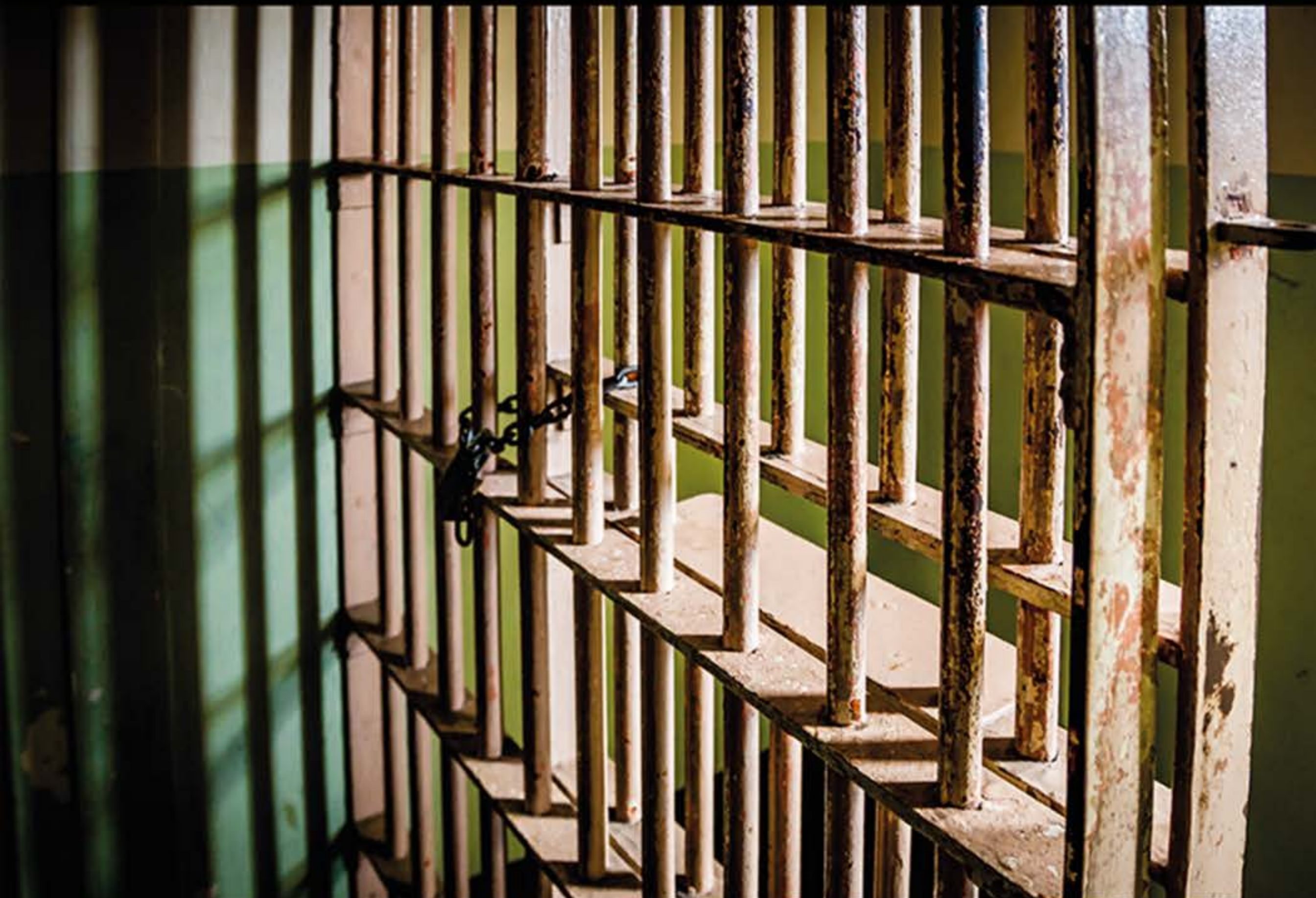
# PUNISHMENT AND CRIME

## THE LIMITS OF PUNITIVE CRIME CONTROL



GARY KLECK
BRION SEVER

# PUNISHMENT AND CRIME

This book summarizes and synthesizes a vast body of research on the effects of legal punishment on criminal behavior. Covering studies conducted between 1967 and 2015, *Punishment and Crime* evaluates the assertion that legal punishment reduces crime by investigating the impacts, both positive and negative, of legal punishment on criminal behavior, with emphasis on the effects of punitive crime control policies via the mechanisms of deterrence and incapacitation.

Brion Sever and Gary Kleck, author of the renowned *Point Blank: Guns and Violence in America*, present a literature review on legal punishment in the United States that is unparalleled in depth and scope. This text is a must-read for students, researchers, and policymakers concerned with the fields of corrections and crime prevention.

**Gary Kleck** is David J. Bordua Professor Emeritus at Florida State University. He earned a Ph.D. and M.A. in Sociology from the University of Illinois and a B.A. in Sociology from the University of Illinois. He is the author of numerous books and articles, including *Point Blank: Guns and Violence in America*, which won the 1991 Michael J. Hindelang Award of the American Society of Criminology.

**Brion Sever** is Associate Professor of Criminal Justice at Florida Gulf Coast University. Previously, he taught for sixteen years at Monmouth University. He earned a Ph.D. in Criminology from Florida State University. He has published research in over a dozen journals, including *Criminology*, *Police Quarterly*, and the *Journal of Criminal Justice Education*.

# PUNISHMENT AND CRIME

## The Limits of Punitive Crime Control

*Gary Kleck and Brion Sever*

Routledge
Taylor & Francis Group
NEW YORK AND LONDON

First published 2018
by Routledge
711 Third Avenue, New York, NY 10017

and by Routledge
2 Park Square, Milton Park, Abingdon, Oxon, OX14 4RN

*Routledge is an imprint of the Taylor & Francis Group, an informa business*



Library of Congress Cataloging-in-Publication Data
Names: Kleck, Gary, 1951- author. | Sever, Brion, author.
Title: Punishment and crime : the limits of punitive crime control /
Gary Kleck and Brion Sever.
Description: 1 Edition. | New York : Routledge, 2018. | Includes index.
Identifiers: LCCN 2017023129| ISBN 9781138307254 (hardback) |
ISBN 9781138307261 (pbk.) | ISBN 9781315142258 (master ebook)
Subjects: LCSH: Punishment in crime deterrence. | Punishment—Philosophy.
Classification: LCC HV8693 .K54 2018 | DDC 364.601—dc23
LC record available at https://lccn.loc.gov/2017023129

ISBN: 978-1-138-30725-4 (hbk)
ISBN: 978-1-138-30726-1 (pbk)
ISBN: 978-1-315-14225-8 (ebk)

Typeset in Bembo
by Apex CoVantage, LLC

**Gary Kleck would like to dedicate this book to his wife Diane, his children Matt and Tessa, and his mentor, David Bordua. Brion Sever would like to dedicate this book to his wife Melissa Sever, his children Brody and Eason Sever, and his parents, Buddy and Joan Sever.**

**Finally, both authors would like to dedicate the book to the late Professor Ray Paternoster, who dominated the study of deterrence for three decades, and whose wisdom will be greatly missed.**

# CONTENTS

# TABLES

# ACKNOWLEDGMENTS

The authors wish to acknowledge the contributions of Marc Gertz, Spenser Li, J. C. Barnes, Kelly Roberts, Dylan Jackson, Raquel Velez Oliveira, and Diane Gomez. We would also like to extend our appreciation to the library staff and instructional technology staff at Monmouth University who aided us in this endeavor over the past two decades, particularly graphic web designer Wayne Elliot, who worked tirelessly to keep the appendix up online through much of the duration of the project. Similarly, our thanks goes to Web designer Joseph Stanis at Florida Gulf Coast University for setting up the online appendix in its current format. Finally, we would like to thank the dozens of graduate assistants who worked on this project at Monmouth University, particularly Ana Camarotti, Matthew Doyle, Joseph Gill, Ryan S. King, Gwen Meltzer, Antonia Tsiandi, and Samantha C. Wilson, who made themselves available during weekends, the Summer, and even holiday breaks, and whose professionalism, passion for this project, and overall efforts to assist went far beyond the call of duty.

We thank the American Society of Criminology for allowing adaption of Kleck, Gary, Brion Sever, Spencer Li, and Marc Gertz. 2005. "The missing link in general deterrence research." *Criminology* 43(3):623–660 in Chapter 9.

# 1

# INTRODUCTION

## The Scope of This Book

This book is intended to summarize and synthesize research on the effects of legal punishment on criminal behavior. We have conducted a systematic review of the English-language research published on this topic between 1967 and June 30, 2015. No one can promise absolute coverage of any research literature, and we have undoubtedly missed some studies. Nevertheless, we have earnestly tried to cover as much of the published literature as possible.

The book is not about the sources of the urge to punish deviance, the causes of variation in punishment, or the ethics, morality, or philosophy of punishment. Readers interested in those topics may consult a wide variety of other sources (e.g., Feinberg and Gross 1975; Garland 2001; Golash 2005; Rothman 1971; van den Haag 1975). Our focus is solely on the utilitarian justification for legal punishment of criminals—that it reduces crime. Thus, the focus is very much on issues of public policy concerning crime control. We do not address the far broader topic of the full set of informal social controls (such as expressions of disapproval, shunning and social isolation of deviants, parental punishment of their children's rule-breaking, and so forth) that may influence crime, except insofar as their effects need to be separated from those of legal punishment. Nor do we discuss how punishment is brought about—that is, how police catch criminals, prosecutors obtain convictions, or prisons incarcerate criminals. Rather, our goal is to bring together between the covers of a single book what science has to tell us about the impact, both beneficial and harmful, of legal punishment on criminal behavior, with particular emphasis on research bearing fairly directly on the effects of punitive crime control policies via the mechanisms of deterrence and incapacitation. Finally, our focus is primarily on legal punishment in the United States and on research undertaken in that nation, based on the assumption that this is the research that is most relevant to public policy in the U.S.

To understand the policy implications of research on the impact of punishment on crime, it is helpful to first put the work in the context of recent American punitive polices and to see the degree to which the U.S. of the past few decades has been exceptional in both historical and cross-national terms.

## The Historical Context: Recent American Trends in Punishment

By the waning years of the twentieth century, American public debate over what to do about crime had, to a great extent, narrowed to a debate over punishment. Discussion of the effectiveness of various crime control policies among those with the power to determine them had become simplified

**TABLE 1.1** The scale of legal punishment in the United States, 2014

| | |
|---|---|
| *Persons Incarcerated* | |
| State Prisoners | 1,263,800 |
| Federal Prisoners | 209,600 |
| Local Jail Inmates | 744,600 |
| Juveniles in Residential Facilities (2013) | 54,148 |
| Territorial Prisons, Military Prisons, Jails in Indian Country | 17,800 |
| Total Incarcerated | 2,289,848 |
| *Adults Under Other Correctional Supervision* | |
| Probation | 3,864,100 |
| Parole | 856,000 |
| Total Under Probation or Parole Supervision[a] | 4,708,100 |
| **Total Under Correctional Supervision** | 7,046,089 |

*Sources*: U.S. Bureau of Justice Statistics (15; 2016); Office of Juvenile Justice and Delinquency Prevention (16).

*Note*: [a] This total is somewhat less than the sum of probation and parole populations since some people can be on both probation and parole.

down to little more than debate over the fine details of how best to deliver punishment to criminals in the way most likely to have the biggest impact. Treatment and rehabilitation were rarely part of the public discourse about crime anymore because many had become convinced that these approaches had been proven ineffective. And in a turnabout from the 1960s, poverty reduction and social reform were rarely linked with crime reduction in public discourse. These alternatives to punitive measures had not in fact been proven ineffective. Rather, they had become politically irrelevant due to the growing dominance of conservatives in the halls of power (Zimring, Hawkins, and Kamin 2001). The result was that by 2014 over 7 million Americans were under correctional supervision of some kind, 2.3 million of whom were locked up in prisons and jails (Table 1.1).

The punitive emphasis manifested itself in innumerable ways. Legislatures passed laws requiring mandatory minimum sentences or add-on penalties for those who committed certain crimes, such as drug selling or violent crimes committed with guns, or that had other "aggravating" characteristics. In response to heinous crimes committed by repeat offenders, legislatures passed "three strikes" laws that imposed especially harsh sentences on those who had previously been convicted of two crimes or just one violent crime (Jacobsen 2005). Encouraged by Federal government incentives, most states enacted "truth-in-sentencing" laws in the 1990s, which required offenders to serve a minimum proportion (often 85 percent) of their sentence before being released. Since inmates previously had served less than half their sentence, this step alone had the potential to nearly double the average length of sentences served (Ditton and Wilson 1999). Or legislators simply increased the maximum terms that judges were allowed to impose. For their part, prosecutors sought, and judges imposed, longer prison sentences, and imposed prison sentences rather than less severe alternatives, such as probation, on a larger share of those convicted (Table 1.2).

Further, many states rolled back the ability of correctional systems to limit prison populations through the release of inmates onto parole. By 2001, at least 16 states had eliminated discretionary release of prisoners by parole boards for all offenders, while others eliminated it for some types of offenders (Hughes, Wilson, and Beck 2001). Admissions to prison due to violations of parole increased even faster during the 1990s than those due to new court commitments, and many of these parolees were returned to prison for "technical" violations, i.e. violations of required parole conditions (such as remaining employed), rather than for committing new crimes (Langan and Levin 2002).

To accommodate the flood of criminals into prisons, legislatures authorized correctional budgets that financed an explosion in prison building unprecedented in the nation's history. Over just 38 years, the nation's prison population grew by 692 percent, from 196,092 in 1972 to 1,552,669 in 2010 (Table 1.3). This occurred at a time when, leaving aside a few short-term upturns, the long-term trend in the rate of serious crime, as well as illicit drug use, was flat or declining (U.S. Bureau of Justice Statistics 2007; U.S. Substance Abuse and Mental Health Services Administration 1999). Crime did not go up; punitiveness did.

**TABLE 1.2** Trends in severity of state sentencing, 1988–2006

| | *Percent of Felons Given Prison or Jail Sentence* | | | | | | *Median Maximum Sentence Imposed (months)* | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *Year* | *Murder* | *Rape* | *Robbery* | *Aggravated Assault* | *Burglary* | *Fraud*[a] | *Murder* | *Rape* | *Robbery* | *Aggravated Assault* | *Burglary* | *Fraud*[a] |
| 1988 | 95 | 87 | 89 | 72 | 75 | – | 240 | 84 | 60 | 24 | 36 | – |
| 1990 | 95 | 86 | 90 | 72 | 75 | 53 | 240 | 72 | 60 | 24 | 36 | 24 |
| 1992 | 97 | 87 | 88 | 72 | 75 | 52 | 252 | 72 | 66 | 24 | 36 | 24 |
| 1994 | 97 | 88 | 88 | 75 | 75 | 59 | 300 | 84 | 72 | 36 | 36 | 23 |
| 1996 | 95 | 81 | 87 | 72 | 71 | 50 | 288 | 72 | 60 | 23 | 24 | 24 |
| 1998 | 96 | 84 | 88 | 72 | 75 | 55 | 288 | 72 | 60 | 24 | 24 | 12 |
| 2000 | 95 | 90 | 89 | 71 | 76 | 54 | 264 | 72 | 60 | 16 | 24 | 12 |
| 2002 | 95 | 89 | 86 | 71 | 72 | 59 | 240 | 60 | 52 | 24 | 24 | 12 |
| 2006[b] | 95 | 86 | 85 | 72 | 73 | 59 | 264 | 96 | 60 | 24 | 24 | 12 |

*Source*: U.S. Bureau of Justice Statistics (2004a and other years).

*Notes*: [a] Fraud, forgery, and embezzlement; [b] The 2004 report did not show figures for specific crime types; 2006 was the last year for which a report was prepared.

**TABLE 1.3** Trends in prison admissions and populations, 1926–2014[a]

| *Year* | *Admissions* | *Rate of Admissions per . . .* | | *Prison Pop* | *Prisoners per 100,000 Pop* |
|---|---|---|---|---|---|
| | | *100 Violent Crimes* | *100,000 Pop* | | |
| 1926 | 48,108 | | 41.0 | 97,991 | 83 |
| 1927 | 51,936 | | 43.6 | 109,983 | 91 |
| 1928 | 55,746 | | 46.3 | 116,390 | 96 |
| 1929 | 58,906 | | 48.4 | 120,496 | 98 |
| 1930 | 66,013 | | 53.6 | 129,453 | 104 |
| 1931 | 71,520 | | 57.7 | 137,082 | 110 |
| 1932 | 67,477 | | 54.1 | 137,997 | 110 |
| 1933 | 62,801 | | 50.0 | 136,810 | 109 |
| 1934 | 62,251 | | 49.3 | 138,316 | 109 |
| 1935 | 65,723 | | 51.6 | 144,180 | 113 |
| 1936 | 60,925 | | 47.6 | 145,038 | 113 |
| 1937 | 63,552 | | 49.3 | 152,741 | 118 |
| 1938 | 68,326 | | 52.6 | 160,285 | 123 |
| 1939 | 66,024 | | 50.4 | 179,818 | 137 |

(*Continued*)

**TABLE 1.3** (Continued)

| *Year* | *Admissions* | *Rate of Admissions per . . .* | | *Prison Pop* | *Prisoners per 100,000 Pop* |
|---|---|---|---|---|---|
| | | *100 Violent Crimes* | *100,000 Pop* | | |
| 1940 | 73,104 | | 55.4 | 173,706 | 131 |
| 1941 | 68,700 | | 51.6 | 165,439 | 124 |
| 1942 | 58,858 | | 44.0 | 150,384 | 112 |
| 1943 | 50,082 | | 37.3 | 137,220 | 103 |
| 1944 | 50,162 | | 37.7 | 132,456 | 100 |
| 1945 | 53,212 | | 40.2 | 133,649 | 98 |
| 1946 | 61,338 | | 43.8 | 140,079 | 99 |
| 1947 | 64,804 | | 45.2 | 151,304 | 105 |
| 1948 | 63,777 | | 43.7 | 155,977 | 106 |
| 1949 | 68,925 | | 46.4 | 163,749 | 109 |
| 1950 | 69,473 | | 45.9 | 166,123 | 109 |
| 1951 | 67,165 | | 43.8 | 165,680 | 107 |
| 1952 | 70,892 | | 45.5 | 168,233 | 107 |
| 1953 | 74,240 | | 46.9 | 173,579 | 108 |
| 1954 | 80,900 | | 50.2 | 182,901 | 112 |
| 1955 | 78,414 | | 47.7 | 185,780 | 112 |
| 1956 | 77,924 | | 46.6 | 189,565 | 112 |
| 1957 | 80,482 | | 47.2 | 195,414 | 113 |
| 1958 | 88,633 | | 51.1 | 205,643 | 117 |
| 1959 | 87,192 | | 49.2 | 208,105 | 117 |
| 1960 | 88,575 | 30.7 | 49.2 | 212,953 | 117 |
| 1961 | 83,513 | 32.3 | 51.1 | 220,149 | 119 |
| 1962 | 89,082 | 29.5 | 48.0 | 218,830 | 117 |
| 1963 | 87,826 | 27.7 | 46.6 | 217,283 | 114 |
| 1964 | 87,578 | 24.0 | 45.8 | 214,336 | 111 |
| 1965 | 87,505 | 22.6 | 45.2 | 210,895 | 108 |
| 1966 | 77,857 | 18.1 | 39.8 | 199,654 | 102 |
| 1967 | 77,850 | 15.6 | 39.4 | 194,896 | 98 |
| 1968 | 72,058 | 12.1 | 36.1 | 187,914 | 94 |
| 1969 | 75,277 | 11.4 | 37.4 | 196,007 | 97 |
| 1970 | 79,351 | 10.7 | 38.9 | 196,429 | 96 |
| 1971 | 89,395[b] | 10.9 | 43.2 | 198,061 | 95 |
| 1972 | 99,440[b] | 11.9 | 47.5 | 196,092 | 93 |
| 1973 | 109,484[b] | 11.2 | 51.8 | 204,211 | 96 |
| 1974 | 119,529[b] | 12.3 | 56.0 | 218,466 | 102 |
| 1975 | 129,573 | 12.6 | 60.1 | 240,593 | 111 |
| 1976 | 129,482 | 12.9 | 59.6 | 262,833 | 120 |
| 1977 | 128,050 | 12.4 | 58.3 | 278,141 | 126 |
| 1978 | 126,121 | 11.6 | 56.8 | 294,398 | 132 |
| 1979 | 131,047 | 10.8 | 58.4 | 301,470 | 133 |
| 1980 | 142,122 | 10.6 | 62.5 | 315,974 | 139 |
| 1981 | 160,272 | 11.8 | 70.3 | 353,673 | 154 |

| Year | Admissions | Rate of Admissions per . . . | | Prison Pop | Prisoners per 100,000 Pop |
|---|---|---|---|---|---|
| | | 100 Violent Crimes | 100,000 Pop | | |
| 1982 | 177,109 | 13.4 | 76.5 | 395,518 | 171 |
| 1983 | 187,408 | 14.9 | 80.2 | 419,346 | 179 |
| 1984 | 180,418 | 14.2 | 76.5 | 443,398 | 188 |
| 1985 | 198,499 | 15.0 | 83.4 | 480,568 | 202 |
| 1986 | 219,382 | 14.7 | 91.4 | 522,084 | 217 |
| 1987 | 241,887 | 16.3 | 99.8 | 580,812 | 231 |
| 1988 | 261,242 | 16.7 | 106.8 | 603,732 | 247 |
| 1989 | 316,215 | 19.2 | 128.1 | 680,907 | 276 |
| 1990 | 323,069 | 17.7 | 129.4 | 739,980 | 297 |
| 1991 | 317,237 | 16.6 | 125.4 | 789,610 | 313 |
| 1992 | 334,301 | 17.3 | 130.3 | 846,277 | 332 |
| 1993 | 341,722 | 17.7 | 131.4 | 932,074 | 359 |
| 1994 | 345,035 | 18.6 | 131.1 | 1,016,691 | 389 |
| 1995 | 361,464 | 20.1 | 135.7 | 1,085,022 | 411 |
| 1996 | 353,893 | 21.0 | 131.4 | 1,137,722 | 427 |
| 1997 | 365,085 | 22.3 | 133.9 | 1,194,581 | 444 |
| 1998 | 381,646 | 24.9 | 138.4 | 1,245,402 | 461 |
| 1999 | 383,103 | 26.9 | 137.3 | 1,304,074 | 463 |
| 2000 | 389,734 | 27.3 | 138.1 | 1,331,278 | 469 |
| 2001 | 405,907 | 28.2 | 142.4 | 1,345,217 | 470 |
| 2002 | 433,959 | 30.5 | 150.7 | 1,380,516 | 476 |
| 2003 | 445,556 | 32.8 | 153.2 | 1,408,361 | 482 |
| 2004 | 457,096 | 33.6 | 155.7 | 1,433,728 | 489 |
| 2005 | 470,159 | 33.8 | 159.0 | 1,462,866 | 495 |
| 2006 | 492,315 | 34.3 | 164.9 | 1,504,598 | 504 |
| 2007 | 479,710 | 33.7 | 159.1 | 1,532,850 | 508 |
| 2008 | 477,100 | 34.2 | 156.7 | 1,547,742 | 508 |
| 2009 | 474,997 | 35.8 | 154.7 | 1,553,574 | 506 |
| 2010 | 458,360 | 36.6 | 148.2 | 1,552,669 | 502 |
| 2011 | 454,526 | 37.7 | 145.9 | 1,538,847 | 494 |
| 2012 | 444,214 | 36.5 | 141.5 | 1,512,430 | 482 |
| 2013 | 454,819 | 38.9 | 143.9 | 1,520,403 | 480 |
| 2014 | 448,993 | 38.5 | 140.8 | 1,508,636 | 473 |

*Sources*: *Prison Admissions*. 1926–1976: U.S. Bureau of Justice Statistics (1986, 36). 1977–1998: U.S. Bureau of Justice Statistics, Spreadsheet, "New Court Commitments . . ." at www.ojusdoj.gov/bjs/prisons.htm. 1999–2004: e-mailed spreadsheet sent by U.S. Bureau of Justice Statistics statistician Paige M. Harrison; *Prison Populations.* 1926–2003. "Sourcebook of Criminal Justice Statistics Online" at www.albany.edu/sourcebook/pdf/t6282005.pdf. 2004–2014: U.S. Bureau of Justice Statistics (2015, 5); *Resident Population*. 1926–1959: U.S. Bureau of the Census (1975). 1960–2014: U.S. Bureau of the Census (2016, 8) *Violent Index Crimes*. 1960–1975: U.S. Federal Bureau of Investigation (1976, 36); 1976–1985: U.S. Federal Bureau of Investigation (1986, 41); 1986–2005: U.S. Federal Bureau of Investigation (2008).

*Notes*: [a] Prison "admissions" refers to the number of sentenced prisoners received from courts by state or federal prisons, and does not include parole violators. Figures for 1976 and later pertain only to new court commitments, while earlier figures pertain to all prisoners received from courts. Admissions figures for 1947–1949 cover only males. Rates per 100 violent crimes refers to FBI violent Index crimes (murder, rape, robbery, aggravated assault) known to the police. These crime data are not comparable across years before 1960. "Prison population" refers to sentenced prisoners under the jurisdiction of state or federal authorities (regardless of where housed) as of December 31 of the indicated year. [b]No admissions figures were gathered for 1971–1974 and are estimated here using linear interpolation between the 1970 and 1975 figures.

The result of this extraordinary expansion in governments' capacity for punishment was that larger and larger shares of the nation's population, especially its poor and minority populations, were locked up. At one point the U.S. Bureau of Justice Statistics (2003) estimated that if 2001 incarceration rates remained unchanged, 6.6 percent of Americans would go to prison at some time during their lives, and that this would be true of an astounding *32.2 percent* of black males. And to the extent that families and communities of the incarcerated were also affected, ripple effects would touch still larger shares of the population.

Some of these punitive developments were not so much a unique phenomenon of the late twentieth century as they were a return to earlier levels of American punitiveness, combined with far greater resources to finance them. For example, scattered national data suggest that prison sentences served for homicide in the 1940s and 1950s were about as severe as those imposed in 1993–2005—averaging about five years (Table 1.4). In this light, the lower punitiveness of the 1960s and early 1970s (Table 1.3) can be seen as a deviant interruption in the American historical norm of severe punishment, while the 1980s, 1990s, and early 2000s (Tables 1.2, 1.3) represent a return to that norm.

On the other hand, use of the death penalty for punishing murder was far more common in the 1930s and 1940s than it was in 1995–2013. We can separate the effect of changes in the willingness to punish murder with a death sentence from the effect of changes in the murder rate by computing executions per 1,000 homicides. In 1934 there were 13.8 executions per 1,000 homicides, a rate that remained roughly as high through the end of the 1940s, but declined sharply through the 1950s and 1960s. In the 1990s there was a brief increase in use of the death penalty, peaking at 5.4 in 1999 but declining thereafter (Table 1.4).

Trends in the willingness of judges to sentence felons to prison can likewise be measured in a way that separates trends in sentencing punitiveness from trends in the rate of crime serious enough to qualify for a prison sentence. Most prison inmates are serving terms for violent crimes (53 percent of inmates of state prisons in 2014 [U.S. Bureau of Justice Statistics 2015, 16]), so it is reasonable to examine the number of persons newly committed by the courts to prison as a rate per 100 violent Index crimes known to the police. As recently as 1980, only 10.6 people per 100 known violent crimes were sent by the courts to prison, but by 2004 this rate had jumped to 33.6 (Table 1.3). Thus, the punitiveness of the courts tripled in just 24 years, independent of trends in crime.

The long-term perspective provided by Table 1.3 suggests that the rise in incarceration rates (prison admissions relative to the crime rate) in the 1980–2004 period also represented a return to a more punitive past, rather than an unprecedented development in American history. The incarceration rate measured in this way was 30.7 in 1960, and, if we project backwards the trends of the early 1960s, the rate was probably still higher in the 1950s. Thus, the willingness of judges to send criminals to prison, relative to the level of serious crime, was about the same in 2014 as it had been in 1960 and earlier. In the decades prior to 1970, legislatures were willing to authorize very severe punishments, and judges were willing to impose long prison sentences and to send a large share of convicted criminals to prison.

The nation's aggregate capacity to punish, however, was far more limited prior to the 1970s because governments did not spend so many billions to build and maintain prisons and jails. Direct expenditures by state governments on corrections increased *34-fold* in a single generation, from \$1.05 billion in fiscal year 1970 to \$36.94 billion in 2003. Inflation accounts for only a small share of this increase, since the Consumer Price Index increased by a factor of just 4.7 over this same period (U.S. National Criminal Justice Information and Statistics Service 1973, 24; U.S. Bureau of Justice Statistics 2003, Table 1.9).

Prior to the 1970s, limits on corrections spending held down prison capacity, which in turn effectively constrained the number of criminals that judges could send to prison, and the number of

**TABLE 1.4** Long-term trends in use of death penalty and prison sentence punishment of homicide, 1934–2013

| *Year* | *Executions* | *Executions, per 1,000 Homicides Previous Year* | *Median Time Served, Homicide First Releases* |
|---|---|---|---|
| 1934 | 168 | 13.8 | |
| 1935 | 199 | 16.5 | |
| 1936 | 195 | 18.4 | |
| 1937 | 147 | 14.4 | |
| 1938 | 190 | 19.4 | |
| 1939 | 160 | 18.2 | |
| 1940 | 124 | 14.8 | |
| 1941 | 123 | 14.8 | |
| 1942 | 147 | 18.3 | |
| 1943 | 131 | 16.6 | |
| 1944 | 120 | 17.6 | |
| 1945 | 117 | 17.5 | |
| 1946 | 131 | 17.4 | 69.4 |
| 1947 | 153 | 17.2 | |
| 1948 | 119 | 13.7 | |
| 1949 | 119 | 13.8 | |
| 1950 | 82 | 10.2 | |
| 1951 | 105 | 13.2 | 52.0 |
| 1952 | 83 | 11.1 | 49.6 |
| 1953 | 62 | 7.7 | 52.4 |
| 1954 | 81 | 10.6 | |
| 1955 | 76 | 9.8 | |
| 1956 | 65 | 8.8 | |
| 1957 | 65 | 8.5 | |
| 1958 | 49 | 6.4 | |
| 1959 | 49 | 6.3 | |
| 1960 | 56 | 6.9 | 51.3 |
| 1961 | 42 | 5.0 | |
| 1962 | 47 | 5.5 | |
| 1963 | 21 | 2.3 | |
| 1964 | 15 | 1.6 | 48.5 |
| 1965 | 7 | 0.7 | |
| 1966 | 1 | 0.1 | |
| 1967 | 2 | 0.2 | |
| 1968 | 0 | 0.0 | |
| 1969 | 0 | 0.0 | |
| 1970 | 0 | 0.0 | |
| 1971 | 0 | 0.0 | |
| 1972 | 0 | 0.0 | |
| 1973 | 0 | 0.0 | |
| 1974 | 0 | 0.0 | |

(*Continued*)

**TABLE 1.4** (Continued)

| *Year* | *Executions* | *Executions, per 1,000 Homicides Previous Year* | *Median Time Served, Homicide First Releases* |
|---|---|---|---|
| 1975 | 0 | 0.0 | |
| 1976 | 0 | 0.0 | |
| 1977 | 1 | 0.1 | |
| 1978 | 0 | 0.0 | |
| 1979 | 2 | 0.1 | |
| 1980 | 0 | 0.0 | |
| 1981 | 1 | 0.0 | |
| 1982 | 2 | 0.1 | |
| 1983 | 5 | 0.2 | |
| 1984 | 21 | 1.0 | |
| 1985 | 18 | 0.9 | |
| 1986 | 18 | 0.9 | |
| 1987 | 25 | 1.2 | |
| 1988 | 11 | 0.5 | |
| 1989 | 16 | 0.7 | |
| 1990 | 23 | 1.0 | |
| 1991 | 14 | 0.6 | |
| 1992 | 31 | 1.2 | |
| 1993 | 38 | 1.5 | 42 |
| 1994 | 31 | 1.2 | 42 |
| 1995 | 56 | 2.2 | 46 |
| 1996 | 45 | 2.0 | 48 |
| 1997 | 74 | 3.5 | 52 |
| 1998 | 68 | 3.4 | 58 |
| 1999 | 98 | 5.4 | 62 |
| 2000 | 85 | 5.0 | 69 |
| 2001 | 66 | 3.3 | 77 |
| 2002 | 71 | 3.5 | 82 |
| 2003 | 65 | 3.7 | 83 |
| 2004 | 59 | 3.3 | 87 |
| 2005 | 60 | 3.6 | 87 |
| 2006 | 53 | 2.9 | |
| 2007 | 42 | 2.3 | |
| 2008 | 37 | 2.1 | |
| 2009 | 52 | 3.1 | |
| 2010 | 46 | 2.7 | |
| 2011 | 43 | 2.6 | |
| 2012 | 43 | 2.6 | |
| 2013 | 39 | 2.3 | |
| 2014 | 35 | 2.2 | |

*Sources*: *Executions.* U.S. Bureau of Justice Statistics (1986, 2013). *Homicides.* 1933–1995: U.S. National Center for Health Statistics (various years 1933–1995); 1996–2014: U.S. National Center for Health Statistics (various years 1996–2014); Median Time Served, homicide. 1946–1964: U.S. Bureau of the Census (1948); U.S. Federal Bureau of Prisons (1954, 1955, 1957a, 1957b, 1963a, 1963b, 1967); U.S. Law Enforcement Assistance Administration (1972); 1993–2005: National Corrections Reporting Program, time served spreadsheets on the U.S. Bureau of Justice Statistics website, http://bjs.ojusdoj.gov/index.cfm?ty=pbdetail&iid=2045.

long sentences that they could impose. As the prison system's capacity and thus its ability to absorb new admissions expanded, the courts could afford to both sentence a larger share of convicted criminals to terms of incarceration and to make those prison sentences longer. Thus, what really distinguished the last third of the twentieth century from earlier eras was that the U.S. began to use its considerable national wealth to finance a huge increase in its capacity to punish, and especially its ability to incarcerate huge numbers of criminals.

The size of the imprisoned population measured relative to the size of the general population had been remarkably stable from 1926 to 1974, fluctuating around 110 prisoners per 100,000 population, plus or minus 20. But then it quintupled in just 33 years, from 102 in 1974 to 508 in 2007, while the raw number of prison inmates increased seven-fold (Table 1.3; see Blumstein and Beck 1999 for a detailed discussion). This expansion was funded by correspondingly enormous increases in corrections expenditures—primarily state government spending on prison operating costs and, to a lesser extent, state prison construction costs and federal corrections spending (U.S. Bureau of Justice Statistics 2004a). Thus, an unprecedented amount of serious punishment, in the form of incarceration, was being inflicted by the beginning of the twenty-first century. Despite the absence of any net increase in crime rates over the 1974–2014 period, the total amount of punishment inflicted by the American criminal justice system increased radically, partly as a result of a drastic increase in the willingness of judges and legislators to inflict severe punishment in the form of prison sentences (a return to the historic norm) and partly as a result of an even more drastic and unprecedented rise in the correctional system's capacity to carry out those sentences.

Further, imprisonment is only the most serious tip of the iceberg of state control of criminals. Many offenders are incarcerated in penal institutions other than state or federal prisons, such as local jails, juvenile facilities, and forensic facilities for mentally ill offenders (Table 1.1). And an even larger number of criminals are not incarcerated but are nevertheless "under correctional supervision" because they were sentenced to a term of probation instead of, or in combination with, a jail or prison term, or are serving a term of parole following a period of incarceration. If we include all these correctional populations, the number of those under state control for criminal acts is far larger than prison population figures imply. By the end of 2014, while 2.3 million Americans were incarcerated in state or federal prisons, territorial prisons, local jails, juvenile facilities and various other penal institutions (U.S. Bureau of Justice Statistics 2006a, 1), another 4.7 million were under probation or parole supervision, for a total of 7 million under criminal justice system control. The trends in these other correctional populations closely paralleled trends in prison populations (U.S. Bureau of Justice Statistics 2006b, 1–2).

The punitiveness of American courts, as reflected in sentencing patterns, showed signs of leveling off and even declining as early as the mid-1990s for the most serious crimes (Table 1.2). One might speculate that this was due to declines in public concern over crime and demands for its punishment, but the evidence does not support this proposition, since fear of crime and the belief that courts should be harsher remained high by 1994 (Table 1.5). Instead, declines in punitiveness were more likely the product of elites recognizing the fiscal limits implied by the need for government expenditures in other areas besides corrections against a background of public resistance to tax increases. Nevertheless, despite some declines in judges' imposition of prison sentences, prison populations continued to rise because the long sentences imposed in preceding years insured that the number of inmates released after finishing their sentences would continue to be smaller than the number of offenders being newly admitted to prison.

Since crime rates were about the same in 2013 as they were in 1973, the prison population increases in the intervening decades were not directly due to contemporaneous crime increases as much as they were due to the growing political power of those who favored punitive crime control strategies. The "conservative revolution" of this period, which put Republicans and conservative

**TABLE 1.5** Trends in punishment-related public attitudes, 1936–2014

| *Year* | *% Support Death Penalty, Gallup* | *% Support Death Penalty, NORC* | *% Believe Courts Not Harsh Enough* | *% Oppose Legalization of Marijuana* | *% Afraid to Walk at Night* | *Murder Rate*[b] |
|---|---|---|---|---|---|---|
| 1936 | 61.5 (2) | | | | | 8.0 |
| 1937 | 65 | | | | | 7.6 |
| 1953 | 68 | | | | | 4.8 |
| 1956 | 53 | | | | | 4.6 |
| 1957 | 47 | | | | | 4.5 (Low) |
| 1960 | 53 | | | | | 5.1 |
| 1965 | 45 | | 54 (2) | | 34 | 5.1 |
| 1966 | 42 (*Low*) | | | | | 5.6 |
| 1967 | 54 | | | | 31 | 6.2 |
| 1968 | | | 63 | | 35 | 6.9 |
| 1969 | 51 | | 75 | 84 (High) | | 7.3 |
| 1971 | 49 | | | 84 | | 7.9 |
| 1972 | 53.5 | 53 (*Low*) | 70 (2) | 81 | 41.5 (2) | 8.6 |
| 1973 | | 60 | 73 | 79 (2) | 41 | 9.0 |
| 1974 | | 63 | 78 | | 45 | 9.4 |
| 1975 | | 60 | 79 | 75 | 45 | 9.8 |
| 1976 | 66 | 66 | 81 | 69 | 44 | 9.6 |
| 1977 | | 67 | 83 | 66 | 45 | 8.8 |
| 1978 | 62 | 66 | 85 | | | 9.0 |
| 1979 | | | | 70 | 42 | 9.7 |
| 1980 | | 67 | 83 | 71 | 43 | 10.2 (High) |
| 1981 | 66 | | | | 45 | 9.8 |
| 1982 | | 74 | 86 | 74 | 48.5 (2) | 9.1 |
| 1983 | | 73 | 86 | 77 | 45 | 8.3 |
| 1984 | | 70 | 82 | 73 | 42 | 7.9 |
| 1985 | 73.5 (2) | 76 | 84 | 73 | 40 | 8.0 |
| 1986 | 70 | 71 | 83 | 80 | | 8.6 |
| 1987 | | 70 | 79 | 79 (2) | 38 | 8.3 |
| 1988 | 79 | 71 | 82 | 79.5 (2) | 40 | 8.5 |
| 1989 | | 74 | 84 | 81 | 41.5 (2) | 8.7 |
| 1990 | | 75 (*High*) | 83 | 81 | 40.5 (2) | 9.4 |
| 1991 | 76 | 72 | 80 | 78 | 43 | 9.8 |
| 1992 | | | 83.5 (2) | | 44 | 9.3 |
| 1993 | | 72 | 86 (High) | 73 | 43 (2) | 9.5 |
| 1994 | 80 (*High*) | 74 | 85 | 73 | 43 (2) | 9.0 |
| 1995 | 75.5 (2) | | | 73 | | 8.2 |
| 1996 | | 71 | 78 | | 40.5 | 7.4 |
| 1997 | | | | | 38 | 6.8 |
| 1998 | | 68 | 74 | 69 | 41 | 6.3 |
| 1999 | 71 | | | | | 5.7 |
| 2000 | 66.5 (2) | 63 | 68 | 64 | 36.5 | 5.5 |

| Year | % Support Death Penalty, Gallup | % Support Death Penalty, NORC | % Believe Courts Not Harsh Enough | % Oppose Legalization of Marijuana | % Afraid to Walk at Night | Murder Rate[b] |
|---|---|---|---|---|---|---|
| 2001 | 66.7 (3)[a] | | | 62 | 30 (*Low*) | 5.6 |
| 2002 | 71 (2) | 66 | 67 | 60 | 33.5 (2) | 5.6 |
| 2003 | 72 (2) | | | 64 | 36 | 5.7 |
| 2004 | 71 | 65 | 65 | 59 | 32 | 5.5 |
| 2005 | 64 (2) | | | 60 | 38 | 5.6 |
| 2006 | 65 | 65 | 65 | 60 | 36 (2) | 5.7 |
| 2007 | 67 (2) | | | | 37 | 5.6 |
| 2008 | 64 | | 63 | 57 | 33 | 5.4 |
| 2009 | 65 | 64 | | 54 | | 5.0 |
| 2010 | 64 | 65 | 62 | 47 | 33 | 4.8 |
| 2011 | 61 | | | | | 4.7 |
| 2012 | 63 | 61 | 57 | 49 | | 4.7 |
| 2013 | 60 | | | | | 4.5 |
| 2014 | 63 | 62 | 58 | 42 (*Low*) | 31 | 4.5 |

*Question Wordings*: Death penalty (Gallup 1953–2009)—"Are you in favor of the death penalty for a person convicted of murder?" Death penalty (Gallup 1936)—"Do you believe in the death penalty for murder?" Death penalty (Gallup 1937)—"Do you favor or oppose capital punishment for murder?" Death Penalty (NORC)—"Do you favor or oppose the death penalty for persons convicted of murder?" Courts not harsh enough—"In general, do you think the courts in this area deal too harshly or not harshly enough with criminals?" Legalization of marijuana—"Do you think the use of marijuana should be made legal or not?' Afraid to walk—"Is there any area around here—that is, within a mile—where you would be afraid to walk alone at night?"

*Sources*: U.S. Bureau of Justice Statistics (2005) for 1953–1991 Gallup death penalty results, 1965–2005 "afraid to walk" results; Gallup Organization, Inc. (2010) for 1936–1937 Gallup death penalty results; Roper iPoll (2015) for all others.

*Notes*: [a] Numbers in parentheses after a result indicate that the figure is an average of results from multiple surveys, and denote the number of surveys used; [b] Murder rates prior to 1960 are based on vital statistics death certificate data, and those from 1960 on are based on the FBI's Uniform Crime Reports.

Democrats in control of most state legislatures and governorships, was a more potent cause of the incarceration boom than actual crime increases (Smith 2004). It might also be suspected that Americans became more punitive towards criminals during this period, so we examine national poll data on trends in punitive attitudes to judge whether popular "outrage" over crime moved in directions that would correspond to trends in the punitiveness of the CJS.

## Trends in Public Opinion on Crime and Its Punishment

Many Americans apparently believe that crime is always increasing, regardless of actual trends. For the period for which we have the requisite survey data, 1989–2014, there has never been a time when most Americans believed that crime was going down, even though it was declining for most of this period. Quite the contrary—in every year except 2000 and 2001, a majority believed crime was *increasing*. For example, 89 percent of Americans questioned in 1992 said that they believed crime had increased from the previous year, when the rates of both FBI Index crimes and murder had declined during that period. In 1996, 71 percent of Americans believed that crime in the U.S. had increased in the previous year, even though the murder rate had dropped by a record-breaking 9.8 percent (U.S. Bureau of Justice Statistics 2005; U.S. Federal Bureau of Investigation 2001). That is, most Americans had perceptions of crime trends that were the exact reverse of reality.

The news media surely share some of the blame for this state of affairs. While the nation's crime rates declined virtually every year from 1991 to 1999, the number of crime stories broadcast by the three largest broadcast television networks actually *increased* every year during that period (Dorfman and Schiraldi 2001, 9). Those whose perceptions of crime trends were influenced by the emphases of the network news programs received exactly the wrong impression, an inversion of reality. In combination with poll data on the perception of crime trends, it is reasonable to conclude that increasing news coverage contributed to Americans' misperception of crime as an ever-growing plague. We can be certain that increases in criminal victimization did not cause the growth in anxiety in this period, since there were no increases.

In the face of perceptions of ever-increasing threats from crime, it is no surprise that fear of crime remained high despite strong declines in the actual seriousness of the threat. The poll data in Table 1.5 indicate this is precisely what happened in the 1990s. Across five national surveys conducted in 1989–1991, the share of Americans who said they were afraid to walk in their neighborhood alone at night averaged about 41.5 percent. Despite a 33 percent decline in the murder rate and 36 percent drop in the robbery rate from 1990 to 1998 (U.S. FBI 1999), the share who were afraid in 1998 remained at 41 percent. That is, fear of crime remained just as high despite dramatic declines in the actual risk. In this era, it clearly was not increases in crime that kept fear high or caused people to believe that crime was increasing. It is more likely that news media coverage of crime artificially maintained high levels of public anxiety and a sense that the nation was facing a crisis of ever-increasing crime.

The figures in Table 1.5 also indicate, at least as far back as national poll data can reveal, that Americans have always had fairly punitive attitudes towards criminals. A majority of Americans have supported the death penalty for murder for virtually the entire period from 1936, when the Gallup Poll first asked about the issue, through 2014, excepting a few years within the period from 1957 to 1971. The historical average is that about two-thirds of Americans favor capital punishment for at least some murders. Likewise, since pollsters began asking about the topic in 1968, about three-fourths of Americans have stated that they felt the courts in their area were not harsh enough towards criminals. This share has never dropped below 57 percent. Increases in the actual harshness of courts did not induce Americans to relinquish this belief. Although criminal sentencing got harsher from 1988 to 1994 (Table 1.2), overwhelming majorities of Americans continued to state their belief that courts were still not harsh enough, and the share who felt this way actually *increased* slightly during this period, from 82 percent in 1988 to 85 percent in 1994 (Table 1.5). Conversely, when courts finally did get somewhat less harsh from 1994 to 2002 (Table 1.2, right column)—a development that should have *increased* the share of Americans who felt that courts were not harsh enough—public opinion perversely moved in the opposite direction, with the share thinking that courts were not harsh enough dropping from 85 percent in 1994 to 67 percent in 2002 (Table 1.5). Once again, the realities of crime and punishment do not appear to be the dominant forces driving public perceptions. Given the documented increases in media coverage of crime, it is reasonable to hypothesize that media stories about unusually lenient judges, or criminals who "got away with murder" or got a "slap on the wrist" for a serious crime, were driving public perceptions about punishment levels, rather than actual punishment levels (Roberts 1992).

In sum, public perception of rising crime threats, continuing high levels of fear, and perceptions of punishment as being inadequate appears to be a product of media coverage of crime, rather than of trends in actual levels of crime and punishment. Confirming this hypothesis, Kleck and Jackson (2016) found that public support for harsher punishment of criminals was unrelated to crime rates in their area, their own personal victimization experiences, victimization of others known to them, or their perceived chances of future victimization. Instead, punitive attitudes were associated with being white, the conservative character of their area of residence (as measured by percent Republican), and how many hours of local TV news they watched in a typical week.

American public opinion towards crime and punishment is nevertheless quite changeable, as shown in the poll data in Table 1.5. While these views show little correspondence with reality, they clearly do change over time, whether the changes are due to shifts in media coverage of crime and punishment or to other factors. For example, the share of Americans who believed courts were not harsh enough declined from 86 percent in 1993 to 65 percent in 2004, while the percent supporting the death penalty for murder dropped from 80 percent in 1994 to 64 percent in 2005. The short-term volatility of these attitudes suggests that they may not be as inflexible, strongly held, or salient as one might assume. The share of Americans supporting harsh treatment of criminals was still high in 2014, but was far lower than it had been in the early 1990s. From a longer-term perspective, these drops can be seen as a return to the historical norms that have prevailed throughout the era when public attitudes could be systematically measured with surveys, a "correction back to the mean" following the extraordinarily punitive attitudes prevailing in the 1980s and early 1990s. In a longer-term historical context, punitive opinion was anomalously high in those years, just as it had been anomalously low in 1956–1972.

Another indication of shifts in attitudes towards criminals may be inferred from opinion on the legalization of marijuana, which could be interpreted as one barometer of intolerance for deviant behavior and of the willingness to impose criminal penalties for disapproved behaviors. National surveys have asked about this issue dozens of times since 1969, so we have data for many years. Opposition to legalizing marijuana was as high as 84 percent in 1969 but showed a marked decline after 1990, dropping from 81 percent to 42 percent in 2014. This may indicate declining willingness to use criminal law to control behavior or increased tolerance for (minor) rule-breaking behavior. This trend roughly parallels the decline in punitive attitudes over the same period.

As a whole, the poll data suggest that there may be more of an opening for policy makers to base policy choices on rational considerations of crime control efficacy rather than merely responding to supposedly intense and inflexible public demands for more punishment. Perhaps, for once, reality did influence public opinion—the reality of declining crime in the 1990s may have finally pushed the level of punitive public opinion downward, albeit only after a considerable lag.

## American Punishment Levels Compared to Other Nations

Neither the level nor recent trends in punitiveness in the United States find parallels in the recent history of other Western democracies. The U.S. is far more punitive towards criminals than other developed countries and increased its level of punitiveness at a time when most other Western nations became less punitive. The most detailed comparison of the U.S. with other developed nations was conducted in 2004 by the U.S. Bureau of Justice Statistics and counterpart government agencies in other nations. Key results concerning five selected serious offenses are shown in Table 1.6. For every crime type, the average length of sentence imposed was longer in the U.S. than in any of the other seven nations for which data were available. Sentences actually served were likewise longer in the U.S. than in all other nations for every offense type, with the exception of homicide and assault in Australia.

There is less difference between the U.S. and other nations with respect to the percent of convicted persons given a prison or jail sentence. Since all five of the crime types considered are serious offenses, most nations consider most offenders convicted of these crimes deserving of an incarceration sentence. Nevertheless, even on this dimension, the U.S. ranked high internationally—first among these nations for assault, second for robbery, third for burglary, and fourth for homicide and rape. Not surprisingly, the per capita rate of imprisonment is far higher in the U.S. than in any other nation

**TABLE 1.6** Punitiveness of sentencing in the U.S. compared with other developed nations, c. 1996[a]

*Percent of Convicted Offenders Given Incarceration (Custodial) Sentences*

| *Nation* | *Homicide* | *Rape* | *Robbery* | *Assault* | *Burglary* |
|---|---|---|---|---|---|
| United States | 94.5 | 75.6 | 75.0 | 59.3 | 57.7 |
| England and Wales (1997) | 97.1 | 97.6 | 71.7 | 27.8 | 56.0 |
| Australia (1995) | 96.0 | 41.0 | 53.0 | 6.0 | 19.0 |
| Canada | – | – | 80.3 | 49.2 | 61.8 |
| Netherlands | 89.0 | 73.0 | 64.0 | 13.5 | 66.0 |
| Scotland (1995) | 92.1 | 85.7 | 60.0 | 14.0 | 44.0 |
| Sweden | 98.5 | 92.0 | 66.0 | 29.9 | 50.9 |
| Switzerland (1997) | 79.8 | 55.1 | 20.9 | 16.6 | 38.1 |

*Average Sentence Imposed/Time Served 1996 (in months)*

| *Nation* | *Homicide* | | *Rape* | | *Robbery* | | *Assault* | | *Burglary* | |
|---|---|---|---|---|---|---|---|---|---|---|
| | *Imposed* | *Served* | *Imposed* | *Served* | *Imposed* | *Served* | *Imposed* | *Served* | *Imposed* | *Served* |
| United States | 250.0 | 126.2 | 115.5 | 67.6 | 76.4 | 37.4 | 40.4 | 21.6 | 43.4 | 18.6 |
| England and Wales (1997) | 223.4 | 103.5 | 80.7 | 46.2 | 38.8 | 19.3 | 14.0 | 6.5 | 17.4 | 7.7 |
| Australia (1995) | 171.5 | 129.4 | 78.2 | 57.3 | 73.7 | 32.8 | 34.5 | 27.0 | 31.0 | 16.3 |
| Canada | 109.8 | – | 58.5 | – | 25.2 | – | 5.1 | – | 8.0 | – |
| Netherlands | 107.0 | 71.3 | 25.8 | 18.6 | 16.5 | 14.0 | 5.0 | 5.0 | 12.7 | 11.4 |
| Scotland (1995) | 181.5 | 103.5 | 62.0 | 34.7 | 35.5 | 18.8 | 11.9 | 6.1 | 6.2 | 3.1 |
| Sweden | 94.8 | 47.4 | 36.9 | 18.8 | 28.8 | 14.9 | 5.4 | 3.4 | 9.7 | 6.0 |
| Switzerland (1997) | 93.7 | 65.5 | 47.2 | 24.8 | 30.0 | 22.2 | 15.6 | 12.9 | 19.4 | 13.1 |

*Source*: U.S. Bureau of Justice Statistics (2004b). Compiled from appendix tables for each nation.

*Note*: Data pertain to 1996 unless otherwise indicated.

(Sparks 2003, 29), though this is partly attributable to America's higher rates of violent crime. Nevertheless, the U.S. is clearly far more punitive towards criminals than other Western nations.

Although the U.S. is not totally unique in becoming more punitive in the past several decades, its trends are not typical of the more developed nations. Data reported by Sparks (2003, 29) showed that prison populations per capita declined in most European nations between 1985 and 1995, and among those with an increase, the rise was generally far smaller than in the U.S. The general trend in other Western democracies appears to be a decline in use of the most serious punishments.

The scale of America's commitment to punishment as its primary response to crime is extraordinary by any standard, unprecedented from an historical standpoint, and nearly unique among economically advanced nations. The dominant utilitarian justification for this expansion of punishment capability is that it suppresses crime. Thus, we come to the questions that this book addresses: To what degree does punishment reduce crime? What is the empirical foundation for the belief that punishment of criminals reduces crime rates? Is the evidence strong enough to justify the extraordinary magnitude of America's commitment to punitive crime control? What harmful effects result from punishment?

The prevention or reduction of crime is obviously not the only justification for punishment and may not even be the most important one. Many people argue for punishment of criminals largely on the basis of justice or retribution and place little emphasis on crime control as a rationale. It is, however, equally obvious that crime control is *one* important justification. Indeed, in the historical long-term, if cultural heterogeneity increases and moral consensus decreases, we are likely to see even more emphasis on utilitarian rationales for public policies in general and declining stress on purely moral justifications. Kahan (1999, 498–499) argued that justifying crime control policies on the basis of their anticipated practical consequences, such as deterring crime, provides a socially beneficial way to debate such contentious policies: "By muting expressive controversy, deterrence arguments make it easier for citizens of diverse moral and cultural commitments to agree on policy outcomes . . . Deterrence theory secures the goals of liberal public reason, which enjoins us to disclaim privileged moral insight when we engage in public deliberations."

To be sure, some of those who express utilitarian justifications for punitive policies may be merely rationalizing support that is actually based largely in a thirst for vengeance or a belief in the moral rightness of retribution and punishing the wicked. Nevertheless, even such individuals are unlikely to be totally indifferent to the actual crime-control effects of the policies they support, notwithstanding their more emotionally powerful motives for favoring punitive policies.

We did not try to take on the herculean task of reviewing all research that has evaluated the impact of specific legal changes or crime control programs that might have had some of their effects via their punitive components. While our findings certainly are relevant to, and help to place in context, the impacts of specific punishment-based interventions, studies of such specific efforts are beyond the scope of this book. While many crime-control interventions involve a punitive component, evaluations typically make only a global assessment of the total impact of the policies without regard to the separate contributions attributable to deterrence or incapacitation and without distinguishing the effects of punitive elements from nonpunitive elements. Examples would include studies of individual episodes of police "crackdowns" on crime (e.g., Sherman 1990), assessments of the impact of specific new laws imposing harsher penalties (e.g., Kovandzic, Sloan, and Vieraitis 2002, 2004), or campaigns against drunk driving (e.g., Ross 1982).

We are instead concerned with the effects of formal legal punishments and the threat of such punishments on the criminal behavior of those punished and of those who might be punished. We explore what research has to say on this question, with primary emphasis on the deterrent and incapacitative effects of arrest, conviction, and incarceration or the threat thereof, as well as the effects of the death penalty on murder. Other effects of punishment might well be important yet difficult to detect, such as its effects on social solidarity or the collective level of commitment to the moral norms whose violation was punished. Because there is so little empirical evidence bearing on these effects, we have little to say about them. We likewise do not address the effects of treatment or rehabilitation efforts that may accompany punishment, except insofar as these efforts are discussed in the concluding chapter as alternatives or complements to punitive strategies. Research on these effects has already been extensively reviewed elsewhere (Lipsey 1992, 1995; Pearson and Lipton 1999; Pearson, Lipton, Cleland, and Yee 2002).

These are the primary questions that we do address:

1. Does the threat of legal punishment deter crime?
2. What qualities of punishment are most important in generating its effects—certainty, severity, or swiftness?
3. What types of crimes are most or least deterrable by punishment?
4. What kinds of people are most deterrable; that is, what characteristics of people make them more aware of punishment risks or more responsive to those perceptions?
5. Does the experience of being punished cause the punished person's criminal behavior to increase, decrease, or remain the same?

6. Does capital punishment have any effect on murder rates beyond the effects produced by long prison sentences?
7. Does increasing the number of imprisoned criminals reduce crime rates to a degree that justifies the costs of imprisonment? Is there a point where further additions to the prison population are not cost effective and may even increase crime?
8. What crime-*increasing* effects might punishment have?

We intend our assessment to be balanced, so we consider both intended and unintended consequences of punishment for crime, both desirable and undesirable effects. Thus, we review the evidence on the stigmatizing and social isolating effects of arrest and conviction, the "prisonizing" effects of incarceration on inmates, and the impact of the incarceration of criminals on the inmates themselves, their families and their communities, to the extent that these bear on the effects of punishment on crime.

## A Word About Meta-Analysis

The popularity of multivariate meta-analysis as a technique for summarizing large bodies of research findings necessitates an explanation of why this technique was not used in this book. The vast majority of the significant studies of the impact of legal punishments on criminal behavior use some variant of multiple regression to analyze data generated by nonexperimental procedures and to produce estimates of effects. The nation's leading authority on the application of meta-analysis to topics related to criminal justice is Professor Mark Lipsey, senior author of the textbook *Practical Meta-Analysis* (Lipsey and Wilson 2001). He and his co-author flatly state that meta-analysis cannot be applied to research findings from regression-based studies because "multiple regression results cannot generally be represented in an effect size statistic," explaining that "meta-analysts have not yet developed effect size statistics that adequately represent this form of research finding, and, indeed, their complexity and the diversity across studies with regard to the selection of variables involved may make this impossible" (Lipsey and Wilson 2001, 15–16, emphasis added; see also Cooper 2010, 192–193 for the same point, in another standard meta-analysis textbook). One can meta-analyze crude bivariate measures of association, such as correlation coefficients, for the unrepresentative minority of studies reporting such statistics, but since these are largely meaningless as estimates of causal effect, and frequently misleading, such an analysis would be pointless. Because meaningful and comparable measures of the effects of punishment on crime cannot be computed for multiple regression findings, complex multivariate analyses of effect sizes are impossible with this body of research. While meta-analysis is a useful tool for synthesizing the results of experimental studies, it is not suitable for synthesizing the findings of nonexperimental multivariate studies of the effects of legal punishment. Instead, the best that can be done to analyze the latter is to conduct a systematic review in which we cross-tabulate findings with respect to methodological or substantive factors that may condition the estimated effects of punishment on crime. This is what we have done.

## References

Blumstein, Alfred, and Allen J. Beck. 1999. Population growth in U.S. prisons, 1980-1996. In *Crime and Justice*, *Volume 26*, eds. Michael Tonry and Joan Petersilia. Chicago: University of Chicago Press.

Cooper, Harris. 2010. *Research Synthesis and Meta-Analysis*. Los Angeles: Sage.

Ditton, Paula, and Doris Wilson. 1999. *Truth in Sentencing in State Prisons: BJS Special Report*. Washington, DC: U.S. Government Printing Office.

Dorfman, Lori, and Vincent Schiraldi. 2001. *Off Balance: Youth, Race & Crime in the News*. Washington, DC: Building Blocks for Youth.

Feinberg, Joel, and Hyman Gross. 1975. *Punishment*. Encino, CA: Dickenson.

Gallup Organization, Inc. 2010. *The Gallup Poll*. Available online at www.galluppoll.com.

Garland, David. 2001. *Culture of Control*. Chicago: University of Chicago Press.

Golash, Deirdre. 2005. *The Case against Punishment: Retribution, Crime Prevention, and the Law*. New York: New York University Press.

Hughes, Timothy, Doris Wilson, and Allen Beck. 2001. *Trends in State Parole 1990–2000, BJS Special Report*. Washington, DC: Bureau of Justice Statistics.

Kahan, Dan. 1999. The secret ambition of deterrence. *Harvard Law Review* 113:413–500.

Kleck, Gary, and Dylan Jackson. 2016. Does crime cause punitiveness? *Crime & Delinquency*.

Kovandzic, Tomislav, John Sloan, and Lynne Vieraitis. 2002. Unintended consequences of politically popular sentencing policy. *Criminology & Public Policy* 1:399–424.

Kovandzic, Tomislav, Joan Sloan, and Lynne Vieraitis. 2004. "Striking out" as a crime reduction policy: The impact of "three strikes" laws on crime rates in U.S. cities. *Justice Quarterly* 21:207–239.

Langan, Patrick, and David Levin. 2002. *Recidivism of Prisoners Released in 1994: BJS Special Report*. Washington, DC: U.S. Government Printing Office.

Lipsey, Mark. 1992. Juvenile delinquency treatment: A meta-analytic inquiry into the variability of effects. In *Meta-Analysis for Explanation*, eds. Thomas Cook, Harris Cooper, David Cordray, Heidi Hartmann, Larry Hedges, Richard Light, Thomas Louis, and Frederick Mosteller. New York: Russell Sage Foundation.

Lipsey, Mark. 1995. What do we learn from 400 research studies on the effectiveness of treatment with juvenile delinquents? In *What Works: Reducing Reoffending*, ed. James McGuire. New York: John Wiley.

Lipsey, Mark, and David Wilson. 2001. *Practical Meta-Analysis*. Thousand Oaks, CA: Sage.

Lipton, Douglas, Frank Pearson, Charles Cleland, and Dorline Yee. 2002. The effects of therapeutic communities and milieu therapy on recidivism. In *Offender Rehabilitation and Treatment: Effective Programs and Policies to Reduce Re-Offending*, ed. James McGuire. Chichester: John Wiley & Sons.

Pearson, Frank, and Douglas Lipton. 1999. A meta-analytic review of the effectiveness of corrections-based treatments for drug abuse. *The Prison Journal* 79:384–410.

Pearson, Frank, Douglas Lipton, Charles Cleland, and Dorline Yee. 2002. The effects of behavioral/cognitive-behavioral programs on recidivism. *Crime & Delinquency* 48:476–496.

Roberts, Julian. 1992. Public opinion, crime, and criminal justice. *Crime and Justice* 16:99–180.

Roper iPoll. 2015. Public Opinion Research Archive. Cornell University: Roper Center of Public Opinion Research. Available online at https://ropercenter.cornell.edu/.

Ross, Laurence. 1982. *Deterring the Drinking Driver: Legal Policy and Social Control*. Lexington, MA: Lexington Books.

Rothman, David. 1971. *The Discovery of the Asylum*. Boston: Little, Brown.

Sherman, Lawrence W. 1990. Police crackdowns: Initial and residual deterrence. *Crime and Justice* 12:1–48.

Smith, Kevin. 2004. The politics of imprisonment. *Journal of Politics* 66:925–938.

Sparks, Richard. 2003. State punishment in advanced capitalist countries. In *Punishment and Social Control, Enlarged Second Edition*, eds. Thomas G. Blomberg and Stanley Cohen, 19–44. New York: Aldine de Gruyter.

U.S. Bureau of the Census. 1948. *Prisoners: 1946*. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of the Census. 1975. *Historical Statistics of the United States, Colonial Times to 1970*. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of the Census. 2016. *Statistical Abstract of the United States 2016*. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 1986. *United States Historical Corrections Statistics: 1850–1984*. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2003. *Prevalence of Imprisonment in the U.S. Population 1974–2001*. Bureau of Justice Statistics Special Report. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2004a. *Felony Sentences in State Courts 2002*. BJS Bulletin. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2004b. *Cross-National Studies in Crime and Justice*. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2005. *Sourcebook of Criminal Justice Statistics 2003*. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2006a. *Prisoners in 2005*. BJS Bulletin. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2006b. *Probation and Parole in the United States 2005*. BJS Bulletin. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2007. *Prison and Jail Inmates at Midyear 2006*. BJS Bulletin. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2013. *Prisoners Executed Under Civil Authority in the United States, by Year, Region, and Jurisdiction 1977–2013*. Available online at www.bjs.gov/index.cfm?ty=pbdetail&iid=2079.

U.S. Bureau of Justice Statistics. 2015. *Prisoners in 2014*. BJS Bulletin. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2016. *Correctional Populations in the United States 2015*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 1976. *Crime in the United States—1975*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 1986. *Crime in the United States—1985*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 1999. *Crime in the United States—1998*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 2001. *Crime in the United States—2000*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 2008. *Crime in the United States—2007*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Prisons. 1954. *Prisoners in State and Federal Institutions 1950*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Prisons. 1955. *Prisoners in State and Federal Institutions 1951*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Prisons. 1957a. *Prisoners Released from State and Federal Institutions 1950*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Prisons. 1957b. *Prisoners Released from State and Federal Institutions 1952 and 1953*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Prisons. 1963a. *Characteristics of State Prisoners 1960*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Prisons. 1963b. *Prisoners Released from State and Federal Institutions 1960*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Prisons. 1967. *State Prisoners: Admissions and Releases 1964*. Washington, DC: U.S. Government Printing Office.

U.S. Law Enforcement Assistance Administration. 1972. *State Prisoners: Admissions and Releases 1970*. Washington, DC: U.S. Government Printing Office.

U.S. National Center for Health Statistics. various years 1933–1995. *Monthly Vital Statistics Report—Final Mortality Statistics [year]*. Hyattsville, MD: Public Health Service.

U.S. National Center for Health Statistics. various years 1996–2014. *National Vital Statistics Reports—Deaths: Final Data for [year]*. Hyattsville, MD: Public Health Service.

U.S. National Criminal Justice Information and Statistics Service. 1973. *Sourcebook of Criminal Justice Statistics 1973*. Washington, DC: U.S. Government Printing Office.

U.S. Substance Abuse and Mental Health Services Administration. 1999. *Summary Findings from the 1998 National Household Survey on Drug Abuse*. Rockville, MD: SAMHSA.

van den Haag, Ernest. 1975. *Punishing Criminals: Concerning a Very Old and Painful Question*. New York: Basic Books.

Zimring, Franklin, Gordon Hawkins, and Sam Kamin. 2001. *Punishment and Democracy: Three Strikes and You're Out in California*. New York: Oxford University Press.

# 2

# THEORY

## The Mechanisms by Which Legal Punishment Might Reduce Crime

Punishment is just one form of negative reinforcement, cost, or disincentive for socially disapproved behaviors, and legal punishment is just one subtype of punishment. We cannot hope to address the full array of all possible negative consequences of criminal acts, nor all socially imposed negative consequences and their social control effects, though we will occasionally touch on these matters as they bear on our main topic. Rather, we focus solely on the effects of legal punishments, threatened or imposed, such as arrest, conviction of a crime, adjudication as a juvenile delinquent, fines, probation, short jail terms, prison sentences, or execution. This chapter is concerned with the theoretically plausible, potentially crime-reducing effects of legal punishment, outlining the full array of such effects that legal sanctions may have. Possible criminogenic or crime-*increasing* effects of legal punishment will be addressed in Chapter 11. Our discussion in this chapter owes most to the writings of Johannes Andenaes (1952, 1966, 1974) and Jack Gibbs (1975).

The extent of empirical support for some of these effects will be the focus of subsequent chapters—primarily those involving deterrence or incapacitation. For now, it is only important that readers understand the considerable variety and complexity of possible effects, because it bears on whether one can automatically assume that such effects are bound to be crime-reducing, and, if crime-reducing, that they must be due to deterrent effects.

While we discuss many different possible ways that legal punishment might discourage criminal behavior, we do not claim to be able to synthesize large bodies of evidence pertaining to any one of them other than deterrence and incapacitation, since there are no large research literatures on any of the other mechanisms. Nor can we definitively say that actual deterrent effects in the real world can be convincingly distinguished from the numerous other possible linkages between punishment and crime. Indeed, some might agree with Jack Gibbs' pessimistic assessment in 1975: "Since there is no truly adequate way to control for possible preventive consequences of punishment other than deterrence, evidential problems in tests of a deterrence theory are currently insurmountable" (Gibbs 1975, 219). In this chapter, we seek only to provide readers with an appreciation of the variety and complexity of these potential linkages and to persuade them that any preventive effects that punishment may have are not necessarily attributable to either deterrence or incapacitation.

## Theoretically Plausible Mechanisms by Which Legal Punishment Could Affect Crime

### *Deterrence*

Gibbs (1975, 2) defined deterrence as "the omission of an act as a response to the perceived risk and fear of punishment for contrary behavior." He elaborated on this definition by specifying that the punishment in question was legal punishment, and the "contrary behavior" in question was behavior in violation of criminal law. Thus, we can rephrase the definition of deterrence to be *the omission of a criminal act in response to the perceived risk and fear of legal punishment for the act.* It bears emphasizing that one cannot "omit" a criminal act in any meaningful sense unless one had some inclination to commit the criminal act in the first place. Thus, by definition, deterrence can only occur among persons with some minimal willingness to commit crimes. Note also that the word "deterrence" implies more than just a process of thinking about legal risks of a possible criminal act, as some have suggested (Jacobs 2010, 417–418). The word also clearly implies a particular *outcome* of that process—the prospective offender refrains from committing a crime. If a prospective offender merely considers legal risks, but does not as a consequence refrain from even a single crime, deterrence has not occurred.

Further, omitting or refraining from committing a crime implies more than merely delaying its commission or committing it in a different place than intended before considering the legal risk. This is mere displacement of crime. Rather, deterrence implies not committing at least one particular contemplated crime at all. Without this definitional requirement, instances of displacement would be a subset—probably a very large subset—of instances of deterrence. Under such conditions, the occurrence of "deterrence" would, for all practical purposes, be placed beyond the need of any empirical test. "Deterrence" defined this way would be virtually inevitable and universal among all persons aware of any specific source of legal threat; it scarcely needs empirical verification that some prospective criminals at least momentarily delay their crimes when faced with a source of legal risk as conspicuous as, say, the immediate presence of a police officer. Differentiating deterrence from displacement is critical for policy reasons, as well as for the sake of conceptual clarity, since preventing the commission of crimes is socially valuable but displacement may not be. Merely displacing crimes to different times or places does not necessarily have any value and can even be harmful, as when robbers are displaced from prosperous businesses as robbery targets to low-income individuals less able to bear the losses.

On the other hand, the occurrence of "deterrence" does not imply that a prospective offender refrains from all crime subject to possible legal punishments. He or she might instead commit fewer crimes, in which case it could be said that "restrictive deterrence" has occurred—that is, some of the crimes that otherwise would have been committed by a prospective offender were not committed, at least partly because he feared the possibility of suffering a legal punishment (Gibbs 1975, 33–34).

To clarify the distinction between restrictive deterrence and displacement, imagine a potential offender who otherwise would have committed five robberies in neighborhood X, but who, in response to an increased probability of arrest due to increased police patrols in X, decides to instead commit five robberies in neighborhood Y. This person has been displaced, not deterred, because no crimes were prevented. Now suppose instead that the same person was unwilling to risk the associated legal punishment so many times and committed only three robberies. This person *was* deterred, since two robberies were prevented; thus restrictive deterrence occurred. The alternative is "absolute deterrence," which would occur only if a person refrained altogether, over his or her entire lifetime, from committing even a single instance of a particular type of crime due to fear of legal punishment for that type of crime (Gibbs 1975, 32). Most deterrence in the real world is restrictive deterrence.

Although prospective offenders may refrain from crime due to the fear of legal punishment, the punishment itself is not necessarily the consequence that they fear the most. Instead, they may primarily fear various social consequences likely to follow from legal punishment, such as loss of employment or the respect of significant others. Gibbs (1975, 84–86) conceptualized this effect as a "stigmatization effect" distinct from deterrence, but it can also be regarded merely as one mechanism by which deterrence occurs. The power of legal threats may be contingent on the social assets a person possesses—the impact of legal threats may be stronger for those who possess more social assets and thus stand to suffer more ancillary harms as a byproduct of legal punishment. We will discuss this further when we address contingencies in deterrent effects later in this chapter.

### *General Deterrence*

Two major varieties of deterrence have traditionally been distinguished. *General deterrence* occurs when a prospective offender refrains from a criminal act because he fears suffering legal punishment due to *offenders in general* being punished, rather than the prospective offender's own prior experience of punishment. Thus, general deterrence can prevent criminal acts both among those who have personally experienced legal punishment and those who have not, i.e. it can occur within the entire population and, consequently, could potentially have very broad impact on the criminal behavior of many people.

### *Specific (Also Called "Special" or "Individual") Deterrence*

Specific deterrence occurs when an offender who has personally experienced legal punishment refrains from subsequent crimes because his *own* experience of punishment has increased his fear of future punishment. More specifically, the fact of having been punished for past crimes may (a) increase the offender's perception of the certainty of also being punished following future crimes, or (b) dramatize or emotionally intensify the perceived consequences of crime (Andenaes 1966). As we will see in Chapter 6, the first proposition has not been supported by recent perceptual deterrence research—the experience of being legally punished does not appear to generally increase the perceived certainty of future punishment (e.g., Paternoster and Piquero 1995; Piquero and Paternoster 1998; Piquero and Pogarsky 2002; Pogarsky and Piquero 2003).

Andenaes (1952) argued that personally experiencing punishment made its pains more vivid, and thus more capable of dissuading the criminal from reoffending. It is, however, at least equally plausible to argue the opposite, that actually experiencing a punishment reduces its deterrent power. Prior to experiencing a punishment like imprisonment, even the least imaginative prospective offender can imagine a host of potential horrors that might be a part of the experience. The breadth and depth of fears are not limited by any actual personal experiences, but rather only by the power of the human imagination. Once the punishment is actually experienced, however, only some of the anticipated pains come to pass, and many criminals may react to the more routine pains of restraint and boredom with the thought, "This is not as bad as I thought it would be—I can handle this." The actual experience of punishment, then, produces a more realistic sense of its pains, and for many criminals the severity of the realistically assessed pains of punishment is less than that prospectively anticipated. Further, it has long been known that repeated exposure to noxious stimuli can desensitize a person to their noxiousness (Lewin 1935). Consequently, it is not at all self-evident that personally experiencing punishment increases its power to deter subsequent misconduct.

Stafford and Warr (1993) argued that general and specific deterrence should be reconceptualized as the direct and indirect experience of punishment because they can operate together, and the same person can be influenced by both the direct experience of their own punishment and the indirect or vicarious experience of others' punishment. Nevertheless, their reconceptualization still acknowledges that there are two important varieties of deterrent effect that need to be distinguished. Their point is well taken, but we prefer to retain the traditional terminology because it helps convey the far greater breadth of general deterrence's potential impact—it can hypothetically influence the behavior of anyone in the general population, while "specific deterrence" connotes the far narrower effect of punishment on the specific persons punished. In Stafford and Warr's terms, the indirect, *vicarious* experience of legal punishment is far more prevalent in the population than direct *personal* experience of it.

### Incapacitation

Incapacitation is the prevention of criminal behavior by the physical restraint of criminals, primarily through incarceration but also through capital punishment. Incapacitation makes it physically impossible for the affected offender to commit crimes against the general public, or at least those requiring direct physical contact. Some also argue that house arrest, enforced by electronic monitoring, could be regarded as a form of incapacitation by virtue of its restraint of the movement of convicted criminals, but offenders subjected to such monitoring are not actually physically restrained. Rather, they are deterred from making unauthorized trips outside their home because their movements are very likely to be detected. Consequently, it is more appropriate to regard electronic monitoring as a specific source or intensifier of deterrence due to the prospective offender's increased fear that his crimes would be detected and punished. The evidence on incapacitation effects is addressed in Chapter 10.

### Moral Education

Legal punishments can change the meaning of the criminal act from acceptable to unacceptable, or strengthen beliefs as to how immoral the act is, for both persons punished and those aware of others' punishment (Andenaes 1952, 179–180, 1966). Andenaes (p. 179) phrased it thusly: "The idea is that punishment as a concrete expression of society's disapproval of an act helps to form and to strengthen the public's moral code and thereby creates conscious and unconscious inhibitions against committing crime." Moral education, as it pertains to punished offenders, is essentially the same as Gibb's concept of reformation, but moral education can also affect the vast majority of the population who are not punished. Punishment may intensify the force of the popular moral definition, insuring that it will be taken more seriously, and the severity of penalties may further communicate the immorality of the act.

Gibbs (1975, 68–71) described the closely related effect of "enculturation," a socializing effect of punishment. The fact that an act is forbidden by law, and at least occasionally punished, can cause some people to become newly aware that the act is wrong, even though they had not previously regarded the act as immoral. This learning experience can then cause some people to refrain from committing the act "because of an uncritical obedience to law" (p. 68).

### Normative Validation—The Reinforcement of Morality and Respect for Law

Punishing crimes can help maintain and intensify the general public's moral condemnation of the punished acts. This intensified moral condemnation of the act, as distinct from the fear of being punished for it, can cause a person to refrain from crime (Gibbs 1975, 79–82; Williams and Hawkins 1986, 559–560). This is distinct from enculturation because it does not entail anyone becoming newly aware of the immorality of a legally prohibited act, but instead involves the strengthening of preexisting notions of immorality. Durkheim did not use the term "normative validation" in this context, but seemed to be implying a similar effect when he argued that punishment's true function was not so much deterrence of crime as it was to maintain social solidarity, primarily among the law-abiding, which in turn discourages crime (1893[1933], 108–109). More specifically, punishment of a given crime serves to strengthen social solidarity in the sense that it strengthens "collective sentiments" about crime—that is, moral condemnation of the punished act. Thus, Durkheim believed that punishment of crimes did not merely *reflect* prevailing morality, it *strengthened* moral norms by increasing consensus about the wrongness of the acts.

### *Reformation*

Like specific deterrence, reformation is a process that affects only those individuals who are punished, but unlike specific deterrence, it is not a product of fear of being punished. Nor is reformation the same as rehabilitation, since the latter is the alteration of an offender's behavior by nonpunitive means—that is, by "treatment." Nor is it "moral education," since that can affect anyone, not just individuals who are themselves punished. Instead, Gibbs (1975, 72–79) conceptualized reformation as the alteration of an offender's criminal behavior by punitive means but independent of any specific deterrent effects attributable to fear of future punishment. He hypothesized that the personal experience of punishment may cause the offender to experience a "moral jolt" that makes him more fully recognize the immorality of the punished act. This could reduce recidivism among the punished and thereby reduce the crime rate. We are not aware of any empirical evidence directly bearing on this possible effect.

### *Strengthening Habitual Obedience to the Law*

Law-abiding people do not newly decide, each day or upon encountering each new criminal opportunity, to refrain from crime. Instead, much obedience to the law is habitual, a general behavioral tendency that continues to routinely restrain rule-breaking impulses without much conscious thought. The law-abiding habit, however, must somehow be acquired in the first place, and some scholars have argued that legal punishment helps build and reinforce this habitual tendency (Andenaes 1952, 179–180; Gibbs 1975, 88–92; Zimring and Hawkins 1973, 85). This effect is not necessarily entirely independent of deterrence. A person might initially refrain from crime as a result of deterrence, i.e. because they fear punishment, but after a while they no longer consider or fear punishment but continue obeying the law out of habit.

An alternative way of stating this idea is that people develop "standing rules" of behavior and routinely obey these rules unless new experiences and circumstances compel them to deviate from the rules (Simon 1957). The standing rules, however, had to be developed in the first place, and threats of legal punishment may have contributed to this initial adoption of prosocial standing rules. Once adopted, learning of the punishment of others for violations of those rules might help strengthen an individual's own continuing commitment to these standing rules.

### *Reintegrative Shaming*

Reintegrative shaming entails shame that is directed at the act rather than the actor and holds open the prospect of reform and reacceptance of the violator back into the community (Braithwaite 1989). Such punishment tells the offender that he remains a part of the community and is capable of reform but also clearly conveys the shamefulness of the act. If the offender still feels a part of the community, he will want to obey its rules, and if he can be made to feel the act is shameful, he will want to avoid doing it in future. Legal punishment in the U.S. rarely incorporates this sort of shaming. Instead, the punishment tends to separate the punished person from the community and to reject him as a person "unfit to associate with decent people." For example, many states are so committed to placing criminals outside the boundaries of the community that they deprive criminals of the right to vote, even after they have completed their sentences and "paid their debt to society." Consequently, the possible beneficial effects of reintegrative shaming remain mostly a theoretical possibility that will become important in the future only to the extent that scattered pilot programs are expanded so as to become a more common part of criminal justice. It is one way that punishment could reduce crime but one that rarely operates at present.

### *Punitive Surveillance*

Most convicted criminals are not punished with incarceration, and few serious common law crimes are punished with fines or community service alone. Instead, some form of enforced supervision in the form of probation is the most common punishment of serious crime. Likewise, parole is a form of enforced supervision that commonly follows an incarceration sentence. Both are clearly punishments, as they are likely to be perceived by convicted criminals as onerous or discomforting, and are inflicted involuntarily. Violation of the rules accompanying the supervision can result in additional sanctions, including incarceration. The occasional, intermittent surveillance of convicted criminals, in the forms of periodic contact with probation or parole officers, can suppress crime. While it may do this partly via deterrence mechanisms, it can also control some kinds of criminal behavior more directly as a product of surveillance, broadly construed, e.g. through the use of drug testing to detect illegal drug use. Gibbs (1975, 65–68) argued that probation and parole may not be extremely effective but are so cheap, especially compared to incarceration, that they may nonetheless be cost-effective.

### *Legal Retribution as a Means of Discouraging Private Vengeance*

One of the oldest arguments for the necessity of law, i.e. governmental social control, was that, in its absence, wrongs done by one private party against another would result in endless rounds of revenge as each victimized party sought retribution against the offending party, which would in turn trigger counter-vengeance. Instead, if the state, perceived as a neutral third party, inflicted punishment on the wrongdoer, the motivation for private vengeance by victims or their kin would be reduced. Public retribution would substitute for private retribution, and punishment could thereby reduce a very specific kind of crime—that committed as revenge for a prior offense (Gibbs 1975, 82–84).

### *Stigmatization*

This topic is usually discussed as a crime-increasing consequence of crime, as indeed we will address in Chapter 11. Gibbs (1975, 84–86), however, noted that legal punishment could discourage crime in a way that operates independently of fear of future punishment itself, because legal sanctions commonly bring with them other undesirable consequences for the person punished. For example, being punished by the legal system is, for most people, degrading and shameful. Punishment can alter how the punished person is viewed by others, especially respectable others. Thus, people might refrain from crime not because they fear the pains of punishment itself but rather because they fear losing the good opinion of others who matter to them. Degradation by the legal system can result in the loss of friends and other valued associates. Thus, apart from their fear of the formal punishments that may follow future criminal acts (incarceration, probation, fines), people may fear the other nonlegal consequences that often follow legal punishment, which would add another form of deterrence beyond that produced by fear of legal sanctions themselves. On the other hand, among those not deterred, who commit crime anyway and are caught and punished, stigmatization can have crime-increasing effects, which will be discussed in Chapter 11.

### *Normative Insulation*

Certain kinds of punishment—incarceration, banishment, and capital punishment—have the effect of isolating the punished criminal from other people and thereby preventing the offender from exerting antisocial influence on other people. To the extent that criminals would influence the norms and values of other people in a pro-criminal direction, this isolation could have crime-reducing effects. Gibbs (1975, 87–88) called this effect "normative insulation" and suggested that it would be most important regarding members of the offender's family and other close associates. He also pointed out, however, that isolating a criminal from his or her family can also have very detrimental effects on family members, starting with the loss of income resulting from punishment of a family's breadwinner.

Research on most of the effects discussed thus far in this chapter, aside from general and specific deterrence and incapacitation, is negligible to nonexistent, so we are not in a position to say anything definitive or persuasive about them based on sound empirical research.

## The Conditions Under Which Punishment Is Most Likely to Reduce Criminal Behavior

The threat or inflicting of legal punishment does not have the same effects on all types of criminal behavior, under all conditions, or for all people (Nagin and Paternoster 1993; Wright, Caspit, Moffitt, and Paternoster 2004). It has long been recognized that punishment effects are contingent upon various conditions under which the punishment is used and vary depending on the attributes of the persons punished or threatened (Tittle and Rowe 1974). The impact of the threat or experience of punishment on criminal behavior may depend on the social position, personality, or prior history of the prospective offender; aspects of the situation in which committing a crime is contemplated; the perceived character of the individuals or institutions delivering the punishment; attributes of the punishment itself, such as its certainty, severity, or swiftness with which it is inflicted; the type of crime being considered; and numerous other kinds of contingencies.

Researchers refer to such contingencies as "interactions," asserting that punishment interacts with various contingent attributes in affecting crime. Thus, punishment is more likely to reduce (or increase) crime under some conditions than others, and any serious theory of punishment needs to take account of these contingencies. We summarize some of the more likely contingencies in this section. Few of these contingencies have been studied often enough to merit a systematic assessment of the relevant evidence but, regarding those contingencies that have been the subject of substantial research, we assess that research in later chapters. Here, we instead merely describe some of the more plausible contingencies to provide readers with a richer understanding of the complex and variable ways in which punishment might influence criminal behavior.

### *Properties of Punishment That Could Condition Its Deterrent Impact*

Many of the hypothetical effects of punishment could vary in strength depending on certain key properties of punishment. For example, as will be discussed at length in Chapter 10, the incapacitative effect of incarceration depends greatly on whether severity of punishment (especially sentence length) or certainty of punishment (e.g., the percent of crimes resulting in punishment) is emphasized in crime control policy. For now, however, we focus on how deterrent effects can vary with the certainty, severity, and swiftness of punishment.

## *Certainty*

The certainty of punishment is the probability that crimes will result in punishment. Thus, the certainty of punishment is higher if the percent of crimes that result in the arrest of the perpetrator is higher, the fraction of arrested persons who are convicted is higher, the fraction of those convicted who are sentenced to any given punishment is higher, or any combination of these probabilities. One of the longest ongoing debates about punishment is whether increasing certainty of punishment or severity of punishment is more likely to yield deterrent effects or reduce crime (Beccaria 1764/1963, 58; Bentham 1789/1988; Gibbs 1975; Grogger 1991). It is commonly argued the severity of punishment is irrelevant to deterrence if the certainty of the punishment being inflicted is low. It is, however, equally reasonable to argue that higher certainty of punishment is not likely to increase deterrent effects if punishment severity is low. One cannot simply reason one's way to a convincing conclusion on this point. It requires empirical investigation to determine whether variations in certainty or variations in severity have the most influence on crime.

## *Severity*

From almost the beginning of modern research on deterrence, it was hypothesized that any deterrent effect of the severity of punishment was likely to be contingent upon certainty—the perceived severity of punishment would affect only the criminal behavior of those who perceived a fairly high certainty of punishment (Tittle 1969). Most macro-level deterrence research has found no effect of severity levels on crime rates (Chapter 7, Table 7.14; Doob and Webster 2003; Nagin 1978). In contrast, Mendes and McDonald (2001) insisted that more severe punishment really does reduce crime but that most studies failed to find this effect because they "unbundled" severity from certainty. They argued that the true deterrence argument derived from economic theory requires analysts to measure the expected cost of legal punishment—that is, the certainty of punishment multiplied by its "cost," i.e. average severity, echoing sociologist Charles Tittle's (1969) earlier argument that severity levels will affect crime rates but only when combined with relatively high certainty levels. Mendes and McDonald went beyond Tittle, however, arguing that severity effects cannot be assessed separately from certainty effects and that one cannot detect severity effects at all when the severity level (typically measured as the average length of prison sentences) is included as a separate variable in crime rate equations. They argued that severity effects could only be detected if severity was combined with certainty in a multiplicative term (severity times certainty).

This is a *non sequitur*. Even if there were interactive effects of punishment severity on crime rates (that is, severity effects depended on minimum certainty levels being achieved), one should still find a negative association between the severity level (included as a separate variable) and crime rates, as long as those minimum certainty levels were achieved in at least a few times and places included in the sample studied. The only exception to this generalization would be if higher severity combined with lower certainty sometimes *increased* crime, which neither Mendes and McDonald nor any other scholars known to us have asserted. Thus, additive tests of severity effects *are* relevant, though less satisfactory, even if the severity effect is an interactive or contingent one. Although analysts would miss the interactive nature of some effects, coefficients for separate severity variables should still be negative and significant if severity contributes to deterrence. Further, if a punishment severity measure is buried in a combined "expected cost" measure (severity times certainty), as Mendes and McDonald urged, one could generate apparent support for a nonexistent severity effect because "ineffective" severity was combined with "effective" certainty—severity coasting on certainty's coattails.

Mendes and McDonald's review of research on the impact of severity was extremely misleading because they excluded most of the research that did not support the notion that greater severity is effective. They excluded all studies that did not express the effect of severity measures on crime rates using a specific metric, the "elasticity," for expressing the magnitude of each variable's effects. Elasticity measures the percent change in the dependent variable associated with a 1 percent change in the independent variable. Thus, an elasticity of 0.8 for an analysis in which researchers estimated the effect of average sentence length on crime rates would mean that a one percent increase in average sentence length was associated with a 0.8 percent decrease in the crime rate.

This was a highly arbitrary standard for the selection of studies, especially since most studies that have yielded findings directly assessing the impact of punishment severity, including many very sophisticated studies, do not use this measure of association. Not coincidentally, most of the studies that did report elasticities were done by economists, who, by and large, are sympathetic to the deterrence doctrine in general, and, specifically, the hypothesis that increasing the prospective costs of a behavioral alternative (punishment severity in deterrence studies) will reduce the likelihood of a person choosing that alternative. The authors correctly stated that most of the handful of studies that they reviewed supported the hypothesis that greater severity of punishment reduces crime but did not share with readers the less supportive findings obtained in the far larger number of relevant studies that did not report elasticities. As we will see in Chapter 7, most research does not indicate that greater average punishment severity reduces crime rates (see also Doob and Webster 2003).

On the other hand, it is still possible that differences in severity have effects on crime rates but that the effects are missed because research has assumed a linear relationship between the two variables. The relationship between severity of punishment and criminal behavior may be sharply nonlinear, subject to strong ceiling effects. That is, increases in severity at low levels may reduce crime, but once punishment is as severe as, say, a short term of incarceration, further increases in severity do not produce any further reductions in crime. Since most studies of severity effects, and virtually all macro-level studies of the subject, examine only differences in sentence length, they may miss any increment in deterrent effect produced by, say, the difference between a large fine and a small one, or between a longer term of probation and a shorter one.

Despite the large volume of evidence indicating that more severe sentences do not produce greater deterrent effects on crime, it should not be thought that this issue is a settled matter among researchers and that scholarly support for "tougher penalties" has disappeared. As recently as 2009, even a generally sophisticated analyst of crime control strategies like Mark Kleiman could assert that it was very plausible ("though not yet shown to be true") that "toughening sentences for gun trafficking" would reduce gun acquisition by criminals and thereby reduce violent crime (2009, 143). This endorsement of longer sentences, equivocal though it was, was all the more remarkable in that it appeared in a book containing an extended earlier discussion explaining why longer sentences would *not* necessarily produce greater deterrence (pp. 76–78).

### *Swiftness (Celerity)*

Can swifter punishment have more effect on criminal behavior than punishment that follows crime only after long delays? Justice delayed may well be justice denied, but is delayed justice also less effective in reducing crime? Also known as "celerity," swiftness of punishment has rarely been studied. It plays virtually no role in economic versions of deterrence theory and is most likely to be emphasized by psychologists, who see learning via operant conditioning as being facilitated by temporally closer links between behaviors and their consequences. Gibbs (1975) argued, probably correctly, that operant conditioning does not provide a persuasive rationale for a celerity effect but went even further, asserting that there was *no* plausible theoretical rationale for celerity having an effect. Nagin and Pogarsky (2001) disagreed, arguing that prospective offenders will assign less "value" to punishments likely to be deferred into the remote future—that is, offenders are "future discounters." Criminals are often described as present-oriented persons who discount consequences that are likely to only be experienced in the remote future. This implies that they are less likely to be influenced by punishments that only follow criminal acts after a long passage of time. We agree with Nagin and Pogarsky that there is a plausible theoretical rationale for greater deterrent effects when punishment is swifter, one that does not rely on operant conditioning. Theoretical plausibility, however, is at best a weak basis for believing a celerity effect to be a reality. As we will see, there is very little empirical evidence bearing on the effect of the swiftness of legal punishment on criminal behavior.

### *Justice*

The justness with which punishment is imposed is usually regarded as an important value apart from the crime control effectiveness of legal sanctions, but there is also evidence that justice can enhance effectiveness. Tyler (1990) asserted that the crime control effectiveness of sanctions increases in accordance with the legitimacy that the punished person attributes to the sanctioning agent—especially the procedural justice and respect shown by the enforcement agent to the punished person. Thus, punishment or the threat of punishment is more likely to reduce criminal behavior when the person or institution delivering the punishment is perceived as legitimate, the punishment is regarded as just, and the person or institution inflicting punishment is seen as obeying rules of procedural fairness in imposing punishments. For example, legal punishment is more likely to be effective when laws are enforced by honest police officers rather than corrupt police who take bribes, use brutality, or plant evidence; by honest prosecutors rather than those who suppress or conceal exculpatory evidence; or is administered by impartial judges rather than those who allow personal biases or vindictiveness to influence their decisions (Tyler 1990).

Sherman (1993) went further, proposing that punishment perceived by the punished person as unfair or excessive could even stimulate defiance rather than deterrence, increasing criminal behavior if the defiance effect outweighed the deterrent effect. This will be discussed in detail in Chapter 11.

### *The Visibility of Legal Threats*

Deterrent effects logically require that a legal threat be perceived. A risk of which a prospective offender is unaware cannot deter that person from committing a crime. This principle is so fundamental that Jack Gibbs (1986), in stating his version of the deterrence doctrine, stated as the first of three fundamental propositions that "a direct relationship obtains between the objective properties of punishment and their perceptual properties" in order for deterrence to occur. Some punishments, legal threats, or changes in punishment levels, however, are more likely to be perceived by prospective offenders than others. Those that are highly publicized in the news media or more widely disseminated through informal communication channels are more likely to be perceived by prospective offenders. Likewise, some punishment-generating activities are more conspicuous than others. People can see police patrol cars moving past them, reminding them of the risk of arrest, but are less likely to see a judge passing sentence on a convicted criminal if one is not the defendant or to see a person serving a prison term if one is not an inmate. Some changes in punishment policy, such as the enactment of a new law providing for a harsh mandatory minimum sentence, may be the subject of news stories, while increases in police patrol levels may go unnoticed. In short, the "visibility" or perceptual availability of legal threats varies, and more visible threats should exert more deterrent effect than less visible ones.

It is worth noting that it is not necessarily those who most accurately perceive risks who are most likely to be deterred. Exaggerated perceptions of punishment risk may deter criminal behavior more than lower but more accurate perceptions. Although it is unlikely that many prospective offenders have perceptions of risk that are consistently the *reverse* of actual risks, certainly the perceptions of some may well be too high, thereby generating greater deterrence, while misperceptions to the low side may decrease deterrence and encourage crime. It is sensible to keep in mind Gary Jensen's (1969) insight that some deterrence is the result of a "shared misunderstanding" among prospective offenders that punishment is a likely consequence of crime. It is, however, equally important to remember Jensen's finding that this socially useful misunderstanding rapidly declines with age as people accumulate experience (personal and vicarious), demonstrating that punishment usually does *not* follow crime. Those who do not attempt crime may never get the chance to learn, via personal experience, how unlikely punishment is, which may partly explain why they continue to refrain from crime. Conversely, those who commit crimes soon learn the low-risk truth of the matter, partly explaining why they continue to commit crimes.

### *Attributes of Prospective Offenders That Could Condition the Deterrent Effects of Punishment*

Attributes of prospective offenders could also condition the deterrent effects of punishment (readers interested in a more detailed review of this topic may consult Piquero, Paternoster, Pogarsky, and Loughran 2011). For example, the threat of punishment cannot prevent criminal behavior among those who would never want to commit crime, regardless of the risks of legal consequences. They may instead refrain from criminal acts solely because they regard them as immoral and believe this is sufficient reason to refrain from committing them. Thus, the threat of punishment will reduce criminal behavior only if *the person threatened is crime-prone to some degree*, i.e. has some minimal criminal propensity (Wright et al. 2004). There is, however, some danger of this assertion being a tautology, since some starting motivation to commit crime is built into our previously stated definition of deterrence: a deterrent effect occurs when a person *otherwise inclined to commit a crime* refrains from doing so because of the fear of legal punishment. Nevertheless, it is worth noting that this is a necessary condition of legal deterrence because it could help explain instances where threats of punishment fail to have any effect. Wright and his colleagues (2004) found that, among New Zealand youth, the threat of legal punishment is primarily relevant to those who seriously contemplate committing crime, i.e. those with a higher criminal propensity (higher "criminality"). People disinclined to commit crimes on moral grounds do not think about legal risks. Thus, their findings suggest that deterrence works best with those with a propensity to commit crime and has little relevance to those without.

At the other extreme, some people (such as drug addicts or terrorists) may be so powerfully motivated to commit crime, or be so convinced that they have nothing valuable to lose by offending and being caught, that the threat of punishment has little or no impact. Thus, the threat of punishment is most likely to deter *persons at an intermediate level of criminal motivation—willing to some degree to commit crime if circumstances are sufficiently favorable but not so powerfully compelled to do so that no credible legal threat can deter them*. Thus, one is less likely to find evidence of deterrence among either (a) virtuous people living in very low-crime areas or (b) among groups of hard-core offenders, such as prison inmates. The ideal study sample for detecting deterrent effects, then, might be noninstitutionalized persons living in high-crime urban areas. The high crime rate of their neighborhood suggests that the average level of criminal propensity among residents is substantial, but the fact that residents included in the study sample are not incarcerated suggests that they are less likely to be hard-core career criminals.

Punishment is more likely to reduce criminal behavior if *the person being punished is strongly bonded to the conventional social order* being reinforced with the punishment. Scheff and Retzinger (1991) proposed that the effect of punishment depends on the degree to which the punished person is emotionally bonded to the sanctioning agent or the community he represents. Note that this is somewhat contradictory to the notion that deterrence is irrelevant to those with no criminal propensity to the extent that those most strongly bonded to the conventional social order are also likely to have little criminal propensity.

The threat of punishment is more likely to reduce criminal behavior if *the person threatened with punishment has a greater stake in conformity, i.e. more to lose if they are punished.* Everyone has something to lose as a result of punishment, such as their freedom or their life, but some have considerably more to lose than others. The threat of punishment may have more deterrent power with persons with a greater stake in conformity because legal punishment is likely to produce additional nonlegal negative consequences for the punished person beyond the legal sanction itself. These consequences could include moral condemnation from law-abiding family members, friends, and respectable associates, damage to valued relations with these others (in particular, a family that would be hurt by the offender's punishment), loss of income from a lucrative job, harm to a valued career path, or injury to a reputation as a respectable law-abiding person. Of course, it is necessary that a person possess these assets in the first place if they are to fear losing them as a by-product of legal punishment. Thus, threats of punishment may best deter those people who have more to lose than just their freedom or money if they are punished (Sherman and Smith 1992; Smith and Gartin 1989; Wright et al. 2004). Conversely, punishment threats are less likely to deter those who feel they have nothing to lose. At the other extreme, some people may be virtually undeterrable because they neither fear the punishment itself nor believe they possess anything of value that would be lost if they were punished.

On the other hand, it is possible that *actually* punishing a person, rather than merely threatening to do so, has predominantly criminogenic effects (Chapter 11) and that these effects are stronger for persons who have greater stakes in conformity. Once punishment is actually inflicted, it can be more devastating for those with more to lose and may increase subsequent offending more for those whose lives were more seriously damaged by legal sanctions. Losing future career prospects is more disrupting for those who possessed such prospects in the first place. Likewise, disrupting progress towards higher education is only possible for those who intended to pursue higher education in the first place. Thus, there is more potential for the labeling and other crime-increasing effects for some people than for others (see Chapter 11).

The lack of a criminal record may be considered a proxy for one particular type of stake in conformity—a reputation as a law-abiding citizen. Those with no known official record of criminal behavior possess this asset and would risk losing it if they committed a crime. A given amount of legal risk may therefore have a greater deterrent effect on a person *with no official criminal record* than on a person with a record. Actually inflicting legal punishment on a person can therefore reduce the power of legal threats to have any further effect thereafter. This is one reason why threatening punishment can have more crime-reducing impact than actually inflicting it. On the other hand, it can also serve as a reason to inflict more severe punishments on persons with longer prior criminal records—it may take more punishment of a person with no respectable reputation to lose to produce the same amount of deterrence that would be produced with a person possessing this social asset.

Braithwaite (1989) proposed that punishment is more likely to reduce criminal behavior when *the punished person experiences, and accepts, feelings of shame over the punished act* rather than rejecting the feeling of shame. The former is a more likely reaction if punishing agents focus shame on the act rather than the actor, stressing the potential for the actor to reform and to reject the punished acts in future. This makes it possible for the violator to be reintegrated back into the community. The latter is less likely to happen if punishment agents stress the person as being shameful rather than the act, because this encourages the offender to avoid the pain of feeling shame by taking pride in their actions and rejecting the moral authority of the punishment agents and the institutions they represent. Thus, deviants adapt to their shame by rejecting their rejecters and taking pride in their isolation from the conventional community. Under these circumstances, punishment pushes the deviant outside the conventional community, reducing its ability to control his behavior (Braithwaite 1989).

Punishment or the threat of punishment is more likely to deter criminal behavior if *the punishment would be stigmatizing to the prospective offender* if he committed a crime and were caught. Stigma is an undesirable consequence, but punishment is not equally stigmatizing to everyone in all contexts. It is more stigmatizing in social contexts where it is used sparingly and selectively. In contexts where it is commonplace, it produces less shame for the punished. Thus, increasing punishment levels in the aggregate reduces the stigma of punishment and thereby reduces its deterrent effect by reducing one major cost of criminal conduct. One could therefore observe a "diminishing returns" pattern in which further increases in aggregate levels of punishing produce smaller and smaller deterrent or other crime-reducing effects on the population, perhaps to the point where the reductions in crime are not large enough to justify the costs of generating the punishment increases needed to produce the crime reductions. In this regard, the stigma associated with punishment works better to reduce criminal behavior when punishment is threatened than when it is actually inflicted. Being caught, punished, and officially labeled as a criminal can increase crime via stigmatizing effects and the blocking of opportunities, thereby canceling out its crime-reducing effects (Chapter 11), whereas merely threatening punishment has no stigmatizing effects on those threatened.

Punishment or the threat of punishment is more likely to reduce criminal behavior among *people who have high self-control, are less impulsive, and are more future-oriented* (Gottfredson and Hirschi 1990, 255–256; Nagin and Pogarsky 2001; Zimring and Hawkins 1973, 98–101). These personality traits might be treated separately, but this would be artificial because they seem to be both analytically overlapping and highly correlated across individuals. That is, to have high self-control is to be less impulsive and more oriented toward future consequences of one's actions. Some evidence, however, suggests that deterrence works most effectively for those with *low* self-control. Perhaps the people who most need external controls are those whose internal controls are ineffective (Wright et al. 2004).

Those who are more likely to consider long-term consequences of a contemplated action are more likely to think seriously about the prospect of legal punishment, which is almost exclusively a long-term consequence of persistent criminal behavior. It is unlikely to follow any one criminal act (other than murder) but is much more likely to occur in the long run if a person persists in repeated criminal conduct. The idea that threats of punishment more strongly affect more future-oriented people might seem to contradict the idea that effects are stronger for persons with more criminal propensity, since persons who possess a higher degree of self-control and are more future-oriented might be thought of as also having little propensity to commit crime. There is, however, no necessary contradiction here. Two things could both be true: (1) Deterrence works only for those with some minimal propensity to commit crime, in the sense of a willingness to exploit criminal opportunities if the conditions are right (low risk, high gain) and is irrelevant for those who have internalized social norms forbidding the acts and who therefore have strong moral objections to them. (2) Deterrence works *less* well for those who are either incapable of controlling their impulses or who tend not to take account of, or place much weight on, long-term consequences of their decisions, including legal punishment. That is, it will have less effect on those who "discount" future consequences of their actions.

The threat of legal or other risks may have more impact on people are who *more risk-averse than average*—that is, less willing to take greater risks to gain greater benefits. It is unclear, however, to what degree low risk aversiveness—also called "risk affinity" by some—is conceptually or empirically distinct from present orientation or a tendency to discount the future.

Another personality trait that seems likely to be strongly correlated with risk affinity is overconfidence. Some people are more willing to take what others would regard as a serious risk because they are so sure that they can avoid being caught (Loughran, Paternoster, Piquero, and Pogarsky 2011). Thus, the threat of legal sanctions is likely to have less impact on *persons who are overconfident about their ability to evade punishment.*

The threat of punishment is more likely to reduce criminal behavior among persons who, for whatever reasons, *assign more negative "value" or weight to a given legal punishment.* For example, a fine of $500 is more consequential to a poor person than a rich one and thus likely to be assigned greater subjective weight. Some people perceive a one-year prison sentence as a more appalling prospect than others, even independent of the income, social capital, and other assets one might stand to lose from incarceration, because prison is more frightening or distasteful to them. The loss of freedom may be viewed as more horrifying for those whose life in the free world is more pleasant. Further, the threat of prison is less frightening to those who have already experienced it and become more desensitized to some of its pains than to those who have not experienced it and who can imagine every horror their minds are capable of conceiving, both realistic and not so realistic. Thus, the deterrent threat of incarceration may weaken as a criminal career proceeds and an offender personally experiences legal sanctions.

The threat of punishment is more likely to reduce criminal behavior if a person *assigns less value to the rewards of crime*, thereby making the legal risk seem larger in comparison. A wealthy person would assign a lower subjective value or weight to a $1,000 payoff for a crime than a poor person would, so the perceived benefit would therefore be less likely to outweigh the perceived risk of legal punishment. Thus, punishment may deter ordinary "common law" crime better among richer people not only because they have greater stakes in conformity (more to lose if punished), but also because they assign less subjective value to the benefits of such crimes than do poor people, and the benefits are therefore less likely to outweigh the value assigned to legal punishment and related costs.

On the other hand, some attempts to discover interactions between the effect of legal threats on criminal behavior and various other conditioning factors, such as gender, have failed to find supporting evidence. Although males commonly perceive less legal risk from offending than females do (e.g., Finley and Grasmick 1985; Richards and Tittle 1981), the effect of a given level of perceived risk on criminal behavior seems to be about the same for males and females. Carmichael, Langton, Leuking, Reitzel, and Piquero (2005) found no significant differences by gender in the deterrent effects of perceived punishment certainty on self-reported delinquency—the threat of legal punishment was about equally effective, or ineffective, for males and females. Piquero and Paternoster (1998) and Smith and Paternoster (1987) likewise found no differences in the effects of legal threat across genders.

### *Situational Factors That Could Condition the Deterrent Effect of Punishment Threats*

The effect of the threat of punishment could depend not only on lasting properties of prospective offenders but also on their emotional and physical state at the time an offense is contemplated.

Persons who are *angry or intoxicated* are less likely to be influenced by the prospect of legal punishment than calm, sober individuals (Exum 2002). This can be especially important in light of the fact that so many criminals are angry or intoxicated at the time they commit crimes. The fact that some potential offenders are calm and sober may explain why they take account of potential sanctions and therefore refrain from crime, while the fact that others are angry or intoxicated may explain why they do not consider potential sanctions and commit the crime (Tunnell 1992).

More generally, *persons experiencing fear, pain, drug withdrawal, extreme hunger, or other intense need-related emotions* are less likely to make the choices predicted by the rational choice or utility-maximization model and less likely to be deterred by legal punishments that are likely to be experienced, if at all, only in the distant future. Instead, the desire to immediately satisfy powerfully felt needs may overwhelm any consideration of delayed negative consequences such as legal punishment. Thus, the experience of powerful needs may reduce, and possibly negate, the deterrent effect of criminal penalties.

Social aspects of the situation in which a criminal decision is made may also condition the effect of sanction threats. The *presence of co-offenders* may contribute to a sense of heightened anonymity and invulnerability, lowering the impact of legal threats (Stafford and Warr 1993, 132). Thus, the comfort provided by accomplices may neutralize the effect of the legal risks that a prospective offender would otherwise experience.

There is evidence indicating that the impact of punishment on criminal behavior differs considerably depending on the *type of crime* being contemplated. While evidence of the deterrent impact of legal sanctions is often weak with regard to the violent crimes the public fears most, one type of crime seems considerably more vulnerable to control via deterrence: business crime. The decision to commit business crimes such as corporate crime is likely to be made under calm circumstances, with prolonged and deliberate consideration of the costs and benefits of possible alternatives. These conditions provide greater potential for the threat of legal punishment to influence the criminal decision to commit offenses like consumer or tax fraud. The decision to commit business crime, like business decision-making in general, may be particularly rational because it is guided by an unusually large volume of relatively accurate information about costs and benefits. The greater volume of available information in turn is to a considerable degree the result of the collective nature of business decision-making. Decisions are either made by groups of people or by single individuals guided by information provided by numerous others. With more people involved, more benefits and costs can be taken into account, more information about the probability and magnitude of each factor can be considered, and the probability that actual costs and benefits are accurately perceived increases. This rationality might conceivably increase the deterrent effect of legal threats, but if the rewards of a contemplated business crime are great, and the likelihood of legal punishment is low—as it usually is—this same rationality will increase the likelihood of crime.

Traffic offenses are not as premeditated as business crime, but may also be unusually deterrable because of the peculiarly public nature of the infractions. Moving about in a huge steel box on public roads is as visible and public a category of behavior as most people ever engage in, which makes traffic-related crimes, such as driving while intoxicated, far easier for authorities to detect than the types of offenses that first spring to mind when most people think about "the crime problem." Drivers are well aware of the unusually visible character of many kinds of driving infractions, making such behavior unusually deterrable. It is especially likely that a given driver will be apprehended if the type of infraction in question is one that is committed repeatedly. While it is unlikely that any one infraction will result in apprehension, it is much more likely that the driver will eventually be caught for at least one of many repeated infractions. For these very reasons, however, it would be imprudent to generalize from deterrence findings regarding driving infractions or business crimes to other types of crime.

## Nonlinearity of the Effects of Punishment—Threshold and Diminishing Returns Patterns

From the earliest days of modern deterrence research, scholars have suspected that the effects of increasing legal punishment levels may be nonlinear, i.e., that the amount of impact on crime rates of a given unit of change in amounts of punishment inflicted is not constant across levels of punishment (Logan 1972; Tittle and Rowe 1974). The most common hypothesis pertaining to this nonlinearity was the idea that the certainty of punishment had to reach a certain minimum "threshold level" or "tipping point" before further increases in punishment certainty would begin to reduce crime. The underlying assumption was that prospective offenders do not perceive or take seriously low certainty punishments, and only begin responding to further increases in certainty once the threshold level is reached. Up to that point, increasing the certainty of punishment has little effect.

Some researchers have indeed found macro-level evidence of a threshold effect (Brown 1978; Chamlin 1991; Tittle and Rowe 1974; Yu and Liska 1993). Studying arrest certainty rates for Florida municipalities and counties, Tittle and Rowe (1974) found evidence of a tipping point around 0.3. That is, increasing the certainty of arrest did not seem to have much crime-reducing effect until it reached about 30 percent, at which point further increases in arrest certainty appeared to reduce crime rates. This study did nothing to establish the causal order between arrest certainty and crime rates, however, so it is possible the negative association merely reflected the fact that higher crime rates can overload police ability to solve crimes and that this effect only becomes pronounced once crime rates are very low. If the associations are interpreted as deterrent effects, however, the findings suggested that aggregate levels of arrest certainty must reach fairly high levels before they begin to have perceptible effects on crime rates. Brown (1978) similarly obtained evidence of a tipping effect of arrest rates around 0.25 to 0.35, though this was evident only in small communities. Chamlin (1991) found evidence of a tipping point around 0.4, which he also believed applied only to smaller cities. Yu and Liska (1993) obtained evidence of a tipping effect, but one that only applied to blacks, which they attributed to the possibility that certainty of arrest was higher among blacks and therefore may have reached the threshold value for blacks but not for whites.

It is also possible that there is nonlinearity at the high end of the certainty scale, in the form of a "diminishing returns" pattern whereby the impact of further increases in punishment certainty diminishes as it gets very high. Logan (1972) and Erickson, Gibbs, and Jensen (1977) found macro-level evidence of a diminishing returns pattern, while Yu and Liska (1993) found indications of both a threshold pattern and a diminishing returns pattern (which they called a "ceiling effect"), though only for blacks. Based on survey-derived self-reports of delinquent acts among juveniles, Erickson et al. (1977) concluded that "[b]eyond some point any further increase in the perceived certainty of punishment is associated with only very small decreases in the mean number of self-reported acts" (p. 311). Likewise, perceptual deterrence research by Loughran, Paternoster, Piquero, and Pogarsky (2012) provided strong individual-level support for a tipping effect pattern whereby perceived certainty of punishment did not begin to affect offending until it reached the 30–40 percent range.

After these departures from linearity had been discovered by criminologists in early macro-level research on crime rates, psychologists Kahneman and Tversky (1979, 282–283) rediscovered them via laboratory experimentation and incorporated them into their prospect theory of decision-making. They found that people making decisions under conditions of uncertainty often discount altogether low-certainty contingencies and sometimes make little distinction between contingencies that are highly likely and those that are certain. Thus, people effectively treat probabilities greater than 0 but less than, say, 0.1 or 0.2 as if they were 0, while probabilities greater than 0.8 or 0.9 are treated as if they were 1. The first pattern is consistent with criminologists' conclusions that increasing the certainty of legal punishment from 0 to 0.3 have no effect on criminal behavior and that only increases beyond that point reduce crime. The second pattern implies that increasing the certainty of punishment up to, say, 0.8 or 0.9 might reduce criminal behavior but further increases beyond that point would have no additional crime-reducing effect.

As a theoretical matter, both nonlinearities may well exist. That is, threshold effects may prevail at low levels of punishment certainty, while diminishing returns (ceiling) patterns may prevail at high levels. The principle difference, however, is that in the real world, the actual certainties of legal punishment for nearly all ordinary crime types cluster at the low end of the certainty spectrum, so a threshold effect is far more likely to actually operate than a diminishing returns pattern. The probability of arrest or punishment comes nowhere near certainty ($p = 1.0$) for any common crime type, so ceiling effects are, in most places and regarding most types of crime, little more than a theoretical possibility with regard to *actual* certainty levels. On the other hand, some people may misperceive legal risks as approaching certainty under some circumstances, so ceiling effects might still be relevant to *perceived* certainty of punishment.

Among the eight "Index" offense types on which the FBI gathers statistics, the share of crimes known to the police that result in the arrest of an offender exceeds 0.3 only for the most serious violent crimes—0.612 for murder, 0.400 for forcible rape, and 0.541 for aggravated assault (U.S. Federal Bureau of Investigation 2008). Taking into account the fact that most crimes are not even reported to the police (U.S. Bureau of Justice Statistics 2010), the actual overall probability of arrest for forcible rape and aggravated assault is probably less than half of what these figures suggest, probably well under 0.25.

Furthermore, these FBI Index crimes are unusually likely to be punished compared to nearly all other crime types. For example, the chance of any one instance of drunk driving resulting in apprehension is estimated to be less than one in 1,000 (Ross 1992, 61–62). A low punishment certainty is even more characteristic of white-collar offenses than of the types of common law "street crime" for which the FBI gathers statistics, as the former rarely result in the perpetrator's arrest. Although there are local variations in arrest rates, it is clear that the certainty of arrest is usually very low and almost never anywhere near certainty. Thus, even though a diminishing returns effect of very high actual levels of punishment certainty might exist under artificial laboratory conditions, it would be extremely rare in the real world.

The implications of a threshold effect for real-world punishment policy, in contrast, could be profound. If the tipping point for certainty of arrest (as measured by clearance rates) really is anywhere near 0.3, it would imply that increases in deterrent effects would not reach detectable levels in most places, most of the time, with regard to most offense types, since currently achievable certainty levels for most types of crime do not reach this threshold. Thus, public expenditures to increase the certainty of arrest anywhere within the range from 0 to 0.3 would yield little benefit in the form of crime reduction due to increased deterrence. Typical local jurisdictions achieve arrest certainties that are near or over a tipping point of 30 percent only for the most serious violent offense types with a direct confrontation between offender and victim, i.e. those where victim eyewitness information is most likely to enable the victim and police to identify a specific offender. For other crimes, police rarely approach a 30 percent success rate.

## The Communication of the Risk of Legal Punishment to Prospective Offenders

Deterrence theory is a perceptual theory, asserting that perceptions of legal risk somehow discourage criminal behavior. Virtually all prospective offenders are aware of the abstract possibility that punishment may follow crime, but what communication mechanisms cause variations in perceived risk across individuals or changes over time? Some have suggested that news outlets communicate the risk by reporting stories of criminals being caught and punished or of legislatures changing the severity of penalties, while others argue that informal communication between friends, relatives, and others convey the legal threat. For example, Cook (1980) asserted that prospective offenders shift their perceptions of legal risk when other offenders of their acquaintance commit crimes and are punished or escape without punishment. He did not, however, present any empirical evidence of this process working with any significant frequency. Recent research undercuts the notion that a person's perceptions of punishment risks are influenced by friends and other close associates. Boman (2013) studied pairs of close friends and found that perceptions of the certainty of punishment of the members of the pairs are "nearly completely unrelated to each other." This finding certainly does not encourage belief in the notion that people learn about punishment risks from their friends. Likewise, Lochner (2007) tested whether a person's perception of the risk of arrest is affected by a sibling being recently arrested and found there was no significant relationship (and the association was in the wrong direction). If prospective offenders gain any accurate knowledge of punishment risks, it apparently is not acquired via the punishment experiences of friends and family.

There is little direct evidence as to the effects of news media on perceptions of punishment risks, and indirectly relevant evidence does not tend to support any impact. There is little association between the volume of news about crime and official crime rates (Barlow, Barlow, and Chiricos 1995, 7–8; Davis 1952, 327–329; Garofalo 1981; Jones 1976, 241–242; Marsh 1989; McClellan 1997). Since punishment events are given less media attention than crimes, there is even less basis for expecting a close association between news accounts and the frequency of punishment events such as arrests, convictions, impositions of sentences, or admissions to prison. Confirming this, scholars have commonly found that the news media routinely provide inaccurate impressions of the risks of legal punishment (Parker and Grasmick 1979, 371; Roberts 1992; Roshier 1973, 37).

In sum, it appears to be more likely that prospective offenders acquire their perceptions of punishment risks from their own personal experiences than from the experiences of associates or the news media. Research on the impact of personal and vicarious experiences with punishment, and the avoidance of punishment, on risk perceptions will be reviewed in detail in Chapter 6.

## References


Andenaes, Johannes. 1952. General prevention: Illusion or reality? *Journal of Criminal Law, Criminology, and Police Science* 43:176–198.

Andenaes, Johannes. 1966. The general preventive effects of punishment. *University of Pennsylvania Law Review* 114:949–983.

Andenaes, Johannes. 1974. *Punishment and Deterrence*. Ann Arbor: University of Michigan Press.

Barlow, Melissa, David Barlow, and Theodore Chiricos. 1995. Economic conditions and ideologies of crime in the media: A content analysis of crime news. *Crime and Delinquency* 44:3–19.

Beccaria, Ceasare. 1764/1963. *An Essay on Crimes and Punishments*. Indianapolis: Bobbs-Merrill.

Bentham, Jeremy. 1789/1988. *The Principles of Morals and Legislation*. Buffalo, NY: Prometheus Books.

Boman, John H. 2013. Deterrence theory and peers: Do close friends perceive sanction certainty in a similar manner? *American Journal of Criminal Justice* 38:266–275.

Braithwaite, John. 1989. *Crime, Shame, and Reintegration*. Cambridge: Cambridge University Press.

Brown, Don. 1978. Arrest rates and crime rates: When does the tipping effect occur? *Social Forces* 57:671–682.

Carmichael, Stephanie, Lynn Langton, Gretchen Leuking, John Reitzel, and Alex Piquero. 2005. Do the experiential and deterrent effect operate differently across gender? *Journal of Criminal Justice* 33:267–276.

Chamlin, Mitchell. 1991. A longitudinal analysis of the arrest-crime relationship: A further examination of the tipping effect. *Justice Quarterly* 8:187–199.

Cook, Phillip. 1980. Research in criminal deterrence: Laying the groundwork for the second decade. In *Crime and Justice: An Annual Review of Research*, vol. 2, eds. Michael Tonry and Norval Morris, 211–268. Chicago: University of Chicago Press.

Davis, F. James. 1952. Crime news in Colorado newspapers. *American Journal of Sociology* 57:325–330.
Doob, Anthony, and Cheryl Webster. 2003. Sentence severity and crime: Accepting the null hypothesis. *Crime and Justice* 30:143–195.
Durkheim, Emile. 1893/1933. *The Division of Labor in Society*. New York: The Free Press.
Erickson, Maynard, Jack Gibbs, and Gary Jensen. 1977. The deterrence doctrine and the perceived certainty of legal punishments. *American Sociological Review* 42:305–317.
Exum, M. Lyn. 2002. The application and robustness of the rational choice perspective in the study of angry and intoxicated intentions to aggress. *Criminology* 40:933–966.
Finly, Nancy, and Harold Grasmick. 1985. Gender roles and social control. *Sociological Spectrum* 5:317–330.
Garofalo, James. 1981. Crime and the mass media. *Journal of Research in Crime and Delinquency* 18:319–350.
Gibbs, Jack. 1975. *Crime, Punishment and Deterrence*. New York: Elsevier.
Gibbs, Jack. 1986. Deterrence theory and research. In *The Law as a Behavioral Instrument: Nebraska Symposium on Motivation*, ed. Gary B. Melton. Lincoln, NE: University of Nebraska.
Gottfredson, Michael, and Travis Hirschi. 1990. *A General Theory of Crime*. Stanford, CA: Stanford University Press.
Grogger, Jeffrey. 1991. Certainty versus severity of punishment. *Economic Inquiry* 29:297–309.
Jacobs, Bruce A. 2010. Deterrence and deterrability. *Criminology* 48:417–441.
Jensen, Gary. 1969. Crime doesn't pay: Correlates of a shared misunderstanding. *Social Problems* 17:189–201.
Jones, Terrence. 1976. The press as metropolitan monitor. *Public Opinion Quarterly* 40:239–244.
Kahneman, Daniel, and Amos Tversky. 1979. Prospect theory: An analysis of decision under risk. *Econometrica* 47:263–291.
Kleiman, Mark. 2009. *When Brute Force Fails*. Princeton, NJ: Princeton University Press.
Lewin, Kurt. 1935. The psychological situations of reward and punishment. In *A Dynamic Theory of Personality*, ed. Kurt Lewin. New York: McGraw-Hill.
Lochner, Lance. 2007. Individual perceptions of the criminal justice system. *American Economic Review* 97:446–460.
Logan, Charles. 1972. General deterrent effects of imprisonment. *Social Forces* 51:64–73.
Loughran, Thomas, Raymond Paternoster, Alex Piquero, and Greg Pogarsky. 2011. On ambiguity in perceptions of risk: Implications for criminal decision making and deterrence. *Criminology* 49:129–161.
Marsh, H. L. 1989. Newspaper crime coverage in the U.S. 1983–1988. *Criminal Justice Abstracts* 21:506–514.
McClellan, Steve. 1997. Crime spree on network news. *Broadcasting & Cable* 127:28, 30.
Mendes, Silvia, and Michael McDonald. 2001. Putting severity of punishment back in the deterrence package. *Policy Studies Journal* 29:588–610.
Nagin, Daniel. 1978. General deterrence: A review of the empirical evidence. In *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, eds. Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin. Washington, DC: National Academy Press.
Nagin, Daniel, and Raymond Paternoster. 1993. Enduring individual differences and rational choice theories of crime. *Law and Society Review* 27:467–496.
Nagin, Daniel, and Greg Pogarsky. 2001. Integrating celerity, impulsivity, and extralegal sanction threats into a model of general deterrence: Theory and evidence. *Criminology* 39:865–889.
Parker, Jerry, and Harold Grasmick. 1979. Linking actual and perceived certainty of punishment: An exploratory study of an untested proposition in deterrence theory. *Criminology* 17:366–379.
Paternoster, Raymond, and Alex Piquero. 1995. Reconceptualizing deterrence: An empirical test of personal and vicarious experiences. *Journal of Research in Crime and Delinquency* 32:251–286.
Piquero, Alex, and Raymond Paternoster. 1998. An application of Stafford and Warr's reconceptualization of deterrence to drinking and driving. *Journal of Research in Crime and Delinquency* 35:3–39.
Piquero, Alex, Raymond Paternoster, Greg Pogarsky, and Thomas Loughran. 2011. Elaborating the individual difference component in deterrence theory. *Annual Review of Law and Social Science* 7:335–360.
Piquero, Alex, and Greg Pogarsky. 2002. Beyond Stafford and Warr's reconceptualization of deterrence: Personal and vicarious experiences, impulsivity, and offending behavior. *Journal of Research in Crime and Delinquency* 39:153–186.
Pogarsky, Greg, and Alex Piquero. 2003. Can punishment encourage offending? Investigating the resetting effect. *Journal of Research in Crime and Delinquency* 40:95–120.
Richards, Pamela, and Charles Tittle. 1981. Gender and perceived chances of arrest. *Social Forces* 59:1182–1199.

Roberts, Julian. 1992. Public opinion, crime, and criminal justice. *Crime and Justice* 16:99–180.

Roshier, Bob. 1973. The selection of crime news by the press. In *The Manufacture of News*, eds. Stanley Cohen and Jock Young. London: Constable.

Ross, Laurence. 1992. *Confronting Drunk Driving: Social Policy for Saving Lives.* New Haven, CT: Yale University Press.

Scheff, Thomas J., and Suzanne M. Retzinger. 1991. *Emotions and Violence: Shame and Rage in Destructive Conflicts.* Lexington, MA: Lexington Books.

Sherman, Lawrence W. 1993. Defiance, deterrence, and irrelevance: A theory of the criminal sanction. *Journal of Research in Crime and Delinquency* 30:445–473.

Sherman, Lawrence W., and Douglas A. Smith. 1992. Crime, punishment and stake in conformity: Legal and informal control of domestic violence. *American Sociological Review* 57:680–690.

Simon, Herbert. 1957. *Models of Man.* New York: Wiley.

Smith, Douglas, and Patrick Gartin. 1989. Specifying specific deterrence: The influence of arrest on future criminal activity. *American Sociological Review* 54:94–106.

Smith, Douglas, and Raymond Paternoster. 1987. The gender gap in theories of deviance: Issues and evidence. *Journal of Research in Crime and Delinquency* 24:140–172.

Stafford, Mark, and Mark Warr. 1993. A reconceptualization of general and specific deterrence. *Journal of Research in Crime and Delinquency* 30:123–135.

Tittle, Charles. 1969. Crime rates and legal sanctions. *Social Problems* 16:409–423.

Tittle, Charles, and Alan Rowe. 1974. Certainty of arrest and crime rates: A further test of the deterrence hypothesis. *Social Forces* 52:455–462.

Tunnell, Kenneth. 1992. *Choosing Crime.* Chicago: Nelson-Hall.

Tyler, Tom. 1990. *Why People Obey the Law.* New Haven, CT: Yale University Press.

U.S. Bureau of Justice Statistics. 2010. *Criminal Victimization in the United States—Statistical Tables.* Tables for 2007. Washington, DC: Bureau of Justice Statistics. Available online at http://bjs.ojp.usdoj.gov/content/pub/pdf/cvus/current/cv0737.pdf.

U.S. Federal Bureau of Investigation. 2008. *Crime in the United States—2007.* Washington, DC: U.S. Government Printing Office.

Williams, Kirk, and Richard Hawkins. 1986. Perceptual research on general deterrence: A critical review. *Law and Society Review* 20:545–572.

Wright, Bradley, Avshalom Caspi, Terrie Moffitt, and Ray Paternoster. 2004. Does the perceived risk of punishment deter criminally prone individuals? Rational choice, self-control, and crime. *Journal of Research in Crime and Delinquency* 41:180–213.

Yu, Jiang, and Allen Liska. 1993. The certainty of punishment: A reference group effect and functional form. *Criminology* 31:447–464.

Zimring, Franklin, and Gordon Hawkins. 1973. *Deterrence: The Legal Threat in Crime Control.* Chicago: University of Chicago Press.

# 3

# DETERRENCE AND THE RATIONAL CHOICE MODEL OF CRIMINAL BEHAVIOR

## The Case of the Disappearing Theory

### The Rational Choice Model

Scholars' assumptions about the deterrent effects of legal punishment are largely grounded in a specific theory of criminal behavior commonly called the rational choice model (RCM). This model has been applied to the explanation of virtually all human behaviors, not just criminal behavior, and is particularly prominent as an explanation of economic behavior. Scholars trained as economists are especially likely to rely on it when explaining criminal behavior, but they are by no means the only ones. While the "economic approach" to crime of economist Gary Becker (1968) was basically a rational choice theory, the same basic model underlies the "calculus of pleasure and pain" of utilitarian philosopher Jeremy Bentham (1789), the "rational choice" and "situational crime prevention" perspectives of criminologists Clarke and Cornish (2001), and the "routine activities theory" of sociologists Cohen and Felson (1979).

The subtitle of this chapter is not intended to suggest that the RCM is becoming less popular as a way of explaining criminal behavior. Quite the contrary—its popularity has grown in recent decades. It is instead intended to convey the notion that the theory as it once existed in a simple form—with clear, easily tested predictions and clear implications for crime control policy—has gradually disappeared as the theory was modified to better comport with empirical evidence of human behavior.

The simplest version of the RCM was based on neoclassical economics' principle of the maximization of expected utility. It proposed that individuals choose among alternative courses of action, such as achieving goals via criminal means versus doing so via lawful means, and that this choice is made on the basis of a comparison of the anticipated costs (risks, losses) and benefits (pleasures, profits, gains) of each alternative. Each cost or benefit is weighted by the value that the actor attributes to it and by the probability of each cost or benefit being experienced; the sum of these weighted costs and benefits is called the "expected utility" of that choice. The theory predicts that, on average, people will choose the alternative course of action with the highest expected utility. In short, humans are regarded as utility maximizers (Bentham 1789; Becker 1968; Ehrlich 1973). Early versions of this theory sometimes seemed to imply that choices were made consciously, in a deliberate way, and possibly even with some sort of "calculation" of anticipated costs and benefits (Beccaria 1764; Bentham 1789), but few contemporary adherents would accept this view (Levitt and Miles 2007).

One of the basic problems in assessing the RCM is whether it can be defined solely by the process that supposedly goes into making decisions, irrespective of the results produced by that process. If people consider costs and benefits, as they understand them, in making decisions, but consistently make decisions that do not maximize utility and that produce substantially suboptimal results, does such a pattern of behavior still support the RCM? One obvious reason the *process* might in some sense be "rational" yet consistently lead to suboptimal *results* is that people usually possess very little accurate information about likely costs and benefits of alternative courses of action and take account of only a fraction of what they do possess. Thus, limited or poor quality information might be assessed rationally, leading to suboptimal decisions.

## The Significance of Limits on Information

Expected utility theory has two variants. The first could be called "objective expected utility theory" and the second "subjective expected utility theory." The difference between the two is that the objective version assumes that the actors accurately know the actual probabilities of contingencies (such as arrest), whereas the subjective version makes no such assumption. Instead, it assumes that people have a variety of subjective, often erroneous ideas about the probabilities. In applications to populations, however, scholars applying the subjective variant of the theory effectively assume that the expected value (mean) of the distribution of subjective estimates in a population equals the actual probability (Cook 1980; Nagin 1998, 10). Thus, they assume a correspondence between perception and reality in the aggregate, even though there may be substantial deviation of perceptions from reality for any one individual and consideration variation in the degree of this deviation across individuals.

There are probably few, if any, remaining advocates of the objective version of the expected utility theory today. Almost from the beginning, those who adhered to various versions of RCM, going back at least as far Jeremy Bentham (1789), had acknowledged the imperfections of human knowledge in making decisions. More recently, concerns about this problem were forcefully articulated by Herbert Simon (1957), who argued for a modified version of the RCM revolving around the concept of "bounded rationality." Simon pointed out that virtually all real-world decisions are bounded by significant limits on the information possessed by actors. Not only do actors possess limited relevant information, but also much of the information they possess is inaccurate. Thus, decisions are only rational within these information limits—actors assess costs and benefits of alternate courses of action, but these are only a subset of the possible consequences of each course, and each consequence is only imperfectly understood.

The growing recognition of the importance of limits on information about crime and its punishment (e.g., Jacob 1979; Cook 1980) necessitated important changes in the RCM. The most extreme variant of a subjective RCM would be one that assumed that perceived contingencies (costs and benefits) bear *no* relationship to actual contingencies, which would mean that knowing actual contingencies, by itself, would provide no ability to predict or explain criminal decisions. People might make decisions consistent with their understanding of the consequences of different decisions, but their understanding would not correspond at all with actual consequences. Under these conditions, decisions would routinely be suboptimal, failing to maximize utility. Economists such as Becker (1968) acknowledged, in a *pro forma* way, that there were limits on human abilities to acquire, store, and retrieve information, but nevertheless held to a version of expected utility theory that was traditional in that it effectively assumed enough correspondence between perceived and actual contingencies that one could still predict the *average* response of *populations* to shifts in actual contingencies, such as punishment risks. Thus, one could predict the average response of the population of prospective criminals to changes in the certainty or severity of legal punishment

(Cook 1980, 220, 225). Actual applications of expected utility theory to criminal behavior, however, almost never attempt to model real-world limitations on information and how these limits affect decisions (Williams and Hawkins 1986, 548). Nor has there been any empirical demonstration that the average perceptions of punishment levels of populations correspond closely to actual punishment levels.

The deterrence doctrine underlying punitive crime control policies can be seen as a derivative of the RCM. One compact statement of the deterrence doctrine, derived from Beccaria and Bentham, was: *the greater the certainty, severity, and swiftness of punishment, the lower the crime rate* (e.g., Gibbs 1975, 5). This bald statement of statistical patterns is, however, based on a background theory of *why* such patterns should prevail: people decide whether to commit crimes, and this decision is influenced by, among other things, the risks of legal punishment. Thus, the deterrence doctrine is essentially an application of the RCM to criminal behavior and its control, but one in which the focus is selectively concentrated on costs rather than benefits, and the focus on costs in turn is concentrated even more narrowly on the legal risks of punishment as the primary or sole cost explicitly considered.

Leaving aside *pro forma* acknowledgements of the possible difference between the reality and perception of legal punishments, economists carrying out empirical research on crime have given virtually no attention to the process by which perceptions of risk are formed, and have not given any serious consideration to the possibility that actual legal risks may have little or no impact on those perceptions. Many say nothing about the issue (e.g., Mendes and McDonald 2001), while others fall back on the faith that the average perceptions of populations will correspond reasonably close to actual risks (Cook 1980). No research procedures are implemented to test these assumptions. This may be a reflection of the preference among economists for macro-level research on populations, rather than individual-level research and skepticism about the validity of responses to questions asked in the survey research that is nearly always used to measure risk perceptions of individual persons. Without survey-based or other measures of individual perceptions of legal risk, however, economists can only indirectly test the proposition that legal punishments deter crime, inferring it from negative associations between aggregate levels of punishment and crime rates. Indeed, some economists writing on deterrence seem barely aware of the existence of a body of individual-level research that directly tests the links between measured perceptions of legal risk and criminal behavior and how enormous this body of evidence is (e.g., Levitt 2002, 435).

Economists hold to a strong disciplinary faith, based on price theory, that higher costs of a behavior will, other things being equal, reduce the frequency of that behavior. This faith appears to be unshakable among many economists. One distinguished economist, Ronald Coase, put it bluntly: "Punishment, for example, can be regarded as the price of crime. An economist will not debate whether increased punishment will reduce crime; he will merely try to answer the question, by how much?" (1978, 210). Or, as another economist put it, the economic perspective "leads naturally to a presumption that deterrence works—that crime rates will be inversely related to the likelihood and severity of punishment" (Marie 2013). The rigid unwillingness of many economists to deviate from this simple proposition might be attributable to the fear that if it were to prove false, it would cast much of economic theory into doubt. Some may therefore cling to an ultra-orthodox set of beliefs about what economic theory predicts about crime and punishment and are loathe to accept empirical results that seem to challenge these beliefs (e.g., Levitt and Miles 2007).

A more sophisticated economic theory, however, need not be threatened by findings that more punishment does not always (or even usually) lead to less crime by explicitly acknowledging that both individual and average population perceptions of legal risk can deviate substantially from actual risks and, conceivably, have little or no correlation with them. Thus, even though increased punishment frequently would have no effect on crime rates, a more sophisticated version of economic theory would be perfectly compatible with such a null finding. The core idea that people are influenced by their perceptions of cost would remain unchallenged, but it would be recognized that the strength of the perception/reality link may be far weaker for legal sanctions than it is for the prices of consumer goods, stocks, labor, or other costs traditionally addressed by economics.

## Weak or Invalid Criticisms of the RCM as Applied to the Deterrence Doctrine

The deterrence doctrine has been subjected to a substantial amount of fairly shallow, irrelevant, or ill-considered criticism, and it is worth taking time to dispose of these, both for the sake of eliminating distracting misconceptions and to make it clear that the present critique does not in any way rely on them. First, it should be understood that the deterrence doctrine cannot be argued or reasoned away on purely *a priori* logical grounds. Attempts to show that deterrence-based crime control policies "cannot work," based solely on logical argumentation and "common sense" have generally amounted to little more than one-sided pleading of a case in which logic and facts are selectively applied in the service of antipunishment policy conclusions. Some examples follow.

### *"Criminals Are Not Rational"*

Some critics argue that criminals act impulsively, do not consider legal risks, or consider only the short-run consequences of their actions, so punishment does not deter crime. The problem with these criticisms is that, because they apply only to those who commit crime, they are perfectly consistent with the RCM. Those who believe there are strong deterrent effects of legal punishment would respond that (1) more "rational" people take account of legal risks and consequently refrain from crime, while (2) less "rational" people fail to take account of these risks and commit crime. The less "rational" character of criminals is thus perfectly compatible with any version of the RCM that permits the degree of rationality (in the sense of taking account of potential consequences such as legal punishment) to vary across individuals. Whether any traditional or orthodox version of the RCM allows such variation is another matter.

### *"The Warden's Fallacy"*

The failure to understand this point leads to what has been called "the warden's fallacy." Lewis Lawes, the warden of San Quentin prison reported that all the inmates on death row with whom he had spoken had told him they did not consider the death penalty before committing their murders, leading Mr. Lawes to conclude that the death penalty does not deter (cited by Zimring and Hawkins 1973, 31). Some scholars have drawn similar conclusions, based on similar lines of reasoning (e.g., Bohm 2007, 189; Doob and Webster 2003, 181–184). The warden's mistake was in drawing conclusions based solely on the study of criminals, who are all, by definition, failures of deterrence (Andenaes 1966, 955). One cannot judge the number of successes of deterrence based on the number of failures, nor will such knowledge allow one to know whether the successes outnumber failures. The fact that a criminal has committed a crime necessarily indicates that deterrence has failed, but then it also indicates the failure of all other crime-suppressing factors or efforts at crime prevention, of whatever character, including poverty reduction, conscientious parenting, and offender treatment programs. The continued existence of crime as a serious social problem obviously demonstrates that all currently operating crime preventive measures taken together are not completely effective. Observing that some criminals, at some times, do not consider the possibility of legal punishment is of relevance to the efficacy of deterrence only in that it confirms the already self-evident point that the deterrent effects of punishment are not totally effective in preventing all crime. The contrary, albeit equally extreme, possibility is that all those who *refrained* from crime did so solely because they *did* take possible legal penalties into account and were thereby deterred. For every offender who was not deterred by the prospect of punishment, there might be a hundred who were deterred. One simple methodological implication is that one cannot estimate the deterrent effect of punishment on crime by studying only criminals—it requires the comparison of less criminal persons with more criminal persons.

### *"Some Types of Crime Are Committed Impulsively, Under Conditions of Emotional Stress, in Which People Do Not Take Account of Legal Risks"*

At best, this criticism implies a limit to the application of RCM; at worst it is no criticism at all. More than a half-century ago, Andenaes (1952) argued that the threat of legal punishment can reduce criminal behavior by strengthening law-abiding habits, which in turn can govern even highly emotional behaviors, since habits operate automatically and require little conscious or extended mental effort. Similarly, Cook (1980), following Simon (1957), argued that people develop "standing rules" of behavior, which they are capable of applying even under emotionally charged circumstances because the rules have become habitual and can be automatically applied with little conscious or prolonged thought. These habits or standing rules originally developed over a long period of time, under calmer circumstances, and might have originally been influenced by calm and deliberate consideration of legal risks of criminal behavior. On the other hand, if a person had not developed an applicable standing rule with regard to a particular potential crime, stressful circumstances might well interfere with the consideration of legal risks. For some people, under some circumstances, then, decisions are less "rational" and involve less consideration of the risks of punishment. Conversely, some people, under some circumstances, *do* consider the prospect of punishment and might thereby be deterred from committing the crime they had contemplated.

### *"Criminals Do Not Calculate the Consequences of Their Actions" (e.g., Bohm 2007, 188)*

This criticism seems to be based on the view that deterrence theory presupposes offenders who either literally compute risks and benefits of crime, doing some sort of rough arithmetic in their heads, or at least that criminals engage in some kind of deliberate, prolonged, conscious thought about risks. Deterrence theory, or at least any modern variant that is more than a straw man, does not necessarily assume any such thing. Deterrence theory assumes that prospective offenders *somehow* take account of legal risks. There may be calculation in some sense, but thoughts about risks can be unconscious as well as conscious, can be brief as well as prolonged, and can be emotionally charged as well as coldly deliberative. Indeed, deterrent effects may depend as much on fear as on calm deliberation, and fear can be experienced in seconds.

The human brain certainly works fast enough that consideration of possible consequences could transpire in a few seconds, and there is no evidence that decisions to commit even the most impulsive, least premeditated crimes are made in less time than that. This does not mean, however, that the degree of premeditation is irrelevant to deterrence. Contemplating the commission of a crime for a longer period of time favors more consideration of its possible consequences, including legal punishment. Thus, crimes that are more premeditated, and prospective criminals who give more prolonged thought to a crime, may well be subject to *more* deterrent effects of possible punishment. Nevertheless, even the least premeditated crimes, and the most impulsive criminals, could be subject to *some* deterrent effects.

Conversely, deterrence theory does not necessarily predict that even very prolonged deliberation about a crime will always cause a prospective offender to refrain from committing the crime. It may instead merely result in more careful planning to avoid being caught, which reduces anxiety about whatever punishments might be inflicted if the offender were caught. Ethnographic research on active offenders, however, indicates that most "street" crime is opportunistic, does not involve elaborate planning, and potential costs are given relatively little consideration (Jacobs 2000; Jacobs, Topalli, and Wright 2003; Shover 1996; Wright and Decker 1994, 1997). Even when offenders do consider the potential costs of crime, they also take account of their ability to manage or eliminate these potential costs, which negates to some extent the deterrent impact of punishment threats (Jacobs and Miller 1998). This body of research suggests that criminals are extremely confident—perhaps too confident—about their abilities to control a situation and minimize the risk of arrest (Jacobs 2000; Wright and Decker 1997). Thus, even prolonged consideration of costs such as legal punishment does not guarantee a deterrent effect; rather, deterrence is merely more likely if such consideration occurs.

### *"Most Criminals Who Are Punished Continue Committing Crimes Thereafter, Proving That Deterrence Does Not Work"*

This criticism confuses specific deterrence with general deterrence and is commonly based on evidence that casts doubt on the former but has no necessary bearing on the latter. Psychologists Andrews and Bonta (2006, 378–380) argued that "get tough" policies to reduce crime do not work, and they devoted a long discussion to explaining why punishment does not work (386–392). They cite numerous laboratory studies by psychologists that establish the conditions for effective punishment and argue that these conditions do not prevail in the criminal justice system's punishment of criminals. Their entire discussion, however, was relevant only to specific deterrence, i.e. the effects of punishment on the behavior of the person punished. The vast majority of both theoretical and criminological argumentation, as well as much of the advocacy in the public policy arena in favor of "get tough" measures, is based instead on the concept of *general* deterrence, i.e. the effects of the punishment of a relatively few criminals on the behavior of the general population, most of whom are not punished. That is, the general deterrence argument is that, regardless of whether punishment makes punished persons stop committing crime, it will discourage the rest of the population from committing crimes. It will help keep the law-abiding obeying the law even if it does not make punished criminals stop doing crime. Andrews and Bonta did not even discuss general deterrence, and showed no signs of being aware of the distinction between the effects of punishment on punished persons and its effects on the rest of the population. Because only a tiny fraction of all actual and potential criminals are punished, the potential aggregate impact of specific deterrence is slight compared to the potential impact of general deterrence, and this may help explain why, as we shall see, so much less research has focused on specific deterrence than on general deterrence. Further, a wide array of detrimental effects of legal punishment on the punished (discussed in Chapter 11) may cancel out much of any crime-reducing effects of specific deterrence, rendering it undetectable.

## Arguably Valid Criticisms of the Model

Other criticisms of the RCM are at least arguably valid or are irresolvable. The model's use of the word "choice" is, for many, meaningful only if one assumes that human behavior is to some degree voluntaristic—that is, not entirely determined by biological or environmental factors beyond the actor's control. One highly deterministic psychological view is that human behaviors are completely determined by their contingencies, e.g. by the positive and negative reinforcements and punishments for different behaviors. People learn to repeat the behaviors that have been reinforced in the past and avoid those that have not. This is the position taken by behaviorist psychologists such as B. F. Skinner (1978), but similarly deterministic positions have been staked out by biologically-oriented psychologists like Hans Eysenck (1964) who stress genetic and hormonal causes of behavior, and by those who argue that human behavior is determined by the social and economic environments in which each actor exists (Bonger 1916/1967). There is considerable evidence from behavioral genetics that most or all complex human behaviors, and antisocial behavior in particular, are influenced to some degree by genetic factors, usually operating in conjunction with environmental factors (Rhee and Waldman 2002; Ferguson 2010). Given that people have no ability to control their genetic inheritance, this certainly implies significant limits on the scope of human choice, if not the death of free will. Many would say that if free will is denied, the conventional understanding of human choice must be abandoned as well. And if humans do not choose, they cannot choose rationally. It remains very much in doubt, among both philosophers and scientists, whether humans possess free will (Mele 2009).

On the other hand, one might sidestep the thorny issue of free will by arguing that the conventional lay understanding of "choice" is not essential to the RCM model. "Choice" could instead be reinterpreted as merely a convenient verbal shorthand for an unknown process that does not necessarily involve any exercise of free will but instead could describe a predetermined reaction to a set of environmental contingencies without committing one to exactly how the reaction comes about. This was the position adopted by Skinner, who regarded the process of "choosing" courses of action as an unknown "black box" process. Criminologist Travis Hirschi argued that causation as understood by positivists does not preclude choice, since some of the causes of a criminal or delinquent act can be the "calculations and desires of the actor himself" that go into the criminal choice (1986, 112). Cornish and Clarke asserted that the "rational choice perspective is . . . neutral with respect to the free will-determinism debate" (1986b, 13). This position, however, seems to require distorting the ordinary sense of the word "choice," since one suspects that, for many, free will is an inherent part of the meaning of "choice."

From the standpoint of judging the effectiveness of crime control policies, it may not be necessary to adopt a position on free will. What matters is whether raising penalties or reducing rewards for criminal behavior somehow reduces crime. Whether people make conscious free will decisions to refrain from crime or respond in an automatic and predetermined way to changes in legal risks might be irrelevant from that standpoint. The issue is, however, significant from a moral standpoint, since the absence of free will would make it harder to morally justify the punitive crime-control policies that many believe follow from the RCM. Many would find it unfair to punish people for acts that they had no power to refrain from, even if punishment did result in less crime.

Alternatively, those who want to apply the RCM to human action but are uncomfortable with a totally deterministic model of human behavior can adopt a position of soft determinism, or "conditional free will." That is, people may make choices from within sets of options that are more limited for some than for others. They are hemmed in by constraints on the alternative courses of action that are realistically available to them, their knowledge of the options available, and their ability to acquire, retain, and process information. The array of realistically available choices may be dependent on the income, education, power, and other resources a person possesses, so that the freedom to choose is not a species-wide constant, but a factor that varies across individual humans. Some individuals may have many very attractive options to choose from while others have only a few options, none of them very attractive. Thus, "free will" could be regarded as both limited and a property that varies across individuals rather than a humanity-wide constant.

The concept of bounded or situational rationality implies that people make choices but only within a set of conditions that limit what choices are realistically available to them. As Felson (1986, 119) put it, "people make choices, but they cannot choose the choices available to them." Further, their choices are made rationally but within the limits of available information about options and their costs and benefits, and limits on their ability to acquire and process the information. In any case, it is not at all clear whether the RCM requires free will and whether a deterministic world would render it irrelevant as a theory of human behavior.

## Stronger Criticisms of the Model

Any theory may be modified, and the RCM is no exception. As empirical evidence has exposed flaws and limitations in its earlier, simpler versions, the theory has been modified to take account of new findings. As this happened, however, the theory moved further and further away from the earlier versions that were so intuitively appealing and seemed to have such clear implications for crime control policy. In particular, recognition of the perspective's limits, and the revisions made in response to that recognition, have cast increasing doubt on the proposition that increasing punishment will reduce crime by producing more deterrent effect.

### *Severe Limits on Information Regarding Legal Punishment*

There are numerous ways in which the theory has had to be modified to be consistent with research evidence, based on the recognition that humans make decisions in ways that seem contrary to what an objective and accurate assessment of costs and benefits would predict and, in that sense, seem to be "irrational." First, people have limited capacities or inclinations to acquire, retain, and later make use of information, so their decisions are only "rational" within those limits (Simon 1957), which are typically substantial, especially with respect to crime and punishment (Kleck 2003). Defending the deterrence doctrine, Cook (1980, 220) argued that deterrent effects could still exist in the face of limited information about the prospects of punishment, implying that incorporating these limits required only a modest revision of deterrence theory. He did not, however, consider the implications of information being so severely limited for so many people that perceived legal risks, even when considered in the aggregate across populations, bore little or no relationship to actual risks. Research reported in Chapter 9 indicates that this is more than a mere hypothetical possibility.

### *People Do Not Consistently Decide in Accordance With the Principle of Expected Utility Maximization*

Secondly, research has shown that people are not actually thorough-going utility maximizers in making decisions, even with respect to what they *believe* are the prospective costs and benefits of alternatives. Psychologists who study decision-making have long noted that people make use of very little of the information available to them and simplify the process by taking account of only a few of the contingencies of which they are aware. Thus, for example, prospective criminals may think only about the rewards of crime but give little or no thought to its costs (Carroll 1978).

Further, setting aside limits on information that people possess, and limits on their ability to take into account much of the information they do possess, people also do not consistently act in accordance with decision rules that would produce optimal outcomes. Psychologists Amos Tversky and Daniel Kahneman (1974; Kahneman and Tversky 1979) laid the groundwork for the field of "behavioral economics," demonstrating in a long series of experiments that decisions made under conditions of uncertainty show persistent deviations from utility maximization. They found that people faced with the need to decide under conditions of uncertainty make use of "a limited number of heuristic principles which reduce the complex tasks of assessing probabilities and predicting values to simpler judgmental operations" (1974, 1124). These simplified decision rules may sometimes produce reasonably successful outcomes, but they also result in consistent deviations from what utility maximization would predict, yielding decisions that, from the standpoint of an orthodox RCM, are "irrational."

In a typical experiment, Tversky and Kahneman would present subjects with various sets of hypothetical choices in which the value and probability of the choices were clearly stated and ask the subjects to choose. Their results indicated that people deviate from simple rationality in consistent ways that the authors called "biases." A few of the biases that are especially relevant to criminal decision-making are as follows:

1. People tend to ignore the "prior probability" of an outcome of a decision and thus ignore the "base rate," or population-wide probability, of the outcome occurring. One reading of this finding, with respect to the deterrent effects of punishment, is that even if people knew the population-wide probability of being caught when committing a crime they would not be significantly influenced by this knowledge. This may be because people give far more weight to their own experiences than to those of others, even if their own experiences may be very limited, may be substantially unrepresentative of the experiences of the entire population, and may be unlikely to be repeated in the future, and even if the experiences of a large population would provide a more reliable basis for forecasting future risks for the individual.
2. People tend to give too much weight to information that is consistent with their prior expectations, even if those expectations are unrealistic, and too little weight to information contrary to expectations. Thus, people who start out optimistic about their chances of avoiding punishment may largely ignore information concerning friends and acquaintances getting caught and punished for crimes, because it is contrary to their expectations and thus discounted as unrepresentative of the experiences of all criminals.
3. People take little account of information that is less "available." Information about the possible consequences of a decision is more available if it is more emotionally salient. Thus, something one has personally experienced or witnessed is more available than something one has merely heard or read about. Likewise, more recently acquired information is more available than information acquired in the more remote past. The former is given more weight in making a choice, even if it is not representative of all information acquired, and thus ineffective in predicting the likely consequences of a future choice.
4. People tend to ignore low probability contingencies altogether, but when they do take them into account they give them excessively large weight. Legal punishment, of nearly all crimes except murder, is just such a low-probability contingency. Either of these biases could result in less rational decisions because they do not accurately reflect actual probabilities of punishment.
5. In simplifying information related to a decision, people round probabilities and outcomes. Thus, a probability of punishment of 0.01 or 0.02 would be rounded to 0, while a probability of 0.98 or 0.99 would be rounded to 1, i.e. a certainty. The former would work against deterrence of crime while the latter would work in its favor. In practice, however, only rounding down to zero is likely because the probabilities of punishment for nearly all crimes are quite low and are almost never anywhere near one.
6. People start with initial estimates of a risk, but when exposed to further information that calls for revising the estimate, they make insufficient adjustments to their initial estimate. Thus, if the actual risks of punishment for crime were to go up, prospective offenders might adjust their own estimates of risk upward but would tend to do so inadequately, making small adjustments even in response to information indicating large changes in actual risk. On the optimistic side, if actual risks declined, offenders would also tend to inadequately adjust their assessments of risk downward. Either way, the implication is that criminal justice policies producing changes in the risk of punishment will have weaker effects than a simple RCM would predict.

7. When tasks require the success of multiple elements (e. g., all criminal confederates in a group crime properly carrying out their tasks), people tend to underestimate the probability of failure, because they do not take account of how vulnerable the task is to just one participant failing. This helps explain why some criminals tend to be unduly optimistic about their chances of avoiding being caught and punished, and as a result they are not deterred by risks of punishment that may actually be fairly substantial.

Defending the deterrence doctrine, Cook (1980, 218–222) acknowledged some of these deviations from utility-maximizing rationality, but instead of discussing them as fundamental challenges to deterrence theory, presented them as if they merely implied a need for modest revisions to a RCM of criminal behavior, and thus only mild changes in what could be reasonably expected from deterrence-based crime control policies. He did not confront the question: If new evidence necessitates making more and more revisions to a theory, is there not eventually a point where it ceases to be the same theory?

Within economics, the discoveries of behavioral economists encouraged a shift away from the subjective expected utility model of decision-making to Kahneman and Tversky's (1979) prospect theory, a fundamentally different model of human decision-making. The expected utility theory that guided the work of Becker (1968) and Ehrlich (1973, 1975) has lost favor even among economists. As economists Lattimore and Witte (1986, 148) put it, "in recent years the [expected utility] model has come under increasing attack, as neither the model's predictions nor its behavioral axioms appear to closely reflect actual decision-making behavior in either the criminal or other decision-making realms." Oddly enough, adherence to the expected utility model seems to have survived more among some criminologists trained as sociologists or psychologists than among economists (e.g., Matsueda, Kreager, and Huizinga 2006; Cornish and Clarke 1986b).

It is hard to view prospect theory as nothing more than a modest revision of the RCM since it implies radically different predictions in too many important ways, with respect to too many relevant factors, that apply in too many decision-making situations to be considered a mere revision (for a thorough summary and comparison of the theories, see Lattimore and Witte 1986). Expected utility theory implies a clear prediction about what would happen if more punishment were inflicted on criminals—other things being equal, criminal behavior would decline. In sharp contrast, prospect theory does not offer any clear prediction at all on this point.

Prospect theory proposes that people make decisions under conditions of uncertainty in two phases. In the "editing" stage, the possible choices the person might select are filtered and simplified—some options are ignored, while others are assessed in ways that account for only a few of the possible consequences that might follow from choosing the option. In the second phase, the person evaluates these simplified options and chooses the most highly valued option.

One possible implication of prospect theory for criminal decisions is that a prospective offender may "edit out" consideration of legal punishment altogether. Research testing prospect theory indicates that part of the editing process of decision-making involves people treating low probability consequences the same as if their probability was zero. Conventional applications of the RCM do not consider such a possibility and simply assume that the more likely punishment is, the less likely it is that crime will be chosen. That is, even variation in certainty at the low end of the scale is assumed to affect criminal conduct. This is especially problematic for crime control strategies based on the RCM, since actual probabilities of legal punishment for almost all crimes cluster at the very low end. Except for a few very serious violent crimes, the probability of crimes resulting in arrest, conviction, and punishment is generally under 0.1 (Table 3.1), i.e. low enough that they might be effectively treated as if they were zero.

**TABLE 3.1** The probability that crime will result in incarceration, U.S. 2014

| *Offense Type:* | *Murder, Nonnegligent Manslaughter* | *Rape* | *Robbery* | *Aggravated Assault* | *Burglary* | *Larceny* | *Motor Vehicle Theft* |
|---|---|---|---|---|---|---|---|
| Total Offenses[a] | 14,249 | 284,350 | 664,210 | 1,092,090 | 2,993,480 | 11,760,620 | 534,570 |
| Offenses Known to Police[c] | (14,249) | 116,645 | 352,802 | 741,291 | 1,729,806 | 5,858,496 | 689,527[b] |
| % Known to Police[d] | (100) | 41.0 | 53.1 | 67.9 | 57.8 | 49.8 | (100.0) |
| % Cleared by Arrest[e] | 64.5 | 38.5 | 29.6 | 56.3 | 13.6 | 23.0 | 12.8 |
| % Convicted[f] | 70 | 68 | 66 | 56[g] | 68 | 66 | 67 |
| % Sentenced to Prison or Jail[f] | 100 | 89 | 89 | 81 | 79 | 72 | 77 |
| Probability That Crime Will Result in Prison[g] | 0.45 | 0.10 | 0.09 | 0.17 | 0.04 | 0.05 | 0.07 |

*Notes*:

[a]Based on estimates of number of crime incidents, whether reported to police or not, from National Crime Victimization Survey (NCVS) (U.S. Bureau of Justice Statistics 2015).

[b]This is not a typo. The number of motor vehicle thefts known to police was larger than the total number of such crime as estimated in the NCVS.

[c]Crime incidents reported or otherwise known to police. Based on *Uniform Crime Reports* (U.S. Federal Bureau of Investigation 2015).

[d]This is the ratio of offenses known to police over the number of crime incidents as estimated by the NCVS. Numbers in parentheses are assumed.

[e]Percent of crimes known to police that were cleared by arrest of one or more offenders believed to have committed the crimes or by "exceptional means" (U.S. Federal Bureau of Investigation 2015).

[f]These figures pertain to courts in the 75 largest counties in the U.S., as of 2009. There are no corresponding data for the U.S. as a whole or for the 75 largest counties for more recent years.

[g](% reported to police/100) × (% cleared by arrest/100) × (% convicted/100) × (% of convicted defendants who were sentenced to prison or jail/100).

### *Instrumental Rationality Does Not Apply in the Presence of Strong Moral Norms*

The conventional RCM assumes that people take into account their subjective assessments of the costs and benefits of a potential criminal act in deciding whether to commit the crime. Empirical attempts to directly study this process, however, suggest that people take account of instrumental incentives and disincentives only if they have not internalized moral norms against the acts. For those with strong moral objections to the prospective crime, consideration of its other costs and benefits are simply irrelevant and have no effect on their decisions whether to commit crimes (Kroneberg, Heintze, and Mehlkop 2010).

### *People Do Not Necessarily Make Separate Decisions Whether to Commit Individual Crimes*

Some applications of the RCM seemed to assume that people make separate decisions each day as to whether they will commit a crime. Somewhat more sophisticated variants (Cook 1980), however, suggest that people may gradually develop "standing rules" to act (or not) on criminal opportunities rather than repeatedly making short-term decisions immediately preceding each potential crime. If the standing rule is to not take advantage of such opportunities, it does not have to be renewed each day but remains in force until important changes in circumstances produce a change. The costs and benefits associated with a particular opportunity are largely irrelevant to those who have adopted such a standing rule. In light of the preceding discussion, these considerations may be irrelevant to most people because they have internalized strong moral norms forbidding the crimes. The decision to commit specific crimes is influenced by the anticipated pay off and perceived risks of the specific criminal opportunity being considered only if a person's standing rule is to take advantage of sufficiently attractive opportunities to commit crime.

If this model is correct, deterrence is relevant to criminal behavior primarily with respect to the initial development of the standing rule to not act on criminal opportunities. This is something that may have occurred in the remote past but is less relevant with regard to decisions made in the present, which are largely habitual. Conversely, for criminals, the prospect of legal punishment may be insufficient for the actor to adopt a standing rule to pass up criminal opportunities but could influence decisions as to whether to commit particular crimes at particular times and places. This might result in an offender refraining altogether from committing the crime, or it might merely induce the offender to modify his tactics to manage the risk or seek a more attractive target—the offender might merely be displaced rather than deterred. Thus, from this perspective, deterrence-based policies seem more likely to yield substantial crime control benefits if they increase the numbers of people who adopt standing rules to not commit crime than if they merely increase target-specific risks of punishment.

### People Do Not Decide in Social Isolation

People are social beings, not just isolated individual actors. They respond to what they believe are the likes and dislikes of those around them, as well as being guided by their own likes and dislikes. While one could imagine a version of the RCM in which actors are embedded in networks of social links to other people, this certainly does not describe the versions actually applied to criminal behavior by the perspective's contemporary adherents. Instead, these applications seem to conceptualize actors as isolated assessors of what will help or hurt themselves, without regard to the views of others. Assuming such narrow individualistic selfishness as the norm among humans is not so much a realistic assessment of the ways things are as it is an unduly constricted simplification of how people make decisions. Even Bentham incorporated many *social* "pains" and "pleasures" in his utilitarian model of human choice, including the "pleasures" of possessing a good name, or of acting benevolently, and the "pains" of "an ill name," or experiencing the enmity of others (1789, 36–39). Modern variants of the RCM, as operationalized in empirical research, may require a substantial shift in emphasis to make them more realistic in this respect, stressing how criminal acts and their punishment might produce purely social harms or benefits, such as damage to relationships with those who disapprove of the acts or strengthening of relationships with those who approve and admire the acts. Certainly, sociological theories of criminal behavior, from differential association theory (Sutherland 1947) to social control (Hirschi 1969), social learning (Akers 1973), and control balance theories (Tittle 1995) have emphasized what could be regarded, from the standpoint of a more sophisticated RCM, as social costs and benefits of criminal behavior.

### Nonmaterial Costs and Benefits May Matter More Than Material Ones

The concepts of costs and benefits could likewise encompass other nonmaterial consequences of decisions, such as the "benefit" of acting in accord with one's conscience or the "cost" of acting contrary to its dictates and experiencing guilt. Some students of crime even place primary emphasis on nonmaterial, noninstrumental benefits of committing crime, such as "thrills" (Katz 1988).

Ironically, however, the risk of legal punishment can itself provide some of those pleasurable thrills and thus be a *benefit* rather than just a threatened cost. Modern adherents to the RCM model certainly could incorporate such costs and benefits in explaining decisions to commit crime but rarely do so in their empirical research. Instead, when it comes to actually measuring variables thought to reflect the expected consequences of a criminal decision, adherents—especially economists—tend to focus almost entirely on material gain as the primary benefit, and the possibility of legal punishment, along with the expenditure of time and possible loss of legal employment and income, as the primary costs. Isaac Ehrlich (1973) set the pattern early on. After specifying explicit measures of legal risk and monetary returns to criminal and noncriminal employment in his "supply of offenses" equation, he added to his model a single vague "vector of environmental variables" that was nothing more than a miscellany of correlates of crime and an unmeasured variable that represented "the effect of psychic and other nonquantifiable variables on the crime rate," which were not specified (537). None of these intangible consequences of the crime decisions were measured or incorporated into Ehrlich's empirical research, nor have they played any significant role in the crime research of economists in subsequent decades (e.g., Jarrel and Howsen 1990; Mendes and McDonald 2001; Levitt and Miles 2007).

People are guided by moral rules, and obeying these rules prevents criminal behavior even though obedience does not bring any material benefits and does not, under most circumstances, avoid any material costs. When people turn away from a relatively risk-free criminal opportunity because committing the crime would be morally wrong, the choice rarely brings any material benefits to the actor, nor is it even likely to be *perceived* as bringing any such benefits. The reward for obeying the moral norm under such circumstances is that the person is not bothered by his or her conscience. Thus, a "cost" is avoided, but it is an emotional cost, not a material one.

### *People Are Driven by Emotion, Much of It Genetically Determined*

Even if one accepted the idea that humans in some sense choose courses of action, the choice may be governed largely by strongly felt emotions favoring one course over others. Thus, one may "choose" to follow one's passions because it is easier than curbing them. The option of defying one's passions exists, but it may be possible to choose it only with great effort and exceptional self-control. For example, we may feel genetically governed impulses to aggress, which are triggered by environmental circumstances such as threats or frustration, and we either can or cannot resist the impulses (Miles and Carey 1997). If one "chooses" to follow such impulses, is it really a choice? And if doing so usually produces, on the whole, suboptimal outcomes for the chooser, is it useful to describe it as a rational choice? Aggression might well have enhanced the survival chances of man's primate ancestors over millions of years of early evolution, yet it may consistently yield very suboptimal consequences in the contemporary world. If an impulse to aggress in the presence of certain environmental triggers is indeed genetically determined, then one does not choose to experience the impulse; at most, one can only choose whether or not to resist it once it is experienced.

### *Punishment Can Encourage Crime as Well as Discourage It*

Legal punishment cannot be realistically conceptualized as only a cost of crime, which can only discourage criminal conduct. Many kinds of punishment, especially the more severe and prolonged punishments such as incarceration can serve to encourage crime among those punished by limiting associations to pro-criminal others, restricting prosocial opportunities such as lawful employment or formal education, enhancing criminal skills and knowledge, or a host of other mechanisms. Further, the threat of punishment may provide one of the main thrills of committing crime (Katz 1988).

These issues are discussed at length in Chapter 11 and need not be addressed further here. It suffices to note here that it would be hard to derive any clear prediction about even the direction of the impact of increases in legal punishment of crime, never mind its magnitude, from a more complete and realistic revision of the RCM that took into account the foregoing complexities.

## What Kinds of Human Behaviors *Do* Accord With the Rational Choice Model?

Considering the full range of human decisions, choices of a narrow economic nature, such as investor and consumer choices, may be the exceptions to the general rule that human behaviors are heavily emotional, and only partial exceptions at that. The degree to which consumer choices can be swayed by advertising that manipulates emotions and the ill-founded enthusiasms of investors for faddish stocks suggest that even these market-related decisions are often based on emotions rather than just cold calculation about likely returns versus purchase costs. Nevertheless, the ability to predict human decisions based on rational choice is probably better in the narrow realm of market behavior where the RCM has flourished the most than with respect to criminal behaviors such as violent crimes. The volume of accurate information typically available to decision makers is unusually large with regard to traditional market behaviors and, in this respect, should be seen as the exception, not the rule, of human behavior. That is, market-related behaviors are the very small subset of human behaviors where people have a nonnegligible amount of good information about the likely consequences of alternative courses of action.

Thus, some decisions fit the RCM better than others, and, more specifically, some decisions as to whether to commit crimes may accord with the model better than others. When business executives make decisions to evade taxes, defraud customers or investors, or violate pollution or worker safety laws, their decisions may accord well with the RCM, primarily because they *are* business decisions and only incidentally criminal decisions. The same decisions could have been made for business reasons even in the absence of laws forbidding them. Indeed, prior to the advent of modern legal regulations of business, such as antitrust and consumer protection laws, many of the outcomes of these decisions would not even have been crimes. Given the variability of laws concerning business activity across nations and historical eras, the same acts may not even be illegal in other times and places. Consequently, there is good reason to regard business crime as more "business" than "crime" and to view any success of the RCM in modeling such decisions as an instance of the model's fit to economic behavior narrowly construed rather than as evidence of its value in explaining criminal behavior in general.

One implication of the foregoing for public policy is that the arena in which deterrence-based crime control is most likely to be effective is corporate crime, since decisions to commit crimes are especially likely to be in accord with the RCM. In the years preceding the 2008 crash, the corporate officials who sold securities backed by subprime mortgages not likely to be paid back went to great lengths to conceal the true value of these securities from their customers, carefully planning these frauds in ways that point to rational decision-making in which forethought about benefits (massive profits) and costs (negligible risk of prosecution) played an important role (Patterson and Koller 2011). Thus, enacting laws forbidding such practices, providing for significant penalties for violations, and making substantial investments in enforcement of the laws could deter these types of unusually rational "crimes in the suites," even if deterrence-based control efforts largely fail to reduce "crime in the streets." The irony of course is that business crime is precisely the category of crime on which the legal system has been least likely to inflict significant punishment.

These considerations imply that corporate crime is relatively more explainable by the RCM than ordinary street crime, but empirical evidence suggests that even corporate crime is not affected much by existing punitive policies. Schell-Busey, Simpson, Rorie, and Alper (2016) reviewed 58 studies of the impact of what the authors loosely described as "corporate crime deterrence strategies," analyzing 80 separate tests of effects. Most of the efforts to control corporate crime that were evaluated were laws or policies, and studies of their impact could not isolate the effect of fear of punishment. The studies that came closest to directly assessing deterrent effects were those that evaluated the impact of "punitive sanctions," i.e. "studies that examine the impact of imposed or threatened sanctions including fines, prosecution, conviction, imprisonment, or the avoidance of punishment" (397).

The authors' cautious summary of findings bearing on this type of intervention was that "we do not have enough evidence to conclude that punitive sanctions have a deterrent effect on individual- or company-level offending" (397). This conclusion was based on nonsignificant negative associations between sanctions and offending and evidence of publication bias tending to exclude studies obtaining null (no effect) results. In sum, even corporate crime seems to be, at best, only marginally responsive to threats of punishment and the inflicting of punishment. It should be stressed, however, that these weak findings may be largely a reflection of the weak sanctions and enforcement efforts aimed at corporate crime in contemporary America. Tougher penalties, imposed through more rigorous enforcement, might have stronger effects.

## The Disappearing Theory

The problem with the RCM of criminal decision-making may be that the empirically discovered exceptions to its seemingly simple predictions eventually swallowed up the theoretical rule. In order to make the model consistent with empirical observations of human behavior, so many exceptions to basic principles had to be made that, in the end, little of the simpler, and thus more intuitively appealing, versions of the theory remains. A theory that began with the elegant simplicity of a Greek temple ended up looking like a child's tree house, with ugly additions sticking out all over.

The issue can be summarized like this: (1) People may not really "choose" to do crime or not do crime, in the sense of exercising free will. (2) People make decisions on the basis of perceptions about the probability and value of consequences of their actions that have little correspondence to reality. (3) People routinely deviate from decision-making rules that accord with conventionally understood rationality based on expected utility, instead applying shorthand rules that entail leaving out consideration of most of the actual contingencies of decisions. (4) People display persistent biases in their decision-making that deviate from rules that would consistently produce optimal outcomes. (5) People make decisions that persistently yield *results* that are far from optimal.

If people do not make choices, or make choices but not in accordance with ordinary notions of rationality, can it be meaningfully said that human behavior results from a process of "rational choice"? If so many modifications are made to a theory that it no longer generates any of the clear predictions its original version implied, can it be regarded as basically the same theory with which one began? Is there any value to continuing to call it the Rational Choice Model? An anonymous American Major during the Vietnam War was once quoted as saying, "It became necessary to destroy the town in order to save it" (Associated Press, February 8 1968). Those who have sought to preserve some version of the RCM appear to have decided that "we needed to destroy the theory in order to save it."

Gary Becker (1968) and his student Isaac Ehrlich (1973) advocated versions of RCM that were theoretically simplistic, based largely on the assumptions of orthodox expected utility theory. Further, Ehrlich's empirical tests of the theory effectively assumed a close correspondence between actual and perceived costs of crime, given that the former were used as proxies for the latter. Cook (1980) recognized the serious limits of this theory in explaining criminal decision-making and tried to improve it by incorporating the more sophisticated views of (post-Becker) students of decision-making, such as Tversky and Kahneman, regarding choices made under conditions of uncertainty and risk. By doing so, however, he effectively lost much of what made the orthodox approach appealing in the first place—its simplicity, its seemingly commonsensical nature, and, most importantly, its relatively clear predictions as to how criminal behavior would change in response to changes in punishment policies. Cook, an economist by training, did not, understandably enough, present his modified perspective as a rejection of neoclassical economics and expected utility theory. His discussion of deterrence, however, pointed out so many exceptions and qualifications to what neoclassical expected utility theory would predict that it is not at all self-evident what policies the highly modified theory implied by his discussion would prescribe.

One problem with the term "rational choice" is that it carries an everyday lay meaning that goes well beyond the narrow and banal assertion that some people sometimes take account of costs and benefits in making choices. When lay people (and apparently many scholars) say that human behavior is rational, they also seem to mean that behavior is generally "sensible" or "reasonable," i.e., that it is at least approximately utility-optimizing and tends to produce optimal results. Outside of the narrow realm of market behavior, however, there is little evidence that human decisions, even when averaged over large populations, result in even approximately optimal outcomes. To the contrary—decisions are routinely made in accordance with principles that systematically undercut utility maximization, such as discounting remote future consequences in favor of immediate ones or discounting consequences experienced by many others in favor of the very limited, often unrepresentative consequences experienced by one's self (Tversky and Kahneman 1974; Kahneman and Tversky 1979). Likewise, when there is little correspondence between perceived and actual costs and benefits of alternative choices, it is unlikely that rational choice will result in optimal outcomes for the decision-maker, and thus unlikely that the choice is "reasonable" or "sensible." Such considerations raise the question: Is it linguistically useful to describe decision-making patterns that consistently result in such suboptimal results as "rational"? Or does this customary use of the word produce more confusion than clarity?

## Is Street Crime "Rational" in the Sense of Yielding More Benefit Than Cost?

Under the RCM, deciding to commit crime is partly the result of a consideration of its costs and benefits, and one of the primary costs is legal punishment. How likely is it that commission of a crime in contemporary America will result in punishment? Table 3.1 displays evidence for all seven FBI Index crimes for which we have the necessary data. We generously assume that all murders and motor vehicle thefts become known to the police, though that is probably only approximately accurate. Nevertheless, even generous estimates of the probability that these crimes will result in even a short term of incarceration indicate that only murder results in such punishment more than 17 percent of the time, and only murder and aggravated assault are punished with incarceration more than 10 percent of the time. Indeed, the vast majority of Index crimes do not result in any legal punishment at all, given that only a small minority result in a conviction. These data do not cover white collar crimes like corporate crime, fraud, or writing bad checks or nonpredatory crimes like illicit drug selling, drug use, or prostitution, but the probability of any one of these crimes resulting in punishment is almost certainly even lower than for Index crimes. In sum, crime resulting in legal punishment is, in absolute terms, rare, while crime resulting in punishment avoidance is very common.

The actual certainty of punishment is also lower than the general public believes it to be. Kleck, Sever, Li, and Gertz (2005) found that the nation's urban population believed that 43 percent of robberies and 38 percent of burglaries known to the police result in an arrest (640) when the actual probabilities of arrest in were 27 percent and 14 percent, respectively, in the urban areas studied (and just 26 percent and 12 percent respectively for the nation as a whole in 2007 [Table 3.1]). These erroneous beliefs in high punishment probabilities might themselves produce deterrent effects that could persist as long as one does not learn the reality of low punishment certainty. This sort of deterrent effect is produced by what has variously been referred to as a "shared misunderstanding" (Jensen 1969) or the "shell of illusion" (Tittle 1980). This misunderstanding, however, can be whittled away by criminal experience—by an offender repeatedly committing crimes and not being punished. Deterrence scholars call this the "experiential effect," and it will be discussed at length in Chapters 5 and 6.

From one standpoint, then, one might initially consider most street crimes to be rational in the sense that one is unlikely to be punished for committing any one such crime. The rational choice model, however, explains criminal behavior on the basis of the relative balance of benefits and costs, not costs alone. Is criminal conduct rational in the sense of its benefits exceeding its costs? If people rationally decided to commit ordinary street crimes like robbery or burglary, one would expect that the outcomes of their decisions would, on average, be at least reasonably optimal. Given that the primary motive of these particular types of crime is profit, one would expect them to be, on average, profitable relative to law-abiding alternatives such as lawful employment. Of course, for many people, the lawful alternatives to crime are not very lucrative. Legal jobs, even when available, may yield no more than minimum wage returns. Thus, "street crimes" do not have to be highly rewarding in an absolute sense to be consistent with the RCM—they merely need to be more profitable than the lawful alternatives available to prospective offenders.

Therefore, it is worth assessing the average costs and benefits of ordinary "street crime." Table 3.2 presents some relevant statistics on the costs and benefits of robbery and burglary using national data from the National Crime Victimization Survey, the Uniform Crime Reports, and the National Judicial Reporting Program. Focusing on property crimes is useful because if any kind of crime is likely to be the product of rational decision-making, it should be crimes yielding material benefits.

The data show the probability of these crimes being punished with some kind of incarceration (jail or prison), the average length of that incarceration, and the average dollar yield of the crime.

**TABLE 3.2** The costs and benefits of ordinary property crime—robbery and burglary, 2007

| | *Robbery* | *Burglary* |
|---|---|---|
| **Costs** | | |
| *Probability of Punishment (P)* | | |
| Probability That the Crime Will be Reported to Police | 0.656 | 0.501 |
| Probability That a Reported Crime Results in Arrest | 0.259 | 0.124 |
| Probability That an Arrest Results in Conviction | 0.332 | 0.328 |
| Probability That a Conviction Results in Incarceration | 0.650 | 0.440 |
| Overall Probability That Crime Results in Incarceration | 0.037 | 0.009 |
| *Severity (Value) of Punishment (S)* | | |
| Mean Maximum Sentence Length Imposed, 1 Conviction Offense, in Months | 69 | 36 |
| *Expected Cost (P x S), Months in Prison per Crime* | 2.567 | 0.430 |
| **Tangible Benefits** | | |
| *Average Dollar Value per Crime* | $140 | $220 |
| **Ratio, Benefit/Cost** | | |
| *Dollars Stolen per Expected Month of Incarceration* | $55 | $512 |

*Sources*: U.S. Federal Bureau of Investigation (2008); U.S. Bureau of Justice Statistics (2010, Tables 82, 91); U.S. Bureau of Justice Statistics (2009b, Tables 2.3, 2.4).

These are generous estimates of the material gains of robbery and (especially) burglary, since they are based on estimates of losses provided by the victim. Some victims may overstate losses to the police for purposes of making claims on insurance policies or for the purpose of encouraging police to regard the crime as sufficiently serious to be worthy of their best investigative efforts. Further, these figures do not reflect the property losses in crime not reported to the police, which are likely to be lower those in reported crimes. Finally, a victim's loss of X dollars of property does not necessarily imply a gain worth of X dollars to the offender. If the criminal steals a television that cost the victim $300 when purchased new, it is not worth $300 to the offender, both because it is no longer new and because he cannot sell it to others for that much.

In order for a criminal to suffer the "cost" of being legally punished with incarceration (we ignore the costs of other kinds of legal punishment as well as intangible costs), his crime must result in (1) being reported, or otherwise becoming known, to the police; (2) the offender's arrest; (3) a conviction; and (4) a jail or prison sentence. In Table 3.2 each of these probabilities of these results occurring are multiplied times each other to yield the overall probability that a given robbery or burglary will result in an incarceration sentence. These probabilities reaffirm the implications of the Table 3.1 data that punishment of any one crime is highly unlikely. Only 3.7 percent of robberies and 0.9 percent of burglaries result in any kind of incarceration. In the short run, punishment has a very low probability of occurring. On the other hand, if a person continued committing many of these crimes, as career offenders in fact do, it would eventually become likely that he would be punished at least once.

The expected cost of these crimes can be computed in the conventional way used by economists under the expected utility model by simply multiplying the probability of an outcome by its value. In this case, we consider just one kind of cost—incarceration—and are therefore computing a conservative estimate of costs. The mean maximum sentences imposed on robbers is 69 months for those convicted of just one felony at the time of sentencing and 119 months for those convicted of multiple offenses (which could include nonrobbery offences). The corresponding figures for burglary are 36 months and 48 months. As we noted in Chapter 1, criminal sentences in the contemporary U.S. are extraordinarily severe by either historical or international standards. For robbery the expected cost of a given crime is 2.567 months, i.e. a robber could expect to be incarcerated that many months per robbery committed, averaged over all robberies. For burglary, the expected cost is 0.430 months of incarceration.

The material benefits of these ordinary property crimes, on the other hand, are meager by any standard, even those of low-income people. The average robbery in the U.S. in 2007 generated a loss to the victim of $140 for all robberies. The average household burglary generated a loss of just $220. As previously noted, the material benefit to offenders is likely to be even lower than these amounts as estimated by victims. Thus, we can compute the "wages of crime" for these offenses in the sense of how many dollars of gain (generously estimated) are earned by criminals per month of cost in the form of incarceration. The average robbery in 2007 yielded just $55 per month of incarceration that the robber could expect to serve per offense, while the average burglary yielded a better but still modest $512. It is unlikely that even the lowest income offender, with the poorest job prospects and the most hardened attitude to serving time in prison, would consider it "worth" $512 to be incarcerated for a month or $6,144 a year. Even if one ignored the pains of incarceration, risks of assault while in prison, loss of contact with family, and all the other less quantifiable costs of incarnation, the loss of lawful income alone, even, for a minimum wage worker would be far greater than $512 per month. The federal minimum wage in 2007 was $5.85 per hour, so even a minimum wage worker employed 40 hours per week and 50 weeks per year would earn $11,700 a year, or about $975 per month.

In sum, notwithstanding the low absolute probability that crime results in legal punishment, ordinary street crime does not pay, even relative to lawful minimum wage employment. Even using generous estimates of material benefits, and taking account of only one kind of cost (legal punishment in the form of incarceration), the benefits do not exceed the costs, and the benefit/cost ratio is not attractive even relative to unattractive legal alternatives. These data certainly do not support the notion that commission of these kinds of crimes is the result of a rational, utility-maximizing decision-making process, at least with regard to material gains, even for economically disadvantaged persons.

Yet millions of people *do* commit these crimes. These facts could lead one to any of several conclusions—either (1) the choice to commit these crimes is not rational in the sense of producing approximately optimal outcomes, (2) offenders grossly misperceive the costs and benefits of crime (but choose rationally based on their misperceptions), or (3) the crimes must have considerable intangible benefits, such as the thrill of risk or the pleasure of comradery with one's co-offenders, great enough to outweigh the costs of punishment. In any case, this simple comparison of costs and benefits certainly does not encourage one to believe that these offenses were the product of rational assessments of their more obvious costs and benefits or a comparison of criminal alternatives with legal alternatives for making money.

### *The Implications of Constricted Rationality for Target Selection, Situational Crime Prevention, and Crime Displacement*

Another implication of the low rewards of ordinary street crime is that offender choice of targets for property crime—or at least for burglary and robbery—seems distinctly suboptimal. That is, the choices are not rational in the sense of consistently yielding optimal results for offenders or maximizing their utility. In an affluent nation swimming in valuable, minimally guarded property, criminals seem to regularly "select" crime targets that are remarkably unrewarding by any standards, even those of low-income criminals. It is almost as if the choices were made quite haphazardly, if not totally randomly, with little attention paid to the full range of feasible, attractive potential targets or the likely payoffs and risks associated with them. If target selection was the product of even minimally rational decision-making, one would think that the average payoffs of victimizing those targets would not be so extraordinarily meager.

Nevertheless, when scholars interview property offenders, the criminals often present themselves as highly rational, thoroughgoing professionals rather than bumbling amateurs who impulsively make foolish decisions. Many criminals describe extended assessments of criminal opportunities in which they carefully considered a wide array of potential crime targets and attended to numerous relevant cues as to their relative merits (e.g., Wright and Decker 1994). There is, however, little evidence aside from these dubious self-presentations that this is actually the way most criminals operate most of the time. Cromwell and Olson (2004) questioned incarcerated burglars and asked them how they went about selecting targets. The burglars described lengthy searches and highly rational decision-making strategies. The authors, however, made efforts to verify these claims by conducting field simulations with the burglars after their release from prison, in which a previous crime was reconstructed at the original scene. The authors concluded that "the characteristics of the target sites and the techniques used to burglarize those targets were seldom congruent with the completely rational approach [the burglars] had constructed during the initial interview" (19). Instead, target selection was actually quite casual and opportunistic, favoring conspicuous targets with which the offender was already familiar or those the burglar happened to pass by. Further, the burglars gave only limited attention to indicators of likely payoffs from potential targets (called "gain cues" by the authors). Thus, even though Cromwell and Olson somewhat inexplicably characterized burglary as "a highly rational crime" (32), they observed that "most burglars in the study expended minimal energy and time assessing gain cues" (21).

There may be a few criminal masterminds who meticulously plan crimes like those portrayed in Hollywood "caper" movies, and certainly some criminals are more professional than others in selecting targets. Marvell (2002) even speculated that a small fraction of offenders may account for a very large share of crimes and that these few might be unusually rational, careful in selecting targets and skillful in avoiding arrest. On occasion, even generally careless offenders may engage in more extended thought and planning than they customarily do. Perhaps these exceptional crimes are the ones that incarcerated criminals selectively recall when questioned by scholars and journalists. The evidence concerning *average* targets of the sort actually chosen on a routine basis, however, belies the impression promoted by such criminals. If target selection really were that routinely rational, the results should have been far better than we know them to be. Robbers and burglars in practice choose targets that usually provide very meager rewards (Table 3.2).

Instead, most property offenders seem to make target choices quickly and impatiently, devote little effort to the process, consider only a handful of candidate targets, and take account of only the most conspicuous features of these candidates (Cromwell and Olson 2004). They may favor targets on or near the travel routes they follow in their ordinary daily routines, over more lucrative yet still relatively risk-free targets located further away that would require more search time and patience to identify. Once having chosen a target, planning as to how to carry out the crime may likewise be minimal.

This picture of target selection and crime execution certainly comports better with the hard information we have on the modest rewards of ordinary street crime—if target selection was careful, surely the rewards would be far better than they are. It also comports better with scholarly knowledge of the personality traits that differentiate criminals from noncriminals. Criminals tend to be impatient, impulsive, disinclined to consider long-term consequences of their actions, and averse to extended intellectual activity (Gottfredson and Hirschi 1990; Vold, Bernard, and Snipes 2002, 77–80)—all attributes that work against rational crime planning.

These facts have important implications for the issue of crime displacement and the evaluation of the impact of localized crime prevention efforts. Some of the scholars who evaluate the impact of situational crime prevention (SCP) efforts make some effort to check whether the efforts merely displaced crime to other locations outside the intervention area rather than reducing crime. These efforts, however, seem to be based on the implicit assumption that if crimes are displaced, they are most likely to be displaced to nearby areas bordering on the intervention area, and that "displaced crimes" would require more effort and time to commit than the crimes that would have been committed in the absence of the intervention (e.g., Cornish and Clark 1986, 5). This assumption is unwarranted and inconsistent with what scholars know about criminal target selection. If target selection is poor in the first place and tends to result in the selection of less-than-optimal targets, then if efforts are made to block criminal access to those targets, criminals would not necessarily have to make *any* extra effort, spend any more time, endure more risk, or accept lower payoffs if they sought substitute targets. Quite the contrary—the more poorly chosen the initially preferred target was, the more likely it is that alternative targets exist that are equally or more attractive. Blocking access to a few of the more obvious and conspicuous (but far from optimal) potential targets, in an affluent nation filled with adequate targets for crime, may simply force offenders to make the minimal additional efforts to identify slightly less conspicuous alternatives, many of which might require *less* travel or effort to carry out, entail *less* risk, and have *higher* payoffs than the initially preferred targets. Under these conditions, there is no sound reason to expect anything less than 100 percent displacement (and thus no crime reduction) in response to most localized efforts aimed at property crime.

Advocates routinely claim that SCP efforts have been successfully implemented, with little or no evidence of displacement (e.g., Guerette and Bowers 2009). This claim, however, must be judged in light of the fact that the typical evaluation of a SCP program actually incorporates *no* effort whatsoever to look for evidence of displacement and that the minority that do make some effort make only the most superficial efforts. In an otherwise quite favorable assessment of SCP research, Guerette and Bowers reviewed 206 studies of SCP and found that 104 of them *failed to do anything* that "allowed for some observation of displacement" (2009, 1355). Further, of the minority that looked for evidence of displacement, most looked for it only in areas near the intervention area (1356). These commonly only looked at areas within a few blocks of the area in which an SCP intervention was implemented. The authors conceded that the literature they reviewed can "tell us little about the possibility of displacement to more distant locations" (1356) while downplaying the significance of this limitation. Typical of studies looking for displaced crimes only in bordering areas, Braga and Bond (2008) evaluated the impact of increasing police presence in high-crime areas and looked for displacement only in the *two block* area surrounding intervention areas (596); the same size displacement area was used by Ratcliffe, Taniguchi, Groff, and Wood (2011, 815). In neither study did the authors provide an explicit rationale for the use of this very constricted displacement area.

While such researchers do not necessarily assume that *all* displaced crimes will be found in such nearby areas, they presumably assume that these are the areas to which offenders are *most likely* to be displaced. The underlying assumption seems to be that criminals would start their search for alternative targets in the intervention area, perhaps because the offenders were assumed to reside in that area and to prefer to victimize targets likewise located in that area. Then, facing an intervention that denied them targets within the intervention area, they would search for other targets. The nearest alternative targets would be located just outside the intervention area. Based on this assumption, researchers may reason that if there is little or no crime increase in these bordering areas, it is even less likely that crime was displaced to more remote areas, since identifying and travelling to and from the alternative targets would require more time and energy. In the absence of crime increases in bordering areas, crime decreases in the intervention area are interpreted as crime reduction and not mere displacement.

Research on the movement of offenders and their target selection procedures, however, provides little reason to expect that all, most, or even a large minority of displaced crime will be found in bordering areas very near to a SCP intervention area. While criminals generally prefer targets *fairly* close to their homes over targets further away, they do not favor targets *very* close to their homes. Rengert, Piquero, and Jones (1999, 432) characterized scenarios "in which the criminal always begins the crime search at the home" as "unrealistic" and reported that criminals avoid a "buffer zone" very close to home. Their own data on urban burglars indicated that most burglaries were committed over a mile from the burglar's home (439). Other researchers have likewise reported even longer average "trips to crime" for property offenses, as well as evidence of a buffer zone. Thus the search for targets often begins in locations other than the offender's home, and even when it begins in the home, the offender rarely selects a target within a few blocks of his home.

Consequently, if an SCP intervention blocked access to a given prospective target, the offender's search for alternative targets generally begins in a location that is neither extremely close to his residence nor necessarily inside or near the area in which the intervention was introduced. Evaluators who search for evidence of displacement only in locales within a few blocks of the intervention area's borders are, for the most part, looking in the wrong places, since these are neither the only, nor even the most likely, places to which property crimes would be displaced.

Unfortunately, evaluators of SCP rarely look for displaced crimes anywhere else. And if researchers make no serious effort to look for evidence of displacement, they are not likely to find it. In this light, there is little foundation for believing claims that SCP efforts caused actual reductions rather than displacement of crime. To take one specific example relevant to deterrence-based crime control, in the absence of serious efforts to search for evidence of more remote displacement following localized increases in the risk of police apprehension of criminals, there is no sound basis for believing that they have caused anything other than 100 percent displacement. In sum, extant evaluations of highly localized changes in legal risk at present offer little credible evidence that they reduce crime rather than just moving it around.

## The Predictive Ability of the Rational Choice Model of Criminal Behavior

The ultimate test of a theory is its ability to predict observable patterns in the real world. In its simpler forms, the RCM seemed to offer clear predictions: (1) the greater the costs of a behavior, the less likely the behavior, and (2) the greater the benefits, the more likely. By incorporating more and more costs and benefits and measuring their probability and value to the actor, the theory should grow in predictive ability. Once the objective RCM was replaced by the subjective RCM, however, most of the ability to derive clear predictions from the theory was lost. While it is sometimes feasible to estimate the average (population-wide) probability and value of some objective costs and benefits of crime, under most real-world conditions it is difficult or impossible to accurately measure individual, subjective assessments of those costs and benefits. Accurate information about actual costs and benefits is generally more easily and widely available than information about individuals' subjective perceptions of these consequences or information about the average values of these perceptions in large populations. Under artificial research conditions, subjects may, to the best of their ability, convey their own perceptions to researchers, at the moment the study is being conducted. It is, however, quite another matter to acquire such information on a regular basis across large populations, or to accurately predict the perceptions produced by a policy change such that the information is available on a routine basis for making real-world policy decisions.

As long as it was believed (or implicitly assumed) that there was a reasonable correspondence between actual and perceived risks, at least when averaged across large populations, objective risks, which could be measured, could be used as proxies for average perceived risks, which usually could not. The wider the gap between perceptions and reality, however, the less credible it is to use actual risks as proxies for perceived risks, and thus the less predictive ability the theory will have when applied to aggregates such as the populations of cities, counties, or states.

Direct tests of the RCM's ability to account for individual differences in criminal behavior have so far yielded disappointing results. Matsueda et al. (2006) mounted what is arguably the most ambitious effort to test an especially sophisticated "rational choice model of theft and violence." Their self-report perceptual research incorporated an unusually rich array of measures of perceived rewards of crime, perceived risk, prior experience with both punishment and unpunished offenses, and even the opportunity costs of criminal behavior. They tested their model with a large probability sample of youths, employed questions specially designed to measure perceived risks and benefits of crime, and applied sophisticated statistical estimation procedures. Nevertheless, their models accounted for only 7 percent of the variation in theft and 9 percent of the variation in violence, shares that even the authors described as "modest" (117). They did not assess the degree to which the youth's perceptions corresponded to actual risks, but even with respect to the link between those perceptions (accurate or not) and delinquent behavior, the model's predictive ability was slight by any reasonable standard. Had the authors not been able to directly measure perceptions and been forced to rely on actual risks as proxies for those perceptions, the predictive ability of their model would have been even worse.

Even when models testing the RCM appear to show somewhat better predictive ability, most of it is attributable to variables outside of the orthodox RCM framework. For example, the models of Piliavin et al. (1986, 113) explained as much as 22 percent of the variation in crime among youth, but most of this predictive power came from age, sex, drug use, and other variables besides perceived costs and benefits. No doubt a better measurement and a more exhaustive cataloging of potential contingencies will improve predictive ability to some degree, but these findings at minimum raise doubts about the predictive ability that the RCM can have under real-world conditions, where the ability to measure perceived contingencies would normally be far more limited than it is for scholars working in a research context. Perceptions of costs and benefits have *some* predictive value, but they may not have much (Chapter 5). Further, much of the theoretical "action" in the link between punishment and crime may turn out to lie in explanations of how and why perceptions of costs and benefits arose in the first place and why these perceptions depart so radically from actual costs and benefits.

Whether the modest links between perceived contingencies and behavior have any real-world implications for making policy to reduce crime heavily depends on the strength of the link between actual and perceived consequences of crime. If perceptions are wildly inaccurate, the modest ability of those perceptions to predict behavior will mean very little for attempts to reduce criminal conduct (Nagin 1998). Although policymakers have some ability to alter the actual costs and benefits (primarily costs) of criminal conduct, they have far less ability to directly manipulate perceptions of those consequences, apart from the effects that actual contingencies have on perceived contingencies.

## Conclusions

Levitt and Miles (2007, 462) correctly noted that one of the principle benefits of the "economic approach" (their name for the RCM) was its "relative simplicity" and "the set of sharp behavioral predictions [it generated] that empirical inquiry may validate or refute." The simplicity of the RCM, however, has largely disappeared in the face of empirical refutations of the straightforward utility maximization versions of the model and the long and growing list of modifications to the theory that were necessitated by unsupportive empirical results. The modified version is anything but simple, and its predictions for human behavior in the realm of ordinary crime are anything but "sharp." Prospect theory cannot be regarded as merely a modest modification of expected utility theory, as Cook (1980) seemed to hint. They are fundamentally different in what they predict will happen when levels of legal punishment are altered. As economists Lattimore and Witte (1986, 148–149) put it, "These two models are likely to lead to quite different conclusions regarding the way in which changes in criminal justice policy (i.e., changes in the probability of apprehension and punishment and severity of punishment) . . . affect criminal behavior."

By the 1980s, many advocates of the RCM had abandoned simplistic utility maximization variants of rational choice and conceded many of its limitations. Perhaps influenced by the behavioral economists, prospect theory, and the disappointing results of individual-level tests of the RCM, they had gravitated instead to some variant of "limited rationality" (Cook 1980; Cornish and Clarke 1986; Clarke and Cornish 2001, 24; Brezina 2002). So much of the original RCM had to be abandoned or significantly revised, however, that it is questionable whether there is much point to retaining the term "rational choice" at all. The term has been so thoroughly watered down that it scarcely means anything anymore. Consider, for example, Brezina's (2002) characterization of what he termed the "wide" model of rational choice. He conceded that decisions arrived at (1) are routinely *not* optimal, (2) are commonly based on inaccurate assessments of the situation, and (3) rely on extremely limited quantities of information. He further acknowledged that decision makers (4) have limited inclination to acquire information and (5) have severely limited abilities to process the information they do acquire. Under this loose conception of rational choice, there is scarcely anything left of the original theory beyond the banal fact that *some* decisions entail *some* consideration of *some* costs and benefits (however inaccurately perceived) of *some* alternative behavioral choices. It is impossible to derive any firm predictions regarding the impact of threats of legal punishment from such a feeble variant of the rational choice model. Given that this is what has survived of the RCM, it is not surprising that the predictive ability of the model is so slight.

Under current usage, there appears to be only negligible requirements for scholars to assert that criminal behavior is the result of "rational choice," or that it is "rational behavior." Cornish and Clarke (1986b, 7) conceded that "people pay attention to only some of the facts at their disposal, that they employ short cuts or rules of thumb to speed the decision process, that they often decide poorly as a result of fatigue or alcohol, and under pressure of time they may make last-minute changes of plan," yet the authors nevertheless insist that criminal behavior is rational because "the decisions made represent the offender's best efforts to maximize the benefits for himself." For Cornish and Clarke, all that appears to be required for criminal decision-making to be considered rational is that at least *some* prospective offenders think, however briefly, about *some* possible costs and benefits, however inaccurately understood, of criminal choices, no matter how consistently suboptimal the results may be (see also Tunnell 1992; Cromwell and Olson 2004 for similar examples).

Likewise, Feeney (1986, 66) concluded that the decisions of robbers "are clearly rational" and "meet the standards of minimum rationality" because (1) "there is clearly a thinking process involved," and because the robbers had (2) "needs that they chose to satisfy by committing robberies." Feeney did not require, as a condition of rationality, that there be any actual connection between the robbers' decisions and whether they did in fact satisfy those needs, that any of the robbers' thinking entail even approximately accurate reflections of their world, or that the thinking take account of any particular costs or benefits.

An alternative view is that some criminals do crime precisely because they do *not* take much account of costs or benefits of alternatives, other than the anticipated, intended short-term benefits of the crime. It is, ironically, the minimally rational character of the decision-making process that leads to crime. The criminal decision is not the result of a rational process but rather of the absence of rationality. The RCM itself (or at least any orthodox version of it) does not offer any help in predicting who will be habitually less rational in making decisions, since it assumes or takes rationality as a given, albeit to varying degrees, in all humans. To be sure, many supporters of the RCM do acknowledge differences across individuals, such as some people being more likely than others to "discount" future consequences of their actions. But once the advocates do this, their explanation becomes based more on the personality traits that determine a person's degree of rationality than on the process of rational choice itself or the actual costs and benefits of alternate courses of action.

A relative absence of rationality in some individuals may be largely the product of genetic inheritance in interaction with an environment favoring irrationality—especially parental child-rearing practices that fail to discourage impulsive decision-making. If so, the explanatory "action" might not be in the operation of a rational thought process or the cost/benefit contingencies of alternative choices, but rather with the degree to which the actor is rational in the sense of being inclined to routinely take account of many costs and benefits when making decisions. Thus, the concept of impulsivity stressed by the low self-control theory of criminologists Gottfredson and Hirschi (1990) could be reconceptualized as low rationality—a persistent inability or disinclination to consider a wide array of consequences, especially long term consequences, in making decisions.

### *Is the Theory Falsifiable? The Rational Choice Model as Moving Target*

Scientific theories are supposed to be falsifiable. That is, if the theory were false, it should be possible to show that it is false. Advocates of the rational choice theory might be lauded for their flexibility and willingness to revise the theory to accord with newly discovered facts, since responsiveness to empirical evidence is surely a virtue. Another way to view the same characteristic, however, is that it makes RCM something of a moving target.

Even if its central claims were false, the model can never be shown to be false if its adherents regard all evidence, regardless of its character, to be somehow supportive of some version of RCM, albeit one so revised from earlier versions as to be virtually unrecognizable as a RCM. Under those conditions, the possibility of any decisive tests of crucial propositions would be an ever-receding goal. It is more worrisome than reassuring when proponents of a theory can respond to *any* evidence by saying that "it just goes to show" that the theory is valid, playing a game of "heads we win, tails you lose."

For example, if evidence points to criminals carefully selecting targets of higher value and lower risk, it certainly would be reasonable to cite this as evidence supporting RCM. When critics of RCM, however, note that offenders consistently make suboptimal choices to commit low reward/high risk crimes, RCM defenders can say this too supports their theory, since individuals who are more rational refrain from crime because they recognize the unrewarding, risky nature of crime, while those who are less rational commit crime.

If research finds that people are less likely to offend when faced with higher legal risks, this understandably would be interpreted as support for the RCM. However, if many people proceed to commit crimes even when risks are high, RCM advocates can argue that this is *still* rational behavior because it just goes to show that the person must have a "taste" for risk and that their crimes provide the offender with the benefits of enjoyable risk (for an example of this reasoning, see Shapiro and Votey [1984], 601).

If research reveals little connection between actual risks of crime and criminal behavior, defenders can argue that it just goes to show that perceptions of costs are imperfect, but we can still retain the principle that *perceived* risks affect offending. And if the best extant research also finds little connection between *perceived* costs and offending (Chapter 5), defenders can argue that perceptions were badly measured, that they were not measured within the relevant subset of the population likely to be affected, or that they did have a deterrent effect but it was obscured by counterbalancing criminogenic effects (e. g., Nagin 2013b).

If most empirical studies effectively test only for short-term effects, defenders of the RCM can speculate that punishment might have long-term crime-reducing effects that have not yet been discovered (e.g., Nagin 1998, 4). The beauty of this sort of speculation about what future research might reveal is that it is infinitely flexible—one can selectively speculate whatever one likes. Those strongly wedded to a theory, however, can easily succumb to the temptation to be one-sided in these speculations, tending to imagine only future results supportive of the theory and not those undercutting it.

If even the best tests of the RCM indicate that very little variation in offending can be explained by variations in costs actual or perceived (Matsueda et al. 2006), defenders can respond that the right costs were not measured or were measured badly and that, in any case, no one ever claimed that the theory provided a *complete* explanation of crime.

If criminals are shown to have little awareness of, or responsiveness to, legal risks as they pertain to the population as a whole (see Chapters 5, 9), defenders can argue that criminals must be rationally responding to their own *personal* experiences with punishment and offending (Cook 1980). And if it is shown that offenders do not respond rationally to even their *own* punishment experiences (see Chapter 6), RCM defenders can speculate that flaws in the methods prevented researchers from detecting those responses or that such responses prevail only among some kinds of people, those who were not included or were underrepresented in the research samples studied (Nagin 2013b). In the context of this sort of special pleading, it is fair to ask "Are there *any* empirical results that advocates would concede could disconfirm the RCM?"

If no evidence of deterrent effects is found in numerous individual-level studies, defenders can respond that there is "heterogeneity in the deterrence response to the threat of" punishment and that researchers just did not include in their study samples of enough members of those subsets of the population who are deterrable, citing a selected few studies generating findings more friendly to the deterrence doctrine (Nagin 2013b, 227). This response can encourage data dredging, looking for subsets of the population where deterrence apparently works better. To be sure, it can be enlightening to discover whether the effects of punishment vary across subsets of prospective offenders and crime types, but data-dredging through huge numbers of subsamples can lead to chance findings not likely to be replicated when other bodies of data are examined. Until evidence consistently shows stronger deterrent effects within the same subsets of the population, this sort of support for deterrence should be regarded skeptically. It is also worth considering how relevant to crime-control policy it would be if punishment effects varied across subsets of the population but the net effect across all subsets of the population combined was zero.

There can be a thin line between an open-minded willingness to revise a theory in accordance with new evidence and rendering the theory unfalsifiable. The RCM has turned out to be something of a moving target, always changing enough to stay ahead of its critics. If it turns out there are no core principles to which the theory is irrevocably committed, how useful is it?

### *What the Rational Choice Model Is and Is Not Good For*

The RCM may be most useful as a loose organizing principle bringing together many different insights about criminal behavior by conceptualizing a wide array of causal factors as costs or benefits, incentives or disincentives. Its more simplistic versions may serve as a useful starting point for explaining criminal behavior, as long as its users recognize (1) that one has to move a great distance from this starting point to arrive at a realistic model with significant predictive power and (2) that the theory has no clear implications for crime control policy. Most of the perspective's explanatory "action" may well lie in the distorted perceptions, severe constraints on information, and seemingly nonrational rules that people follow in processing information and making decisions—the cognitive shortcuts and deviations from simple utility-maximizing rationality. The theory may even have significant power to explain business-related behavior that happens to be forbidden under various legal codes, since decision-making in this narrow realm of human experience apparently has a substantial rational component to it. To the extent that criminal behavior of the sort the public fears and scholars commonly study is only minimally rational, the RCM is only minimally useful in explaining that behavior and in helping to devise strategies for reducing its frequency.

More specifically, it is now clear that the RCM does not offer any clear predictions or implications regarding the impact on crime of changes in punishment levels. In particular, because the link between actual contingencies and perceived contingencies may be extremely weak or even nonexistent, it is questionable whether changes in punishment levels routinely alter the rate of criminal behavior via deterrence mechanisms (Kleck 2003).

### *An Alternative Perspective: Constricted Rational Choice*

The claim that the RCM has great predictive power appears to be based entirely on the fact that research sometimes finds a statistically significant, though not necessarily very strong, association between criminal choices and the costs and benefits of legal and illegal alternatives. That is, the studies establish that the association is not so small that it is likely to be entirely attributable to random chance factors, such as random sampling or measurement error. This does not constitute great or even substantial predictive power. The best available studies indicate that the relative costs and benefits of criminal vs. noncriminal choices—or at least those that are measurable—explain only a tiny share of the individual variation in criminal behavior. It is possible that this share would be larger if researchers had measured more costs and benefits, or measured them more accurately, but it is pure speculation that the predictive power of the RCM would become large once these research improvements were made.

An alternative perspective that is more compatible with the extremely low documented predictive ability of the RCM could be called the constricted rational choice model (CRCM). This perspective retains the seemingly self-evident notion that humans consider some costs and benefits when making some behavioral decisions, but it also recognizes the very modest association of these contingencies with decisions as to whether and how often crime is committed. Under this alternative view, humans possess very little information about the contingencies of criminal choices and much of what little information they possess is inaccurate or outdated (Kleck 2003).

The term "constricted rationality" is used in preference to the term "limited rationality" (Simon 1957) to convey the extreme degree to which the operation of rational choice is limited. Some scholars who use the term "limited rationality" imply that rational choice is the norm in human conduct and that deviations from the predictions of rational choice are special exceptions (e.g. Cook 1980). The CRCM asserts the reverse—that most variation in criminal behavior cannot be accounted for by variations in the actual costs and benefits of criminal and noncriminal choices and that occasions when much of this variation *can* be explained by such contingencies are relatively uncommon exceptions to the rule. Criminal choices may well be influenced to some degree by what actors think the contingencies are, but these perceptions rarely have strong correlations with actual costs and benefits. The difference between the RCM and the CRCM may seem like nothing more than one of degree, but if this is so, the degree is a huge one, because the former implies that increasing punishment risks will generally reduce crime, while the latter does not.

People may view themselves as rational decision-makers and may sometimes even consider some costs and benefits of some alternatives, but they take account of very little information when they do so, and the information they do use is commonly of poor quality. Situations in which prospective criminal offenders possess large amounts of accurate information about the legal risks of crime in general, or specific potential crimes are the exception rather than the rule. As a result, knowing the actual contingencies of lawful vs. unlawful choices provides little guide to predicting or understanding the decisions people make. If this alternative model is valid, the policy implications for crime control would be that one could expect little or no reduction in rates of criminal behavior due to increased deterrence resulting from raising the certainty or severity of punishment.

## References

Akers, Ronald. 1973. *Deviant Behavior: A Social Learning Approach*. Belmont, CA: Wadsworth.

Andenaes, Johannes. 1952. General prevention: Illusion or reality? *Journal of Criminal Law, Criminology, and Police Science* 43:176–198.

Andenaes, Johannes. 1966. The general preventive effects of punishment. *University of Pennsylvania Law Review* 114:949–983.

Andrews, Donald, and James Bonta. 2006. *The Psychology of Criminal Conduct* (4th ed.). New York: Anderson Publishing.

Beccaria, Ceasare. 1764/1963. *An Essay on Crimes and Punishments*. Indianapolis: Bobbs-Merrill.

Becker, Gary. 1968. Crime and punishment: An economic approach. *Journal of Political Economy* 76:169–217.

Bentham, Jeremy. 1789/1988. *The Principles of Morals and Legislation*. Buffalo, NY: Prometheus Books.

Bohm, Robert. 2007. *Deathquest III: An Introduction to the Theory & Practice of Capital Punishment in the United States*. Newark, NJ: Matthew Bender and Company.

Bonger, William. 1916/1967. *Criminality and Economic Conditions*. New York: Agathon Press.

Braga, Anthony, and Brenda Bond. 2008. Policing crime and disorder hot spots. *Criminology* 46:577–607.

Brezina, Timothy. 2002. Assessing the rationality of criminal and delinquent behavior: A focus on actual utility. In *Rational Choice and Criminal Behavior: Recent Research and Future Challenges*, eds. Alex R. Piquero and Stephen J. Tibbetts, 241–264. New York: Routledge.

Carroll, John. 1978. A psychological approach to deterrence: The evaluation of crime opportunities. *Journal of Personality and Social Psychology* 36:1512–1520.

Clarke, Ronald, and Derek Cornish. 2001. Rational choice. In *Explaining Criminals and Crime*, eds. Raymond Paternoster and Ronet Bachman. Los Angeles: Roxbury.

Coase, Ronald. 1978. Economics and contiguous disciplines. *Journal of Legal Studies* 7:210–211.

Cohen, Lawrence, and Marcus Felson. 1979. Social change and crime rate trends: A routine activity approach. *American Sociological Review* 44:588–608.

Cook, Phillip. 1980. Research in criminal deterrence: Laying the groundwork for the second decade. *Crime and Justice* 2:211–268.

Cornish, Derek B., and Ronald V. Clarke. 1986. Introduction. In *The Reasoning Criminal: Rational Choice Perspectives on Offending*, eds. Derek B. Cornish and Ronald V. Clarke, 1–16. New York: Springer-Verlag.

Cromwell, Paul, and James N. Olson. 2004. *Breaking and Entering*. Belmont, CA: Wadsworth.

Doob, Anthony, and Cheryl Webster. 2003. Sentence severity and crime: Accepting the null hypothesis. *Crime and Justice* 30:143–195.

Ehrlich, Isaac. 1973. Participation in illegitimate activities: A theoretical and empirical investigation. *Journal of Political Economy* 81:521–565.

Ehrlich, Isaac. 1975. The deterrent effect of capital punishment: A question of life and death. *American Economic Review* 65:397–417.

Eysenck, Hans. 1964. *Crime and Personality*. Boston: Houghton Mifflin.

Feeney, Floyd. 1986. Robbers as decision-makers. In *The Reasoning Criminal*, eds. Derek B. Cornish and Ronald V. Clarke. New York: Springer-Verlag.

Felson, Marcus. 1986. Linking criminal choices, routine activities, informal control, and criminal opportunities. In *The Reasoning Criminal: Rational Choice Perspectives on Offending*, eds. Derek B. Cornish and Ronald V. Clarke, 119–128. New York: Springer-Verlag.

Ferguson, Christopher. 2010. Genetic contributions to antisocial personality and behavior: A meta-analytic review from an evolutionary perspective. *The Journal of Social Psychology* 150:160–180.

Gibbs, Jack. 1975. *Crime, Punishment and Deterrence*. New York: Elsevier.

Gottfredson, Michael, and Travis Hirschi. 1990. *A General Theory of Crime*. Stanford, CA: Stanford University Press.

Guerette, Rob, and Kate Bowers. 2009. Assessing the extent of crime displacement and diffusion of benefits. *Criminology* 47:1331–1368.

Hirschi, Travis. 1969. *Causes of Delinquency*. Berkeley, CA: University of California Press.

Hirschi, Travis. 1986. On the compatibility of rational choice and social control theories of crime. In *The Reasoning Criminal*, eds. Derek Cornish and Ronald Clarke, 105–118. New York: Springer-Verlag.

Jacob, Herbert. 1979. Rationality and criminality. *Social Science Quarterly* 59:584–585.

Jacobs, Bruce A. 2000. *Robbing Drug Dealers: Violence beyond the Law*. New York: Aldine Transaction.

Jacobs, Bruce A., and Jody Miller. 1998. Crack dealing, gender, and arrest avoidance. *Social Problems* 45:550-569.

Jacobs, Bruce A., Volkan Topalli, and Richard Wright. 2003. Carjacking, streetlife and offender motivation. *British Journal of Criminology* 43:673–688.

Jarrell, Stephen, and Roy Howsen. 1990. Transient crowding and crime: The more strangers in an area, the more crime except for murder, assault and rape. *American Journal of Economics and Sociology* 49:483–494.

Jensen, Gary. 1969. Crime doesn't pay: Correlates of a shared misunderstanding. *Social Problems* 17:189–201.

Kahneman, Daniel, and Amos Tversky. 1979. Prospect theory: An analysis of decision under risk. *Econometrica* 47:263–291.

Katz, Jack. 1988. *Seductions of Crime*. New York: Basic Books.

Kleck, Gary. 2003. Constricted rationality and the limits of general deterrence. In *Punishment and Social Control, Second Edition*, eds. Thomas Blomberg and Stanley Cohen, 291–310. New York: Aldine de Gruyter.

Kleck, Gary, Brion Sever, Spencer Li, and Marc Gertz. 2005. The missing link in general deterrence research. *Criminology* 43:623–660.

Kroneberg, Clemens, Isolde Heintze, and Guido Mehlkop. 2010. The interplay of moral norms and instrumental incentives in crime causation. *Criminology* 48:259–294.

Lattimore, Pamela, and Ann Witte. 1986. Models of decision making under uncertainty: The criminal choice. In *The Reasoning Criminal: Rational Choice Perspectives on Offending*, eds. Derek Cornish and Ronald Clarke, 129–155. New York: Springer-Verlag.

Levitt, Steven. 2002. Using electoral cycles in police hiring to estimate the effect of police on crime: A reply. *American Economic Review* 92:1244–1250.

Levitt, Steven D., and Thomas J. Miles. 2007. Empirical study of criminal punishment. In *Handbook of Law and Economics*, vol. 1, eds. A. M. Polinsky and S. Shavell, 455–495. New York: North Holland.

Marie, Olivier. 2013. Lessons from the economics of crime. *CentrePiece* Winter 2013/14 issue.

Marvell, Thomas. 2002. The impact of lambda skewness on criminology. In *Rational Choice and Criminal Behavior*, eds. Alex Piquero and Stephen Tibbetts. New York: Routledge.

Matsueda, Ross, Derek Kreager, and David Huizinga. 2006. Deterring delinquents: A rationale choice model of theft and violence. *American Sociological Review* 71:95–122.

Mele, Alfred. 2009. *Effective Intentions: The Power of Conscious Will.* New York: Oxford University Press.

Mendes, Silvia, and Michael McDonald. 2001. Putting severity of punishment back in the deterrence package. *Policy Studies Journal* 29:588–610.

Miles, Donna, and Gregory Carey. 1997. Genetic and environmental architecture of aggression. *Journal of Personality and Social Psychology* 72:207–217.

Nagin, Daniel. 1998. Criminal deterrence research at the outset of the twenty-first century. *Crime and Justice* 23:1–42.

Nagin, Daniel. 2013b. Deterrence in the twenty-first century. *Crime and Justice* 42:199–263.

Patterson, Laura, and Cynthia Koller. 2011. Diffusion of fraud through subprime lending: the perfect storm. In *Economic Crisis and Crime (Sociology of Crime Law and Deviance, Volume 16)*, ed. Mathieu Deflem, 25–45. Bingley, United Kingdom: Emerald Group Publishing.

Piliavin, Irving, Craig Thornton, Rosemary Gartner, and Ross Matsueda. 1986. Crime, deterrence, and rational choice. *American Sociological Review* 51:101-119.

Ratcliffe, Jerry, Travis Taniguchi, Elizabeth Groff, and Jennifer Wood. 2011. The Philadelphia foot patrol experiment: A randomized controlled trial of police patrol effectiveness in violent crime hotspots. *Criminology* 49:795–831.

Rengert, George, Alex Piquero, and Peter Jones. 1999. Distance decay reexamined. *Criminology* 37:427–445.

Rhee, Soo, and Irwin Waldman. 2002. Genetic and environmental influences on antisocial behavior: A meta-analysis of twin and adoption studies. *Psychological Bulletin* 128:490–529.

Schell-Busey, Natalie, Sally S. Simpson, Melissa Rorle, and Mariel Alper. 2016. What works: a systematic review of corporate crime deterrence. *Criminology & Public Policy* 15: 387–416.

Shapiro, Perry, and Harold Votey. 1984. Deterrence and subjective probabilities of arrest: Modeling individual decisions to drink and drive in Sweden. *Law and Society Review* 18:583–604.

Shover, Neal. 1996. *Great Pretenders: Pursuits and Careers of Persistent Thieves*. Boulder, CO: Westview Press.

Simon, Herbert. 1957. *Models of Man.* New York: Wiley.

Skinner, B. F. 1978. *Reflections on Behaviorism and Society*. Englewood Cliffs, NJ: Prentice-Hall.

Sutherland, Edwin. 1947. *Criminology* (4th ed.). Philadelphia: Lippincott.

Tittle, Charles. 1980. *Sanctions and Social Deviance: The Question of Deterrence.* New York: Praeger.

Tittle, Charles. 1995. *Control Balance: Toward a General Theory of Deviance*. Boulder, CO: Westview.

Tunnell, Kenneth. 1992. *Choosing Crime.* Chicago: Nelson-Hall.

Tversky, Amos, and Daniel Kahneman. 1974. Judgment under uncertainty: Heuristics and biases. *Science* 185:1124–1131.

U.S. Bureau of Justice Statistics. 2009b. *Felony Sentences in State Courts, 2006 – Statistical Tables*. Washington, DC:U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2010. *Criminal Victimization in the United States – Statistical Tables, Tables for 2007*. Washington, DC: Bureau of Justice Statistics. http://bjs.ojp.usdoj.gov/content/pub/pdf/cvus/current/cv0737.pdf.

U.S. Bureau of Justice Statistics. 2015. *Prisoners in 2014.* BJS Bulletin. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 2008. *Crime in the United States—2007*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 2015. *Crime in the United States—2014*. Available online at https://ucr.fbi.gov/crime-in-the-u.s/2014/crime-in-the-u.s.-2014/cius-home.

Vold, George, Thomas Bernard, and Jeffrey Snipes. 2002. *Theoretical Criminology* (5th ed.). New York: Oxford.

Williams, Kirk, and Richard Hawkins. 1986. Perceptual research on general deterrence: A critical review. *Law and Society Review* 20:545–572.

Wright, Richard T., and Scott H. Decker. 1994. *Burglars on the Job*. Lebanon, NH: Northeastern University Press.

Wright, Richard T., and Scott H. Decker. 1997. *Armed Robbers in Action: Stickups and Street Culture*. Boston: Northeastern University Press.

Zimring, Franklin, and Gordon Hawkins. 1973. *Deterrence: The Legal Threat in Crime Control*. Chicago: University of Chicago Press.

# 4

# GENERAL METHODOLOGICAL PROBLEMS IN PUNISHMENT RESEARCH

The findings of research on the effects of punishment on criminal behavior are often contradictory, which leads some to throw up their hands and conclude that research can tell us nothing. In fact, much of the variation in findings is attributable to methodological flaws, which differ across studies and distort findings in different ways. Thus, there may be more consistency than initially meets the eye once these technical flaws are taken into account. Therefore, it is important to understand the more common methodological problems afflicting research in the field so that we may separate the wheat from the chaff.

There is a temptation to reject evidence when it does not conform one's worldview. Citing flaws in the methods used to generate the evidence can serve as a pretext for rejecting research findings and falling back on one's ideological biases and preconceptions as guides to reality. Rejecting the relevance of research to policy on the basis of methodological flaws is foolish since there is no sound reason to believe that any alternative ways of deciding policy questions, unaided by research findings, are better. The alternatives are likely to be based on political self-interest, cultural and ideological biases, and the unexamined notions loosely referred to as "common sense."

Rather than rejecting research findings because they were generated by flawed methods, we believe that the more sensible course is to distinguish better research from worse research and draw conclusions based on the better—that is, less flawed—research. Good scholars do not treat all evidence equally, but rather distinguish findings generated by methodologically stronger research from those based on weaker research. In sum, they draw conclusions based on the technically best evidence currently available, no matter how flawed that research may be, while also conveying to their readers the limits of the evidence.

Each of the hundreds of studies of the effects of punishment on criminal behavior has its own unique constellation of problems, and any one study may have individual flaws unique to that research. While these more narrowly relevant flaws are sometimes important, these will be discussed (if at all) in later chapters when an issue to which the flaw is relevant is discussed. The present chapter instead provides an overview of the most serious and widespread technical problems that afflict the largest number of studies. We divide the discussion into two parts, one concerning macro-level research on aggregates like cities, counties, or states, and the other concerning individual-level studies.

## Common Methodological Problems of Macro-Level Studies

Many studies of punishment effects are macro-level studies of aggregates such as the populations of cities, counties, or states in which official data from sources like police, court, or corrections agencies are used to measure crime rates and various risks of punishment such as the fraction of crimes that result in arrest or the average length of sentence imposed on persons convicted. Negative associations between official crime rates and official "objective" punishment risk variables are treated as supportive of the hypothesis that greater punishment levels increase the deterrent effect of punishment and thereby reduce crime rates.

### *Aggregate Objective Risks as Proxies for Individual Subjective Perceptions of Risks*

The very fact that analysis is conducted at the level of aggregates raises the possibility of aggregation bias—relationships between variables at the level of aggregates are inaccurate as estimates of how the variables relate at the individual level. Even those scholars who use macro-level data to study deterrence concede that deterrence requires changes in individual perceptions of risk of punishment and that individual-level data would, in a perfect world, be preferable to macro-level data (Nagin 1998). Many studies of aggregates may have been motivated largely by convenience—macro-level data are easy to acquire quickly and cheaply.

Some deterrence issues, on the other hand, probably cannot be effectively studied any other way, even if resources for data gathering were ample. For example, purely individual-level research is not a feasible approach to estimating the deterrent effects of executions (or any other factors) on murder, largely due to the extreme rarity of the crime. Further, macro-level studies are quite appropriate for judging the overall net impact of policies on crime rates. They are often ambiguous as to exactly why the policies influence crime rates, but they address questions of overall effect that cannot be answered using individual-level research.

Some scholars, however, have asserted that aggregate data are actually preferable to individual data. Economists are especially skeptical about the validity of responses to questions about criminal behavior and perceptions of punishment risk in self-report surveys, and prefer official data on crime rates of aggregates and the "actual" risks of punishment. One economist studying deterrence, John Heineke (1988, 303–305), explicitly asserted what many economists very likely believe without overtly expressing it in print—that macro-level approaches are superior to individual-level research using self-report data. While conceding that individual perceptions of legal risk are an essential element in the deterrence process, he argued that aggregate punishment and crime data provide adequate proxies for individual perceptions, preferable to what he regarded as grossly invalid survey measures. He provided an unusually specific statement of the conditions that must be met in order for aggregate risks to adequately proxy individual perceptions of punishment risk: "One can use actual reward and sanction levels from the public record [to proxy their perceived counterparts] *if* the latter are increasing monotonic functions of the former." And while noting that it is an empirical question whether these conditions are met, Heineke concluded that "it seems likely that this condition will often be met" (303), albeit without providing any supporting empirical evidence on the point. As we will see, the best empirical evidence indicates that this condition is *not* generally met (Chapter 9).

For a long time economists seemed unaware that individual-level research on deterrence perceptions was even being done. As late as 1978, by which time more than a dozen such studies had been published (see Chapter 5), economist Charles Manski asserted that "almost all empirical analyses of deterrence have been based on macro models of crime commission," and cited none of the individual-level studies that had already been carried out (1978, 83). Nevertheless, Manski recognized the problems with the study of aggregates and the need for individual-level deterrence research, even discussing the prospects for self-report surveys.

Manski was, however, very much in a minority among economists in this regard. Among nearly all economists studying deterrence rigid opposition to the use of surveys made direct study of perceptions impossible. Even when research was done by economists at the individual level, the researchers still did not directly measure perceptions of legal risk, instead inferring deterrence from negative associations between prior personal experiences of punishment (as recorded in official data) and later criminal behavior, as imperfectly indexed by arrests (e.g., Witte 1980).

Virtually all macro-level research on deterrence, being based on official crime and punishment measures that were used to proxy perceived risk of punishment, is deeply flawed because it does not actually measure perceived risks or valid proxies for such perceptions. Defenders of the deterrence doctrine might ask, "Why, then, are negative crime/punishment associations so often found in macro-level research if they do not reflect deterrence?" As we will see, besides the possibility that these correlations actually reflect deterrence (or some other crime-reducing effect of punishment), there are at least four plausible methodological explanations: (1) causal order is confused, and the associations reflect the resource-straining effects of crime rates on punishment levels rather than the effect of punishment levels on crime rates; (2) the associations are spurious, especially due to the negative effects of levels of public intolerance for crime on crime rates and their positive effects on punishment levels; (3) the negative associations are an artifact of a ratio variable problem in which the (erroneously measured) number of crimes appears in both the numerator of the crime rate and the denominator of the punishment certainty measure, which generates an artifactual negative association; and (4) macro-level researchers' reliance on limited existing data hobbles their ability to control for confounding variables.

### *Causal Order—Two-Way Causation Between Crime and Punishment*

At the level of aggregates such as cities, counties, or states, higher levels of punishment might reduce crime rates, but it is also possible that higher crime rates can drive down punishment levels. Early on in the history of macro-level research on deterrence, researchers recognized that increases in crime could at least temporarily overwhelm the resources of the criminal justice system, reducing the certainty and severity of punishment delivered by the system (Fisher and Nagin 1978; Geerken and Gove 1977). For example, if the crime burden on police was suddenly increased but budgets and hiring could not be immediately increased in response, the ratio of resources to crime would go down, and, consequently, the ability to solve crimes and apprehend offenders would decline. Likewise, crime increases that resulted in a large absolute increase in prison admissions could force resource-strapped correctional authorities to release more inmates early, reducing the average time served and thus the severity of punishment.

The authors of many early studies, and a fair number of more recent ones, simply ignored the possibility of two-way causation and did nothing to address it in their statistical techniques. Some researchers did not even acknowledge the issue, while others raised it only to dismiss it, trying to argue it away on theoretical grounds. These scholars simply treated crime rates as the dependent variable, punishment levels as an independent (exogenous) variable, and estimated their crime rate models with ordinary least squares (OLS) methods, or some variant thereof, that effectively assumed one-way causation. Later deterrence scholars, however, believe that there is reciprocal causation between punishment levels and crime rates and that punishment levels must be treated as "endogenous" variables—that is, influenced by other variables in the model, such as crime rates (Nagin 1998). If OLS methods or other analytic techniques that do not address simultaneity are used to estimate the effects of punishment levels, the estimates will be biased and inconsistent and thus uninterpretable. The findings of these studies can therefore be given very little weight.

Other early scholars acknowledged the possibility of two-way causation and attempted to do something about it, but they used a totally inadequate method. They would use a punishment variable that was "lagged"—it was measured for the time period preceding the period to which the crime rate pertained. For example, researchers would measure the association between punishment levels in 1969 and crime rates in 1970. This approach had a superficial intuitive appeal because the current year's crime rate clearly cannot affect last year's punishment levels. Thus, it seems to solve the causal order problem. This impression is misleading, however, because including only the lagged version of the punishment variable really amounts to simply assuming the simultaneity problem away. By specifying *only* the lagged punishment variable as affecting crime rates, analysts effectively assume that there is no immediate, unlagged effect of punishment on criminal behavior. While some deterrent effect of punishment might well be lagged, it is highly unlikely that punishment would have no immediate effect yet influence criminal behavior after a year had passed when memories of punishment events had surely faded. The opposite is more likely—deterrent effects of punishment events will occur immediately or not at all (Jacobs 2010, 424). Further, the main statistical problem of simultaneous two-way causation is that punishment levels will be correlated with the error term for the crime rate equation. Since the lagged version of a punishment variable is almost perfectly correlated with the current, unlagged version, it will have virtually the same correlation with the error term as the unlagged version did. Consequently, the use of the lagged punishment variable will do almost nothing to reduce this correlation and the resulting distortion of estimates. Thus, studies that use this lagged punishment variable method are little better than the studies that simply ignore the causal order problem altogether.

Some variant of instrumental variables (IV) methods, most often estimated using two-stage least-squares (2SLS) methods, are generally the most feasible approach, if properly applied, to this causal order problem. In most studies, however, it is impossible to tell if the methods were properly applied because it is impossible to tell if models were properly identified and made use of relevant, valid, and exogenous instrumental variables (Fisher and Nagin 1978). Instrumental variables used to instrument punishment levels must be exogenous (they are not affected by crime rates), have reasonably strong and direct effects on punishment levels (i.e., are "relevant"), and have no effect (apart from their indirect effects via punishment) on crime rates (i.e., are "valid"). Studies using IV methods rarely report tests of the validity and exogeneity of the instrumental variables used (information on their relevance is reported somewhat more often), and some do not even describe the instruments used, making it uncertain whether the models were properly identified (e.g., Ehrlich 1973, 1975; Kleck 1979). Indeed, studies that properly document the adequacy of their instruments are so rare that there was no point to be served by noting in our review tables the few that did or by separately tabulating their findings. More recent deterrence research is not significantly better in this regard than older research (for a recent example of an IV study that did not establish exogeneity or validity of instruments, see Baltagi 2006).

Even the handful of deterrence studies that test for the adequacy of instruments commonly obtain weak or irrelevant results. Levitt (1996) used the occurrence of prison overcrowding lawsuits as instruments to predict prison population independent of crime rates. His own estimates, however, indicate that the litigation variables have little "relevance," i.e. have little effect on the size of the prison population. Levitt obscured this by focusing on the effect of litigation on *rates of change in prison growth*, rather than the size of the prison population; the net effect of litigation on the number of criminals in prison was negligible. The annual rate of prison growth in the U.S. during Levitt's study period averaged 6 percent (334), and the mean state prison population was 168 prisoners per 100,000 population (329). Levitt concluded that all of the effect of litigation was confined to the three years following a final court order to reduce overcrowding and estimated that prison growth in litigation states in this period was "almost 15 percent below the rest of the nation" (332). Since the national rate of growth was 6 percent, this implies that litigation states experienced an average growth rate of about 5.1 percent (6 percent, minus 15 percent of 6 percent). Applying these figures to states with the national average prison rate of 168, they imply a growth from 168 to 176.6 for the average litigation state, compared to a growth from 168 to 178.1 for the nation as a whole. Thus, Levitt's own figures, even if taken at face value, indicate that prison litigation reduced prison population rates less than 1 percent. Given that it is unlikely that prison population figures are even accurate to within 1 percent of their true values, it is hard to regard so slight an effect as reliable. In sum, Levitt's prison litigation status variables appear to be very weak instruments.

Another reason that these variables cannot be used as instruments is that they are not likely to be exogenous with respect to the variables they predict, which is a minimum required condition for instrumental variables (Liedka, Piehl, and Useem 2006, 255). While Levitt used the filing of overcrowding litigation to predict changes in the size of the prison population, it is obvious that growth in the prison population itself, in combination with limited prison capacity, triggers overcrowding litigation. Levitt supported the exogeneity of his litigation variables with an overidentification test, but he conceded that such tests can be artificially biased in favor of accepting the exogeneity of instruments when there are a large number of them—he used ten litigation variables in one set of estimates, and five in another (340). More importantly, these overidentification tests are valid only if one can be confident that at least one of the instruments is valid, which is more likely to be true when a diverse set of instruments is used, each very different from the others. This was not true in Levitt's study, since all his instruments were simply different measures of overcrowding litigation status and all shared the same likely problem of not being exogenous to the prison population. Inexplicably, Nagin (1998, 26–27) assessed this strategy for solving the simultaneity problem as "clever" and "plausible."

Other proposed instruments are questionable from the validity standpoint—they might affect punishment levels, but it is debatable whether they can legitimately be excluded from the crime equation. For example, in another study, Levitt (1997) sought to test the effect of police levels on crime rates and tried to identify his models using "electoral cycles" as instruments. He asserted, reasonably enough, that in election years, mayors and other local officials sometimes hire more police as a highly visible vote-getting stratagem. Thus, in a sense, election years can cause higher police levels. To properly use election years as instruments, however, he also needed to assume that election years would have no effect on crime rates other than any effect produced by the beefed-up police forces. This assumption effectively required one to believe that local authorities do nothing else in election years that reduces crime rates other than hiring more police, thereby making it legitimate to omit the election year variable from the crime equations.

This assumption is plainly implausible, since there are many other steps within the powers of local governments that can reduce crime rates and that can also be carried out in highly visible ways that could garner more election-year support for their sponsors. For example, Wilson and Kelling's (1982) "Broken Windows" thesis asserted that visible signs of disorder, such as vandalism, graffiti, and uncollected trash, can increase crime by conveying to prospective offenders the impression that local residents do not care about their neighborhood, do not pay attention to problematic behaviors, and do not effectively mobilize providers of local services, including the police. Locally elected officials obviously have the power to spend bigger shares of their budgets in election years to repair broken streetlights, paint over graffiti on public vehicles and facilities, and promptly collect trash. Such actions could make a favorable impression on the electorate in their own right as cleanup measures but could also cause lower crime rates by reducing visible signs of disorder. Likewise, local authorities could expand local jail facilities and incarcerate a larger share of the offenders arrested, reducing crime via an incapacitative effect in election years. Local prosecutors could convict more criminals, seek prison sentences for a higher share of convicted offenders, or seek longer prison sentences. Elected judges could likewise pass harsher sentences to impress the voters. If election years thereby reduce crime independent of police levels, election year variables cannot be legitimately excluded from crime rate equations and cannot serve as instruments.

Therefore, Levitt's models were probably underidentified, and his estimates of the effects of police levels on crime rates uninterpretable.

In any case, reanalyses of Levitt's data revealed that his estimates were the product of a weighting error and that when the error was corrected, there was no longer a significant effect of police levels on crime rates (McCrary 2001). In response, Levitt (2002) abandoned the election year instruments, switched to firefighters per capita as his instrument, and claimed that results using the latter instrument indicated that higher police levels reduce homicide and auto theft. Kovandzic, Schaffer, Vieraitis, Orrick, and Piquero (2016), however, showed that all of the instruments proposed by Levitt were "weak instruments" and that they therefore "cannot be used to address the potential endogeneity of police in crime equations" (133).

To put this research in perspective, Levitt was actually more careful about testing the adequacy of his instrumental variables than most other deterrence researchers who have used IV methods. Although his instruments were inappropriate, there is even less reason to place confidence in the instruments used by others. Consider the work of Wilson and Boland (1978), who sought to estimate the effect of proactive or aggressive police patrolling on crime rates. Their identification strategy required that they assume that aggressive patrol practices affected the arrest ratio (arrests/crimes), which in turn affected crime rates, but otherwise aggressive patrolling had no direct effect on crime rates (373–374). This effectively requires one to assume that aggressive patrolling cannot raise the visibility of police to prospective offenders by increasing contacts of police with suspects and thereby exert its own deterrent effect apart from any effects of higher arrest rates. Wilson and Boland, unlike Levitt, did not report any overidentification tests or any other tests of their assumption that aggressive patrolling could be excluded from their crime rate equations, so there is little basis for believing that their instruments were valid, their models identified, or their estimates believable.

Although some version of the instrumental variables/structural equation approach remains potentially the strongest method of analyzing macro-level data if applied appropriately, the approach has heretofore not been appropriately applied in much punishment research. It therefore remains unclear whether the negative crime/punishment association often observed in macro-level research reflects (1) the impact of punishment levels on crime rates, or (2) the resource-straining or other negative effects of crime rates on punishment levels.

Further, theoretical work in economics has suggested that once the possibility of crime affecting punishment levels is taken into account, the impact of increased law enforcement and punishment levels is no longer easily predictable, and the policy implications derived from early economic theories of crime such as those of Becker (1968) and Ehrlich (1973) are no longer valid. Bar-Gill and Harel (2001) argued that crime rates could have either positive *or* negative effects on either certainty or severity of punishment, and they concluded that once the "oscillating dynamic" between crime and punishment was taken into account, the deterrent effects of increased law enforcement could be either greater or smaller than those implied by the older economic theories, depending on whether, and the degree to which, changes in crime rates produce changes in punishment certainty and severity.

### *Confounding Factors—The Degree of Social Condemnation of Crime*

There are many variables that affect crime rates and that may also be correlated with punishment risks. To the extent a study fails to measure and control for them, the estimated effects of punishment will be biased because they will partly reflect the effects of these omitted variables. No study has controlled for all of them and probably no study ever could. Thus, it is a logical possibility that virtually any association between a measure of punishment risk and criminal behavior could be spurious (noncausal and attributable to the effects of one or more other factors), either totally or partially. To the extent this were true, any policy that increased punishment might produce either no reduction in crime or far less than was implied by the punishment/crime (P/C) association.

To vaguely criticize studies that find punishment effects on the grounds of a failure to control unspecified confounding factors is uninformative. One particular confounding factor, however, *can* be specified. We believe this has had an extremely widespread distorting effect on the results of macro-level deterrence studies because, to our knowledge, *every* macro-level study finding a negative association between punishment and crime failed to control for this factor. This variable almost certainly has a negative effect on crime rates and almost certainly is positively correlated with the aggregate level of legal punishment inflicted on criminals, whether that is measured by the rates of arrest, conviction, or imprisonment, the average severity of sentences imposed by courts, or the total number of people incarcerated. This factor is the average level of social condemnation of (public intolerance for or outrage over) crime.

It is common to conceptualize punishment of crime as an expression of the community's condemnation of the behavior. If the public's outrage over, or intolerance towards, a given behavior is increased, this change will be reflected in the frequency and severity of legal punishment for those who engage in the behavior. Public hostility towards criminal behavior is clearly not a constant, but rather changes over time and varies across different populations. Certainly, all individuals are not equally intolerant of crime, so one would not expect all populations, such as the residents of different cities, states, or nations, to be equally intolerant of crime. Indeed, one reason that legislatures "create" new crimes by passing laws that proscribe some previously lawful behavior, such as selling alcohol or possessing marijuana, is that "moral entrepreneurs" have stimulated elevated levels of public outrage over the target behavior (Becker 1963, 147–163). A behavior that was previously tolerated or even ignored becomes the object of public outrage. Likewise, public support for the death penalty, longer prison sentences, hiring more police, building more prisons, and generally more punitive attitudes towards criminals all reflect public outrage over crime and intolerance for deviance. Variations in moral condemnation of a behavior cause variations in whether it is punished, the degree to which it is punished, and public support for seeing that the laws forbidding it are enforced.

The level of public intolerance for crime, however, also affects whether crimes are committed independent of the law and the activities of its enforcement agents. People who live amongst, and associate with, others who have negative attitudes towards a given behavior—people such as parents, other relatives, friends, neighbors—are less likely to engage in that behavior themselves (Grasmick and Bursik 1990; Nagin and Paternoster 1994; Tittle 1980; Williams and Hawkins 1992). This is the central principle underlying many major theories of criminal behavior, especially social learning and cultural theories such as differential association theory and the subculture of violence perspective (Vold, Bernard, and Snipes 2002, Ch. 9). Thus, variations in local levels of intolerance for crime will cause variations in crime rates across areas and over time.

These variations can be very large across different areas and change fairly quickly over time. If support for more punishment of criminals is an indicator of greater disapproval of crime, then trends in punitive opinion indicate that the level of public disapproval of crime varies greatly across subsets of the American population and changes substantially over time, even over fairly short periods of time. For example, the percent who supported punishing murder with the death penalty increased from 42 in 1966 to 66 in 1981, a proportional increase of 57 percent in just 15 years. This percent peaked at 80 in 1994, then declined to 64 in 2005. That is, 20 percent of the support for punishing murder with death disappeared within a decade. Other punitive attitudes show similarly substantial variation over time (Table 1.5) and across regions and subpopulations (U.S. Bureau of Justice Statistics 2005).

The degree of public disapproval for some behaviors can even vary enough so that large shares of the population change their minds as to whether the behavior should even be illegal and thus whether it should be legally punished at all. For example, national surveys indicated that the percent of American adults who felt that the use of marijuana should be made legal increased from 20 in 1975 to 28 in the very next year—a 40 percent increase, far in excess of what random sampling error is likely to have produced. The percent favoring legalization peaked at 30 in 1978, plummeted to just 16 in the next nine years, and then went back up from 18 in 1991 to 32 by 2000. By 2009, less than half of Americans opposed legalization (Table 1.5; U.S. Bureau of Justice Statistics 2012, 158–159). Thus, the degree to which Americans disapproved of marijuana use—enough to make it legally punishable conduct—varied a great deal and changed substantially over short periods of time in America. Note that these figures represent national averages across all areas and subpopulations. Variation over time within subsets of the population, such as the residents of cities or states, is even larger, since trends in public opinion tend to be more unstable in smaller populations.

In sum, levels of public disapproval of criminal behavior vary greatly across time and space, negatively affect the frequency of that behavior, and positively affect the level of punishment directed at the behavior. Thus, levels of disapproval have opposite-sign effects on punishment and crime, tending to produce a spurious, i.e. noncausal, negative association. There appears to be little reason to question that punishment/crime associations in deterrence research are biased by the failure to control for public intolerance of crime in favor of pro-deterrence findings; only the magnitude of this distorting effect is in serious doubt.

Criminologists and sociologists have recognized this problem for decades. Glaser and Zeigler (1974) pointed it out in connection with murder rates and the death penalty, while Williams and Hawkins (1986, 548) identified this as a serious flaw in macro-level economic research on crime and deterrence in general, noting that the crime-suppressing effects of moral condemnation can easily be misinterpreted as deterrent effects of punishment (see also Williams, Gibbs, and Erickson 1980). Erickson, Gibbs and Jensen (1977) had previously noted the corresponding problem in connection with individual-level perceptual research on deterrence—individuals' perceived certainty of punishment for various acts was so highly correlated with their level of moral condemnation of those acts that it was difficult or impossible to establish if there was any effect of the former beyond the effects of the latter. They concluded that "*until defenders of the [deterrence] doctrine show that the relation between properties of legal punishments and the crime rate holds independently of the social condemnation of crime, then all purported evidence of general deterrence is suspect*" (316, emphasis added). That is, if levels of social condemnation of crime are not adequately measured and statistically controlled, e.g. by including the variable in a multivariate model of crime rates, any observed negative association between punishment levels and crime rates is almost certainly at least partially spurious. As it happens, macro-level deterrence studies have never included direct measures of moral condemnation of crime in their models, and thus all remain subject to the grave doubts expressed by critics decades ago. The problem remains as serious today and just as unsolved as it was when it was first recognized decades ago.

Closely related to public moral condemnation or intolerance of crime is the capacity to express this intolerance in a way that affects the behavior of prospective offenders. Independent of the operation of the criminal justice system and its paid specialists in crime control, people exert informal social control over their associates through mechanisms such as overt criticism, disapproving glances, gossip, ostracism, shunning, withholding of favors, and a host of other social mechanisms. The strength of the social institutions that exert control, however, varies sharply across space and time. The residents of neighborhoods characterized by social disorganization—as manifested by weak family structures, population transience, and a weak sense of neighborliness or belonging to a community—are less effective in controlling rule-breaking behavior. Neighbors who do not know one another or care about one another's opinions are not likely to alter their behavior when they are shunned or ostracized by others, reducing the effectiveness of informal controls and increasing crime. Likewise, people who do not even know their neighbors are unlikely to inform them when they witness or hear of misconduct by a neighbor's child (Sampson 1986).

Unfortunately these sorts of socially disorganized areas tend to be the same areas where formal, legal controls are ineffective. Citizen cooperation with police is poor, the reporting of crimes to the police is spotty, and assistance with police investigations is often grudging and limited. Police patrol frequency is low relative to crime levels, and arrest clearance rates are correspondingly low (Sherman 1986). Criminal sentences imposed in high-crime urban areas also tend to be less severe, perhaps because crimes have less power to shock and evoke harsh sentences from judges in places where they are more common (Pope 1975). In sum, residents of socially disorganized neighborhoods tend to be subject to weaker levels of both formal legal controls and of the informal social controls that, under more favorable circumstances, would be exerted by families, friends, teachers, neighbors, and others.

But if the strength of formal legal controls is low in the same places where these informal controls are also weak, it is easy to wrongly attribute high crime rates to weak legal controls that were actually due to weak informal social controls. High crime rates partly due to weak family structure might be mistakenly attributed to the shorter prison sentences imposed by local courts or the lower frequency of police patrols relative to crime. Thus, isolating the impact of levels of legal punishment in macro-level studies requires measuring and controlling for the strength of institutions exerting informal control, such as families, churches, schools, or other community organizations.

Macro-level punishment researchers rarely acknowledge the potential risks of omitting measures of social condemnation of crime, social disorganization, or the strength of informal control mechanisms, from their models of crime rates. Those who conclude that higher punishment levels cause lower crime rates almost never measure and control for these specific confounding factors and rarely even mention them in order to temper readers' confidence in their conclusions.

One possible explanation for these critical omissions may be that some scholars, especially economists, are uncomfortable with dealing with concepts that are hard to quantify, however real their effects might be. To be fair, however, statistics that could be used to measure the operation of informal controls are rarely as routinely generated by governments, as statistics on arrests and prison populations are. This makes it hard for macro-level researchers to separate the effects of informal controls from those of punitive crime control efforts. Governments track what governments do to a greater extent than they track what families and local communities do. Thus the tendency of researchers to attribute to governmental controls that which is actually due to informal nongovernmental controls is to some extent a byproduct of the kinds of controls that governments document in official statistics. Nevertheless, it is often possible to at least control for indirect indicators of the strength of informal control institutions, such as measuring the strength of families in an area or time period by the divorce rate or the share of households headed by a single parent, even though many researchers fail to use such data.

### *Confusing Deterrence With Displacement*

Displacement was discussed in Chapter 3 in connection with the implications of constricted rationality for offender target choice. Displacement also creates methodological problems. When legal risks are raised in a relatively small area, such as a neighborhood, prospective offenders might not commit fewer crimes but might instead merely shift their activities to another area not subject to the elevated risks. Similarly, risks raised only for a limited span of time can lead some offenders to merely delay the crimes they otherwise would have committed until the risks returned to the previous levels that had not been sufficient to deter the offenders. Likewise, when the risks of one type of crime are raised, offenders might shift to other crime types not subject to those increased risks, committing the same amount of crime. These are different forms of displacement, not deterrence (see the Chapter 2 discussion of the distinction). There is generally no value in mere displacement unless offenders are displaced to less harmful crimes or to victims better able to sustain the harms, while displacement to more harmful crimes or to victims who are less able to absorb losses is counterproductive.

Consequently, it is essential that macro-level research evaluating the deterrent effects of locally focused risk-elevating interventions, such as beefed-up police activity, carry out thorough checks for displacement, especially spatial displacement. That is, the research methods must provide a credible answer to the question: "Were prospective criminals deterred from committing some crimes, or were they merely displaced to other areas, times, or offense types?" The unfortunate reality is that most research of this type does nothing to check for displacement, and the minority of studies that do address the problem carry out only the most cursory checks for spatial displacement, such as checking for crime increases only in areas immediately contiguous to the intervention areas (Chapter 3). Such studies have little to say of a persuasive nature about whether the intervention actually deterred or otherwise reduced crime rather than merely displacing it.

### *Ratio Variables, Measurement Error, and Artifactual Associations*

The certainty of punishment in macro-level studies is typically measured with a ratio variable, such as the number of arrests (A) divided by number of crimes (C). A study that relates this ratio to the crime rate—crimes divided by population (C/P)—is estimating the association between two ratio variables, (A/C) and (C/P). We know that crimes are measured with a great deal of error, since most crimes are not reported to the police and many of those reported are not recorded by police or otherwise not passed on to authorities in charge of compiling crime statistics. Thus, the quantity C is likely to be substantially too low in both ratio variables, causing A/C to be higher than it should be and C/P to be lower than it should be. Thus, the same places or times that had misleadingly high measured arrest rates would also have artificially low measured crime rates. Even if greater certainty of arrest actually had no effect whatsoever on crime rates, the former would tend to be negatively correlated with the latter solely as a result of the substantial error in measuring crime (Blumstein, Cohen, and Nagin 1978; Gibbs and Firebaugh 1990).

Sometimes the problem arises in disguised form, as when a specific crime type is just one of the components in the denominator of the punishment ratio and/or the numerator of the crime rate. For example, Sampson (1986) concluded that the local risk of incarceration (jail rate) had a deterrent effect on robbery rates, but his measure of local incarceration risk was number of jail inmates per 100,000 violent offenses known to police. Since robberies are a major component of the violent offenses total, this meant it was both a major component in the denominator of the punishment variable, offenses known to police, and the sole component in the numerator of the robbery rate. Police statistics on robbery frequency are subject to substantial error, so Sampson's negative association was at least partly, and possibly entirely, an artifact of his use of ratio variables. Significantly, when Sampson performed the same analysis with the homicide rate as the dependent variable, he found no significant association with the local incarceration risk. Since homicide claims only about 1 percent of violent offenses total (U.S. Federal Bureau of Investigation 1999), it contributed little to the denominator of Sampson's punishment variable and thus generated little artifactual negative association that could be misinterpreted as reflective of a deterrent effect. Thus, Sampson's evidence only supported deterrence when it benefited from the spuriousness produced by his use of ratio variables.

Levitt's (1998) state-level study involved a similar problem of common components in ratio variables. One of his dependent variables was the estimated number of violent crimes committed by juveniles, divided by population, while one of his measures of juvenile incapacitation was the number of juveniles in custody divided by the estimated number of violent crimes committed by juveniles. Again, since the common component (violent crimes committed by juveniles) is subject to substantial measurement error and appeared in both the denominator of the punishment variable and the numerator of the crime rate, the estimate of the punishment variable's effect was biased in a negative direction. Levitt conceded (1163) the existence of "ratio bias" and presented alternative results using a different measure of the incapacitation of juveniles not subject to this problem—juveniles in custody divided by population. Levitt noted that this latter measure also has a significant negative association with juvenile crime rates and implied that his conclusions did not depend on estimates afflicted by "ratio bias." He buried in a footnote, however, the fact that support for a punishment effect was far weaker when it was not helped by this bias—the ratio of the punishment coefficient over its standard error was just 2.9 for the measure without the "ratio bias" compared to 6.3 when the association was helped by ratio bias (1166). Notwithstanding Levitt's efforts to downplay the problem, his own results indicated that common components in ratio variables more than doubled the appearance of support for punishment effects.

This problem afflicts *most* macro-level research on the supposed deterrent effect of certainty of punishment, and findings concerning certainty in turn provide *most* of the macro-level support for any kind of deterrent effect of punishment on crime (see Chapter 7). Thus, the enormous and seemingly impressive body of macro-level evidence supposedly showing crime-control effects of punishment rates on crime rates is actually far more fragile than it seems. Most macro-level deterrence research does nothing to solve this problem. Gibbs and Firebaugh concluded that there was "no definitive way to demonstrate whether the negative correlation between the crime rate and the objective certainty of punishment reflects deterrence or merely measurement error" (1990, 347). This may have played a role in some scholars' decision to refrain from further use of macro-level tests of deterrence.

Other scholars have reacted to this problem by continuing to do macro-level research on deterrence but dropping the use of ratio variables. They instead relate crime rates or counts to raw numbers of arrests, convictions, or imprisonments. Unfortunately, while this strategy sidesteps the ratio problem, it does violence to the basic concept of certainty of punishment, since certainty is an inherently ratio-related concept—that is, it necessarily reflects the relative frequency of punishments and crimes. Thus, studies dropping the use of ratio variables to measure certainty of punishment fail to capture the essence of the concept and thereby fail to test its effect on crime.

On the other hand, it could be argued that it is not the certainty of punishment that matters, but rather the sheer frequency of punishment events. It is the former that determines how large an audience receives the deterrence message, how often they receive it, and possibly how certain they perceive punishment to be, even though raw frequency is not itself a measure of certainty. Thus, the raw number of punishment events in an area or period may affect the average perceived certainty, even though it does not measure perceived certainty. From the standpoint of the effectiveness of the communication of legal threats, one could argue that it is not essential to measure certainty at all. This would, however, necessitate abandoning the conventional expected utility model of criminal behavior, since the certainty or probability of punishment is one of the main components of utility.

The ratio variable problem is not limited to macro-level research. Some individual-level studies that have explored the relationship between personally experienced punishment and later criminal behavior also have used ratio variables to measure the *experienced* certainty of punishment—for example, previous arrests divided by prior (self-reported) criminal acts (Chapter 6). Grogger (1991) studied young men released from California prisons to test for deterrent effects of experienced punishments on their criminal behavior during a single post-release year, 1986. His proxy measure of criminal behavior was the number of arrests experienced during that year, while his measure of punishment certainty was the number of convictions in the previous seven years (c. 1978–1985) divided by the number of arrests during that same period. The number of arrests in 1986 was almost certainly positively and strongly correlated with the number of arrests in the immediately preceding period, given that they basically measured the same thing. Grogger's dependent variable (arrests in 1986) thus was virtually a proxy for the denominator of his measure of punishment certainty (arrests in 1978–1985)—tantamount to having the same variable as the dependent variable and the denominator of the independent variable measuring certainty of punishment. Since arrest counts are certainly subject to measurement error, this tends to bias the certainty/crime association in a negative direction, artificially favoring the deterrence hypothesis (for similar problems in other individual-level deterrence studies, see Tauchen, Witte, and Griesinger 1994; Witte 1980).

In contrast, macro-level measures of punishment *severity*, such as average length of prison sentences imposed or served, do not have this ratio variables problem—there are no common components built into both the punishment variable and the crime rate. It is therefore noteworthy that most macro-level tests of the impact of severity find no measurable impact on crime rates (Chapter 7). Part of the explanation may therefore be methodological rather than substantive—estimates of severity's deterrent effects cannot "coast" on the contributions of artifactual negative associations, as estimates of certainty's effects can.

### *Distinguishing Deterrent Effects of Punishment From Incapacitative Effects*

Nearly all the measures of punishment certainty and severity used in macro-level studies could contribute to the size of the incarcerated criminal population, and thus the incapacitative effects of locking up criminals. If a larger fraction of crimes result in arrest, a larger share of arrestees are convicted, or a higher percent of those convicted are given prison sentences, all of these will tend to produce a larger prison population, other things being equal. Likewise, if the average prison sentence imposed on criminals is longer, this would tend to result in larger prison populations if there were no countervailing forces, because it could produce, in any given year, a growing accumulation of prisoners still serving terms imposed in previous years, even those imposed a decade or more in the past. Combined with even stable numbers of new admissions to prison, the result of inmates finishing longer prison terms would still be a growing prison population, unless counterbalanced by increased releases.

Thus, unless researchers measured and controlled for the size of the prison population, they could not convincingly isolate the deterrent effects of higher certainty or severity of punishment for crime (Gibbs 1975, 58–65). As Levitt put it, "most purported [macro-level] tests of deterrence are in reality joint tests of deterrence and incapacitation" (2002, 436). While a handful of macro-level deterrence researchers have controlled for prison population size to help isolate deterrence effects (e.g., Kleck 1979; Levitt 1995; Vandaele 1978), the vast majority have not. Thus, even when macro-level researchers do adequate jobs in addressing the other methodological problems and therefore have some foundation for interpreting findings as indicating some kind of crime-reducing impact of punishment, they usually cannot know whether there were any deterrent effects of punishment levels, or any other crime-reducing effects, above and beyond incapacitative effects.

In an attempt to separate the two effects, Kessler and Levitt (1999) employed a strategy that required strong, and ultimately implausible, assumptions about the timing of deterrent vs. incapacitative effects. They reasoned that when new laws imposed additional penalties for crimes, these laws could have immediate deterrent effects but could not immediately add to the incapacitative effect of punishments because it would take at least a year before criminals sentenced under the new laws would be admitted to prison and affect the size of the prison population. Kessler and Levitt, however, provided no direct evidence that perceptions of legal risk among prospective offenders changed after the new law went into effect, either in the short or long run, or even any plausible reason to believe that criminals were immediately aware of the new law and thus should have been deterred from crimes within the first year after the law's implementation. Indeed, there is just as much reason to expect lagged deterrent effects as lagged incapacitation effects, assuming it takes a while for "word to get out" or for enough offenders to have personally received enhanced sentences for it to have a measurable impact on the crime rate.

On the whole, the methodological problems afflicting macro-level research on deterrence eventually came to be regarded as so serious that the approach lost favor and was largely replaced, among all but economists, by individual-level survey-based perceptual studies. The two major exceptions were research on the death penalty (Chapter 9), for which individual-level work seemed to be infeasible, and research on the incapacitative effect of the size of the prison population on crime rates (Chapter 10), which was an inherently macro-level issue. Nevertheless, macro-level research still has a role to play in assessing the impact of punitive crime control policies. The approach can provide global estimates of the net, overall effect of some change in policy, even if it is not able to test how much of that effect can be attributed to any given mechanism, such as deterrence. Direct tests of deterrence hypotheses, on the other hand, require perceptual data, and thus an individual-level approach.

This long list of largely unsolved problems with macro-level deterrence research lead statisticians Stephen Brier and Stephen Fienberg, after reviewing the first decade of "econometric modeling of crime and punishment," to conclude that there was "no reliable empirical support in the existing economics literature either for or against the deterrence hypothesis" (1980, 147). As to how the effects of punishment should be studied, they stated: "we believe that much more attention in the future should be focused on studies of individual criminal behavior" (151). Most of the flaws in official crime and punishment data identified by Brier and Fienberg still exist, and many of the statistical problems (such as underidentified models, arbitrary selections of control variables, and the failure to control for the public's moral condemnation of crime) remain unsolved, so there is no reason to draw a different conclusion about research done in the decades since their review.

## Common Methodological Problems of Individual-Level Studies

In reaction to these grave, and in some cases unsolvable, problems with macro-level research, many scholars, beginning in the 1970s, turned to individual-level strategies for assessing the effects of legal punishment on crime. Most of this research is called "perceptual" research because it uses survey methods to directly measure perceptions of the risk of legal sanctions and to relate them to criminal behavior. It can address the effects of punishments actually experienced by offenders as well as the effects of perceptions of potential punishment. Most of it uses survey methods to directly measure individuals' perceptions of legal risks and measures criminal behavior through respondents' self-reports rather than relying solely on official records. Thus, it is able to examine both crimes known to police or other authorities and self-reported crimes unknown to the authorities. Individual-level research, however, is afflicted by its own methodological problems.

### *Measurement of the Dependent Variable—Measuring Criminal Behavior Through Self-Reports*

Although some perceptual research uses official measures of criminal behavior, such as police records of arrests, most of it relies on self-report measures of criminal or delinquent behavior. That is, researchers ask people whether, or how many times, they have committed various kinds of illegal acts. The principle problem with this methodology is validity—whether people can be relied upon to fully and accurately confess to illegal acts.

Where it is possible to carry out relatively strong tests of the validity of self-reports of criminal behavior, the results indicate low validity. Claims to the contrary typically rely on reviews of research that primarily focus on assessments of *reliability* (the tendency of subjects to give consistent answers) rather than validity. Good measurement reliability may merely indicate that people lie about criminal conduct in consistent ways, not that they rarely lie. When the more optimistic assessments of the measurement adequacy of self-reports do address validity per se, they apply vague standards of adequacy as to "how valid is valid enough?" (e.g., Junger-Tas and Marshall 1999).

Tests of measurement validity in self-reports of criminal or delinquent behavior are commonly weak ones, such as establishing a correspondence between self-reported arrests and official arrest records (e.g., Hindelang, Hirschi, and Weis 1981). Since most survey respondents presumably know that the surveyors can check their responses about arrests against arrest records, they know that any misreports they might offer concerning arrests could be detected by the researchers. In contrast, a respondent who has committed a crime for which he was not arrested is free to deny that crime in a self-report survey without fear of his lie being detected. Consequently, a correspondence between self-reports and recorded arrests is likely to tell us little about whether there is a similar correspondence between self-reports and crimes whose perpetrators are unknown to the police.

Stronger direct tests of validity tell a more pessimistic story. Violations of criminal laws forbidding use of certain drugs are among the few forms of criminal behavior that can be definitively measured in a way that permits checks of the accuracy of self-reports, since the use of substances like marijuana, cocaine, or heroin can be detected with near-perfect accuracy using modern physiological tests of urine or hair samples. Validity studies have first asked respondents (Rs) to self-report illicit drug use then tested the Rs for the presence of the metabolites of drugs in their urine, allowing the researchers to determine the share of those who in fact were using drugs and had denied it in the survey. In one especially large-scale study, thousands of arrestees in six cities were interviewed then urine tested. Of those found to be drug users, based on urine tests, 52 percent of the crack cocaine users had falsely denied crack use in the self-report interviews, as did 36 percent of marijuana users, 54 percent of opiate users, and 44 percent of methamphetamine users (Lu, Taylor, and Riley 2001; see also Fendrich and Johnson 2005). For other kinds of criminal behavior, admission rates are likely to be even worse, since some drug users must be aware that the accuracy of their answers could be checked via drug testing, whereas the accuracy of self-reports of other types of criminal behavior cannot be checked against anything except official records of the few offenses known to the authorities.

The sheer rate of concealing or other underreporting, however, is not determinative of whether findings based on self-reports can be believed. Even if the average person concealed most of their criminal acts, as long as the tendency to conceal (as measured, e.g., by the percent of criminal acts not reported) was not correlated with perceptions of punishment risk or criminal behavior, estimates of the effect of those perceptions on criminal behavior would be unaffected. Unfortunately, underreporting is likely to be neither uniform nor random. For example, Lu et al. (2001) found that failure to report illicit drug use was significantly related to the user's age, sex, ethnicity, city, prior arrest record, and how much the user spent on drugs.

More specifically, those who are most fearful of legal punishment might for that very reason be the most likely to conceal their criminal acts. Surely one reason why some people are reluctant to confess their crimes to survey researchers is that they fear that the information could somehow find its way to the authorities and result in the offender's punishment. This means that people who were fearful of punishment but who nevertheless committed many crimes would be mismeasured as having committed few or no crimes as a result of their concealment of a larger-than-average share of those crimes. If greater fear of punishment motivates more concealing of crimes, such persons should be classified as counterexamples to the deterrence thesis, since they committed many crimes despite perceiving higher risk of punishment. They would not, however, be properly detected as such if the pattern of reporting bias in self-reports is as we have hypothesized. Unfortunately, this hypothesis has not been empirically tested—we do not know that greater fear of punishment motivates more concealing of criminal acts in self-report surveys. While it is plausible on logical grounds, and could seriously bias findings of such studies in favor of the deterrence thesis if it were true, it must remain a speculation until deterrence researchers perform credible validity checks on self-reports of criminal behavior and test whether levels of validity differ by the self-reporter's perceptions of the risks of legal punishment.

### *Measuring the Chief Independent Variables—Perceptions of Punishment Risks*

Should researchers ask about the R's perceptions of legal risk to themselves, to persons *like* themselves, to friends and other people they know, or risks prevailing in the larger population? Each strategy has its strengths and weaknesses. Asking about risks to the R himself has the advantage of directly measuring what would seem to be the perception most likely to influence the R's own behavior; perception of risks applying to others might be irrelevant to the R's own behavior. Further, the R is almost certainly more knowledgeable about his own perceptions than those of other people. It has the disadvantage that people may be less willing to be candid about their own perceptions. They may be reluctant to admit they think it is unlikely they will be caught because it might suggest they would be willing to commit crimes. Thus, people may be more willing to talk about others, but less knowledgeable, while being more knowledgeable about their own perceptions of risk but less willing to share them with strangers.

Asking about both the R's perceptions of their own risks *and* the risks of others known to the R may provide a more complete measure of the full set of risk perceptions that could affect criminal behavior, if one assumes that people are influenced to at least some extent by the experiences of others in arriving at their own assessments of risk. Some people may even be aware of the dangers of forecasting their own future risks based on such a slim evidentiary foundation as their own limited experiences, and so they take account of not only their own risks of punishment, but also what they believe are the risks applying to others they know and what they believe are the risks applying to people in general.

Further, some (e.g., Teevan 1976) have argued that by asking the R about other people's risks, the analyst avoids the problem of two-way causation, in which the R's past criminal behavior (especially committing crimes and not being punished for them) influences their current perception of the risk of punishment. The optimistic hope is that the R's past experience with crime and punishment will not influence their assessments of *other peoples'* risks of punishment, so any association between perceptions of risk and criminal behavior must reflect the effect of risk perceptions on criminal behavior, rather than the reverse.

Stafford and Warr (1993) argued that the distinction between special and general deterrence was merely the distinction as to whether people were influenced by punishment risks and experiences involving themselves (direct personal experiences), or by their indirect, vicarious "experience" of other people's punishment. In this light, a researcher might either measure the individual's perception of his own chances of punishment or his perception of the chances prevailing among other people he knows or among people in general. Logically, the individual's perception of his own chances of punishment should be more likely to influence criminal behavior, and this is indeed what research generally has found (Chapter 5; Williams and Hawkins 1986, 550). On the other hand, perceptions of risk as they pertain to the larger population, because they pertain to a larger number of people, may be more likely to correlate with actual (objective) punishment risks prevailing in that same population. And if the perceptions of risks prevailing in the general population are *not* correlated with actual punishment levels, it is highly unlikely that perceptions of an individual's own individual legal risks would be correlated with aggregate (e.g., city-wide or state-wide) punishment risks, given that the risks do not even pertain to the same people.

If two Rs both perceive that the average severity of punishment for robbery is three years in prison, can we assume that they perceive the same severity of punishment? They may objectively have the same belief about severity of punishment, but one person may perceive a three-year prison term to be far more severe than the other person does. A person who has previously served a prison term is likely to assess a three-year prison term to be a considerably less serious punishment than a person who has never spent a day in jail. Likewise, subjective assessments of severity are likely to be influenced by how pleasant or unpleasant one's life outside of prison is, if the quality of time in prison is assessed relative to the quality of life outside of prison.

If deterrence is generated by a sense among prospective offenders that punishment is likely if they commit crimes, is this perception generated by some kind of rough-and-ready comparison of the number of punished crimes with the total number of crimes (committed by an individual actor or by the population in a given area and time period)? Or does the sheer volume of punishment events determine the perception of punishment certainty independent of any awareness of the volume of crime? If prospective offenders are deterred by the former, the appropriate measure of perceived punishment certainty should take account of this comparison, e.g. by asking a question like "Of every 100 people who commit a burglary in your area, about how many do you think will be arrested for the crime?" On the other hand, if the latter process accurately describes how individual perceptions of risk are generated, only the actor's perception of the absolute volume of arrests or persons sentenced to prison would be relevant. Likewise, in macro-level research, only raw counts of punishment events (or perhaps per capita rates of such events) would be relevant, and there would be no point to measuring the percent of crimes resulting in arrest or the percent of arrests resulting in conviction.

### *Should Tests of Deterrence Be Offense-Specific?*

It has often been suspected that some crimes are more deterrable than others. Most commonly, scholars have argued that instrumental crimes are more deterrable than expressive crimes. Instrumental crimes are those committed as a rational means to achieve some other goal, such as robbery or burglary committed to gain property, while expressive crimes are those that express an emotion and bring pleasure or reduce pain in and of themselves, such as illicit drug use or anger-instigated assaults. Others hypothesize that crimes that are typically planned are more deterrable than those that are not (Andenaes 1952, 1966). If the impact of perceived legal risk has more effect on some types of criminal behavior than on others, estimating deterrent effects using methods that lump many different types of criminal behavior will at minimum miss this variation and, at worst, can yield conclusions that are misleading. If primarily less deterrable offense types are studied, estimated deterrent effects will tend to be low, while studying the more deterrable types will have the opposite effect.

This has implications for how both dependent and independent variables should be measured. If effects of legal threats are offense-specific, measures of legal threats or individuals' perceptions of the threats need to be offense-specific, and their relationship to specific kinds of criminal acts must be analyzed. For example, in a perceptual study, it may be necessary to measure Rs' perceptions of the risk of being arrested for burglary and relate these perceptions to burglary behavior. Further, composite scales that lump together many different types of criminal behavior implicitly assume there is just one dimension of general criminality underlying all the offenses, when in fact there may be multiple dimensions that would be better captured if scales measuring a small, homogenous set of offenses or even variables measuring single offenses were used.

Deterrence research has not yet revealed any clear pattern of variation in deterrent effects by offense-type. Nevertheless, it is commonly found that there *are* differences in estimated deterrent effects across offense types, however unpatterned they seem to be (Chapter 5; Paternoster 1986). Therefore, throughout our own reviews of deterrence research, we have favored presenting offense-specific results whenever the original authors provided them.

### *Causal Order and Two-Way Effects*

Deterrence researchers recognized early on that there could be an effect of criminal behavior on perceptions of the risk of punishment, as well as an effect of those perceptions on crime (Saltzman, Paternoster, Waldo, and Chiricos 1982). For example, those who committed many criminal acts without getting caught would tend to lower their perceived levels of risk. Thus, a negative association between these perceptions and criminal behavior could reflect an "experiential effect" of criminal behavior on perceptions of risk rather than a deterrent effect of perceived risk of punishment on criminal behavior. There is now a large enough body of sound research to conclude that more criminal offending causes perceptions of the risk of legal punishment to be reduced (Lochner 2007; Paternoster 1987; Paternoster, Saltzman, Waldo, and Chiricos 1985; Pogarsky, Piquero, and Paternoster 2004; Saltzman et al. 1982). Therefore, it is impossible to isolate any deterrent effects of punishment unless one takes account of these possible two-way effects.

There is also another possible two-way relationship that has been ignored in deterrence research. The intention to commit crime can be conceptually distinguished from the actual commission of crime, and the two can have distinct effects on perceptions of punishment risk. The experiential effect involves actual criminal behavior, most of which goes unpunished, reducing perceived risk. The intention or desire to commit crimes can, however, also cause people to reduce their estimates of the certainty and severity of punishment as a psychological stratagem for reducing the anxiety that comes from both desiring to commit crimes and believing that punishment is likely or severe. Thus, intentions to commit crime can reduce perceptions of risk in a process separate and distinct from the experiential effects of committing crimes and not being punished. Because intentions to commit crime are often not acted upon, and thus are not perfectly correlated with criminal behavior, controlling for prior criminal behavior will not completely account for this effect.

Panel designs are probably the strongest strategy for disentangling (a) the effects of the perception of punishment risk on criminal behavior from (b) the effects of criminal behavior on perceptions of risk. In panel studies the same set of individuals are questioned at multiple points in time, e.g. interviewed at one-year intervals for three years. Panel designs allow a clear temporal ordering between risk perceptions measured in an earlier wave of interviews and self-reported criminal behavior committed in a later time period. They do not eliminate the problem of simultaneous (or "synchronous") two-way causation, but they permit more plausible methods for identifying and estimating models of such relationships (Finkel 1995). The major shortcomings of panel studies are that (a) they are expensive and time-consuming, as they effectively require carrying out the same survey project multiple times, repeatedly relocating the same set of respondents, and waiting years for data gathering to be completed and (b) they measure perceived risks at times that are generally remote from when criminal decisions were made.

### *Vignette Methods*

One of the more popular ways used to address the causal order problem, without resorting to expensive panel designs, is to carry out a one-time cross-sectional study in which researchers use a subject's statements about his intentions to offend, or forecasts (predictions, projections) of his future criminal behavior, as the dependent variable and relate it to perceptions of legal risks measured at the same time. Patricia Erickson (1976) pioneered this technique, relating subjects' perceptions of legal risk at the time of interview to their forecasts of their future likelihood of continued marijuana use (see Tittle 1977 for another early use of forecasts). More recently, forecasts of future offending are commonly used in conjunction with a vignette technique. In these studies, each subject is presented with hypothetical vignettes or scenarios in which they would have an opportunity to commit a crime and are then asked either (a) whether they would commit the crime or (b) how likely they estimate it to be that they would commit the crime (sometimes referred to as the "willingness to offend"). Researchers either assign a punishment risk by incorporating it into the vignette, or they ask the respondents for their own estimates of punishment risk.

Advocates of the forecast, or vignette, method suggest that they have reduced the causal order problem because the criminal behavior measure refers to the future, while perceptions of legal risk pertain to the present. (A few cross-sectional researchers tried to elicit respondents' recollections of *past* perceptions of risk and relate them to their current self-reports of recent criminal behavior, but this strategy never became popular, probably because of well-founded doubts about whether people could accurately recall their subjective states, such as past perceptions of legal risk, for even relatively recent times in the past.) In this respect, the vignette method can be regarded as a less expensive and time-consuming substitute for panel methods. The method is also thought to better capture very short-term deterrent effects of risk perceptions that change quickly and might be missed in panel studies (Grasmick and Bursik 1990; Piliavin, Thornton, Gartner, and Matsueda 1986).

In addition to its cheapness, another attractive feature of the vignette approach is that it lends itself easily to experimental manipulation. Respondents can be presented with scenarios in which various elements are randomly varied across respondents to determine the effect of a varied attribute (such as the probability of being caught) on the respondent's predicted behavior. By randomly assigning values of the experimentally manipulated variables, researchers ensure that these variables are not correlated with any other determinants of criminal behavior, allowing the analyst to more easily separate the effect of the manipulated variable. The assigning of certainty levels by the researchers, however, has been criticized for introducing additional artificiality into the vignettes, forcing on many respondents beliefs about risk levels that they would never hold in real life (Klepper and Nagin 1989, 729). As a result, stated likelihoods of offending may be correspondingly artificial, having little to do with how respondents would behave in real life.

On the other hand, the artificiality of the vignette method may offer an advantage in getting respondents to "confess" to crime. People may be more disposed to confess to a willingness to commit hypothetical crimes, as they are asked to do in vignette research, than they are to confess to actual criminal acts committed in the past, as they are asked to do in panel and other non-vignette studies. One can be legally punished for committing or attempting to commit crimes but not for merely being willing to do so.

There are, nevertheless, serious problems with the forecast/vignette approach. First, the impression that the causal order problem has been solved because risk perceptions and criminal behavior pertain to different time periods is largely illusory. Both variables are measured at the same time, including the forecasted criminal behavior or "willingness to offend." Thus, even though *actual* future criminal behavior cannot affect current perceptions, the present-time *forecast* of hypothetical future behavior *can* affect present-time reported perceptions of legal risk, as well as the reverse. For example, if a person forecasts a high likelihood of future criminal behavior, this present-time perception can motivate them to also assess the risk of punishment as low, as a matter of wishful thinking. Or a person who had stated that they perceived a high level of risk might understate their likelihood of offending as a way of presenting themselves as rational based on the theory that only a fool would predict a high likelihood of committing a crime if the risk of being punished for it was high. We discuss this latter possibility in further detail later.

Second, the forecasted measure of criminal behavior is hypothetical, not a report of actual behavior, and may be less valid as a proxy for criminal behavior than self-reports of actual past acts for this reason alone. Self-reports of past criminal behavior are themselves questionable enough from a validity standpoint, but shifting the focus to hypothetical future acts aggravates the problem. Respondents may not give serious thought to hypothetical questions because they do not pertain to their experiences or the realities of their own lives. Further, they can more easily misstate future intentions without feeling any sense of guilt for doing so, since it would not be clear to them that they were in fact mischaracterizing their likely future behavior. In contrast, while survey respondents asked about actual crimes they have committed can always lie, they are likely to be conscious of the misstatements as lies and to anticipate a feeling of guilt if they lie, which would discourage many Rs from telling the falsehoods.

Third, responses to hypothetical vignettes may be especially artificial because the vignettes are perceived by respondents as irrelevant to themselves. Some vignette researchers favor very specific, detailed scenarios of potential criminal acts, based on the rationale that they are more realistic. Unfortunately, some Rs may treat these highly specific scenarios as irrelevant to themselves because they describe particular acts in particular circumstances that the respondents would never consider committing. The more details the researcher provides in the vignette, the more opportunities there are for the R to treat the scenario as irrelevant to themselves. Some Rs may exploit the very specific nature of the vignettes as a way of providing the socially desirable response that they would not commit that specific crime, even though they might well commit a similar or analogous crime. Thus, the respondent's answer about the specific scenario presented to them cannot be generalized to any broader set of potential criminal acts. Further, the more irrelevant the scenario is to the respondent, the less meaningful and more artificial the respondent's assessment of the likelihood of committing the crime will be.

Fourth, soliciting self-reported intentions to offend "may encourage 'trash talk' or boastfulness among those with a criminal propensity. This trash talk would take the form of responding to a scenario with the claim that they would commit a criminal act even in the face of certain and severe punishment but acting in the real world in a more prudent manner" (Wright, Caspi, Moffitt, and Paternoster 2004, 181). This raises the possibility that, at least among adolescents, deterrent effects are *under*estimated due to this measurement error exaggerating the number of people who supposedly would commit crimes in the face of high risks of punishment.

Fifth, the belief in the validity of measurement of criminal intentions using the vignette method relies on an assumption that people are *able* to accurately forecast how they would act in future when facing a decision as to whether to commit a crime, that their stated "willingness to offend" would be strongly correlated with actual offending. A person's stated intentions or forecasts of their future behavior, however, may bear a rather weak relationship to their actual later behaviors. In particular, forecasts made in the calm circumstances of an interview or filling out a questionnaire may not simulate very well how people would actually behave in the often emotion-charged circumstances in which real-world offending decisions are made (Wright and Decker 1994, 212–213). The simulated decisions are therefore likely to look more rational than the actual ones.

Sixth, and perhaps most seriously, predicted responses to hypothetical scenarios may be worse than merely inaccurate—they could systematically bias estimates of the punishment/crime association in a negative direction through a process of respondent impression management aimed at artificially presenting the respondent as a rational person. Because risk perceptions and forecasts are elicited at roughly the same time, subjects can artificially harmonize their forecasts of criminal behavior to accord with their perceptions of legal risk. Most people presumably like to regard themselves, and to present themselves to others, as rational beings. If so, it would be foolish for a person to read a vignette stating that there was a strong chance they would be caught and punished if they committed a particular type of crime and then turn around and predict that it is very likely they would commit that crime anyway. Likewise, if a person had been asked to assess the likely risks of doing a crime and estimated the risk to be high, it is unlikely they would also predict that they would do the crime, even if they had committed crimes with similar levels of risk in the past.

Providing responses consistent with the predictions of the rational choice model is one way of presenting one's self in a positive light—a variant of social desirability response bias. Even if perceptions of legal risk had little actual effect on criminal behavior, there would still be strong methodological reasons to expect that research subjects would provide responses generating a negative association between legal risk and predicted criminal behavior. Although vignette-based studies are intended to answer the question: "Do higher perceptions of legal risk reduce criminal behavior?", they may only answer the question: "Do most people like to present themselves as rational?" Indeed, this potential problem was recognized by some of the earliest adopters of the forecast method. Tittle (1977), for example, noted the possibility that "an individual may perceive or report that he . . . does not fear sanctions because he intends to break the rules. Similarly if one has strong . . . sanction fears, he may distort his perception or reports of his conduct in order to achieve cognitive consistency" (587). Many later users of the forecast method did not take this problem so seriously, or at least not seriously enough to dissuade them from using the approach or to mention the problem to their readers.

Schneider and Ervin (1990) measured both intended (forecast) criminal behavior and actual criminal behavior in a later period. When the deterrence hypothesis was tested by relating perceived certainty and severity of punishment to the Rs' *intended* (future) criminal acts, the results appeared to confirm the deterrence hypothesis—those who perceived higher legal risks of punishment reported lower intentions of committing crime. That is, using forecasts of criminal behavior as the dependent variable supported the deterrence hypothesis. However, when the researchers tested the hypothesis using *actual* later criminal behavior as the dependent variable, the deterrence hypothesis was not supported—those who perceived higher punishment risks in the earlier period were *not* less likely to commit crimes in a later period. Indeed, there was a significant *positive* association, contrary to the specific deterrence hypothesis (596). Thus, when causal order was more clearly established using a panel design and it was impossible for Rs to artificially harmonize their self-reports of criminal behavior with their perceptions of punishment risk, the deterrence hypothesis was not supported.

Because behavioral forecasts and risk perceptions are measured literally within minutes of each other in a "one-shot" cross-sectional survey, it is easy for subjects to make their responses accord with a self-presentation as a rational person. A survey respondent is scarcely likely to forget their responses to the legal risk questions (or risk elements of crime vignettes) by the time they are asked the questions about their willingness to offend (or vice versa) a few minutes later. In contrast, in the typical panel study, six months to a year or more passes between the time when questions concerning perception of legal risk in an earlier wave were asked and the time, in a later wave, when questions eliciting self-reports of recent criminal acts were asked. It is unlikely that respondents could artificially harmonize the two sets of responses in this situation, since it would require remembering responses from the distant past. The forecast/vignette approach, on the other hand, virtually invites subjects to provide responses that fit the image of a rational decision-maker—making it is easy for subjects presented with a vignette describing a high punishment risk to falsely claim that it is unlikely they would engage in the behavior. To the extent that people prefer to regard themselves as rational deciders, stated risk perceptions and the stated likelihood of engaging in hypothetical criminal behaviors are likely to be negatively related regardless of whether perceptions of legal risk actually deter criminal behavior in real-world circumstances.

Vignette studies are nearly always based on one-shot surveys conducted at a single point in time, so it is ordinarily impossible for the researchers to become aware of the contrary findings that would have been obtained if criminal behavior had been measured at a later point in time than the time when risk perceptions were measured. In this light, the findings of one-shot vignette surveys can be given little weight as tests of deterrence. Seemingly supportive results obtained in these studies may simply indicate that Rs prefer to present themselves as rational and are easily able to artificially project this image in one-shot surveys, even if they are not rational at all in their real daily lives.

Consider one typical example of a vignette-based study. Piquero and Pogarsky (2002) used the vignette method to assess the impact of perceived risk of legal punishment and prior punishment experiences on willingness to drive drunk. The study was conducted at a single point in time, so the researchers addressed the causal order issue by relating (a) present-time perceptions of legal risk to (b) stated willingness to drive while intoxicated in future hypothetical scenarios. Respondents were asked to estimate their chances of being pulled over by the police if they drove drunk. They were also presented with a hypothetical scenario in which they might be tempted to drive drunk and were asked whether they would do so. The authors found the predicted significant negative association between perceived risk of punishment and drunk driving and interpreted it as evidence of a deterrent effect. The problem is that one would also expect the same negative association even if legal risks had no effect on the respondents' drunk driving, but respondents wanted to present themselves, however inaccurately, as rational, risk-minimizing individuals. Those who estimated a high risk of police pulling them over may have thought they would appear foolish if they also admitted that they would drive while drunk. The negative association may thus be little more than an artifact of the hypothetical scenario research design and the desire of most subjects to appear rational.

The vignette method is also artificial in another important way. In the real world, for legal risks to deter, a prospective offender must first (1) think (however briefly) about the risk of punishment, then (2) decide not to do the crime due to their fear of suffering the punishment. If either mental step is not taken, no deterrence occurs. With the vignette method, however, researchers bypass the first step by forcing the issue of legal risks upon the attention of all respondents. All respondents, including those who in real life would not have thought at all about legal risks prior to considering a criminal act, automatically bypass the first essential step by being told what the risk is or being asked to provide their own estimate of the risk. In contrast, in non-scenario studies where the dependent variable is measured via self-reports of actual criminal acts committed in the past, the researchers do not provide such artificial "reminders" of legal punishment. Confirming this view, Bouffard, Exum, and Collins (2010, 400) found that the consequences of committing crimes are "more likely to be perceived as possible outcomes (i.e., receive a non-zero probability) when they are presented by researchers than when they are self-generated."

Advocates of the forecast method assert that present-time forecasts of future behavior can serve as a good proxy for what would be obtained if a panel approach was used, and subjects provided self-reports of criminal acts actually committed in a later period. For support of this claim of measurement validity, the advocates cite a few studies that directly measured the association between time 1 forecasts of criminal behavior and time 2 self-reports of actual criminal behavior in the intervening time period—studies that supposedly found a strong correspondence (e.g., Pogarsky 2004). In fact, these studies generally indicate only a very modest correspondence, with some correlations as low as 0.35. Further, the few high correlations generally pertained to simple yes/no measures of frequently committed criminal behaviors like drunk driving or marijuana use. These tests do not establish that people can predict how much criminal behavior they will commit in the future, how often they will commit crimes, or how many different types of crime they will commit, but only the simple binary matter of whether they will commit a specific oft-committed type of crime at all.

Even this crude binary "forecast" may be little more than a description of past, repeated behavior, rather than a true forecast of future behavior. For example, Murray and Erickson (1987) found that people could fairly accurately forecast whether they would use marijuana in the future, as did

Erickson for drug use in general (1976, 226, fn. 37). Green (1989) found a 0.83 correlation ($r^2$ = .69) between "estimated future behavior," i.e. forecasts at time 1 and self-reported behavior at time 2. These findings, however, all pertained to crude binary measures of whether a person would use marijuana or other drugs or drive drunk. These simple binary validations may be little more than a reflection of (1) the fact that most people predicted that they would continue to behave in future as they had in the past and (2) the reality that these types of behaviors tend to be stable over short periods of time. Thus, the supposed "forecasts" of future hypothetical behavior in these studies were little more than indirect reflections of *past* behavior, indicating only that those who have engaged in common, frequently repeated criminal behaviors in the recent past generally believe that they are likely to continue doing so in the future.

The predictive validity supposedly demonstrated in these studies takes advantage of human inertia—the tendency of behavioral patterns to remain stable over time. Of course, if future criminal behavior was always identical to past criminal behavior for any given person, i.e. an individual's criminal behavior never changed, the very possibility of increased punishment levels reducing criminal behavior could be ruled out *a priori* as a logical impossibility. Assuming, more realistically, that criminal behavior of individuals does change over time, a stronger test of the validity of forecasts as proxies for actual future criminal behavior would be their ability to predict *changes* in criminal behavior, i.e. deviations from past behavioral patterns. We are not aware of any validation studies of forecasts that have demonstrated this ability.

The most damaging evidence of all regarding the use of the vignette method for testing deterrence was obtained in an experimental study in which respondents were randomly assigned to be either promised or not promised a financial reward for being "as candid and thoughtful about their answers as possible" (Loughran, Paternoster, and Thomas 2014, 689). The researchers found that "the negative correlation between perceived risk and willingness to offend that is often observed in scenario-based deterrence research does not emerge in conditions where respondents are incentivized to be accurate and thoughtful in their survey responses" (677). These findings suggest that the appearance of support for perceptual deterrence in vignette studies is merely an artifact of less candid and thoughtful reporting by respondents.

If this is correct, we would expect to find that the full set of findings generated by the vignette method in past research has been more likely to support deterrence than those based on other methods. Our results in Chapter 5 indicate this is indeed the case. Unfortunately, the method is likely to continue to be popular regardless of grave doubts about its utility, because it is so cheap, easy, and quick to implement.

### *Panel Studies: The Time Interval Between Waves and Two-Way Causation*

Despite their considerable merits, panel studies also have problems beyond their greater expense and time to complete. If the intervals between waves are even moderately long, such as a year or more, the possibility arises that short-term deterrent effects of punishment are missed, since they were evident only for a brief part of the interval between waves. Long intervals between waves of the survey also aggravate another problem. If longitudinal designs such as panel studies are to be useful in assessing causation, the phenomena studied must change over time—otherwise there is no variation in the dependent variable to be explained. Likewise, independent variables that did not change over time could not be causes of changes over time in the dependent variable. If risk perceptions do change, however, this means that perceptions measured at time 1 may be a poor proxy for perceptions prevailing during the period *between* times 1 and 2. Indeed, perceptual deterrence researchers who carried out panel studies discovered early on that risk perceptions change considerably, even within periods shorter than a year (Paternoster, Saltzman, Waldo, and Chiricos

1983; Piliavin et al. 1986; Saltzman et al. 1982). The longer one makes the intervals between waves of a panel study, the worse earlier measures of risk perception are as proxies for later perceptions (see Chamlin, Grasmick, Bursik, and Cochran 1992 for a discussion of how the same problem may occur in macro-level panel studies of deterrence). Paternoster and Simpson (1993, 51) observed that the relevant perception of risk for deterrence of offenders is their perception "at the time that they are contemplating offending." Thus, if perceptions at the earlier wave do not serve as good proxies of perceptions at times between waves, they will not approximate perceptions at the moment that any given potential crime was contemplated.

One might shorten the time intervals between panels as a way of reducing this measurement problem (as suggested by Piliavin et al. 1986, 116), but it is rare for even well-funded panel studies to question Rs more often than every six months or once a year. And even if funding were ample, more frequent questioning would run into the problem that variation in criminal behavior (or at least criminal behavior serious enough for anyone to care about) would be sharply restricted, since larger shares of Rs would self-report zero serious crimes as the between-waves interval shrank. The reduction in variation in criminal behavior would in turn make it harder to test what factors influence this limited variation. One could compensate for this problem of limited variation by increasing sample size, but the combination of both large samples and more waves of surveys would make studies prohibitively expensive for all but the most lavishly funded researchers.

Further, panel designs do not eliminate the problem of simultaneous two-way relationships. Deterrence researchers using panel designs conventionally relate perceptions of legal risk at an earlier time (e.g., time 1) to criminal behavior at a later time (e.g., time 2). To be precise, however, criminal behavior measured at time 2 actually describes behavior committed between times 1 and 2, so analysts relate behavior in this interval to perceptions of risk as measured at the start of the interval. The time ordering, then, seems quite clear—perception of risk precedes criminal behavior. If risk perceptions are to influence criminal behavior between times 1 and 2, however, perceptions as measured at time 1 must be regarded as proxies for perceptions prevailing between times 1 and 2, when criminal acts were contemplated by the subject. If this were not true, and perceptions at time 1 were unrelated to those prevailing between times 1 and 2, there would be no logical basis for expecting an effect of those perceptions on criminal behavior during that interval. On the other hand, if it *is* true, and perceptions at time 1 are treated as proxies for perceptions prevailing between times 1 and 2, the analyst must address the possibility that criminal behavior between times 1 and 2 affected perceptions of risk during that period, as well as the reverse. Thus there is a possibility of either simultaneous two-way causation between perceptions of legal risk and criminal behavior or one-way causation running from criminal behavior to perceived risk of punishment (the "experiential effect"). These possibilities are effectively ignored in most panel studies of deterrence, which usually assume that causal effects operate only between waves, not within waves.

This is especially unrealistic if the special deterrent effects of experiencing punishment are short-lived and are evident only within a small share of the time between waves. For example, if panel surveys were fielded in October 2015 and again in October 2016, and a respondent experienced an arrest shortly after October 2015, any effect on perceived legal risk might well fade by October 2016 and not be evident in perceptions as stated in the later survey. Further, any deterrent effect on criminal behavior that was limited to, say, the first month after the arrest might not be detectable within the self-reports of criminal behavior covering the entire year between surveys.

### *Failing to Control for Informal Social Controls*

Analogous to the failure of macro-level studies to control for levels of public intolerance for crime, many individual-level studies fail to control for the degree to which study subjects are subject to informal social controls, such as emotional ties with conventional others, commitment to conventional future lines of action such as a career in a legitimate occupation, involvement in conventional, legal activities, or belief in conventional moral rules, each of which can exert its own effect on criminal behavior (Gibbs 1986; Hirschi 1969). If the same individuals who have been arrested or imprisoned are also the ones raised by uncaring or ineffective parents, who associate only with people who tolerate or encourage criminal behavior, and who do not anticipate a serious chance at material success via legitimate work, it is hard to isolate the effect of either past punishment experiences or perceptions of future punishment risk from the effects of the ineffective social controls to which the same people are subject. As we will see in Chapters 5 and 6, most individual-level studies of deterrence do not control for level of informal social control.

### *The Use of Convenience Samples of Low-Criminality Middle-Class Students*

Most self-report deterrence studies use nonprobability samples largely confined to predominantly middle-class students—most often junior high, high school, or college students. There is little serious criminal behavior in these middle-class populations, so the constricted nature of the sample also necessitates restricting the kinds of criminal behavior that researchers ask about. There is little reason to waste questions about serious kinds of criminal behavior if virtually everyone in the sample will give the same negative response to the questions. As a result, many of these studies address criminal behaviors that, in terms of seriousness, could be said to "run the gamut from A to B." Dozens of studies, for example, concern themselves exclusively with marijuana use or drunk driving—offenses so common in the population as to virtually constitute "folk crimes." Others inquire about a wider array of offenses, but the bulk of the actual variation in the indexes of criminal behavior computed by researchers is provided by variation in the less serious criminal behaviors because the study sample included so few subjects who had committed any of the serious ones.

The disproportionate concern with less serious offense types in turn necessitates confining the types of legal punishments addressed to the correspondingly less serious penalties commonly imposed on these sorts of behaviors. In sum, many of the studies address only relatively narrow ranges of both crimes and punishments within samples that rarely engage in the more serious sorts of crimes that are of greatest concern to the public and that are punished by the more serious kinds of sanctions. This sharply limits the formal basis for generalizing from the findings of these studies to the real world of serious crimes and severe punishments.

Further, because few middle-class people have committed serious crimes, they are also less likely to have personally experienced correspondingly severe legal punishments or even know someone who has experienced such punishments. Consequently, legal punishment is a far more hypothetical concept for middle-class people than for lower-class people, and the former are therefore less likely than the latter to have well-formed opinions about the certainty or severity of legal sanctions. Their responses to punishment questions may therefore be less meaningful because they reflect the "demand characteristics" of the survey situation more than concrete life experiences. If fear of negative consequences fades as one experiences them, i.e. people become desensitized to them with repeated exposure, this implies that the members of middle class samples will seem especially responsive to the prospect of serious legal punishment precisely because neither they nor anyone they know has actually experienced it.

The largely middle-class character of study samples, such as sets of college students, could also bias findings in favor of the deterrence thesis because middle-class persons have a greater "stake in conformity," i.e. more to lose if arrested and punished. Prior studies have found that criminal penalties appear to have more deterrent effect for those with more social assets, such as a job or family members who care about the person's well-being (Sherman and Smith 1992; Smith and Gartin 1989). Even among juveniles, one would expect that those who plan on going to college or entering a profession would perceive the consequences of arrest as extending well beyond the possibility of suffering a legal punishment. Thus, the findings of self-report deterrence studies largely confined to middle-class samples may overstate the deterrent effects one could expect from punishments on the entire population, including lower-class persons with less to lose.

Further, the use of college student samples may bias results in favor of the deterrence thesis because college students are more rational, in the sense of being more responsive to anticipated costs and benefits of their choices, and more likely to take long-term consequences into account. The very fact that a person has decided to make an investment of four or more years of labor in hopes of economic rewards that will only be enjoyed, if at all, in the long run is an indication that the person gives considerable weight to long-term consequences. Since legal punishment generally follows criminal acts only in the long run, this is the sort of consequence that one would expect college students to give greater weight to. Supporting this, comparisons of college student samples with incarcerated offender samples indicate that the former were more likely to consider legal costs in deciding whether to commit a (hypothetical) crime. The perceived certainty and severity of costs of a criminal act were significantly and negatively correlated with willingness to commit crimes within the college student sample but were unrelated within the offender sample (Bouffard, Bry, Smith, and Bry 2008, 712).

On the other hand, Pogarsky (2002) argued that samples dominated by less criminal persons, such as college students, could bias results against finding deterrent effects because these samples did not include enough "deterrable" persons. In particular, he argued that if most members of such samples would refrain from crimes because of their moral disapproval of the acts, they were undeterrable in the sense that the prospect of legal punishment would be irrelevant to their decision-making. Deterrent effects could not be detected within such subgroups, and the more they dominated study samples, the less chance there was to detect deterrent effects, as long as analysts studied the entire sample. Pogarsky recommended doing analyses of subsamples, divided so that deterrable persons could be analyzed separately from either those who would not consider crimes because of their moral objections or the "incorrigibles" who do not respond to legal risks. And of course simply studying more diverse and representative samples of the population would likewise alleviate this problem. These considerations explain why we are careful in later chapters to describe the nature of the samples used in each study summarized in our tables.

## *Unmeasured Criminal Propensity Differences in Studies of Special Deterrence*

Possible special deterrent effects of punishment on the offenders punished is commonly studied either by (a) comparing criminal behavior of punished persons after a punishment event (such as a prison term) with their criminal behavior before that event, or (b) comparing the criminal behavior of criminals punished with that of other criminals not punished. If the latter approach is to yield persuasive estimates of punishment effects, researchers must control for other factors besides punishment that affect criminal behavior. This would not be a problem if punished criminals and unpunished criminals were essentially identical in their criminal propensities. This notion, however, would make sense only if one believed that punishment was randomly imposed on criminals e.g., that prosecutors randomly selected arrestees to charge with crimes, try to convict, and seek severe sentences from judges or that judges imposed more severe sentences on some convicted offenders and not others without regard to the apparent criminal propensities of those they sentenced. This is clearly implausible, as the weight of an enormous body of research on sentencing indicates that defendants' prior criminal records significantly influence decisions to prosecute and the severity of sentences imposed.

Thus, to the extent that records of prior criminal behavior are proxies for criminal proclivities in the future, punished criminals have higher criminal propensities than unpunished criminals, and more severely punished criminals have higher criminal propensities than more lightly punished criminals. This means that even if punishment did reduce criminal behavior of the persons punished, this effect might be muted or even completely obscured by the higher initial criminal propensities that prevailed among punished persons even before punishment and that presumably continued to operate afterwards as well. Likewise, studies of labeling or other crime-increasing effects of punishment run the risk of confusing the effects of higher criminality among punished persons with deviance amplification effects of some sort. In sum, studies of the effects of punishment on the punished persons should control for prior criminal behavior as thoroughly as possible.

## The Evolution of Research Methods on Deterrence

We will see in our reviews of the relevant empirical research that the methodological sophistication of research on deterrence evolved radically in a very short period of time, especially in the early years of the field, between 1967 and 1977. Early deterrence research, prior to 1967, was almost entirely focused on capital punishment, which accounts for only a tiny share of the instances of punishment inflicted by the criminal justice system. Research now encompasses noncapital punishments as well as capital punishment. Research progressed quickly from the simple bivariate analyses common before the 1970s to more complex—and realistic—multivariate analyses thereafter. A larger number of potential confounding variables are now controlled, though the most important ones, such as levels of public condemnation of crime or strength of informal controls, are still rarely (or never) controlled. After 1983, however, a growing minority of individual-level studies did control for informal social controls on behavior.

Early work assumed a one-way causal relationship between crime and punishment, while more recent studies are more likely to at least consider the possibility that crime might affect punishment, as well as the reverse. Early research was largely cross-sectional, but longitudinal designs were applied with increasing frequency after the mid-1970s with concomitant benefits for inferring causal order. Perhaps most importantly of all, scholars directly measured perceptions of legal risk at the individual level, using survey methods, rather than using official data on macro-level units, like states or cities, and relying on the dubious assumption of a close correspondence between perceived and actual levels of punishment.

Methods will continue to evolve and improve, but there have already been important trends evident in the first few decades of serious empirical research on punishment's effects on crime. Research findings have changed as methods have improved, and the conclusions that seemed so clear after those early decades are not nearly so clear from the vantage point of the twenty-first century. It will be seen that, methodologically speaking, the early case for deterrent effects of punishment on crime was built on a foundation of sand.

## References

Andenaes, Johannes. 1952. General prevention: Illusion or reality? *Journal of Criminal Law, Criminology, and Police Science* 43:176–198.

Andenaes, Johannes. 1966. The general preventive effects of punishment. *University of Pennsylvania Law Review* 114:949–983.

Baltagi, Badi. 2006. Estimating an economic model of crime using panel data from North Carolina. *Journal of Applied Econometrics* 21:543–547.

Bar-Gill, Oren, and Alon Harel. 2001. Crime rates and expected sanctions: The economics of deterrence revisited. *Journal of Legal Studies* 30:485–501.

Becker, Gary. 1968. Crime and punishment: An economic approach. *Journal of Political Economy* 76:169–217.

Becker, Howard. 1963. *Outsiders: Studies in the Sociology of Deviance.* New York: The Free Press.

Blumstein, Alfred, Jacqueline Cohen, and Daniel Nagin. 1978. *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates.* Washington, DC: National Academy Press.

Bouffard, Jeffrey, Jeff Bry, Sharmayne Smith, and Rhonda Bry. 2008. Beyond the "science of sophomores": Does the rational choice explanation of crime generalize from university students to an actual offender sample. *International Journal of Offender Therapy and Comparative Criminology* 52:698–721.

Bouffard, Jeffrey, Lyn Exum, and Peter Collins. 2010. Methodological artifacts in tests of rational choice theory. *Journal of Criminal Justice* 38:400–409.

Brier, Stephen, and Stephen Fienberg. 1980. Recent econometric modeling of crime and punishment: Support for the deterrence hypothesis? *Evaluation Review* 4:147–191.

Chamlin, Mitchell, Harold Grasmick, Robert Bursik, and John Cochran. 1992. Time aggregation and time lag in macro level deterrence research. *Criminology* 30:377–395.

Ehrlich, Isaac. 1973. Participation in illegitimate activities: A theoretical and empirical investigation. *Journal of Political Economy* 81:521–565.

Ehrlich, Isaac. 1975. The deterrent effect of capital punishment: A question of life and death. *American Economic Review* 65:397–417.

Erickson, Maynard, Jack Gibbs, and Gary Jensen. 1977. The deterrence doctrine and the perceived certainty of legal punishments. *American Sociological Review* 42:305–317.

Erickson, Patricia. 1976. Deterrence and deviance: The example of cannabis prohibition. *Journal of Criminal Law and Criminology* 67:222–232.

Fendrich, Michael, and Timothy P. Johnson. 2005. Race/ethnicity differences in the validity of self-reported drug use: Results from a household survey. *Journal of Urban Health* 82 (supplement 3):iii67–iii81.

Finkel, Steven. 1995. *Causal Analysis with Panel Data.* Beverly Hills: Sage.

Fisher, Franklin, and Daniel Nagin. 1978. On the feasibility of identifying the crime function in a simultaneous model of crime rates and legal sanction levels. In *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, eds. Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin. Washington, DC: National Academy of Sciences.

Geerken, Michael, and Walter Gove. 1977. Deterrence, overload and incapacitation: An empirical evaluation. *Social Forces* 56:424–447.

Gibbs, Jack. 1975. *Crime, Punishment and Deterrence.* New York: Elsevier.

Gibbs, Jack. 1986. *Social Control: Views from the Social Sciences.* Beverly Hills: Sage.

Gibbs, Jack, and Glenn Firebaugh. 1990. The artifact issue in deterrence research. *Criminology* 28:347–367.

Glaser, Daniel, and Max Zeigler. 1974. Use of the death penalty v. outrage at murder. *Crime and Delinquency* 22:40–43.

Grasmick, Harold, and Robert Bursik. 1990. Conscience, significant others and rational choice: Extending the rational choice model. *Law and Society Review* 24:837–861.

Green, Donald. 1989. Past behavior as a measure of actual future behavior: An unresolved issue in perceptual deterrence research. *The Journal of Criminal Law and Criminology* 80:781–804.

Grogger, Jeffrey. 1991. Certainty versus severity of punishment. *Economic Inquiry* 29:297–309.

Heineke, John. 1988. Crime, deterrence, and choice. *American Sociological Review* 53:303–305.

Hindelang, Michael, Travis Hirschi, and Joseph Weis. 1981. *Measuring Delinquency.* Beverly Hills: Sage.

Hirschi, Travis. 1969. *Causes of Delinquency.* Berkeley, CA: University of California Press.

Jacobs, Bruce A. 2010. Deterrence and deterrability. *Criminology* 48:417–441.

Junger-Tas, Josine, and Ineke Haen Marshall. 1999. The self-report methodology in crime research. *Crime and Justice* 25:291–367.

Kessler, Daniel, and Steven Levitt. 1999. Using sentence enhancements to distinguish between deterrence and incapacitation. *The Journal of Law and Economics* 42:343–363.

Kleck, Gary. 1979. Capital punishment, gun ownership, and homicide. *American Journal of Sociology* 84:882–910.

Klepper, Steven, and Daniel Nagin. 1989. The deterrent effect of perceived certainty and severity of punishment revisited. *Criminology* 27:721–746.

Kovandzic, Tomislav, Mark Schaffer, Lynne Vieraitis, Erin Orick, and Alex Piquero. 2016. Police, crime and the problem of weak instruments: Revisiting the "more police, less crime" thesis. *Journal of Quantitative Criminology* 32:133–158.

Levitt, Steven. 1995. *Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?* Cambridge, MA: National Bureau of Economic Research.

Levitt, Steven. 1996. The effect of prison population size on crime rates: Evidence from prison overcrowding litigation. *Quarterly Journal of Economics* 3:319–351.

Levitt, Steven. 1997. Using electoral cycles in police hiring to estimate the effect of police on crime. *American Economic Review* 87:270–290.

Levitt, Steven. 1998. Juvenile crime and punishment. *Journal of Political Economy* 106:1156–1185.

Levitt, Steven. 2002. Using electoral cycles in police hiring to estimate the effect of police on crime: A reply. *American Economic Review* 92:1244–1250.

Liedka, Raymond, Anne Piehl, and Bert Useem. 2006. The crime-control effect of incarceration: Does scale matter? *Criminology & Public Policy* 4:245–276.

Lochner, Lance. 2007. Individual perceptions of the criminal justice system. *American Economic Review* 97:446–460.

Loughran, Thomas, Raymond Paternoster, and Kyle Thomas. 2014. Incentivizing responses to self-report questions in perceptual deterrence studies: An investigation of the validity of deterrence theory using Bayesian truth serum. *Journal of Quantitative Criminology* 30:677–707.

Lu, Natalie, Bruce G. Taylor, and K. Jack Riley. 2001. The validity of adult arrestee self-reports of crack cocaine use. *The American Journal of Drug and Alcohol Abuse* 27:399–419.

Manski, Charles. 1978. Prospects for inference on deterrence through empirical analysis of individual criminal behavior. In *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, eds. Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin. Washington, DC: National Academy of Sciences.

McCrary, Justin. 2001. Using electoral cycles in police hiring to estimate the effect of police on crime: Comment. *American Economic Review* 92:1236–1243.

Murray, Glen, and Patricia Erickson. 1987. Cross-sectional versus longitudinal research: An empirical comparison of projected and subsequent criminality. *Social Science Research* 16:107–118.

Nagin, Daniel. 1998. Criminal deterrence research at the outset of the twenty-first century. *Crime and Justice* 23:1–42.

Nagin, Daniel, and Raymond Paternoster. 1994. Personal capital and social control: The deterrence implications of individual differences in criminal offending. *Criminology* 32:581–606.

Paternoster, Raymond. 1986. The use of composite scales in perceptual deterrence research: A cautionary note. *Journal of Research in Crime and Delinquency* 23:128–168.

Paternoster, Raymond. 1987. The deterrent effect of the perceived certainty and severity of punishment: A review of the evidence and issues. *Justice Quarterly* 4:173–215.

Paternoster, Raymond, Linda Saltzman, Gordon Waldo, and Theodore Chiricos. 1983. Perceived risk and social control: Do sanctions really deter? *Law and Society Review* 17:457–479.

Paternoster, Raymond, Linda Saltzman, Gordon Waldo, and Theodore Chiricos. 1985. Assessments of risk and behavioral experience: An exploratory study of change. *Criminology* 23:417–436.

Paternoster, Raymond, and Sally Simpson. 1993. Rational choice theory of corporate crime. *Advances in Criminological Theory* 5:37–58.

Piliavin, Irving, Craig Thornton, Rosemary Gartner, and Ross Matsueda. 1986. Crime, deterrence, and rational choice. *American Sociological Review* 51:101–119.

Piquero, Alex, and Greg Pogarsky. 2002. Beyond Stafford and Warr's reconceptualization of deterrence: Personal and vicarious experiences, impulsivity, and offending behavior. *Journal of Research in Crime and Delinquency* 39:153–186.

Pogarsky, Greg. 2002. Identifying deterrable offenders: Implications for research on deterrence. *Justice Quarterly* 19:431–452.

Pogarsky, Greg. 2004. Projected offending and contemporaneous rule violation. *Criminology* 42:111–135.

Pogarsky, Greg, Alex Piquero, and Ray Paternoster. 2004. Modeling change in perceptions about sanction threats: The neglected linkage in deterrence theory. *Journal of Quantitative Criminology* 20:343–369.

Pope, Carl. 1975. *The Judicial Processing of Assault and Burglary Offenders in Selected California Counties*. National Criminal Justice Statistics and Information Service. Washington, DC: U.S. Government Printing Office.

Saltzman, Linda, Raymond Paternoster, Gordon Waldo, and Theodore Chiricos. 1982. Deterrent and experiential effects: The problem of causal order in perceptual deterrence research. *Journal of Research in Crime and Delinquency* 19:172–89.

Sampson, Robert. 1986. Crime in cities: The effects of formal and informal social control. In *Communities and Crime*, eds. Albert Reiss and Michael Tonry. Chicago: University of Chicago Press.

Schneider, Anne, and Laurie Ervin. 1990. Specific deterrence, rational choice and decision heuristics: Applications in juvenile justice. *Social Science Quarterly* 71:585–601.

Sherman, Lawrence W. 1986. Policing Communities: What Works? In *Communities and Crime. Volume 8, Crime and Justice*, eds. Albert J. Reiss, Jr. and Michael Tonry, 343–386. Chicago: University of Chicago Press.

Sherman, Lawrence W., and Douglas A. Smith. 1992. Crime, punishment and stake in conformity: Legal and informal control of domestic violence. *American Sociological Review* 57:680–690.

Smith, Douglas, and Patrick Gartin. 1989. Specifying specific deterrence: The influence of arrest on future criminal activity. *American Sociological Review* 54:94–106.

Stafford, Mark, and Mark Warr. 1993. A reconceptualization of general and specific deterrence. *Journal of Research in Crime and Delinquency* 30:123–135.

Tauchen, Helen, Ann Witte, and Harriet Griesinger. 1994. Criminal deterrence: Revisiting the issue with a birth cohort. *The Review of Economics and Statistics* 76:399–412.

Teevan, James. 1976. Deterrent effects of punishment: Subjective measures continued. *Canadian Journal of Criminology and Corrections* 18:152–160.

Tittle, Charles. 1977. Sanction fear and the maintenance of social order. *Social Forces* 55:579–596.

Tittle, Charles. 1980. *Sanctions and Social Deviance: The Question of Deterrence.* New York: Praeger.

U.S. Bureau of Justice Statistics. 2005. *Sourcebook of Criminal Justice Statistics 2003*. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2012. *Sourcebook of Criminal Justice Statistics 2010*. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 1999. *Crime in the United States—1998*. Washington, DC: U.S. Government Printing Office.

Vandaele, Walter. 1978. Participation in illegitimate activities: Ehrlich revisited. In *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, eds. Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin. Washington, DC: National Academy of Sciences.

Vold, George, Thomas Bernard, and Jeffrey Snipes. 2002. *Theoretical Criminology* (5th ed.). New York: Oxford.

Williams, Kirk, Jack Gibbs, and Maynard Erickson. 1980. Public knowledge of statutory penalties: The extent and basis of accurate perception. *Pacific Sociological Review* 23:105–128.

Williams, Kirk, and Richard Hawkins. 1986. Perceptual research on general deterrence: A critical review. *Law and Society Review* 20:545–572.

Williams, Kirk, and Richard Hawkins. 1992. Wife assault, costs of arrest and the deterrence process. *Journal of Research in Crime and Delinquency* 29:292–310.

Wilson, James, and Barbara Boland. 1978. The effect of the police on crime. *Law and Society Review* 12:367–390.

Wilson, James Q., and George L. Kelling. 1982. Broken windows. *Atlantic Monthly* 249:29–38.

Witte, Ann Dryden. 1980. Estimating the economic model of crime with individual data. *Quarterly Journal of Economics* 94:57–84.

Wright, Bradley, Avshalom Caspi, Terrie Moffitt, and Ray Paternoster. 2004. Does the perceived risk of punishment deter criminally prone individuals? Rational choice, self-control, and crime. *Journal of Research in Crime and Delinquency* 41:180–213.

Wright, Richard T., and Scott H. Decker. 1994. *Burglars on the Job*. Lebanon, NH: Northeastern University Press.

# 5

# INDIVIDUAL-LEVEL RESEARCH ON GENERAL DETERRENCE

## The Impact of Perceptions of Legal Risk on Criminal Behavior

In this section of the book we begin a systematic review of the empirical evidence bearing on the effects of legal punishment on criminal behavior. We start in Chapters 5 and 6 by examining evidence from two broad categories of individual-level research. Chapter 5 reviews the individual-level evidence bearing on perceptual deterrence—that is, the impact of the *perception* of the risks of legal punishment on criminal behavior—while Chapter 6 reviews the individual-level deterrent effects of the *personal experience* of punishment on criminal behavior of the individuals punished. Thus, Chapter 5 is primarily relevant to the issue of general deterrence—whether a person's perception of the risks of legal punishment causes them to commit less crime due to their fear of punishment, while the Chapter 6 material is relevant to specific deterrence—whether the experience of being punished causes the punished person to commit less crime. In Chapters 7 and 8 we will address macro-level evidence on the association of punishment levels with the crime rates of aggregates like cities and states. Chapter 7 will review macro-level research designed to test the general deterrent effect of levels of *non*capital punishment, such as the certainty or severity of prison sentences, while Chapter 8 will address the impact of capital punishment on homicide rates.

### Review Methods

#### *Criteria for Inclusion of Studies in the Reviews*

In order to be included in our reviews of the evidence on effects of legal punishment, studies had to (a) address the effects of legal punishment on crime using empirical evidence, (b) report a quantitative measure of association between measures of crime and of punishment, (c) be written in English, and (d) had to have been published between January 1, 1967 and June 30, 2015. Excluding studies published prior to 1967 excludes only a few studies that would otherwise qualify, and these early studies generally used methods that would be considered primitive by today's standards. Research published after June 30, 2015 was excluded for the simple reason that our massive research review had to come to an end sometime.

Purely theoretical or qualitative studies were not included. Only studies that tested deterrence or incapacitation effects on legally forbidden behavior or perceptions of the legal penalties for crime were included in the review. Thus, deterrence studies that analyzed noncriminal deviant behavior, such as academic cheating, were not included, nor were studies of alcohol use by adults. On the other hand, since alcohol purchase by minors is unlawful, deterrence studies focusing on alcohol use by minors were included. Studies assessing the impact of specific laws or crime-control strategies, such as "three strikes" laws, intensified "hot spots" police patrols, or revisions of drunk-driving laws were excluded from the systematic reviews, though they were sometimes cited. We considered them to be only tangentially related to the focus of this book, as they generally provide no findings directly testing specific punitive mechanisms by which the interventions affected crime, such as deterrence or incapacitation mechanisms.

We included studies that replicated prior studies if they studied different samples but excluded findings generated by duplicate analyses of the same sample. Where there were such duplicated analyses in multiple studies, we included the findings from the study that was methodologically strongest, omitting the results of weaker research.

### *Search Procedures*

We used a number of search procedures to locate relevant studies. First, we searched the following online bibliographic databases: Criminal Justice Periodicals, EBSCOHost, ECOdatabases, EconLit, and PsychARTICLES. We used an exhaustive set of search terms within each database to locate articles that appeared to match our criteria. Once these items were initially collected, the titles of items in their reference lists were examined to identify any previously undiscovered studies. These studies were obtained and their reference lists were examined for further undiscovered items and so on. This process was repeated on 14 occasions over a 16-year period and more than 3,200 studies were initially examined for our analyses.

The set of studies included in our review undoubtedly excludes at least a few eligible studies despite our efforts to be comprehensive. Journal articles were more readily available than other kinds of publications, and we very likely missed studies published in rarely cited book chapters or state government reports. Therefore, our review is likely to be skewed towards findings reported in peer-reviewed journal articles. Considering the relatively rigorous peer review process used by professional journals, this should increase the credibility of our reviews because it places greater emphasis on methodologically stronger studies. On the other hand, the well-documented bias against publication of research yielding null findings (Cooper 2010) implies that the findings that our review missed were disproportionately findings indicating no relationship between punishment and crime.

### *Methods for Counting Findings*

We regarded each distinct test of the hypothesis that punishment affects crime as a "finding." Many studies reported multiple tests of the punishment effect hypotheses using different methodological approaches and thus had multiple findings. We wanted to avoid counting basically duplicative tests as if they were separate findings. Often multiple tests differed only in purely methodological ways, such as using different statistical estimation procedures, using different measures of the same variables, or controlling for different sets of potentially confounding variables. When this was the case we included in our counts of findings those generated by the most methodologically sophisticated approach used in the study. For example, when multiple studies based on the same body of data tested the impact of certainty of punishment on burglary by using both multivariate regression analysis and bivariate correlation coefficients, only the results of the multivariate regression analyses were recorded. Likewise, if one analysis in a study used instrumental variables to address the question of causal order between variables while no such method was used in a second duplicate analysis, we only included the findings generated by the analysis using instrumental variables methods.

On the other hand, we separately counted findings that differed in substantively important ways rather than purely methodological ways. If a study tested a hypothesis for three different crime types, all three findings were included in our reviews because differences in findings across crime types were deemed to be important for theoretical reasons. We also included multiple findings from a given study if they were based on independent samples. Thus, when a study tested hypotheses using non-overlapping samples, such as males and females or juveniles and adults, we included the separate findings for each of the non-overlapping samples.

We were sometimes faced with less clear-cut decisions as to which of multiple findings to include. For these, we used preliminary analyses of the findings and prior literature to determine important distinctions in findings that should be recognized by retaining multiple findings. For instance, since there is considerable discussion in the literature about potential differences in deterrence findings based on the manner in which perceived punishment and experienced punishment were measured, we included multiple findings differing in these respects in our analyses.

Of the more than 3,200 studies that we initially considered, 724 were included in our analyses reported in Tables 5.1 through 8.11; 410 at the individual-level and 314 at the macro-level. These figures do not reflect the fact that many studies are reviewed in multiple tables, which, if counted separately, would bring the total count of studies used to 1,218, including 758 individual-level studies and 460 macro-level studies. Most of these studies contributed more than one finding, i.e. more than one independent test of the hypothesis that punishment affects crime. Therefore, the number of findings far exceeded the number of studies, with 1,941 findings in individual-level tables and 1,788 in macro-level tables, for a total of 3,729 findings. These totals do not include the incapacitation findings that are reviewed in Chapter 10.

Readers interested in the details of individual studies that we reviewed may consult the book's online appendices at http://scholar.fgcu.edu/bsever/online-appendix/. Appendix A lists studies that did not qualify for our review, though they were on related topics. Appendix B describes in detail the studies we did review whose findings are summarized in the tables that are included in the book's text. Each of the individual findings was placed into an SPSS dataset, allowing us to quickly perform cross-tabulations of findings with various aspects of the studies that generated those findings.

Studies with very large numbers of findings initially appeared to present a challenge for our literature review. Some studies reported dozens of tests of a punishment/crime association while others reported just one. As a consequence, when we tabulated numbers of findings, some studies had more influence on the results of the review than others merely because the study's authors chose to carry out more analyses of their data. Such author decisions can be somewhat arbitrary, reflecting the energy and rigorousness of researchers and not just the strength of the evidence itself.

Because of this concern, we conducted sensitivity checks in which we compared tabulations of findings from which such studies were excluded with tabulations that included them. It turned out that our concerns were baseless, as the conclusions to be drawn were largely unaffected by the inclusion of studies with many findings.

To maintain consistency in our reviews, all findings in our tables were classified as statistically significant or not based on a one-tailed significance level of 0.05. As a result, our study results may sometimes differ from the verbal descriptions provided by the authors of a given study because the author reported a two-tailed test or used a one-tailed test with a significance level of 0.10 or 0.20 rather than 0.05. If a two-tailed significance was reported, we divided that significance in half to judge whether the finding was significant at the 0.05 level, one-tailed.

Our decision to rely on the statistical significance of results in determining deterrence outcomes, though common, is certainly open to criticism. Reliance on statistical significance may skew the conclusions in favor of studies with large samples and statistically significant yet substantively weak findings. An alternative approach would be to focus on measures of the strength of association, but these quantities would not be comparable across studies using the dozens of different statistical procedures applied in the deterrence literature. Punishment/crime associations were most commonly reported in the form of some kind of multiple regression coefficient, which is not generally comparable across studies. Unstandardized (metric) coefficients are not comparable because they depend on differing metrics used to measure the variables involved, while standardized coefficients are not comparable because the standard deviations of variables differ across study samples. Further, multivariate coefficients would usually reflect controls for different sets of control variables, making them substantively noncomparable. Bivariate correlation coefficients are comparable across studies but usually not meaningful as estimates of causal effects in nonexperimental studies (Cooper 2010). Consequently, it was usually not feasible to meaningfully compare magnitudes of punishment/crime associations across studies.

The abbreviations used as column headings in our summary tables are as follows:

| | |
|---|---|
| – sig | The association was negative and statistically significant at the 0.05 level (one-tailed) |
| – ns | The association was negative but *not* significant at the 0.05 level (one-tailed) |
| ? ns | The association was *not* significant at the 0.05 level (one-tailed) and its sign was not reported |
| + ns | The association was positive but *not* significant at the 0.05 level (one-tailed) |
| + sig | The association was positive and significant at the 0.05 level (one-tailed) |
| – p = ? | The association was negative, but its significance level was not reported |
| + p = ? | The association was positive but its significance level was not reported |

Before beginning our systematic review of the research, a strong caveat is in order. Readers should always be cognizant of the "file drawer problem," i.e. the fact that every literature review not only misses some studies, but that it is especially likely to miss those yielding null results. Results of this type are not only less likely to be published but are also less likely to even be written up. For example, Franco, Malhotra, and Simonovits (2014) found that, in an unusually well-documented sample of studies, 64.5 percent of those yielding null results were not even written up, while another 14.6 percent were written up but not published; only 20.8 percent were published. The primary source of the file drawer problem, then, was not publication bias per se, but rather authors suppressing null findings by not writing them up and making them publicly available. In contrast, 61.5 percent of studies yielding strong support for the hypothesis were published, while only 4.4 percent were not written up and 34.1 percent were written up but not published. Thus, supportive studies were three times more likely (61.5 percent vs. 20.8 percent) to be published than those yielding null findings. This same general pattern has been consistently found in a wide array of disciplines and research topics (Cooper 2010). For the topic on which this book is focused, readers should therefore assume that there are very likely a large number of research findings indicating no effect of punishment on crime that could not be included in our systematic reviews and that a review that could somehow include all research findings would generally find less support for the hypothesis that punishment affects crime.

## Part I: Perceptual Deterrence Research

Early research on deterrence—and even a good deal of recent research—was based on macro-level data on crime rates and aggregate punishment levels, like arrest rates or numbers of executions (reviewed in Chapters 7 and 8). This research was rightly criticized for failing to directly test deterrence, due to its failure to measure perceptions of the risk of punishment. Even if it could somehow be shown that higher punishment levels had somehow reduced crime, macro-level research could not show whether the effect was due to deterrence, incapacitation, or even the rehabilitative effects of the increased offender correctional treatment that would often accompany increased punishment levels.

Therefore, many deterrence researchers set out to directly study the impact of perceptions of the risk of punishment by measuring those perceptions using survey research methods. That is, these variables were measured by asking samples of individual people questions about their perceptions of punishment risk. Measurement of criminal behavior generally relied on self-report methods in which survey respondents were, in effect, asked to confess to crimes that they had committed, though a few studies have used official records such as arrest records to measure criminal behavior. The basic hypothesis common to perceptual deterrence studies was that people who perceived legal punishment of crime to be more certain, severe, or swift were, other things being equal, less likely to commit crimes.

## *Methodology of Cross-Tabulations of the Individual-Level Findings*

In this section we review tests of the hypothesis that perceptions of the risk of punishment reduce criminal behavior. We summarize the overall findings and then report the results of cross-tabulations designed to identify factors that may have influenced these findings. Again, the details of the original studies and their findings can be found in the online Appendix stored at http://scholar.fgcu.edu/bsever/online-appendix/.

Many of our analyses of findings concern methodological issues discussed in Chapter 4, issues that could raise questions about the validity of the conclusions that researchers drew. These include issues of time order, the measurement of perceived punishment, controls for important confounders such as informal social controls, and the use of vignette methods. Other analyses address theoretical issues that were raised in Chapters 2 and 3, such as (a) what kinds of crimes are most deterrable, (b) what kinds of people are most deterrable, and (c) which dimensions of punishment—certainty, severity, or swiftness—are most important in producing deterrent effects.

## *Overall Findings on the Impact of Perceptions of Legal Risk on Criminal Behavior*

Table 5.1 summarizes the entire set of findings that test the proposition that greater perception of the risk of legal punishment reduces criminal behavior. We found 569 separate tests of the hypothesis; 38 percent of the findings indicated a significant (at the 5 percent level, one-tailed) negative association between some measure of perceived risk and criminal behavior. This was the most common finding, but our results also mean that only a minority of the findings were significant and in the predicted negative direction. Thus, most individual-level research findings have failed to yield the significant negative punishment/crime association that is predicted by deterrence theory.

This contradicts the widespread belief that most perceptual deterrence research has supported the deterrence doctrine—a view encouraged by a widely cited nonsystematic review written by Daniel Nagin and published in the prestigious *Crime and Justice*. Nagin (1998, 36) concluded, "I am confident in asserting that our legal enforcement apparatus exerts a substantial deterrent effect."

**TABLE 5.1** Total findings of the impact of perceptions of legal risk on criminal behavior

| *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|
| | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

According to the Web of Science database, this review had been cited in journal articles 221 times as of February 18 2016. For an example of authors who uncritically accepted Nagin's characterization of the perceptual deterrence literature, see the brief nonsystematic literature review by von Hirsch, Bottoms, Burney, and Wikstrom (1999, esp. 34–35).

Our results are, however, consistent with a later systematic review of 40 perceptual deterrence studies published up through 2003 (yielding 200 separate hypothesis tests), which concluded that "the mean effect sizes of the relationship between crime/deviance and variables specified by deterrence theory are modest to negligible" and that "empirical support for the effect of formal sanctions on individuals' criminal behavior is most likely an artifact of the failure to control for other 'known' predictors of crime/deviance" (Pratt, Cullen, Blevins, Daigle, and Madensen 2006, 383–384).

## Factors That May Condition the Findings of Perceptual Deterrence Research

It remains possible that there are deterrent effects of perceived risks of punishment that are conditional on other factors, such as attributes of the prospective offender, the crime type being considered, or circumstances in which the offending decision is made (see Chapter 2). Unfortunately, while there are a fair number of studies addressing contingent factors that may condition the impact of perceived punishment risks, there are no more than a handful on any one of them, precluding any firm conclusions and making systematic reviews of the evidence infeasible (Piquero, Paternoster, Pogarsky, and Loughran 2011). Our conclusions about such contingencies must therefore be regarded as tentative.

It is also possible that this simple summary of findings could be misleading if some studies are methodologically better than others and the findings of better studies differ from those of the less sophisticated studies. Phrased in statistical terms, the effects of punishment risk perceptions on criminal behavior may be "contingent," or dependent, on or interact with the level of other variables. We explore these contingencies by subdividing findings in accordance with these contingent factors to determine whether support for perceptual deterrence differs across methodological or substantive categories.

### *Perceptual Deterrence Findings and Academic Discipline*

The disciplinary assumptions of researchers can influence their conduct of research and interpretation of findings. Given that economists are trained to believe that the frequency of a given behavior will decline as its costs are raised, it might be suspected that they would be especially inclined to believe findings indicating that criminal behavior declines as legal punishment increases (see Chapter 3). Conversely, sociologists and criminologists, trained to emphasize the importance of informal nonlegal social controls, might be more hostile to findings supporting an impact of formal legal sanctions. Some scholars have even claimed that deterrence and labeling theorists are so emotionally invested in the debate over the deterrence question that they are more concerned with winning the debate than with the quality of their research (Thomas and Bishop 1984).

We could not *directly* test for effects of researchers' academic discipline because most research reports do not indicate the disciplinary backgrounds of authors, and some studies were conducted by multiple collaborators from different disciplines. We could, however, classify findings published in scholarly journals according to the academic discipline with which the publishing journal was affiliated, which appeared to generally correspond to the authors' discipline when that was reported. We categorized research outlets into one of nine publication types: criminology/criminal justice journal, economics journal, psychology journal, sociology journal, law journal, book chapter,

**TABLE 5.2** Perceptual deterrence findings by academic discipline of publication outlet

| *Publication Field* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Criminology/CJ | 326 | 32.8 | 39.6 | 4.9 | 17.5 | 2.8 | 2.1 | 0.03 |
| Sociology | 117 | 35.9 | 17.1 | 17.9 | 20.5 | 0.9 | 6.8 | 0.9 |
| Economics | 42 | 61.9 | 31.0 | 0.0 | 7.1 | 0.0 | 0.0 | 0.0 |
| Other | 84 | 48.8 | 23.8 | 3.6 | 16.7 | 2.4 | 4.8 | 0.0 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

governmental working paper, book, and "other" journal. Governmental working papers, books and book chapters were not assigned any disciplinary affiliation.

Table 5.2 indicates that there is little difference between deterrence research published in criminology/criminal justice journals and research published in sociology journals—most such research fails to find the predicted significant negative association. Research published in economics journals, however, is far more likely to support the deterrence hypothesis. Economics journals are the only scholarly publishing outlet in which a majority of findings were negative and significant, supporting a crime-reducing effect of punishment perceptions. Economists have not, however, contributed a large share of the perceptual deterrence findings and rarely use individual-level approaches to deterrence research compared to researchers who publish in sociology and criminology journals (Piliavin, Thorton, Gartner, and Matsueda 1986). When economists conduct individual-level studies, however, they are considerably more likely to conclude that higher perceived punishment risks reduce criminal behavior. Whether this is due to disciplinary biases, methodological differences, or some other factor remains to be determined.

### *Changes in Perceptual Deterrence Findings Over Time*

Deterrence research has changed considerably in both methods and substantive focus over the last four decades, and one might reasonably expect changes in the findings as well. The 1970s saw a transition away from the study of macro-level punishment levels and crime rates and towards perceptual deterrence research (Erickson 1976; Minor 1977; Silberman 1976; Tittle 1977) while research in the 1980s attempted to improve the individual-level perceptual research through the use of panel studies and better statistical procedures (Minor and Harry 1982; Paternoster, Saltzman, Chiricos, and Waldo 1982a; Paternoster, Saltzman, Waldo, and Chiricos 1983a; Paternoster and Iovonni 1986). The 1990s brought a focus on the importance of punishment avoidance (Stafford and Warr 1993) and the increasing use of vignette methods to explore factors impacting one's decision to commit crime (Carnes and Englebrecht 1995; Decker, Wright, and Logie 1993; Piquero and Rengert 1999) while the 2000s brought renewed attention to the impact of prior criminal experience on deterrence effects (Bouffard, Bry, Smith, and Bry 2008; Gainey, Payne, and O'Toole 2000; Piquero and Pogarsky 2002; Pogarsky 2002) and to better ways to measure recidivism (Bernburg and Krohn 2003; Ventura and Davis 2005). Braithwaite (1989) suggested that changes in the attention that researchers have paid to informal, nonlegal social sanctions may have altered the outcomes of deterrence research over time. Further, changes in the kinds of subject samples studied in deterrence research may have affected research findings. It was common for researchers to use convenience samples of college students in deterrence research in the 1970s and 1980s (Grasmick and Bursik 1990), but more recent research has

**TABLE 5.3** Perceptual deterrence findings by decade of publication

| *Decade Published* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 1960s and 1970s | 42 | 7.1 | 2.4 | 47.6 | 16.7 | 0.0 | 21.4 | 4.8 |
| 1980s | 189 | 36.0 | 41.3 | 3.7 | 14.8 | 0.5 | 3.7 | 0.0 |
| 1990s | 91 | 34.1 | 36.3 | 9.9 | 14.3 | 4.4 | 1.1 | 0.0 |
| 2000s | 247 | 46.2 | 28.3 | 1.6 | 20.2 | 2.8 | 0.8 | 0.0 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

become somewhat less dependent on these samples and has begun to use more diverse samples of the population. It is plausible that deterrence findings have changed as a result of these changes. College students may be more intelligent, rational, and likely to take account of future consequences than the average person and have a greater stake in conformity. Thus their decisions may be more influenced by perceived risks of legal punishment than is true in the population as a whole. If so, the move away from college student samples may have reduced the share of findings supporting the deterrence hypothesis. We therefore cross-classified perceptual deterrence findings by the decade in which the studies generating them were published.

Table 5.3 shows that support for this hypothesis within the body of individual-level findings has in fact changed only modestly and irregularly over the past several decades. Support increased from the 1970s to the 1980s, declined a bit from the 1980s to the 1990s, and then increased again in the 2000s. In none of these decades, however, did a majority of tests of the deterrence hypothesis yield supportive results in the form of significant negative associations between perceived punishment risks and crime. As will be discussed later in this chapter, many perceptual deterrence findings that were interpreted as supporting a deterrent effect may have actually been reflecting the "experiential effect" of criminal behavior on perceptions of punishment risk (Greenberg 1981; Paternoster 1987). Perceptual deterrence studies with this problem were especially common in the 1980s, and this may help explain the prevalence of so many seemingly pro-deterrence findings in that decade.

A large minority of findings may continue to offer apparent support for perceptual deterrence in recent studies because critical methodological problems continue to afflict the field. For example, one might think that, after decades of discussion about the issue, few researchers would get the causal order of perceived risks and criminal behavior wrong. In fact, research has gotten worse in this regard in recent years. It makes no sense to test the effects of current perceptions of risk on past criminal behavior, yet this is precisely what dozens of researchers have done, clearly getting causal order wrong. Only 16.5 percent of the multivariate findings purporting to test the perceptual deterrence hypothesis in the 1960s and 1970s related current perceptions of punishment risk to past criminal behavior, but this figure rose to 28 percent in the 1990s, dipped to 15.9 percent in the 1990s, and jumped to 39.6 percent in the 2000s. Interestingly, these fluctuations coincide with the changes by decade in support for the perceptual deterrence hypothesis shown in Table 5.3.

There could be a number of explanations for the increase in perceptual deterrence studies having causal order problems, as well as many other flaws, such as an increase of universities encouraging novice scholars to undertake research, the use of more expedient cross-sectional studies due to increased pressure to publish quickly in an increasingly competitive academic job market, and an increase in the number of new academic journals, some of which may not demand the scholarly rigor expected by the more established journals. The impact of erroneous specification of time order on perceptual deterrence findings will be explored further later in this chapter.

Early deterrence research was criticized for its lack of attention to the ways that offender characteristics might condition deterrence effects (Bridges and Stone 1986; Minor 1977; Tittle and Logan 1973). We therefore explore how perceptual deterrence findings differ depending on sample composition with regard to age, gender, enrollment in college, and prior crime experience. Interested readers may consult Piquero et al. (2011) for a more detailed review of related issues.

### *Perceptual Deterrence Findings by the Predominant Ages of the Sample Studied*

A number of researchers have contended that the age of subjects used in perceptual deterrence research may condition the findings of studies (Foglia 1997; Decker et al. 1993; Williams and Hawkins 1986). Williams and Hawkins (1986) suggested that adults may be more affected by the threat of legal punishments because the penalties are more severe than those usually imposed on juveniles and because adults have more to lose from legal punishment, such as a good job, higher income, more property, marriage, and family obligations, all of which could make them more deterrable. Williams and Hawkins also speculated that adolescents may have more knowledge of legal penalties because they are more likely to have contact with offenders, given that offending is more frequent among adolescents. Since this knowledge could reveal the low probability of punishment that actually prevails, it could lower deterrence among adolescents.

To test these ideas, we categorized findings by the predominant ages of the members of the sample on which it was based: (a) juveniles (under 18 years of age), (b) adults (18 and over), or (c) a mixture of adults and juveniles. The patterns reported in Table 5.4 do not provide support for the idea that adults are more effectively deterred by their perceptions of punishment risk than are adolescents or mixed age groups, since only 36 percent of findings based on adult samples were negative and significant, supporting the deterrence hypothesis, while a similar 41 percent of findings based on juvenile samples were negative and significant. Thus, there is no distinct pattern of perceptual deterrence findings differing by the predominant ages of samples studied. If anything, the pattern of findings weakly suggests that juveniles may be more deterrable than adults. Note, however, that this conclusion may be distorted by the fact that many adult samples were samples of college students who may be more deterrable because of their intelligence, education, and stake in conformity, rather than because of their age.

**TABLE 5.4** Perceptual deterrence findings by predominant age of sample

| *Primary Ages of Sample* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Juveniles (Under 18 Years) | 162 | 41.4 | 24.7 | 11.7 | 14.8 | 1.9 | 5.6 | 0.0 |
| Mixed Ages | 91 | 38.5 | 23.1 | 18.7 | 14.3 | 4.4 | 0.0 | 1.1 |
| Adults (18 + Years) | 316 | 36.1 | 38.3 | 1.3 | 19.3 | 1.6 | 3.2 | 0.3 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

### *Perceptual Deterrence Findings by Whether a College Student Sample Was Studied*

It is worrisome that so many perceptual deterrence studies have been based on samples of college students, since there are good reasons to suspect that results of such studies may not be generalizable to the rest of the population (Bouffard et al. 2008). Again, college students arguably have a higher stake in conformity due to their commitment to their college career and hopes for a successful post-college occupational career, so the threat of legal punishment that would disrupt this career could have more deterrent impact than it would for persons not attending college (Foglia 1997). Further, attending college may serve as an outward indicator of greater concern for the future (a longer "time horizon") and greater rationality, so one might also expect more support for deterrence when college students are studied.

Further, because serious crime is rarer among college students than in the rest of the population, many researchers using such samples have been forced to ask largely about the commission of less serious crimes in order to elicit nonneglible numbers of admissions of criminal behavior. Although some scholars using the vignette approach have asked students to respond to scenarios involving serious crimes, these scenarios are unlikely to be plausible for most members of a college sample because it is unlikely that many of them had ever seriously considered committing such crimes (Klepper and Nagin 1989). If less serious crimes are more weakly motivated than more serious crimes, the former may be more easily deterred than the latter. Consequently, samples of college students may show more evidence of deterrent effects than more diverse samples of the general population.

We therefore broke down findings based on whether they were derived from samples of college students, those not attending college, or a mixture of the two groups. It was assumed that most of those in samples of known offenders were not attending college, while samples of adults with no description about college were placed into the "mixed" category.

The results in Table 5.5 do not support our expectations. Findings based on samples of college students are slightly less likely to support the deterrence hypothesis than findings based on persons not attending college—34 percent of findings based on college student samples were negative and significant, while 39 percent of findings based on samples of persons not attending college were negative and significant. We suspect that many studies conducted by college professors who did not report the character of the sample they studied were also based on college samples, which may have understated the negative significant findings in the college samples. In contrast, the review of 40 pre-2004 perceptual deterrence studies done by Pratt and his colleagues (2006) did find more support for a deterrent effect within samples of college students. The difference in conclusions may be due to our inclusion of a much larger set of perceptual findings (569 vs. 200) of more recent

**TABLE 5.5** Perceptual deterrence findings by use of college sample

| *Subjects in College?* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| College | 161 | 33.5 | 46.6 | 0.6 | 18.0 | 0.6 | 0.6 | 0.0 |
| Not College | 211 | 38.9 | 27.5 | 9.5 | 16.6 | 1.9 | 5.7 | 0.0 |
| Unknown/Mixture | 197 | 39.6 | 25.4 | 9.6 | 17.8 | 3.6 | 3.0 | 1.0 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

vintage than were reviewed by Pratt et. al. In any case, we remain uncertain as to whether the excessive reliance of perceptual deterrence researchers on college student samples has resulted in an exaggerated degree of support for the deterrence hypothesis.

### *Perceptual Findings by Gender of Sample*

The findings of deterrence research may also be influenced by the gender distribution of study samples. For example, some social control-oriented research has indicated that women risk more with regard to social stigma and role contradiction when committing crimes than do males (Richards and Tittle 1981, 1982). Most findings included in our review were based on samples that were not broken down by gender. Tibetts (1999) expressed surprise that more deterrence research had not been devoted to gender differences in light of the substantial differences in crime involvement between men and women, as well as research indicating that women perceive penalties for crime differently from men (Blackwell, Grasmick, and Cochran 1994; Carmichael, Langton, Leuking, Reitzel, and Piquero 2005; Demers and Lundman 1987; Finley and Grasmick 1985; Grasmick, Blackwell, Bursik, and Mitchell 1993; Miller and Iovanni 1994; McDonnough, Wortley, and Homel 2002; Miller and Simpson 1991; Minor 1977; Paternoster, Saltzman, Waldo, and Chiricos 1985; Pestello 1984; Piquero and Pogarsky 2002; Pogarsky and Piquero 2003; Richards and Tittle 1982).

Only 61 perceptual deterrence findings were based on samples or subsamples comprised exclusively of a single gender. The results of the cross-tabulation of these findings by gender are shown in Table 5.6. They indicate that findings based on samples of women were somewhat *less* likely to support the deterrence hypothesis than findings based on male samples. The modest difference in support of deterrent effects suggests that the huge difference in crime involvement between men and women is not likely to be substantially attributable to gender differences in susceptibility to the threat of legal penalties. This conclusion, however, is quite tentative since it is based on just 61 findings, only 21 of which were based on female-only samples.

**TABLE 5.6** Perceptual deterrence findings by gender of samples (among samples or subsamples composed exclusively of one gender)

| *Gender of Sample* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Sample of Females | 21 | 42.9 | 28.6 | 0.0 | 23.8 | 0.0 | 4.8 | 0.0 |
| Sample of Males | 40 | 52.5 | 22.5 | 0.0 | 15.0 | 10.0 | 0.0 | 0.0 |
| Total | 61 | 49.2 | 24.6 | 0.0 | 18.0 | 6.6 | 1.6 | 0.0 |

### *Perceptual Deterrence Findings by Crime Type*

Theorists have long believed that threats of legal punishment have different effects on different kinds of criminal behavior. One view states that deterrence is strongest for instrumental crimes, such as property crimes where deliberation and calculation precede commission of the crime (Chambliss 1967; Jacob 1980; Speckart, Douglas, and Deschenes 1989). Conversely, expressive crimes committed for the pleasure of the offender are thought to be more difficult to deter. Violent crimes driven by anger, such as homicide or assault, or by lust, such as sexual assault, are believed to be more compulsive in nature and to involve less rational decision-making and are harder to deter (Bachman, Paternoster, and Ward 1992; Jacob 1980).

Some scholars stress the difference between *mala in se* and *mala prohibita* crimes (Andenaes 1975; Teeven 1977; Waldo and Chiricos 1972). *Mala in se* crimes are widely perceived as strongly immoral, and these moral assessments have their own restraining force, which may make legal penalties less necessary as sources of control for most people. In contrast, *mala prohibita* offenses, like marijuana possession and use, are often viewed as wrong only because they have been prohibited by law. The weaker belief in their immorality has less controlling force, leaving more of a control role to be filled by the threat of legal sanctions. Thus, Andenaes (1975) hypothesized that the threat of punishment has more deterrent impact on *mala prohibita* crimes, such as marijuana use, than on *mala in se* crimes, such as murder or robbery, arguing that legal penalties may fill the gap when informal social sanctions are ineffective but are unnecessary for offenses for which a belief in the immorality of the behavior is sufficient to control them.

In sum, the broad themes of this theoretical literature are that the crime types that are most affected by threats of legal punishment are those that are (1) instrumental crimes, which are more likely to be planned and rational in execution rather than impulsive, (2) acquisitive in motivation rather than motivated by passion, and (3) *mala prohibita* in nature, and therefore less strongly condemned on moral grounds, in contrast to *mala in se* crimes. Recognizing these kinds of theoretical considerations, Paternoster (1986) was critical of the use of composite scales to measure crime in deterrence research, objecting to both the practice of combining serious and minor crimes into a single composite measure and the practice of lumping different types of serious crimes into one measure, since threats of punishment could have different effects on different types of crime.

The studies covered in our review involved dozens of different specific crime types, which were initially grouped into four broad categories: (1) acquisitive, (2) violent, (3) nonpredatory (mostly *mala prohibita*), and (4) mixed/all types. The acquisitive category includes all crimes where the offender attempts to acquire valuables through nonviolent means, such as larceny-theft, burglary, white-collar crimes, drug dealing, and tax evasion. While crimes such as robbery may have economic motivations, we categorized them as violent because they entail the use or threat of physical force to acquire property. Other common crimes that were included in the violent category included aggravated assault, sexual assault crimes, and domestic violence. The nonpredatory category included crimes that were both nonviolent and non-acquisitive in nature and largely consisted of offenses that could be regarded as *mala prohibita* crimes, such as drug use and drug possession crimes and juvenile-only crimes, such as underage drinking and curfew violations. The "mixed" category included all findings pertaining to mixtures of multiple crime types or an index of crimes that did not all belong to any one of the three more specific categories. Thus, measures combining FBI "Index" crimes or some other composite that included both violent and property crimes were included in this mixed category, while a composite of multiple violent crimes would be included under the violent category.

Table 5.7 shows the distribution of findings by broad crime category. As expected, findings favorable to the deterrence hypothesis were substantially more common for acquisitive and nonpredatory crimes than for violent offenses. This pattern is consistent with the contentions of researchers (e.g., Bachman et al. 1992; Jacob 1980) who argued that violent crimes would be less deterrable by legal threats because the offenses are more compulsive, less planned, and less rational in nature, as well as the contentions of those like Andenaes (1975) who believed that *mala in se* crimes are less deterrable than *mala prohibita* crimes, which claimed the bulk of nonpredatory offenses.

These patterns also indicate that support for the deterrence thesis has been inflated by the large numbers of studies that focused largely or entirely on minor nonpredatory, *mala prohibita* offenses like marijuana use or driving under the influence of alcohol. Over a third of all perceptual deterrence findings pertain to nonpredatory offenses, and these findings are more likely to support the deterrence doctrine. Aside from "general crime" omnibus measures, the specific crime types most

**TABLE 5.7** Perceptual deterrence findings by general type of crime

| *Type of Crime* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Violent | 44 | 22.7 | 36.4 | 4.5 | 34.1 | 2.3 | 0.0 | 0.0 |
| Acquisitive | 224 | 37.9 | 37.9 | 4.9 | 15.6 | 3.1 | 0.4 | 0.0 |
| Nonpredatory | 215 | 40.5 | 30.2 | 6.5 | 15.8 | 1.9 | 4.2 | 0.9 |
| Mixed/All Types | 86 | 39.5 | 18.6 | 15.1 | 16.3 | 0.0 | 10.5 | 0.0 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

**TABLE 5.8** Perceptual deterrence findings by specific type of crime

| *Specific Type of Crime* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Marijuana Use | 50 | 38.0 | 10.0 | 12.0 | 24.0 | 2.0 | 10.0 | 4.0 |
| Driving Under the Influence | 79 | 45.6 | 27.8 | 5.1 | 20.3 | 1.3 | 0.0 | 0.0 |
| Theft | 86 | 38.4 | 33.7 | 7.0 | 18.6 | 1.2 | 1.2 | 0.0 |
| Any Crime/Multiple | 86 | 39.5 | 18.6 | 15.1 | 16.3 | 0.0 | 10.5 | 0.0 |
| All Other Crimes | 268 | 34.7 | 41.8 | 4.1 | 14.9 | 3.4 | 1.1 | 0.0 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

frequently studied in perceptual deterrence research are marijuana use, driving under the influence (DUI), and theft. The reliance of university-based researchers on the use of college student samples is one major reason for this narrow focus on less serious offenses, since few college students have committed serious offenses while many have smoked marijuana or driven while intoxicated.

To test whether the perceptual deterrence hypothesis was more strongly supported regarding relatively minor offenses, we broke down the findings by more specific crime types. Table 5.8 reports the distribution of findings for the three crime types that are most commonly examined in studies of college student samples.

Leaving aside the nonspecific "any crime/multiple crimes" category, the patterns in Table 5.8 indicate that findings were more likely to support the perceptual deterrence hypothesis with regard to drunk driving than for theft or marijuana use. These are all less serious crimes, while the "All Other Crimes" category includes some serious offenses as well as minor ones. Thus we may infer that findings are somewhat more supportive of the deterrence hypothesis with regard to less serious crimes than with more serious crimes. If less serious offenses are less powerfully motivated than more serious offenses, one would also expect that less serious offenses are more easily deterred. There may also be some influence on these findings of the degree of compulsiveness involved in the crimes. Since marijuana use is less compulsive than use of more addictive drugs like crack cocaine and heroin, there may be more room for prospective offenders to consider the risks of legal penalties for marijuana use.

The unfortunate result of the disproportionately heavy focus on minor, weakly-motivated offenses is that the body of perceptual deterrence research as a whole has created an overstated impression of the deterrent impact of perceptions of punishment risk on crime in general and serious crime in particular. Conversely, the limited attention paid to the serious, powerfully motivated *mala in se* crimes that are the primary source of public concern about crime renders this body of research less relevant to policy aimed at reducing serious crime. Unfortunately, the threat of punishment may be most capable of deterring the kinds of minor crimes that the public worries about least.

Many important crime types have been largely ignored in perceptual deterrence research. For example, while drug *use* is one of the most frequently studied crimes in the individual-level deterrence research, drug *dealing* has rarely been studied. We located only three perceptual deterrence findings regarding this offense, all of them finding nonsignificant associations (Hjarlmarsson 2009; Horney and Marshall 1992; McCarthy and Hagan 2005). Further, two of these studies (Horney and Marshall 1992; McCarthy and Hagan 2005) actually tested the impact of drug dealing on perceptions of punishment instead of the reverse, given the way the authors handled causal order. Thus we have too little evidence to draw confident conclusions regarding the impact of perception of legal risks on drug dealing. The negligible number of studies of the deterrence of drug dealing is probably not so much an oversight as it is a testament to the difficulty of undertaking such a study. High-school and college students are convenient populations to study regarding drug *use* because many students use drugs, but not for studying drug *dealing*, since few are drug dealers. Dealers are rarer and harder to locate.

Over one million people die in driving accidents each year worldwide (World Health Organization 2015); many of the accidents involving speeding. In this light, violating speed limits would also seem to be an important offense to be addressed in the deterrence literature. We located only 15 findings supposedly addressing the perceptual deterrence hypothesis as it pertains to speeding, all of which unfortunately related *current* perceptions of legal risk (i.e., those prevailing at the time the research was conducted) to *past* speeding. Although most of this research was presented as assessments of deterrent effects, the time order of the variables was obviously wrong for this purpose, and the studies are better viewed as assessments of the experiential effect of speeding on subsequent perceptions of the risk of legal punishment for speeding. Keeping this in mind, 13 of these 15 findings pertained to perceived certainty of punishment for speeding and some of these 13 associations were significant and negative (Beck, Fell, and Yan 2009; Grasmick and Appleton 1977; Grasmick and Milligan 1976; Minor 1977). While these findings might be generously interpreted as supporting a deterrent effect of perceived certainty of punishment, it is more likely that they indicate that speeding without being caught by the police lowers a person's perception of the risk of getting caught for speeding.

On the other hand, Cohen found a positive insignificant association between speeding and perceived *severity* of punishment for speeding (Cohen 1978), and Minor found a significant positive association between speeding and perceived severity of punishment (Minor 1978)—contrary to both the deterrence thesis and the experiential effect thesis. Our review does not cover studies of deterrent effects of legal penalties on driving fatalities or accidents per se, as these do not necessarily involve any criminal behavior.

Deterrence researchers have likewise rarely studied sexual assault and stalking. The omission of stalking is understandable due to its relatively recent criminalization. Rape and sexual assault, in contrast, are not newly illegal, yet we located only six studies, yielding nine findings bearing on deterrence of these crimes. Most tests found insignificant negative associations of perceived certainty (Bouffard 2002a; Gertz and Gould 1995; Strange and Peterson 2013) or severity of punishment (Bouffard 2002a) with forecasts of future sexual offending, though a few findings supported a significant negative association of perceived certainty with forecasts of crime (Bouffard 2002b) and with past crime (Gertz and Gould 1995), the latter possibly reflecting an experiential effect of prior (unpunished) sexual assault behavior on perceived certainty of punishment, rather than a deterrent effect.

The neglect of rape and sexual assault are specific examples of the broader problem that violent crime as a whole has been understudied in the perceptual deterrence literature. The few studies focusing on violent crime that we did locate, beyond the handful of rape/sexual assault studies already discussed, addressed robbery or general crime indexes that only included violent crime as a component. Regarding robbery, there are four studies that yielded a total of 14 findings, but these concerned the effect of *experienced* punishment on criminal behavior, a topic we address in Chapter 6. Most of these findings were nonsignificant (Greenberg and Larkin 1999; Mocan and Rees 2005), indicating that experienced punishment *reduced* perceptions of risks (Kleck, Sever, Li, and Gertz 2005), or were mixed (Horney and Marshall 1992).

There are still other crime types that have yet to be given any significant amount of research attention. For instance, we located only one study that tested the impact of perceptions of punishment on the use of a false ID (Rankin and Wells 1982). Similarly, while there has been some research testing the impact of perceptions of punishment risk on computer hacking (Young and Zhang 2007) and piracy of goods online (Gopal and Sanders 1997; Levin, Dato-on, and Manolis 2007; Morton and Kourteros 2008; Zhang, Smith, and McDowell 2009), as well as the effect of experienced penalties on software piracy (Gopal and Sanders 1997; Gunter 2009; Higgins, Wilson, and Fell 2005; Kartas and Good 2012), we have located no studies testing the effectiveness of legal deterrence on online stalking or predatory computer and phone scams. The rapid increase in these newer forms of crime will hopefully lead to more deterrence studies of these crimes in coming years.

### *Perceptual Deterrent Effects of Different Properties of Legal Punishment: Certainty, Severity, and Swiftness*

A great deal of debate about the deterrent effects of punishment has revolved around which properties of punishment are most important in producing deterrence—the certainty, severity, or swiftness (celerity) of punishment. While political debates about punishments have often focused on whether the severity of punishments should be increased, e.g. in the form of longer prison sentences or increased use of the death penalty, perceptual deterrence research has most often tested the effects of perceived certainty. The next most frequently tested property is the severity of punishment, while tests of the impact of perceived swiftness of punishment are rare.

Table 5.9 summarizes deterrence findings classified according to the dimension of punishment that was assessed. Of the dimensions of punishment widely studied, certainty of punishment is, by far, the dimension most likely to show some apparent deterrent effect. Of 390 findings on perceived certainty effects, over 40 percent are significant and negative. Note, however, that this percentage

**TABLE 5.9** Perceptual deterrence findings by dimension of punishment

| *Dimension of Punishment* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Perceived Certainty | 390 | 40.5 | 33.3 | 8.5 | 11.3 | 1.5 | 4.6 | 0.3 |
| Perceived Severity | 127 | 26.8 | 29.1 | 4.7 | 36.2 | 1.6 | 0.8 | 0.8 |
| Perceived Swiftness | 11 | 27.3 | 45.5 | 0.0 | 27.3 | 0.0 | 0.0 | 0.0 |
| Certainty × Severity | 41 | 48.8 | 12.2 | 12.2 | 19.5 | 7.2 | 0.0 | 0.0 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

obviously also implies that, even for certainty of punishment, most research findings fail to support the deterrence hypothesis, since nearly 60 percent of the certainty/crime associations were either positive or not significantly different from zero. This accords with the conclusions of Pratt and his colleagues (2006), whose review of the pre-2006 perceptual deterrence research concluded that "despite [a] general pattern of weak effects, the mean effect sizes for certainty predictors are consistently the most robust" (381).

The fact that many findings support an effect of perceived certainty of punishment but most do not suggests that it may affect some people and not others or has effects under some circumstances and not others, as noted in Chapter 2.

Crime control strategies based on increasing the severity of punishment are popular among criminal justice policymakers, but most deterrence scholars have concluded that perceived certainty of punishment has greater deterrent effect than perceived severity of punishment (Anderson, Chiricos, and Waldo 1977; Grasmick and Appleton 1977; Grasmick and Green 1981; Jensen, Erickson, and Gibbs 1978; Meier and Johnson 1977; Silberman 1976; Patrick and Marsh 2009). Our review supports this view, showing that tests of the certainty of punishment are substantially more likely to support the perceptual deterrence hypothesis. Only about one fourth of findings regarding perceived severity are significant and negative, supporting a deterrent effect. Put another way, the vast majority of individual-level research findings indicate that perception of more severe punishment does not deter criminal behavior any better than perceptions of less severe punishment.

The swiftness (or celerity) of punishment continues to receive negligible attention. Indeed, we identified only three studies that had tested the impact of perceived swiftness on crime prior to 2000 (Minor 1978; Pestello 1984; Thurman 1989) and six studies since that time (Freeman and Watson 2006; Loughran, Paternoster, and Weiss 2012; Nagin and Pogarsky 2001; Watling and Freeman 2011; Yao, Johnson, and Beck 2014; Yu, Evans, and Clark 2006). Their results are similar to the meager support for deterrent effects of perceived severity, but the body of research on swiftness is far too slender a foundation on which to base any firm conclusions.

### *Is There an Interaction Between Perceived Severity and Perceived Certainty?*

Most researchers have interpreted the poor support for a deterrent effect of more severe punishment as an indication of its ineffectiveness as a source of crime control (e.g., Jensen et al. 1978; Doob and Webster 2003), but others have claimed that greater severity is effective but only when certainty levels are relatively high (Grasmick and Bryjak 1980; Ross, McCleary, and LaFree 1990). It is argued that severity of punishment cannot impact crime when certainty of punishment is low because severity of punishment is of little significance to people who do not believe that they will be apprehended for their crime in the first place (Grasmick and Bryjak 1980; Ross et al. 1990). If this hypothesis is correct, there should be a significant interaction between perceived certainty and perceived severity of punishment, showing a stronger negative effect of severity when certainty is higher.

Table 5.10 Panel B also summarizes findings regarding the interaction between certainty and severity, tested using a multiplicative interaction term, certainty times severity (C × S). The C × S measure is not to be confused with measures that we have described as "combined punishment," as the latter typically consists of severity and certainty measures combined into an index score, often using factor analysis to create the index. In contrast, researchers testing for an interaction between certainty and severity have *separate* measures of certainty and severity and multiply them together to form a multiplicative interaction term.

Table 5.10 Panel B indicates that 48 percent of tests of the C × S interaction term have found it to have a significant negative association with criminal behavior, suggesting that as certainty

**TABLE 5.10** Tests of the interaction between perceived severity of punishment and perceived certainty

| *Study* | *Statistical Method* | *#IVs* | *Crime* | *Severity Finding* |
|---|---|---|---|---|
| *Panel A: Studies Testing Interaction by Subsample Method (Effect of Perceived Severity Within the High Certainty Subset)* | | | | |
| Bailey and Lott (1976) | Correl | 1 | Past Marijuana Use | – , p = ? |
| | | | Past Marijuana Dealing | +, p = ? |
| | | | Past Petty Theft | – , p = ? |
| | | | Past Grand Theft | – , p = ? |
| | | | Past Shoplifting | – , p = ? |
| Teeven (1976a) | Gamma | 1 | Past Marijuana Use | – , $p > 0.05$ |
| | | | Past Shoplifting | – , $p > 0.05$ |
| Teeven (1976b) | Gamma | 1 | Past Marijuana Use | – , $p > 0.05$ |
| | | | Past Shoplifting | – , $p > 0.05$ |
| Anderson, et al. (1977) | Yules Q | 1 | Past Marijuana Use | – , p = ? |
| Grasmick and Bryjak (1980) | OLS | 1 | Past Multiple | **– , $p < 0.001$** |
| Decker et al. (1993) | D. Means | 1 | Forecast of Burglary | – , p = ? |
| Loughran et al. (2011) | OLS | 8 | Later Violent | – , $p > 0.05$ |
| | | | Later Nonviolent Impersonal | – , $p > 0.05$ |
| Jacobs and Piquero (2012) | OLS | 2 | Forecast of DUI | – , $p > 0.05$ |
| Harbaugh, Mocan, and Visser (2013) | OLS | 12 | Forecast of Theft | **– , $p < 0.01$** |
| | | | College Students | **– , $p < 0.01$** |
| | | | High School Students | **– , $p < 0.01$** |
| *Panel B: Studies Using Multiplicative Interaction Term (C × S)* | | | | |
| Cohen (1978) | Correl. | 1 | Past Speeding | ? , n.s. |
| Grasmick and Green (1980)[a] | OLS | 3 | Forecast of Multiple | **– , $p < 0.05$** |
| | | | Past Multiple | **– , $p < 0.001$** |
| Grasmick and Green (1981) | OLS | 7 | Forecast of Multiple | **– , $p < 0.01$** |
| | | | Past Multiple | **– , $p < 0.05$** |
| Grasmick and Bursik (1990) | Logistic | 7 | Forecast of Tax Cheating | **– , $p = 0.03$** |
| | | | Forecast of Petty Theft | **– , $p = 0.03$** |
| | | | Forecast of DUI | **– , $p = 0.02$** |
| Braithwaite and Makkai (1991) | OLS | 25 | Past Corporate Crime | – , $p > 0.05$ |
| Williams (1992) | Logit | 8 | Later Domestic Violence | **– , $p < 0.05$** |
| Grasmick et al. (1993) | OLS | 8 | Forecast of DUI | **– , $p < 0.001$** |
| Paternoster and Iovanni (1986) | OLS | 11 | Later Multiple Nonviolent | – , $p > 0.05$ |
| | | 3 | Past Multiple Nonviolent | +, $p > 0.05$ |
| Green (1989) | Logit | 10 | Past DUI | ? , n.s. |
| Paternoster and Simpson (1996)[b] | GLS | 20 | Forecast of Corporate Crime | – , $p > 0.05$ |
| | | | | +, $p < 0.05$ |
| Piquero and Tibbetts (1996) | Path | 6 | Forecast of Shoplifting | n.s. |
| | | | Forecast of DUI | **– , $p < 0.05$** |
| Tibbets and Herz (1996) | OLS | 6 | Forecast of Shoplifting | +, $p > 0.05$ |

| *Study* | *Statistical Method* | *#IVs* | *Crime* | *Severity Finding* |
|---|---|---|---|---|
| | | | Forecast of DUI | **– , p < 0.05** |
| | | | Forecast of Shoplifting | +, p > 0.05 |
| | | | Forecast of DUI | **– , p < 0.01** |
| Baron and Kennedy (1998) | OLS | 13 | Past Car Burglary | +, p < 0.05 |
| | | | Past Building Burglary | +, p < 0.05 |
| | | | Past Battery | – , p > 0.05 |
| Nagin and Pogarsky (2001) | Tobit | 12 | Forecast of DUI | ? , n.s. |
| Zhang et al. (2009)[a] | OLS | 9 | Past Digital Piracy | ? , n.s. |
| Tittle, Botchkovar, and Antonaccio (2011) | OLS | 6 | Forecast of Theft | |
| | | | 1,400 Adults, Athens | +, p > 0.05 |
| | | | 400 Adults, Greece | +, p > 0.05 |
| | | | 500 Adults in Russia | +, p > 0.05 |
| | | | 500 Adults in the Ukraine | +, p > 0.05 |
| | | | Violence | |
| | | | 1,400 Adults, Athens | **– , p < 0.05** |
| | | | 400 Adults in Greece | +, p > 0.05 |
| | | | 500 Adults in Russia | **– , p < 0.05** |
| | | | 500 Adults in the Ukraine | **– , p < 0.05** |
| Loughran et al. (2012) | OLS | 5 | Forecast of DUI (C × Celerity) | – , p > 0.05 |
| Harbaugh et al. (2013) | OLS | 12 | Forecast of Theft | |
| | | | High School | – , **p < 0.01** |
| | | | College Students | **– , p < 0.01** |
| | | | H.S. and College | **– , p < 0.01** |
| Bouffard and Petkovsek (2013) | Path | 9 | Forecast of DUI | **– , p < 0.01** |
| Yao et al. (2014)[a] | Logistic | 27 | Forecast of DUI | **– , p < 0.05** |

*Notes*:

[a]Authors used a multiplicative C × S variable as a combined legal sanctions variable, not for testing an interaction effect. Likewise, Williams (1992) simply used C × S as a composite punishment variable.

[b]Second finding pertains to C × S when C is perceived *other's* certainty of punishment, while the first finding pertains to C × S when C is perceived *self's* certainty.

*Abbreviations*: Corr = product-moment correlation; D. Means = Difference of Means test; GLS = Generalized least squares; HS & College = High School and College Students; Logit or logistic = Logit or Logistic regression; OLS = Ordinary Least Squares; Path = Path Analysis

increases, the effect of severity becomes more negative, i.e. more crime-reducing. Unfortunately, results of this type of interaction test are ambiguous, since the C × S interaction term may also have a negative association with criminal behavior because severity has a *less positive* effect when certainty levels are higher than when certainty levels are lower. That is, severity might merely have a weaker crime-*in*creasing effect when punishment is more certain, but its effects are nevertheless crime-increasing at both lower and higher levels of certainty.

Research using a less ambiguous method of testing for this interactive effect has yielded less supportive findings. An alternative way to test this interaction is to subdivide the sample of subjects according to their level of perceived certainty of punishment and then estimate the association between perceived severity and criminal behavior within each of those subsets. Table 5.10 Panel A summarizes the findings of this body of research. There were only 18 findings testing for an effect of perceived severity among subjects who perceived a high level of punishment certainty, and 11 of these were generated by research that had the causal order wrong, in that they measured the association of *current* perceptions of punishment risk with *past* criminal behavior. Of the remaining seven findings, five were based on hypothetical, forecast measures of criminal behavior. Thus, only two interaction findings were generated by research in which perceptions of risk were related to actual (not forecast) criminal behavior during a later period (see Loughran, Raymond, Piquero, and Pogarsky 2011). Both of these findings indicated no significant association between perceived severity and later criminal behavior among those who perceived a high certainty of punishment. In sum, there have been almost no strong tests of the hypothesis that perceived severity has a deterrent effect on offending when accompanied by high perceived certainty, and these few strong tests did not support the hypothesis. These deficiencies indicate a need for longitudinal perceptual research on possible interactions between the certainty and severity of punishment that relates perceived punishment at an earlier time to criminal behavior committed during a later time.

If we relax our standards somewhat and summarize all tests of the interaction hypothesis using the subsample method (i.e., that assessed the association between perceived severity and offending among persons who perceive certainty of punishment to be high), there were 18 total findings, of which only four were negative and significant. Thus, only 22 percent of these findings supported the interaction hypothesis. Only when we relax our standards even more, and include findings based on the ambiguous tests using multiplicative interaction terms (C × S) do a substantial minority support the interaction hypothesis. Among all 58 findings shown in Table 5.10 (41 of which are based on the C × S method), 24 of them (41 percent) are significant and negative. All of these supportive findings were based on the ambiguous C × S method.

To be sure, one could speculate that levels of perceived certainty among study samples failed to reach sufficiently high levels for the contingent effect of perceived severity to manifest itself. Even if there were an effect of perceived severity on criminal behavior that depended on achieving high levels of punishment certainty (e.g., over 20 percent), it would have limited applicability to public policy since the criminal justice system has not been able to achieve high certainty levels for any offense other than homicide (Table 3.1).

### *The Impact of Different Punishment Perception Measures on Perceptual Deterrence Findings*

Measures of the perception of punishment certainty can be broken down into four basic types. The first and most commonly used measure is the likelihood of being "caught." This category encompasses two somewhat different measures: the likelihood of being caught by police and the likelihood of simply being caught, without any specification of who was doing the catching. The latter measure was typically used with high school and college students. Many research subjects probably assumed that "being caught" means being caught by the police, i.e. arrested, but the expression is vague enough that it could also encompass being "caught" by one's parents, spouse, employer, or perhaps even the victim. Thus, the measure could refer to the certainty of something happening that does not involve the police or any other part of the criminal justice system, in which case it has no necessary connection with the certainty of legal punishment. Other commonly used perceived certainty measures explicitly pertain to the likelihood of being arrested, prosecuted, convicted, or incarcerated. We have also included an "other" category to encompass findings involving certainty measures that did not fall into any of these categories. Table 5.11 indicates that perceived certainty

**TABLE 5.11** Perceptual certainty findings by measure of certainty

| *Certainty Measure* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| "Caught" | 207 | 49.3 | 27.1 | 1.4 | 12.1 | 2.4 | 7.7 | 0.0 |
| Arrested | 94 | 31.9 | 41.5 | 18.1 | 7.4 | 0.0 | 0.0 | 1.1 |
| Prosecuted/Convicted | 22 | 31.8 | 63.6 | 0.0 | 4.5 | 0.0 | 0.0 | 0.0 |
| Incarcerated | 22 | 9.1 | 45.5 | 40.9 | 4.5 | 0.0 | 0.0 | 0.0 |
| Other | 45 | 37.8 | 24.4 | 8.9 | 2.2 | 2.2 | 4.4 | 0.0 |
| Total | 390 | 40.5 | 33.3 | 8.5 | 11.3 | 1.5 | 4.6 | 0.3 |

analyses using the "caught" measure were considerably more likely to find support for the deterrence hypothesis than were other measures of perceived certainty and in particular far more likely to be supportive than measures of the certainty of arrest. This suggests that it is not the certainty of being arrested and thereby made subject to legal punishment that deters people from crime but rather the certainty of parties other than the police either "catching" the person doing crime (Williams and Hawkins 1986) or learning about the crime through the police. If so, it is more likely that the threat of nonlegal consequences ancillary to arrest affects criminal behavior rather than just the certainty of legal punishment per se.

Table 5.11 makes it clear that the case for perceptual deterrence as it relates to certainty of punishment is heavily dependent on these ambiguous findings based on the "caught" measure. Without the 207 findings based on this measure of certainty, 102 of which were significant and negative, there are just 183 perceived certainty findings, only 30.6 percent of which were significant and negative. Thus, there is considerably less support for the deterrent effect of perceived certainty of punishment if one does not include results that may actually reflect the impact of fears that some nonlegal party, such as a parent, teacher, or employer, might become aware that a person has committed a crime.

Other measures of certainty pertain to the likelihood of being prosecuted, convicted, or incarcerated *if arrested*. The impact of these contingent measures of certainty may be reduced if one's perception of the probability of being arrested in the first place is low. That is, the deterrent effect of the certainty of later steps in the punishment process (prosecution, conviction) may be contingent on perceived certainty of being arrested being relatively high, just as the effect of perceived severity may be contingent on levels of perceived certainty. Future research should explore whether perceived certainty of arrest conditions the impact of perceived certainty of prosecution, conviction, or incarceration on criminal behavior.

### *Perceived Punishment Risk of Self vs. Punishment Risk of Others*

Measures of perceived certainty of punishment also differ by whether they pertain to the person being studied (risk to self) or to other people (risk to others). Some deterrence researchers ask subjects about the risk that a peer, a friend, or generalized others will be caught, arrested, convicted, or imprisoned (e.g., Jensen 1969; Paternoster et al. 1982; Teevan 1976a, 1976b; Waldo and Chiricos 1972; Watling and Freeman 2011), while others ask the respondents specifically about the likelihood that they themselves will experience these consequences (e.g., Bailey and Lott 1976; Baron 2013; Blackwell 2000; Claster 1967; Richards and Tittle 1981, 1982). The summary of findings in Table 5.12 indicates that researchers are slightly more likely to obtain findings supportive of a

**TABLE 5.12** Perceptual certainty findings by perception of risk to Self vs. Risk to others

| *Perceived Certainty of Punishment of:* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Self | 275 | 42.5 | 29.1 | 9.1 | 14.5 | 1.8 | 2.5 | 0.4 |
| Others | 114 | 36.0 | 43.9 | 7.0 | 3.5 | 0.9 | 8.8 | 0.0 |
| Total | 389 | 40.6 | 33.4 | 8.5 | 11.3 | 1.5 | 4.6 | 0.3 |

**TABLE 5.13** Findings by measure of perceived severity

| *Perceived Severity Measure* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| How Much of a Problem? | 57 | 19.3 | 29.8 | 5.3 | 40.4 | 3.5 | 1.8 | 0.0 |
| How Severe the Legal Penalty? | 32 | 45.2 | 22.6 | 6.5 | 22.6 | 0.0 | 0.0 | 3.2 |
| Estimated Legal Penalty | 29 | 6.9 | 37.9 | 3.4 | 51.7 | 0.0 | 0.0 | 0.0 |
| Other Measures | 11 | 63.7 | 18.2 | 0.0 | 18.2 | 0.0 | 0.0 | 0.0 |
| Total | 127 | 26.8 | 29.1 | 4.7 | 36.2 | 1.6 | 0.8 | 0.8 |

deterrent effect of perceived certainty when they measure perceived risks as they pertain to the subject than when they pertain to others.

### *Perceptual Deterrence Findings by How Perceived Severity of Punishment Was Measured*

There are three common types of measures of perceived severity of punishment. The most common measure asks respondents "how big of a problem" being caught for crime would present in their lives (e.g., Grasmick and Bryjak 1980; Jacobs and Piquero 2012; Paternoster and Iovanni 1986). One problem with this measure is that it is not necessarily a measure of the severity of legal punishment per se; any evidence of deterrence may actually be due to the severity of informal negative consequences, like shunning or criticism from significant others, being expelled from school, or losing a job, rather than the legal punishment itself. Problematic though the measure is, studies using this type of severity measure nevertheless dominate research on the deterrent effect of perceived severity of punishment, generating nearly half of the relevant findings.

The other common measures of perceived severity involve asking the respondent to either estimate a specific legal penalty for a given crime (e.g., Bouffard et al. 2008; Feld and Larsen 2012; Silberman 1976) or to provide a less specific prediction about generally "how severe" the legal penalty would be for a given crime (e.g., Meier and Johnson 1977; Watling and Freeman 2011; Williams 1985).

Most of this research indicates that perceived severity of punishment does not generally affect criminal behavior—less than 27 percent of findings were significant and negative. Table 5.13 shows that most of the findings of research using two of the three common measures of perceived severity indicate that perceived severity is not generally related to criminal behavior. Support is not found in studies using the most specific severity measure asking respondents about exact penalties that might be encountered or the most general measure reflecting the full array of problems, legal and nonlegal, caused by being apprehended for a crime. Some support is found, however, for deterrent effects when the severity measure asks respondents about the general level of severity of legal punishments for crime. More research is needed to determine why supportive findings are more likely to be obtained when using that latter kind of severity measure.

### *The Conditioning Effect of Criminal Experience on Perceptual Deterrent Effects*

As we noted in Chapter 2, the threat of legal punishment may affect those who have already engaged in criminal behavior differently from those who have not yet offended in any significant way. Some researchers have theorized that offenders with greater criminal conduct in their past will be less sensitive to the threat of legal penalties (Baron and Kennedy 1998; Hagan and McCarthy 1997). (Note that this is not the same as the "experiential effect," i.e. the effect of prior criminal behaviors on perceptions of punishment risk, a topic addressed later in this chapter.) Chronic criminals presumably have less attachment to the conventional norms of society and have less to lose from the threat of punishment (Baron and Kennedy 1998; Hagan and McCarthy 1997; Zimring and Hawkins 1973). Further, if repeated crime-committing brings repeated legal sanctions, offenders could become inured to the punishments, which become less painful with each repetition. Others have argued that chronic criminals are characterized by an impulsivity to commit crime that makes them less likely to consider the long-term consequences of their actions and, therefore, particularly difficult to deter (Bennett and Wright 1984; Feeney 1986; Shover 1996; Wright and Decker 1994).

We tested these ideas by comparing the results of tests of perceptual deterrence when analysts studied a sample of the general population with results based on a sample of known offenders, such as prison inmates. The relevant findings are summarized in Table 5.14.

The pattern of findings does not support the theoretical expectation that perceptions of the risk of legal punishment exert more effect on the criminal behavior of members of the general population, who generally have little prior experience of serious criminal behavior, than on experienced offenders. The findings show that 38 percent of perceptual deterrence findings were significant and negative when general population samples were studied, while 45 percent of findings were similarly supportive of perceptual deterrence when samples of known offenders were studied. Thus, there is little difference in the level of support for perceptual deterrence between criminals and the general public. This seems to be contrary to the belief that offenders are more present-oriented and therefore should be less responsive to the prospect of legal punishment in the future (Gottfredson and Hirschi 1990). On the other hand, there is some theory and evidence suggesting that deterrence is less likely to affect persons with stronger moral beliefs forbidding criminal acts (e.g., Wright, Caspi, Moffitt, and Paternoster 2004) who are presumably more prevalent in general population samples than in known-offender samples. Perhaps in the aggregate the effects of

**TABLE 5.14** Perceptual deterrence findings by criminal experience of sample

| *Criminal Experience of Sample* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| General Population | 506 | 37.5 | 33.8 | 6.7 | 16.0 | 2.2 | 3.4 | 0.4 |
| Known Offenders | 58 | 44.8 | 19.0 | 1.7 | 29.3 | 1.7 | 3.4 | 0.0 |
| Mixed Sample | 5 | 0.0 | 0.0 | 100.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

present-orientation (discounting of future consequences) and weaker moral beliefs among criminals cancel each other out.

## Perceptual Deterrence Findings and Methodological Artifacts

Research findings can differ across studies due to methodological flaws that vary from one study to the next. In this section, we consider some of the potentially more important methodological variations. Descriptions of each one are brief because more detailed discussions of most of them were already provided in Chapter 4.

### *The Impact of Controlling for Informal Sanctions on Perceptual Deterrence Findings*

Many researchers have argued that informal social controls have a greater effect on criminal behavior than do formal sanctions (e.g., Anderson et al. 1977; Nagin 1998; Paternoster and Iovanni 1986; Paternoster et al. 1983b; Silberman 1976; Tittle 1977). They have further contended that the strength of the informal, nonlegal controls to which an individual is subject may be correlated with their perceptions of the risks of legal punishment, raising the possibility that the effects of the threat of informal sanctions, such as shunning or the criticism of significant others, may be misinterpreted as effects of perceived legal risk. For example, people might refrain from crime not because they feared legal punishment itself but rather because they feared the negative reactions of significant others that would likely accompany being arrested, convicted, or incarcerated. Alternatively, perceptions of legal risk may have no effects of their own at all but appear to do so because they are correlated with the strength of informal controls to which a person is subject. Supporting this latter view, Paternoster and Iovanni (1986) found that once they controlled for informal controls there was no longer any evidence of an effect of either perceived certainty or perceived severity of punishment on criminal behavior.

Estimates of the impact of formal deterrence could therefore be influenced by whether analysts controlled for informal or extra-legal sources of social control (Meier and Johnson 1977; Paternoster and Iovanni 1986; Paternoster et al. 1983a, 1983b; Paternoster, Bachman, Brame, and Sherman 1997). While many scholars assert that threats of legal punishments have deterrent effects of their own apart from the effects of extralegal sanctions (e.g., Grasmick and Appleton 1977; Jensen 1969), others also suggest that perceived legal sanctions interact with extralegal sanctions (Nagin and Paternoster 1994; Wright et al. 2004).

We explored these ideas in a number of ways. First, we compared perceptual deterrence findings generated by analyses with or without controls for extra-legal sanctions. The vast majority of multivariate analyses of perceptual deterrence include controls for at least one variable that at least some scholars have loosely labeled as an extra-legal "social control" variable, such as employment, marriage, or moral views about a given crime. As a consequence, sorting studies as to whether they included some kind of social control variable, construed this broadly, would have been tantamount to sorting them as to whether the analysis was multivariate or bivariate. We instead sorted findings based on whether researchers specifically controlled for perceived informal social sanctions for criminal behavior, such as negative reactions from peers, parents, or society. Studies that merely controlled for marital status, employment, ethics, or morality were not classified as controlling for informal sanctions.

Table 5.15 indicates that findings are considerably less likely to support the perceptual deterrence hypothesis when analysts controlled for the informal social sanctions to which people were subject. When analysts failed to control for informal sanctions, 44.4 percent of findings were

**TABLE 5.15** Perceptual deterrence findings by whether informal sanctions were controlled

| *Controlled for Informal Sanctions?* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Yes | 218 | 27.5 | 35.8 | 10.6 | 20.6 | 3.2 | 1.8 | 0.5 |
| No | 351 | 44.4 | 29.6 | 4.8 | 15.1 | 1.4 | 4.3 | 0.3 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

significant and negative, consistent with the deterrence hypothesis, but when analysts controlled for informal social sanctions, only 27.5 percent were negative and significant. This supports the idea that the effects of perceptions of legal punishment risks were confounded with effects of informal social sanctions in studies that failed to explicitly control for the latter. Since nearly two thirds of perceptual deterrence findings were based on analyses that failed to control for informal sanctions, this suggests that *most* of the apparent support for perceptual deterrence may be at least partly the product of this methodological flaw.

### *Do Informal Social Controls Condition the Effect of Punishment Perceptions on Crime?*

We also explored the possibility that nonlegal sanctions might *condition* the effect of punishment perceptions on crime, in addition to having their own effects. Some theorists, for example, have suggested that threats of legal punishment are largely redundant for persons subject to strong internal controls due to a fear of nonlegal sanctions (Andenaes 1952). Thus, the threat of legal punishment might only affect people not already controlled by the threat of nonlegal punishments.

We initially intended to review only tests of the specific interaction between anticipated *nonlegal sanctions* and perceived legal risk, but we could locate only 15 studies that tested this specific interaction. We also found, however, that there are many studies that tested the conditioning effect that various types of *informal social control* may have on the impact of perceptions of punishment risk on criminal behavior, examining sources of social control such as conventional ties (Jensen 1969), belief in law (Teevan 1977), and moral commitment (Grasmick and Green 1981), as well as determinants of the strength of ties to the community such as socio-economic status (Thistlethwaite, Wooldredge, and Gibbs 1998), employment (Berk, Cambell, Klap, and Western 1992; Sherman, Schmidt, Rogan, Smith, Gartin, Cohn, Collins, and Bachich 1992), and marital status (Berk et al. 1992; Sherman et al. 1992; Thistlethwaite et al. 1998). We therefore extended our focus to encompass tests of interactions between perceptions of legal punishment risk and this broader array of sources of informal social control.

Table 5.16 displays the distribution of findings of analyses testing for an interaction between informal social controls and perceived risk of punishments, either by (1) using an interaction term between informal social control variables and perceived legal risks or (2) by testing the deterrence hypothesis within subsamples defined by the level of informal social control to which people were subject. More specifically, these analyses were attempts to test whether perceptions of legal punishment risk affect criminal behavior only or, to a greater extent, among those subject to weak informal controls.

Readers should be aware of a common flaw in tests of this interaction. Some analysts obtained the same sign and significance of the association between legal punishment and crime across different levels of informal social control and concluded that there was *no* interaction, yet did not

**TABLE 5.16** Summary of findings on whether strength of informal social controls conditions the effects of perceived certainty and severity of punishment on criminal behavior

| *Total Number of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|
| | *– sig* | *– ns* | *ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 74 | 13.5 | 9.4 | 43.2 | 8.1 | 8.1 | 12.2 | 4.4 |

report significant levels of differences between coefficients based on the different subsamples. Other analysts performing this type of interaction test likewise failed to perform tests of significance of these differences but concluded that deterrent effects *were* contingent on informal social control levels, based solely on the fact that the *magnitude* of the punishment/crime association differed across subsamples.

In Table 5.16, a negative finding indicates that the perceived risk of punishment has a stronger deterrent (negative) effect among those subject to stronger informal controls, such as a belief in the immorality of the behavior, anticipated condemnation from significant others, or stronger social bonds in the form of marriage or employment. A positive finding indicates that perceptions of legal risk have a weaker deterrent effect (or even a crime-increasing effect) among those subject to stronger informal social controls. Thus, a positive finding would be consistent with the theoretical expectation that legal controls have less (negative) impact on those already subject to strong informal controls.

The summary of findings suggests that the level of informal social control does not substantially condition the effects of perceived punishment risks on criminal behavior. Only 8.1 percent of the interaction findings were positive and significant, supporting the hypothesis that people subject to stronger informal social controls are affected less by perceived risks of punishment than those subject to weaker informal social controls. Even if we ignore statistical significance, there were more findings (35.1 percent) indicating *weaker* deterrent effects among those subject to weaker social controls (i.e., negative findings) than there were findings suggesting *stronger* deterrent effects among those subject to weaker social controls (20.6 percent) (the sign of the association was unknown for the remainder of the findings). Thus, support for perceptual deterrence appears to be no stronger among those subject to weaker social controls than among those subject to stronger social controls.

### *Perceptual Deterrence Findings by the Number of Variables Controlled*

We have seen that perceptual deterrence findings appear to differ substantially depending on whether researchers controlled for one specific confounding variable—strength of informal sanctions for criminal behavior. We now consider whether researchers who do a better job at controlling potential confounding variables *in general* may likewise obtain different findings. Researchers who controlled many antecedent variables that affect both perceptions of risk and criminal behavior should obtain different estimated effects of perceived risk on crime than those who controlled few. We tested this possibility by classifying findings according to the number of independent variables controlled in the statistical analyses that generated the findings. Table 5.17 shows the results.

The pattern of results does not support the proposition that support for perceptual deterrence was weaker when more variables were controlled, as the share of findings that were significant and negative were nearly identical in studies that controlled for six or more variables as in those that controlled for only five or fewer. On the other hand, if we ignore statistical significance, the share of associations that were negative is slightly higher when few independent variables were included

**TABLE 5.17** Perceptual deterrence findings by number of independent variables in the model

| *# of Independent Variables Controlled* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 6 or More | 405 | 38.3 | 29.6 | 6.9 | 21.0 | 2.7 | 1.2 | 0.2 |
| 0–5 | 164 | 37.2 | 37.8 | 7.3 | 7.9 | 0.6 | 8.5 | 0.6 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

in the analysis (75 percent) than when many were included (68 percent). Nevertheless, it seems prudent to conclude that merely controlling for more variables in general does not appear to consistently affect support for perceptual deterrence. It instead matters more whether the researcher controls for specific confounding variables, such as the strength of informal sanctions to which a person is subject.

### *Perceptual Deterrence Findings and the Use of Hypothetical Crime Vignettes*

As noted in Chapter 4, some scholars tried to solve the causal order problem without benefit of panel designs by relating respondents' perceptions of punishment risk to their forecasts of what they would do if, in the future, they found themselves in hypothetical situations where they had to decide whether to commit crime. Some vignette researchers ask subjects to estimate the certainty or severity of punishment for the crime, while others provide subjects with a randomly assigned hypothetical certainty or severity of punishment. The subject might then be asked to either provide a yes/no response as to whether they would commit the crime or estimate the probability that they would commit crime.

This procedure is highly problematic as a solution to the causal order problem, for reasons discussed at length in Chapter 4. One problem, however, is especially concerning. Because subjects are not asked about actual behaviors they have committed, it is easier for them to provide idealized responses consistent with their preferred self-image as rational persons rather than realistic forecasts of their likely behavior. Thus, when presented with scenarios involving high punishment risks, they may claim that it is unlikely they would do the crime, consistent with their self-image as a sensible person, even if they actually would commit the crime. Such erroneous responses would provide artificial support for the deterrence hypothesis. If this is so, one would expect to see negative and significant findings more often in studies using the vignette method. We tested this proposition by subdividing perceptual deterrence findings according to whether the researchers used vignette methods. The results are shown in Table 5.18, which is divided into three panels. Panel A summarizes all perceptual findings, Panel B summarizes only findings pertaining to perceived certainty of punishment, and Panel C summarizes findings pertaining to perceived severity.

The results strongly indicate that findings seemingly supportive of perceptual deterrence are much more common when the vignette method is used. Panel A shows that nearly half of the findings (49.6 percent) were significant and negative when the vignette method was used, compared to just 34.6 percent when it was not used. Panel B indicates that there is a modest difference in results with regard to findings on the effect of perceived certainty of punishment, again indicating that vignette studies were more likely to yield findings supporting perceptual deterrence. Panel C shows that there is a pronounced difference with regard to findings on the effect of perceived severity—findings supportive of deterrent effects of higher perceived severity were far more likely when vignette methods were used. Subjects in these studies who were told that

**TABLE 5.18** Perceptual deterrence findings by use of vignettes

| | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| *A. All Perceptual Findings, by Use of Vignettes* | | | | | | | | |
| Used Vignette | 127 | 49.6 | 31.5 | 3.9 | 14.2 | 0.8 | 0.0 | 0.0 |
| Did Not Use Vignette | 442 | 34.6 | 32.1 | 7.9 | 18.1 | 2.5 | 4.3 | 0.5 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |
| *B. Findings Regarding Certainty of Punishment by Use of Vignettes* | | | | | | | | |
| Used Vignette | 67 | 47.8 | 38.8 | 3.0 | 10.4 | 0.0 | 0.0 | 0.0 |
| Did Not Use Vignette | 323 | 39.0 | 32.2 | 9.6 | 11.5 | 1.9 | 5.6 | 0.3 |
| Total | 390 | 40.5 | 33.3 | 8.5 | 11.3 | 1.5 | 4.6 | 0.3 |
| *C. Findings Regarding Severity of Punishment by Use of Vignettes* | | | | | | | | |
| Used Vignette | 40 | 45.0 | 25.0 | 5.0 | 25.0 | 0.0 | 0.0 | 0.0 |
| Did Not Use Vignette | 87 | 18.4 | 31.0 | 4.6 | 41.4 | 2.3 | 1.1 | 1.1 |
| Total | 127 | 26.8 | 29.1 | 4.7 | 36.2 | 1.6 | 0.8 | 0.8 |

punishment for the hypothetical crime would be severe may have felt that it would make them seem irrational or foolish if they stated that it was likely they would commit the crime anyway, leading them to understate the likelihood that they would do so. To the extent this happened, it would artificially inflate support for the hypothesis that perception of more severe legal punishment reduces criminal behavior.

## *Do Perceptual Deterrence Findings Differ by Methods for Addressing Causal Order?*

In Chapter 4, we noted that much deterrence research is afflicted by a failure to properly take account of causal order. In the present chapter, we address this issue by asking whether research findings indicate that (1) perceptions of the risk of legal punishment affects criminal behavior, or that (2) committing crimes (usually without being punished for them) affects perceptions of legal risks. The latter effect is commonly referred to as the "experiential effect," i.e. the effect of criminal behavior on perceptions of legal risks. Empirical evidence bearing on this effect is reviewed later in this chapter. At this point, we deal with the matter as a purely methodological issue that impairs researchers' ability to estimate the effect of perceived punishment risks on criminal behavior.

Deterrence researchers who do a better job of dealing with causal order are careful to establish a clear time order between punishment perceptions and criminal behavior, measuring the former with respect to an earlier point in time and the latter with respect to a later period. To review (see Chapter 4 for details), there have been three common ways that perceptual deterrence researchers have addressed this challenge. Some basically ignore the problem, relating *current* risk perceptions

**TABLE 5.19** Perceptual deterrence findings by time order of risk perceptions and crime

| *Perceptions of Punishment Risk Were Related to:* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Past Crime | 164 | 37.8 | 23.2 | 7.9 | 20.7 | 5.7 | 6.1 | 0.6 |
| Forecasts of Future Crime | 231 | 42.0 | 30.3 | 7.8 | 17.3 | 1.3 | 0.9 | 0.4 |
| Actual Later Crime (Panel) | 174 | 32.8 | 42.5 | 5.2 | 13.8 | 1.7 | 4.0 | 0.0 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

to crime committed in the *past*—a clearly unsatisfactory procedure, which is sometimes justified by assuming that perceptions of legal risk are constant over time, allowing the researcher to treat current risk perceptions as adequate proxies for past risk perceptions. This assumption is not supported by most deterrence research (Bishop 1984; Paternoster et al. 1983b; Saltzman et al. 1982). Other scholars relate current perceptions of risk to forecasted or predicted criminal behavior with respect to some future point in time, often with respect to hypothetical vignettes or scenarios. This introduces a form of measurement error to the extent that forecasts are inaccurate measures of actual later behavior. Finally, those who have the resources to carry out panel studies are able to estimate the association between risk perceptions measured at an earlier time point to criminal behavior in a later period. This is probably the best, albeit imperfect, approach because (a) it clearly places the purported cause before the purported effect and (b) it measures actual criminal behavior rather than dubious forecasts of future crime committing. Table 5.19 displays the distributions of findings, classified in accordance with the time order that existed between measures of perceived risks and criminal behavior.

The pattern in Table 5.19 indicates that findings generated by research employing the most methodologically sound approach to the causal order problem were the least likely to support perceptual deterrence. Studies that inappropriately related current perceptions of punishment risk to past crime—that is, studies that failed to simulate the time order involved in the operation of a deterrent effect—obtained significant negative associations 38 percent of the time. Studies that related risk perceptions to hypothetical, forecasted crime were even more likely to produce seemingly supportive results—42 percent of findings were significant and negative. In contrast, only 33 percent of the findings generated by studies that used the methodologically strongest approach—relating earlier risk perceptions to later criminal behavior—were significant and negative.

These results confirm those of Raymond Paternoster (1987), whose systematic review of 27 early (1972–1986) perceptual deterrence studies found that "when researchers employing panel designs have estimated the deterrent relationship with variables in their correct temporal ordering and with more fully specified causal models, the moderate inverse effect for both perceived certainty and severity disappears" (173). Our result is likewise consistent with the more recent review by Pratt and his colleagues (2006) of 40 perceptual deterrence studies published in 1972–2003, who found that estimated deterrent effects of perceived certainty far weaker when researchers "control for the 'experiential effect'" (381).

Overall, the patterns suggest that the use of weaker methodological approaches has produced an exaggerated level of support for perceptual deterrence. This conclusion likewise accords well with Pratt's broad conclusion that "support for the deterrence perspective is most likely to be found in studies that are methodologically the weakest of the bunch" (Pratt et al. 2006, 384).

### *Perceptual Deterrence Findings of Panel Studies by Length of Follow-Up Period*

When a panel design is used to test the impact of perceived legal risks on criminal behavior, the amount of time that passes between (1) the time when perceptions are measured and (2) the later time period for which later criminal behavior is measured, labeled herein the "follow-up period," could affect the estimated deterrent effect of the perceptions. If it is perceived risks of punishment at the time of the possible offense that affect the decision to commit the crime, then measures of risk perceptions are useful only to the extent that they serve as accurate proxies for perceptions held at the time this decision is made. If, however, risk perceptions change, and the perceptions were measured at a time long before criminal decisions were made, they are likely to show a misleadingly weak association with criminal behavior, even if perceptual deterrence actually occurred. Thus, research using longer follow-up periods might understate the deterrent effect of perceptions of legal risk.

We roughly tested this proposition by subdividing findings by the length of the follow-up period, i.e. the period between the time perceptions of risk were measured and the end of the period for which criminal behavior was measured. The results are shown in Table 5.20.

Unfortunately there is not a great deal of variation in the length of this follow-up period, since 73 percent of panel studies' findings were based on a one-year follow-up, and only 13 percent were based on a longer follow-up. We could, however, compare somewhat more substantial numbers of findings based on follow-up periods under one year with those based on one-year follow-up periods. That comparison indicates that studies using shorter follow-up periods—and thus methodologically stronger in this regard—were less likely to support perceptual deterrence.

**TABLE 5.20** Perceptual deterrence findings by length of follow-up period

| *Length of Follow-up* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 0–6 Months | 15 | 13.3 | 46.7 | 0.0 | 33.3 | 6.7 | 0.0 | 0.0 |
| 6.1–11.9 Months | 15 | 20.0 | 33.3 | 0.0 | 6.7 | 6.7 | 33.3 | 0.0 |
| 1 Year | 122 | 29.5 | 47.5 | 6.6 | 13.9 | 0.8 | 1.6 | 0,0 |
| 1.1–2 Years | 4 | 75.0 | 0.0 | 0.0 | 25.0 | 0.0 | 0.0 | 0.0 |
| 2.1–3 Years | 3 | 100.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| Over 3 Years | 9 | 55.6 | 44.4 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| Unclear | 6 | 83.3 | 0.0 | 16.7 | 0.0 | 0.0 | 0.0 | 0.0 |
| Total | 174 | 32.8 | 42.5 | 5.2 | 13.8 | 1.7 | 4.0 | 0.0 |

### *Perceptual Deterrence Findings by Whether Researchers Used a Panel Design and Controlled for Informal Social Controls*

We have seen that findings differ when researchers use the more appropriate way of addressing causal order by employing panel designs in which past perceptions of legal risk are related to later criminal behavior. We have also seen that findings are less likely to support perceptual deterrence when researchers controlled for informal sanctions. We now consider what findings have been obtained when researchers employed both of these crucial methodological features. Table 5.21 displays the results.

**TABLE 5.21** Perceptual deterrence findings by panel design and whether informal sanctions were controlled

| *Panel Design?* | *Controlled Informal Sanctions?* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Yes | Yes | 51 | 37.3 | 33.3 | 9.8 | 19.6 | 0.0 | 0.0 | 0.0 |
| No | Yes | 167 | 24.6 | 36.5 | 10.8 | 21.0 | 4.2 | 2.4 | 0.6 |
| Yes | No | 123 | 30.9 | 46.3 | 3.3 | 11.4 | 2.4 | 5.7 | 0.0 |
| No | No | 228 | 51.8 | 20.6 | 5.7 | 17.1 | 0.9 | 3.5 | 0.4 |
| Total | | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

The most salient result revealed in Table 5.21 is that less than a tenth of perceptual deterrence findings were based on analyses that incorporated both of these fundamental features. Only 51 findings were based on research that, based on these criteria, could be considered methodologically adequate. It is clear that this field of research could use considerable improvement. The studies that were most likely to support the hypothesis of perceptual deterrence were the most technically primitive—those that neither controlled for informal sanctions nor used the better method for addressing causal order, panel designs. Most (52 percent) of these findings were significant and negative, supporting perceptual deterrence. At the opposite end of the scale, only 37 percent of findings based on research that implemented both crucial features were significant and negative. Again, the broad pattern is that only the most technically primitive of the perceptual deterrence studies generally support the deterrence doctrine.

### *Survey Mode and Perceptual Deterrence Findings*

All perceptual deterrence research relies on some version of survey research in the sense that variables are measured by asking questions of the research subjects. Although this is the only way to measure perceptions of the risk of legal punishment, there are different ways of asking the questions using different modes of communication. Subjects might fill out written questionnaires in a group setting such as a classroom, they might be asked questions by an interviewer in a face-to-face context, or they might be asked questions by an interviewer but in the more impersonal context of a telephone conversation. It is not known how these differences affect measurement of risk perceptions, but they do appear to affect how well researchers can get survey respondents to admit to criminal acts. A review of studies randomly assigning different survey modes indicated that written questionnaires, perhaps because they are more impersonal and anonymous, are the more effective way of eliciting self-reports of criminal acts, while both face-to-face and telephone interviews are far less effective (Kleck and Roberts 2012).

Underreporting of criminal acts in self-report surveys could bias estimates of the association between perceived legal risks and criminal behavior if the degree of underreporting were correlated with perceived risks. For example, people who perceive greater risks of punishment may be more fearful of admitting criminal acts to survey researchers, and they consequently underreport their criminal behavior to a greater degree than those who perceived less risk. That is, perceptions of punishment risk may affect the willingness to self-report criminal acts but not the actual frequency of criminal acts. This pattern of underreporting would contribute to an exaggerated negative association between perceived risk and self-reported criminal acts, thereby overstating support for the perceptual deterrence hypothesis. Thus, one might expect that the stronger this potential for errors

**TABLE 5.22** Perceptual deterrence findings by survey data collection method

| *Data Collection Measure* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Written Questionnaire | 311 | 40.8 | 28.6 | 6.4 | 18.3 | 2.3 | 3.5 | 0.0 |
| In-Person Interviews | 200 | 34.0 | 40.0 | 2.0 | 18.0 | 2.5 | 3.0 | 0.5 |
| Telephone Interviews | 52 | 34.6 | 25.0 | 30.8 | 9.6 | 0.0 | 0.0 | 0.0 |
| Interview and Questionnaire | 6 | 50.0 | 0.0 | 0.0 | 0.0 | 0.0 | 33.3 | 16.7 |
| Total | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

in measuring criminal behavior is, the more likely the researcher would obtain results artificially supporting the hypothesis due to this flaw. We tested whether the survey mode affected perceptual deterrence findings by classifying the findings according to the survey mode used (Table 5.22).

The pattern of findings indicates that use of written questionnaires to gather data was most likely to yield findings supportive of perceptual deterrence. Thus, our hypothesis was not supported. In this case, the methodologically superior method for measuring criminal behavior was the one most likely to indicate support. Nevertheless, even within the set of findings based on written questionnaires, only a minority was negative and significant.

## Part II: The Experiential Effect—The Impact of Criminal Behavior on Perceptions of Legal Risk

Perceptions of punishment risk may affect criminal behavior, but it is also possible that criminal behavior affects perceptions of punishment risk (Greenberg 1981; Paternoster 1987). Since most crimes are not followed by punishment, committing many crimes may lower the offender's perception of punishment risk because the person learns more about the low probability of punishment that actually prevails. This raises the problem of casual order, i.e. which variable is cause and which is effect. As has long been recognized, a negative effect of criminal behavior on perceived punishment risk could be misinterpreted by researchers as a deterrent effect of higher perception of legal risks on criminal behavior (Bishop 1984; Greenberg 1981; Minor and Harry 1982; Paternoster 1987; Paternoster et al. 1982; Paternoster, Saltzman, Waldo, and Chiricos 1982b; Paternoster et al. 1983a, 1983b; Saltzman et al. 1982). Consequently, the mere fact that the association between perceived punishment risks and criminal behavior is negative, in the absence of sound evidence regarding causal order does not necessarily point to deterrence. If criminal experience has a negative effect on perceived risks of punishment, a negative association between the two variables would be expected even in the absence of deterrence

Before researchers began to stress the importance of time order in testing the deterrence effect, it was common for researchers to test the hypothesis by examining the association between subjects' perceptions of legal risk prevailing at the time of the research ("current" perceptions) and *past* criminal behavior, e.g. during the year preceding the fielding of the survey. The procedure was justified on the basis of the assumption that perceptions of legal risk remain relatively stable over time so that current perceptions can be regarded as an adequate proxy for perceptions that prevailed during the time period for which criminal behavior was measured. This assumption eventually fell into disfavor in light of evidence from longitudinal research that individuals' perceptions of legal risk changed substantially over time (Bishop 1984; Minor and Harry 1982; Piliavin et al. 1986; Saltzman et al. 1982). Recognition of this problem led to the consensus view that deterrence researchers needed to address the causal order issue more satisfactorily if they hoped to effectively test the deterrence hypothesis.

Most analysts testing the experiential effect of criminal involvement on perceptions of punishment risk believe that those with little experience of criminal behavior have an exaggerated perception of the legal risks of crime, but that as some begin to commit crimes they gradually learn that there is actually a lower chance of being punished than they previously believed. Accordingly, the experiential hypothesis is that the more crime one has committed in the past, the lower one believes the risk of punishment to be (Greenberg 1981; Paternoster 1987; Paternoster et al. 1982a; Paternoster et al. 1982b; Paternoster et al. 1983a, 1983b).

Table 5.23 summarizes 542 findings that tested the experiential effect. We treated findings as bearing on the experiential effect when either (1) the authors explicitly stated that the experiential effect was being tested or (2) it was clear that criminal behavior was measured for a period *prior* to the time when risk perceptions were measured, regardless of how the researchers characterized the phenomenon that they believed they were studying. The only exception to this rule was that we did not include findings from multivariate analyses that treated past crime as the dependent variable and present perceptions as an independent predictor variable, since regression coefficients for the "present perceptions" variables would make no sense as estimates of the effect of prior crime on present perceptions. These findings were included in the perceptual deterrence tables, notwithstanding their causal order problems. We did, however, classify *bivariate* findings relating past crime to later risk perceptions as experiential findings, since bivariate associations are neutral as to causal direction, however primitive they may be as estimates of causal effects. We did this even when the researchers believed they were testing a perceptual deterrence hypothesis.

Nearly half of the tests of experiential effect (summarized in Table 5.23) support the view that experience with crime reduces the offender's perceptions of the risk of legal penalties, buttressing the claims of many scholars that much of the apparent support for the deterrence hypothesis in perceptual deterrence research is actually due to the operation of the experiential effect of criminal behavior on perceptions of legal risk. Table 5.23 also indicates that there has generally been more support for an experiential effect of criminal behavior on punishment perceptions than there has been for the deterrent effect of punishment perceptions on criminal behavior, since 45 percent of tests of the experiential effect yielded significant negative associations, while only 38 percent of the supposed tests of perceptual deterrence yielded significant negative associations.

One factor that limits the share of findings favorable to an experiential effect is the larger number of findings of unknown significance (– p = ? and + p = ?). Because a higher number of experiential findings were derived from correlation matrixes for which no significance levels were provided, 11.6 percent of experiential findings fall into these two "significance unknown" categories compared to just 3.7 percent of the perceptual deterrence findings. If findings of unknown

**TABLE 5.23** Experiential effect findings compared with perceptual deterrence findings

| *Study Type* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Experiential Effect | 542 | 45.0 | 28.0 | 1.3 | 10.7 | 3.3 | 9.4 | 2.2 |
| Perceptual Deterrence (From Table 5.1) | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |

significance are excluded, support for an experiential effect is even stronger; over 50 percent of these findings are then negative and significant.

### *What Dimensions of Punishment Perceptions Are Most Affected by Prior Criminal Behavior?*

If crime experience affects perceptions of punishment risk, which dimensions of punishment are most affected? The findings summarized in Table 5.24 indicate that prior experience in crime is more likely to be negatively and significantly associated with perceived certainty than with perceived severity. People who commit crime become more aware of the actual low certainty of being caught and thus are less likely to believe that they will be caught. While the findings are far short of unanimous, 50.5 percent of the tests of the hypothesis that criminal experience reduces perceived certainty of punishment yielded supportive significant negative findings, while another 26.6 percent were negative but not significant. In contrast, there is much less support for the proposition that criminal experience reduces perceived severity of punishment—only 22 percent of tests were significant and negative.

Swiftness of punishment is neglected in studies of experiential effects, just as it has been in studies of perceptual deterrence. Experiential studies have yielded just nine tests of the effect of prior criminal behavior on perceived swiftness of punishments. Four of these tests yielded a significant negative result and four others were negative but not significant, supportive of an experiential effect, but this is too limited an empirical foundation to draw any firm conclusions about this possible effect.

There have been a number of ways that perceived certainty has been measured in the experiential studies, and we wondered whether these differences might affect the findings. The pattern of findings reported in Table 5.25, however, suggests that the manner in which the certainty of punishment is measured has little impact on the experiential findings. Regardless of whether researchers asked about the chances of getting caught, getting arrested, or being prosecuted or convicted, the

**TABLE 5.24** Experiential findings by dimension of punishment perception

| *Dimension of Punishment* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Perception of Certainty | 428 | 50.5 | 26.6 | 1.6 | 6.3 | 2.8 | 10.3 | 1.9 |
| Perception of Severity | 105 | 22.9 | 32.4 | 0.0 | 28.6 | 5.7 | 6.7 | 3.8 |
| Perception of Swiftness | 9 | 44.4 | 44.4 | 0.0 | 11.1 | 0.0 | 0.0 | 0.0 |
| Total | 542 | 45.0 | 28.0 | 1.3 | 10.7 | 3.3 | 9.4 | 2.2 |

**TABLE 5.25** Experiential effect findings by type of certainty measure

| *Certainty Measure* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Caught? | 212 | 51.9 | 20.8 | 3.3 | 6.6 | 4.8 | 13.2 | 0.5 |
| Arrested? | 161 | 51.6 | 31.7 | 0.0 | 6.2 | 1.2 | 6.8 | 2.5 |
| Prosecuted/Convicted | 25 | 56.0 | 12.0 | 0.0 | 4.0 | 0.0 | 20.0 | 8.0 |
| Other | 30 | 30.0 | 53.3 | 0.0 | 6.7 | 6.7 | 0.0 | 3.3 |
| Total | 428 | 50.5 | 26.6 | 1.6 | 6.3 | 2.8 | 10.3 | 1.9 |

**TABLE 5.26** Experiential effect findings by measure of perceived severity

| *Severity Measure* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Problems in Life | 28 | 17.9 | 42.9 | 0.0 | 35.7 | 0.0 | 3.6 | 0.0 |
| Estimate of Legal Severity | 61 | 26.2 | 31.1 | 0.0 | 24.6 | 4.9 | 9.8 | 3.3 |
| Other Measures | 16 | 18.8 | 18.8 | 0.0 | 31.3 | 18.8 | 0.0 | 12.5 |
| Total | 105 | 22.9 | 32.4 | 0.0 | 28.6 | 5.7 | 6.7 | 3.8 |

results were similarly supportive of the experiential effect hypothesis. Doing crime (and usually avoiding punishment) seems to reduce perceived certainty of legal punishment.

Studies of the effect of criminal experience on perceived severity are summarized in Table 5.26, classified by how perceived severity was measured. While far less than a majority of findings support an experiential effect on perceived severity, regardless of how the latter was measured, researchers who asked respondents to estimate legal punishment severity levels were somewhat more likely to find support for the hypothesis than did those asking them to estimate how big a problem being punished would cause in their lives. Although the results are limited in number, this pattern hints that experience with crime may be more likely to decrease perceptions of the severity of legal sanctions than perceptions of the seriousness of informal social sanctions and other ancillary problems that might result from legal punishments.

### *Effects of Experience With Different Crime Types on Punishment Risk Perceptions*

Tests of the experiential effect have been conducted with respect to the effects of different kinds of past criminal behavior on perceptions of legal risk. Findings broken down by type of crime are displayed in Table 5.27. Support for an experiential effect has been similar across all four broad categories of types of crime that we distinguished, though support is somewhat weaker with regard to violent crime. Experience with violent crime is not as likely to decrease perceptions of punishment as experience with other types of crimes.

It would be more informative to discuss very specific types of crime, but there are a few specific crimes for which there are enough findings to permit us to draw even tentative conclusions. There were 30 or more tests of the experiential effect for six specific crimes: marijuana use, DUI,

**TABLE 5.27** Experiential results by general type of crime

| *Type of Crime* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Acquisitive | 188 | 47.3 | 26.6 | 0.5 | 12.8 | 3.2 | 7.4 | 2.1 |
| Violent | 90 | 36.7 | 44.4 | 0.0 | 12.2 | 1.1 | 5.8 | 0.0 |
| Nonpredatory | 196 | 48.0 | 21.9 | 1.5 | 9.7 | 5.1 | 10.2 | 3.6 |
| Mixed/All Types | 68 | 41.2 | 27.9 | 4.4 | 5.9 | 1.5 | 17.6 | 1.5 |
| Total | 542 | 45.0 | 28.0 | 1.3 | 10.7 | 3.3 | 9.4 | 2.2 |

**TABLE 5.28** Findings on experiential effects by specific type of crime

| *Specific Type of Crime* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Marijuana Use | 88 | 45.5 | 17.0 | 0.0 | 10.2 | 8.0 | 12.5 | 6.8 |
| Theft | 87 | 49.4 | 27.6 | 1.1 | 5.7 | 0.0 | 13.8 | 2.3 |
| Shoplifting | 40 | 37.5 | 17.5 | 0.0 | 30.0 | 7.5 | 2.5 | 5.0 |
| Domestic Violence | 40 | 25.0 | 45.0 | 0.0 | 27.5 | 2.5 | 0.0 | 0.0 |
| Driving Under the Influence | 35 | 34.3 | 22.9 | 2.9 | 22.9 | 2.9 | 11.4 | 2.9 |
| Vandalism | 30 | 60.0 | 26.7 | 0.0 | 0.0 | 0.0 | 13.3 | 0.0 |
| Multiple Crime | 69 | 42.0 | 27.5 | 4.3 | 5.8 | 1.5 | 17.4 | 1.5 |
| Other Crimes | 153 | 50.3 | 34.6 | 1.3 | 5.9 | 3.3 | 4.6 | 0.0 |
| Total, All Crime | 542 | 45.0 | 28.0 | 1.3 | 10.7 | 3.3 | 9.4 | 2.2 |

theft, shoplifting, domestic violence, and vandalism. Table 5.28 summarizes the relevant results and reveals that support for an experiential effect is somewhat higher in connection with marijuana use than with theft or shoplifting. Perhaps the number of prior criminal acts committed without being punished must reach some minimum threshold level before it reduces perceptions of legal risk. The number of individual instances of marijuana use or petty theft is likely to be greater than the number of instances of violence or drunk driving, so experience with unpunished marijuana use or theft is more likely to reach this hypothesized threshold level than experience with less frequently committed offenses.

As previously noted, many tests of an experiential effect were carried out inadvertently by researchers who intended to test the effect of perceived legal risks on criminal behavior using a one-time cross-sectional research design. Because they related *present* (at the time of the survey) perceptions of risk to *past* criminal behavior, however, they were actually testing (at best) the impact of criminal behavior on perceptions of risk (e.g., Carmichael et al. 2005; Claster 1967; Grasmick and Milligan 1976; Jacob 1980; Jensen 1969; Silberman 1976; Waldo and Chiricos 1972). In many "one-shot" cross-sectional studies, testing experiential effects was not an explicit goal of the research (e.g., Baron and Kennedy 1998; Grasmick and Green 1981; Jacob 1980; Paternoster 1988; Paternoster and Piquero 1995; Pestello 1984; Piliavan, Thornton, Gartner, and Matsueda 1986). In contrast, many studies using a panel design explicitly related criminal behavior at an earlier time to perceptions of legal risk measured at a later time (e.g., Miller and Iovanni 1994; Minor and Harry 1982; Paternoster et al. 1983a; Paternoster et al. 1985; Saltzman et al. 1982; Schneider and Ervin 1990).

One would expect the panel studies to yield stronger tests of the experiential effect because they do a better job of establishing causal order by relating past offending to current risk perceptions. In contrast, cross-sectional associations between current risk perceptions and previous criminal behavior are necessarily ambiguous. They may reflect either poor estimates of deterrent effects due to the use of wrongly ordered variables, experiential effects, or both. They should therefore yield less consistent evidence of an experiential effect.

The findings summarized in Table 5.29 indicate that the type of research design does indeed appear to influence the findings. The panel studies were much more likely to find significant negative associations between past crime and later perceptions of punishment risk than cross-sectional studies did—58 percent of findings from panel studies were supportive of an experiential effect

**TABLE 5.29** Experiential findings by general research design

| *Research Design* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Cross-Sectional | 319 | 36.1 | 29.8 | 0.6 | 16.6 | 5.3 | 7.8 | 3.8 |
| Panel | 223 | 57.8 | 25.6 | 2.2 | 2.2 | 0.4 | 11.7 | 0.0 |
| Total | 542 | 45.0 | 28.0 | 1.3 | 10.7 | 3.3 | 9.4 | 2.2 |

while just 36 percent of the ambiguous cross-sectional findings were significant and negative. Based on the results of the technically stronger studies, there appears to be a sound basis for believing that doing more crime causes offenders to perceive less risk of legal punishment from criminal conduct, at least with regard to perceived certainty of punishment. This casts serious doubt on the conclusions of researchers who tried to test perceptual deterrence using cross-sectional research designs and interpreted negative associations between punishment perceptions and criminal behavior as evidence of deterrence.

## *Experiential Effects by Age*

Adults have had a longer time to accumulate more experience with crime than juveniles, so it is reasonable to expect more of an experiential effect in samples of adults than in samples of juveniles, with mixed-age samples yielding an intermediate level of support. Consistent with these expectations, the findings summarized in Table 5.30 indicate that there is more evidence of an experiential effect in samples of adults than in samples of juveniles, with intermediate support observed in mixed samples of adults and juveniles.

Since the reality of legal punishment for crime is that it is very unlikely, then it follows that the more knowledgeable people are, the lower their perceived risks of punishment are likely to be. Certainly, the evidence on the experiential effect in general bears that out—people with more experience with crime are more knowledgeable about punishment risks by virtue of their own experiences with committing crimes. Along these same lines, one might expect that better educated people likewise perceive lower chances of punishment, and indeed prior research indicates that increases in education decrease perceptions of the risk of getting caught for crimes (Blackwell et al. 1994; Bridges and Stone 1986; Cohen 1978; Grasmick et al. 1993).

The findings summarized in Tables 5.31 and 5.32 provide some confirmation of this line of reasoning. Findings on the experiential effect are broken down by the educational level of the subjects

**TABLE 5.30** Experiential findings by predominant ages of sample members

| *Age of Sample* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Juveniles | 165 | 37.0 | 25.5 | 3.6 | 11.5 | 4.2 | 17.0 | 1.2 |
| Mixed Ages | 77 | 44.2 | 35.1 | 0.0 | 7.8 | 2.6 | 7.8 | 2.6 |
| Adults | 300 | 49.7 | 27.7 | 0.3 | 11.0 | 3.0 | 5.7 | 2.7 |
| Total | 542 | 45.0 | 28.0 | 1.3 | 10.7 | 3.3 | 9.4 | 2.2 |

**TABLE 5.31** Experiential findings by K–12 attendance

| *Attending K–12?* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Attending K–12 School | 137 | 40.1 | 25.5 | 4.4 | 5.8 | 2.2 | 20.4 | 1.5 |
| Mixture | 70 | 45.7 | 32.9 | 0.0 | 8.6 | 1.4 | 8.6 | 2.9 |
| Not Attending K–12 School | 335 | 46.9 | 28.1 | 0.3 | 13.1 | 4.2 | 5.1 | 2.4 |
| Total | 542 | 45.0 | 28.0 | 1.3 | 10.7 | 3.3 | 9.4 | 2.2 |

**TABLE 5.32** Experiential results by college attendance

| *Attending College?* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Attending College | 224 | 49.1 | 29.0 | 0.0 | 10.7 | 2.2 | 6.3 | 2.7 |
| Mixture | 109 | 44.0 | 32.1 | 0.0 | 9.2 | 2.8 | 8.3 | 3.7 |
| Not Attending College | 209 | 41.1 | 24.9 | 3.3 | 11.5 | 4.8 | 13.4 | 1.0 |
| Total | 542 | 45.0 | 28.0 | 1.3 | 10.7 | 3.3 | 9.4 | 2.2 |

studied. They indicate that samples of college students are more likely to yield support for the experiential effect than samples of students at the high school level or lower. In sum, the patterns of research findings support the hypotheses that education, as well as age and prior experience with crime, decreases perceived punishment risk.

### *Experiential Effects by Gender*

Some findings on the experiential effect were based on samples or subsamples composed exclusively of males or exclusively of females, allowing indirect tests of whether gender conditions the effect of criminal experience on perceptions of the risk of punishment. The pattern of findings shown in Table 5.33 do not support any such conditioning—samples of males are only slightly more likely than females to support a negative effect of criminal experience on perceived risk of punishment.

### *Methodological Artifacts in Findings on the Experiential Effect*

Variations in the findings on the experiential effect may be partly attributable to differences in the research methods used. In this section we explore whether support for an experiential effect is influenced by the data collection method used by researchers or by the adequacy of controls for potentially confounding variables.

Findings on the experiential effect are broken down by data collection method in Table 5.34. Self-report studies using interviewers are less effective in getting respondents to report criminal acts (Kleck and Roberts 2012), so we might expect the former to be less supportive of experiential effects because they were less effective in measuring criminal experience. The pattern of findings

**TABLE 5.33** Experiential effects by gender—based on single-gender samples or subsamples

| *Gender of Sample* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Sample of Males | 71 | 36.6 | 45.1 | 0.0 | 15.5 | 2.8 | 0.0 | 0.0 |
| Sample of Females | 25 | 32.0 | 32.0 | 0.0 | 32.0 | 4.0 | 0.0 | 0.0 |
| Total | 96 | 35.4 | 41.7 | 0.0 | 19.8 | 3.1 | 0.0 | 0.0 |

**TABLE 5.34** Experiential effect findings by data collection method

| *Data Collection Method* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Self-Administered Questionnaire | 308 | 33.8 | 28.9 | 1.9 | 14.0 | 4.5 | 14.3 | 2.6 |
| In-Person Interview | 133 | 59.4 | 28.6 | 0.8 | 8.3 | 3.0 | 0.0 | 0.0 |
| Telephone Interview | 41 | 53.7 | 24.4 | 0.0 | 7.3 | 0.0 | 9.8 | 4.9 |
| Interview and Questionnaire | 60 | 65.0 | 25.0 | 0.0 | 1.7 | 0.0 | 5.0 | 3.3 |
| Total | 542 | 45.0 | 28.0 | 1.3 | 10.7 | 3.3 | 9.4 | 2.2 |

**TABLE 5.35** Experiential findings by number of control variables

| *# of Independent Variables* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 0–5 | 386 | 50.5 | 20.5 | 1.3 | 8.8 | 4.1 | 12.2 | 2.6 |
| 6 or more | 156 | 31.4 | 46.8 | 1.3 | 15.4 | 1.3 | 2.6 | 1.3 |
| Total | 542 | 45.0 | 28.0 | 1.3 | 10.7 | 3.3 | 9.4 | 2.2 |

did not support this expectation—studies using interviewers were *more* likely than studies using self-administered questionnaires to obtain the significant negative associations between criminal experiences and perceived risks of punishment that support an experiential effect.

Many of the findings on the experiential effect were based on simple bivariate correlations between past criminal behavior and present-time perceptions of punishment risk, controlling for no potential confounding variables, while others controlled for only a handful. As one can see in Table 5.35, less than 29 percent of experiential findings were based on analyses with more than five control variables. More significantly, the pattern of findings displayed in this table indicates that the findings were less likely to be supportive of an experiential effect when more than a handful of other variables were included. This suggests that the significant negative crime/risk perception associations found in studies with few control variables may have been at least partially spurious, i.e. noncausal associations produced by antecedent variables that the researchers failed to control. If the variables controlled in these studies really were confounders, the studies controlling for more variables yielded better evidence on the experiential effect, which would suggest that support for an experiential effect may be weaker than the full set of findings seems to indicate.

## Chapter 5 Summary

We draw the following conclusions from the individual-level perceptual deterrence research reviewed in this chapter:

1. Most research findings on the association between perceived legal risk and criminal behavior do not show the statistically significant negative association that would support the hypothesis of perceptual deterrence (Table 5.1).
2. There was a substantial minority of findings (38 percent) that did appear to support this hypothesis, and many nonsignificant findings did at least show the expected negative association (Table 5.1). Although perceptual deterrence has not been supported in most tests, it remains possible that it operates for some people, under some conditions, in connection with some types of crime.
3. Perceptual deterrence is better supported with regard to acquisitive crimes and nonpredatory offenses than violent crimes (Table 5.7).
4. Perceptual deterrence is more likely to be supported with regard to drunk driving than other types of crime that are commonly tested (Table 5.8).
5. Findings were more likely to support perceptual deterrence with regard to the effect of perceived *certainty* of punishment than with regard to perceived *severity*, but, even among the findings on perceived certainty, most were not negative and significant.
6. Research overwhelmingly indicates that perceived *severity* of punishment does not reduce criminal behavior (Table 5.9).
7. Findings that appear to support an effect of the perceived certainty of punishment to some extent actually reflect the impact of the perceived likelihood of suffering *informal sanctions,* such as expressions of disapproval from significant others, rather than legal punishment itself. When the latter are controlled, findings are less likely to indicate a deterrent effect of perceived certainty of legal punishment (Table 5.15). This could be interpreted to mean either that perceptions of punishment risks have no effect of their own or that they sometimes have effects that operate indirectly by triggering fears of the informal sanctions that could follow from legal sanctions such as arrest or conviction.
8. Support for the hypothesis that perceived risk of legal punishment affects criminal behavior is no stronger in samples of the general public, composed largely of persons with little serious prior involvement in criminal behavior, than in samples of known offenders, such as inmate samples (Table 5.14).
9. Vignette methods appear to overstate the effect of perceived punishment risk—especially perceived severity of punishment—on crime, since they are more likely to support an effect than studies using panel methods that more effectively address the causal order issue (Tables 5.18, 5.19). Research subjects in vignette studies may be giving artificial, idealized responses to researchers that support a "rational" self-image rather than accurate, realistic forecasts about how they would actually behave in future.
10. Studies that use research designs that more effectively establish causal order, using panel methods, generally fail to find support for perceptual deterrence, while studies that either measured only hypothetical forecasts of crime or wrongly tried to relate *current* perceptions of punishment risk to *past* criminal behavior were more likely to appear to support perceptual deterrence (Table 5.19). Research that both controlled for informal sanctions and used panel designs was far less likely to support perceptual deterrence than research that lacked both of these crucial virtues (Table 5.21).

11. Many of the negative associations found between perceived risk of punishment (especially perceived certainty) and criminal behavior are likely to be partly or entirely a reflection of experiential effects of criminal behavior on perceptions of risk rather than of the deterrent effect of perceived risks on criminal behavior. The more crimes people commit, the more they learn how unlikely legal punishment is, causing them to adjust their risk perceptions downward (Tables 5.23, 5.24).

In sum, most empirical research does not support the idea that perceiving a higher risk of legal punishment reduces criminal behavior, and many of the minority of findings that do appear to support the hypothesis do so only because of serious methodological flaws in the research that generated them. Much of the estimated "effect" of perceived legal risks is actually due to the effect of informal social controls or reflects an experiential effect, i.e. the effect of criminal behavior on perceived risks rather than perceptual deterrence.

On the other hand, when evidence does support an effect of perceptions of legal risks, it is largely with regard to perceived certainty of punishment. While most findings on certainty are not significant and negative, there are some good quality studies that have yielded supportive results. For example, Matsueda, Kreager, and Huizinga (2006) used a panel design to address causal order and thereby separate deterrent effects from experiential effects, controlled for some variables that arguably index informal sanctions or social control, and still found a significant negative association between earlier perceptions of arrest certainty and later criminal behavior. They cautioned, however, that these effects were quite small—it would take about a 22 percent increase in perceived certainty of arrest to reduce stealing by just 3 percent and violent behavior by 5 percent. They doubted whether these small effects would have much relevance to policy, remarking that achieving such enormous increases in perceived risk would probably require "draconian steps by the criminal justice system" (117).

## References

Andenaes, Johannes. 1952. General prevention: Illusion or reality? *Journal of Criminal Law, Criminology, and Police Science* 43:176–198.

Andenaes, Johannes. 1975. General prevention revisited: Research policy and implications. *Journal of Criminal Law and Criminology and Police Science* 66:338–365.

Anderson, Linda, Theodore Chiricos, and Gordon Waldo. 1977. Formal and informal sanctions: A comparison of deterrent effects. *Social Problems* 25:103–114.

Bachman, Ronet, Raymond Paternoster, and Sally Ward. 1992. The rationality of sexual offending: Testing a deterrence/rational choice conception of sexual assault. *Law and Society Review* 26:343–372.

Bailey, William, and Ruth Lott. 1976. Crime, punishment and personality: An examination of the deterrence question. *Journal of Criminal Law and Criminology* 67:99–109.

Baron, Stephen. 2013. When formal sanctions encourage violent offending: How violent peers and violent codes undermine deterrence. *Justice Quarterly* 30:926–955.

Baron, Stephen, and Leslie Kennedy. 1998. Deterrence and homeless male street youths. *Canadian Journal of Criminology* 40:27–60.

Beck, Kenneth, James Fell, and Alice Yan. 2009. A comparison of drivers with high versus low perceived risk of being caught and arrested for driving under the influence of alcohol. *Traffic Injury Prevention* 10:312–319.

Bennett, Trevor, and Richard Wright. 1984. *Burglars on Burglary: Prevention and the Offender.* Aldershot: Gower Publishing.

Berk, Richard, Alec Cambell, Ruth Klap, and Bruce Western. 1992. The deterrent effect of arrest in incidents of domestic violence: A Bayesian analysis of four field experiments. *American Sociological Review* 57:698–708.

Bernburg, John, and Marvin Krohn. 2003. Labeling, life chances, and adult crime: The direct and indirect effects of official intervention in adolescence on crime in early adulthood. *Criminology* 41:1287–1317.

Bishop, Donna. 1984. Deterrence: A panel analysis. *Justice Quarterly* 1:311–128.

Blackwell, Brenda. 2000. Perceived sanction threats, gender, and crime: A test and elaboration of power-control theory. *Criminology* 38:439–488.

Blackwell, Brenda, Harold Grasmick, and John Cochran. 1994. Racial differences in perceived sanction threat: Static and dynamic hypotheses. *Journal of Research in Crime and Delinquency* 31:210–224.

Bouffard, Jeffrey. 2002a. Methodological and theoretical implications of using subject-generated consequences in tests of rational choice theory. *Justice Quarterly* 19:747–771.

Bouffard, Jeffrey. 2002b. The influence of emotion on rational decision making in sexual aggression. *Journal of Criminal Justice* 30:121–134.

Bouffard, Jeffrey, Jeff Bry, Sharmayne Smith, and Rhonda Bry. 2008. Beyond the "science of sophomores": Does the rational choice explanation of crime generalize from university students to an actual offender sample? *International Journal of Offender Therapy and Comparative Criminology* 52:698–721.

Bouffard, Jeffrey, and Melissa Petkovsek. 2013. Testing Hirschi's integration of social control and rational choice: Are bonds considered in offender decisions? *Journal of Crime and Justice* 37:285–308.

Braithwaite, John. 1989. *Crime, Shame, and Reintegration.* Cambridge: Cambridge University Press.

Braithwaite, John, and Toni Makkai. 1991. Testing an expected utility model of corporate deterrence. *Law and Society Review* 25:7–39.

Bridges, George, and James Stone. 1986. Effects of criminal punishment on perceived threat of punishment: Toward an understanding of specific deterrence. *Journal of Research in Crime and Delinquency* 23:207–239.

Carmichael, Stephanie, Lynn Langton, Gretchen Leuking, John Reitzel, and Alex Piquero. 2005. Do the experiential and deterrent effect operate differently across gender? *Journal of Criminal Justice* 33:267–276.

Carnes, Gregory, and Ted Englebrecht. 1995. An investigation of the effect of detection risk perceptions, penalty sanctions, and income visibility on tax compliance. *The Journal of American Taxation* 17:26–41.

Chambliss, William. 1967. Types of deviance and the effectiveness of legal sanctions. *Wisconsin Law Review* 3:703–719.

Claster, Daniel. 1967. Comparison of risk perception between delinquents and non-delinquents. *Journal of Criminal Law*, *Criminology and Police Science* 58:80–86.

Cohen, Larry. 1978. Sanction threats and violation behavior: An inquiry into perceptual variation. In *Quantitative Studies in Criminology*, ed. Charles Wellford. Beverly Hills: Sage.

Cooper, Harris. 2010. *Research Synthesis and Meta-Analysis.* Los Angeles: Sage.

Decker, Scott, Richard Wright, and Robert Logie. 1993. Perceptual deterrence among active residential burglars: A research note. *Criminology* 31:135–447.

Demers, David, and Richard Lundman. 1987. Perceptual deterrence research: Some additional evidence for designing studies. *Journal of Quantitative Criminology* 3:185–194.

Doob, Anthony, and Cheryl Webster. 2003. Sentence severity and crime: Accepting the null hypothesis. *Crime and Justice* 30:143–196.

Erickson, Patricia. 1976. Deterrence and deviance: The example of cannabis prohibition. *Journal of Criminal Law and Criminology* 67:222–232.

Feeney, Floyd. 1986. Robbers as decision-makers. In *The Reasoning Criminal*, eds. Derek B. Cornish and Ronald V. Clarke, 53–71. New York: Springer-Verlag.

Feld, Lars, and Claus Larsen. 2012. Self-perceptions, government policies and tax compliance in Germany. *International Tax and Public Finance* 19:78–103.

Finley, Nancy, and Harold Grasmick. 1985. Gender roles and social control. *Sociological Spectrum* 5:317–330.

Foglia, Wanda. 1997. Perceptual deterrence and the mediating effect of internalized norms among inner city teenagers. *Journal of Research in Crime and Delinquency* 34:414–442.

Franco, Annie, Neil Malhotra, and Gabor Simonovits. 2014. Publication bias in the social sciences: Unlocking the file drawer. *Science* 345:1502–1505.

Freeman, James, and Barry Watson. 2006. An application of Stafford and Warr's reconceptualization of deterrence to a group of recidivist drunk drivers. *Accident Analysis and Prevention* 38:462–471.

Gainey, Randy, Brian Payne, and Mike O'Toole. 2000. The relationships between time in jail, time on electronic monitoring and recidivism: An event history analysis of a jail-based program. *Justice Quarterly* 17:733–752.

Gertz, Marc, and Leroy Gould. 1995. Fear of punishment and the willingness to engage in criminal behavior: A research note. *Journal of Criminal Justice* 23:377–384.

Gopal, Ram, and Lawrence Sanders. 1997. Preventative and deterrent controls for software piracy. *Journal of Management Information Systems* 13:29–47.

Gottfredson, Michael, and Travis Hirschi. 1990. *A General Theory of Crime*. Stanford, CA: Stanford University Press.

Grasmick, Harold, and Lynn Appleton. 1977. Legal punishment and social stigma: A comparison of two deterrence models. *Social Science Quarterly* 58:15–28.

Grasmick, Harold, Brenda Blackwell, Robert Bursik, and Suzanne Mitchell. 1993. Changes in perceived threats of shame, embarrassment and legal sanctions for interpersonal violence 1982–1992. *Violence and Victims* 8:313–325.

Grasmick, Harold, and George Bryjak. 1980. The deterrent effect of perceived severity of punishment. *Social Forces* 59:471–491.

Grasmick, Harold, and Robert Bursik. 1990. Conscience, significant others and rational choice: Extending the rational choice model. *Law and Society Review* 24:837–861.

Grasmick, Harold, and Donald Green. 1980. Legal punishment, social disapproval and internalizations as inhibitors of illegal behavior. *Journal of Criminal Law and Criminology* 71:325–335.

Grasmick, Harold, and Donald Green. 1981. Deterrence and the morally committed. *Sociological Quarterly* 22:1–14.

Grasmick, Harold, and Herman Milligan. 1976. Deterrence theory approach to socioeconomic/demographic correlates of crime. *Social Science Quarterly* 57:608–617.

Green, Donald. 1989. Past behavior as a measure of actual future behavior: An unresolved issue in perceptual deterrence research. *The Journal of Criminal Law and Criminology* 80:781–804.

Greenberg, David. 1981. Methodological issues in survey research on the inhibition of crime: Comment on Grasmick and Green. *The Journal of Criminal Law and Criminology* 72:1094–1101.

Greenberg, David, and Nancy Larkin. 1998. The incapacitation of criminal opiate users. *Crime and Criminology* 44:205–228.

Gunter, Whitney. 2009. Internet scallywags: A comparative analysis of multiple forms and measurements of digital piracy. *Western Criminology Review* 10:15–28.

Higgins, George, Abby Wilson, and Brian Fell. 2005. An application of deterrence theory to software piracy. *Journal of Criminal Justice & Popular Culture* 12:166–184.

Hagan, John, and Bill McCarthy. 1997. *Mean Streets: Youth Crime and Homelessness*. Cambridge: Cambridge University Press.

Harbaugh, William, Naci Mocan, and Michael Visser. 2013. Theft and deterrence. *Journal of Labor Research* 34:389–407.

Hjarlmarsson, Randi. 2009. Crime and expected punishment: Changes in perceptions at the age of criminal majority. *American Law and Economics Review* 11:209–248.

Horney, Julie, and Ineke H. Marshall. 1992. Risk perceptions among serious offenders: The role of crime and punishment. *Criminology* 30:575–592.

Jacob, Herbert. 1980. Deterrent effects of formal and informal sanctions. *Law and Policy Quarterly* 2:61–80.

Jacobs, Bruce, and Alex Piquero. 2012. Boundary-crossing in perceptual deterrence: Investigating the linkages between sanction severity, sanction certainty, and offending. *International Journal of Offender Therapy and Comparative Criminology* 57:792–812.

Jensen, Gary. 1969. Crime doesn't pay: Correlates of a shared misunderstanding. *Social Problems* 17:189–201.

Jensen, Gary, Maynard Erickson, and Jack Gibbs. 1978. Perceived risk of punishment and self-reported delinquency. *Social Forces* 57:57–78.

Kartas, Anastasiou, and Sigi Goode. 2012. Use, perceived deterrence and the role of software piracy in video game console adoption. *Information Systems Frontiers* 14:261–277.

Kleck, Gary, and Kelly Roberts. 2012. What survey modes are most effective in eliciting self-reports of criminal or delinquent behavior? In *Handbook of Survey Methodology*, ed. Lior Gideon, 415–439. New York: Springer.

Kleck, Gary, Brion Sever, Spencer Li, and Marc Gertz. 2005. The missing link in general deterrence research. *Criminology* 43:623–660.

Klepper, Steven, and Daniel Nagin. 1989. The deterrent effect of perceived certainty and severity of punishment revisited. *Criminology* 27:721–746.

Levin, Aron, Mary Dato-on, and Chris Manolis. 2007. Deterring illegal downloading: The effects of threat appeals, past behavior, subjective norms, and attributions of harm. *Journal of Consumer Behavior* 6:111–122.

Loughran, Thomas, Raymond Paternoster, Alex Piquero, and Greg Pogarsky. 2011. On ambiguity in perceptions of risk: Implications for criminal decision making and deterrence. *Criminology* 49:129–161.

Loughran, Thomas, Raymond Paternoster, and Douglas Weiss. 2012. Hyperbolic time discounting, offender time preferences and deterrence. *Journal of Quantitative Criminology* 28:607–628.

Matsueda, Ross, Derek Kreager, and David Huizinga. 2006. Deterring delinquents: A rationale choice model of theft and violence. *American Sociological Review* 71:95–122.

McCarthy, Bill, and John Hagan. 2005. Danger and the decision to offend. *Social Forces* 83:1065–1096.

McDonnough, Emma, Richard Wortley, and Ross Homel. 2002. Perceptions of physical, psychological, social and legal deterrents to joyriding. *Crime Prevention and Community Safety: An International Journal* 4:11–25.

Meier, Robert, and Weldon Johnson. 1977. Deterrence as social control: The legal and extralegal production of conformity. *American Sociological Review* 42:292–304.

Miller, Susan, and Leeann Iovanni. 1994. Determinants of perceived risk of formal sanction for courtship violence. *Justice Quarterly* 11:281–311.

Miller, Susan, and Sally Simpson. 1991. Courtship violence and social control: Does gender matter? *Law and Society Review* 25:335–365.

Minor, William. 1977. A deterrence: Control theory of crime. In *Theory in Criminology*, ed. Robert Meier. Beverly Hills, CA: Sage.

Minor, William. 1978. Deterrence research: Problems of theory and method. In *Preventing Crime*, ed. James Cramer. Beverly Hills, CA: Sage.

Minor, William, and Joseph Harry. 1982. Deterrent and experiential effects in perceptual deterrence research: A replication and extension. *Journal of Research in Crime and Delinquency* 19:190–203.

Mocan, Naci, and Daniel Rees. 2005. Economic conditions, deterrence and juvenile crime: Evidence from micro data. *American Law and Economic Review* 7:319–349.

Morton, Neil, and Xenophon Kourteros. 2008. Intention to commit online music piracy and its antecedents: An empirical investigation. *Structural Equation Modeling: A Multidisciplinary Journal* 15:491–512.

Nagin, Daniel. 1998. Criminal deterrence research at the outset of the twenty-first century. *Crime and Justice* 23:1–42.

Nagin, Daniel, and Raymond Paternoster. 1994. Personal capital and social control: The deterrence implications of individual differences in criminal offending. *Criminology* 32:581–606.

Nagin, Daniel, and Greg Pogarsky. 2001. Integrating celerity, impulsivity, and extralegal sanction threats into a model of general deterrence: Theory and evidence. *Criminology* 39:865–889.

Paternoster, Raymond. 1986. The use of composite scales in perceptual deterrence research: A cautionary note. *Journal of Research in Crime and Delinquency* 23:128–168.

Paternoster, Raymond. 1987. The deterrent effect of the perceived certainty and severity of punishment: A review of the evidence and issues. *Justice Quarterly* 4:173–215.

Paternoster, Raymond. 1988. Examining three-wave deterrence models: A question of temporal order and specification. *The Journal of Criminal Law and Criminology* 79:135–179.

Paternoster, Raymond, Ronet Bachman, Robert Brame, and Lawrence Sherman. 1997. Do fair procedures matter? The effect of procedural justice on spouse assault. *Law and Society Review* 31:163–204.

Paternoster, Raymond, and Leeann Iovanni. 1986. The deterrent effect of perceived severity: A reexamination. *Social Forces* 64:751–777.

Paternoster, Raymond, and Alex Piquero. 1995. Reconceptualizing deterrence: An empirical test of personal and vicarious experiences. *Journal of Research in Crime and Delinquency* 32:251–286.

Paternoster, Raymond, Linda Saltzman, Theodore Chiricos, and Gordon Waldo. 1982a. Perceived risk and deterrence: Methodological artifacts in perceptual deterrence research. *Journal of Criminal Law and Criminology* 73:1238–1259.

Paternoster, Raymond, Linda Saltzman, Gordon Waldo and Theodore Chiricos. 1982b. Causal Ordering in Deterrence Research: An Examination of the Perceptions-Behavior Relationship. In *Deterrence Reconsidered: Methodological Innovations*, ed. John Hagan, 55–70. Beverly Hills, CA: Sage.

Paternoster, Raymond, Linda Saltzman, Gordon Waldo, and Theodore Chiricos. 1983a. Estimating perceptual stability and deterrent effects: The role of perceived legal punishment in the inhibition of criminal involvement. *Journal of Criminal Law and Criminology* 74:270–297.

Paternoster, Raymond, Linda Saltzman, Gordon Waldo, and Theodore Chiricos. 1983b. Perceived risk and social control: Do sanctions really deter? *Law and Society Review* 17:457–479.

Paternoster, Raymond, Linda Saltzman, Gordon Waldo, and Theodore Chiricos. 1985. Assessments of risk and behavioral experience: An exploratory study of change. *Criminology* 23:417–436.

Paternoster, Raymond, and Sally Simpson. 1996. Sanction threats and appeals to morality: Testing a rational choice model of corporate crime. *Law and Society Review* 30:549–583.

Patrick, Steven, and Robert Marsh. 2009. Recidivism among child sexual abusers: Initial results of a 13-year longitudinal random sample. *Journal of Child Sexual Abuse* 18:123–136.

Pestello, Frances. 1984. Deterrence: A reconceptualization. *Crime & Delinquency* 30:593–625.

Piliavin, Irving, Craig Thornton, Rosemary Gartner, and Ross Matsueda. 1986. Crime, deterrence, and rational choice. *American Sociological Review* 51:101–119.

Piquero, Alex, Raymond Paternoster, Greg Pogarsky, and Thomas Loughran. 2011. Elaborating the individual difference component in deterrence theory. *Annual Review of Law and Social Science* 7:335–360.

Piquero, Alex, and Greg Pogarsky. 2002. Beyond Stafford and Warr's reconceptualization of deterrence: Personal and vicarious experiences, impulsivity, and offending behavior. *Journal of Research in Crime and Delinquency* 39:153–186.

Piquero, Alex, and George Rengert. 1999. Studying deterrence with active residential burglars. *Justice Quarterly* 16:451–471.

Piquero, Alex, and Stephen Tibbetts. 1996. Specifying the direct and indirect effects of low self-control and situational factors in offenders' decision making: Toward a more complete model of rational offending. *Justice Quarterly* 13:481–510.

Pogarsky, Greg. 2002. Identifying deterrable offenders: Implications for research on deterrence. *Justice Quarterly* 19:431–452.

Pogarsky, Greg, and Alex Piquero. 2003. Can punishment encourage offending? Investigating the resetting effect. *Journal of Research in Crime and Delinquency* 40:95–120.

Pratt, Travis, Francis Cullen, Kristie Blevins, Leah Daigle, and Tamara Madensen. 2006. The empirical status of deterrence theory: a meta-analysis. In *Taking Stock: The Status of Criminological Theory*, eds. Francis T. Cullen, John Paul Wright, and Kristie R. Blevins, 367–395. New Brunswick, NJ: Transaction.

Rankin, Joseph, and Edward Wells. 1982. The social context of deterrence. *Sociology and Social Research* 67:18–39.

Richards, Pamela, and Charles Tittle. 1981. Gender and perceived chances of arrest. *Social Forces* 59:1182–1199.

Richards, Pamela, and Charles Tittle. 1982. Socioeconomic status and perceptions of personal arrest probabilities. *Criminology* 20:329–346.

Ross, Laurence, Richard McCleary, and Gary Lafree. 1990. Can mandatory jail laws deter drunk driving: The Arizona case. *Journal of Criminal Law and Criminology* 81:156–170.

Saltzman, Linda, Raymond Paternoster, Gordon Waldo, and Theodore Chiricos. 1982. Deterrent and experiential effects: The problem of causal order in perceptual deterrence research. *Journal of Research in Crime and Delinquency* 19:172–189.

Schneider, Anne, and Laurie Ervin. 1990. Specific deterrence, rational choice and decision heuristics: Applications in juvenile justice. *Social Science Quarterly* 71:585–601.

Sherman, Lawrence, Janell Schmidt, Dennis Rogan, Douglas Smith, Patrick Gartin, Ellen Cohn, Dean Collins, and Anthony Bachich. 1992. The variable effects of arrest on criminal careers: The Milwaukee domestic violence experiment. *Journal of Criminal Law and Criminology* 83:137–169.

Shover, Neal. 1996. *Great Pretenders: Pursuits and Careers of Persistent Thieves*. Boulder, CO: Westview Press.

Silberman, Matthew. 1976. Toward a theory of criminal deterrence. *American Sociological Review* 41:442–461.

Speckart, George, Douglas Angelia, and Elizabeth Deschenes. 1989. Modeling the longitudinal impact of legal sanctions on narcotics use and property crime. *Journal of Quantitative Criminology* 5:33–56.

Stafford, Mark, and Mark Warr. 1993. A reconceptualization of general and specific deterrence. *Journal of Research in Crime and Delinquency* 30:123–135.

Strange, Emily, and Zoe Peterson. 2013. The relationships among perceived peer acceptance of sexual aggression, punishment certainty and sexually aggressive behavior. *Journal of Interpersonal Violence* 28:3369–3385.

Teevan, James. 1976a. Deterrent effects of punishment: Subjective measures continued. *Canadian Journal of Criminology and Corrections* 18:152–160.

Teevan, James. 1976b. Subjective perceptions of deterrence (continued). *Journal of Research in Crime and Delinquency* 13:155–164.

Teevan, James. 1977. Deterrent effects of punishment for breaking and entering and theft. *Canadian Journal of Criminology and Corrections* 19:121–149.

Thistlethwaite, Amy, John Wooldredge, and David Gibbs. 1998. Severity of dispositions and domestic violence recidivism. *Crime and Delinquency* 44:388–398.

Thomas, Charles, and Donna Bishop. 1984. The effect of formal and informal sanctions on delinquency: A longitudinal comparison of labeling and deterrence theories. *The Journal of Criminal Law and Criminology* 75:1222–1245.

Thurman, Quint. 1989. General prevention of tax evasion: A factorial survey approach. *Journal of Quantitative Criminology* 5:127–146.

Tibbetts, Stephen. 1999. Differences between women and men regarding decisions to commit test cheating. *Research in Higher Education* 40:323–342.

Tibbetts, Stephen, and Denise Herz. 1996. Gender differences in factors of social control and rational choice. *Deviant Behavior* 17:183–208.

Tittle, Charles. 1977. Sanction fear and the maintenance of social order. *Social Forces* 55:579–596.

Tittle, Charles, Keaterina Botchkovar, and Olena Antonaccio. 2011. Criminal contemplation, national context and deterrence. *Journal of Quantitative Criminology* 27:225–249.

Tittle, Charles, and Charles Logan. 1973. Sanctions and deviance: Evidence and remaining questions. *Law and Society Review* 7:371–392.

Ventura, Lois, and Gabrielle Davis. 2005. Domestic violence: Court case conviction and recidivism. *Violence against Women* 11:255–277.

von Hirsch, Andrew, Anthony E. Bottoms, Elizabeth Burney, and P.-O. Wikstrom. 1999. *Criminal Deterrence and Sentence Severity: An Analysis of Recent Research.* Cambridge: Cambridge University Press.

Waldo, Gordon, and Theodore Chiricos. 1972. Perceived penal sanction and self-reported criminality: A neglected approach to deterrence research. *Social Problems* 19:522–540.

Watling, Christopher, and James Freeman. 2011. Exploring the theoretical underpinnings of driving whilst influenced by illicit substances. *Traffic Psychology and Behavior* 14:567–578.

Williams, Frank. 1985. Deterrence and social control: Rethinking the relationship. *Journal of Criminal Justice* 13:141–151.

Williams, Kirk. 1992. Social sources of marital violence and deterrence: Testing an integrated theory of assaults between partners. *Journal of Marriage and the Family* 54:620–629.

Williams, Kirk, and Richard Hawkins. 1986. Perceptual research on general deterrence: A critical review. *Law and Society Review* 20:545–572.

World Health Organization. 2015. *Global Status Report on Road Safety 2015*. Geneva, Switzerland: World Health Organization. Available online at www.who.int/violence_injury_prevention/road_safety_status/2015.

Wright, Bradley, Avshalom Caspi, Terrie Moffitt, and Ray Paternoster. 2004. Does the perceived risk of punishment deter criminally prone individuals? Rational choice, self-control, and crime. *Journal of Research in Crime and Delinquency* 41:180–213.

Wright, Richard T., and Scott H. Decker. 1994. *Burglars on the Job*. Lebanon, NH: Northeastern University Press.

Yao, Jie, Mark Johnson, and Kenneth Beck. 2014. Predicting DUI decisions in different legal environments: Investigating deterrence with a conjoint experiment. *Traffic Injury Prevention* 15:213–221.

Young, Randall, and Lixuan Zhang. 2007. Illegal computer hacking: An assessment of factors that encourage and deter the behavior. *Journal of Information Privacy and Security* 3:33–52.

Yu, Jiang, Peggy Evans, and Lucia Clark. 2006. Alcohol addiction and perceived sanction risks: Deterring drinking drivers. *Journal of Criminal Justice* 34:165–174.

Zhang, Lixuan, Wayne Smith, and William McDowell. 2009. Examining digital piracy: Self-control, punishment and self-efficacy. *Information Resources Management Journal* 22:24–44.

Zimring, Franklin, and Gordon Hawkins. 1973. *Deterrence: The Legal Threat in Crime Control.* Chicago: University of Chicago Press.

# 6

# INDIVIDUAL-LEVEL RESEARCH ON THE EFFECTS OF PUNISHMENT ON THOSE PUNISHED

This chapter reviews the evidence on the effect of the personal experience of legal punishment on criminal behavior of *the person punished*, as distinct from the deterrent effects of legal punishment on the population as a whole—the topic addressed in Chapter 5. When such effects reduce criminal behavior, they are often interpreted as "specific deterrent" effects on the punished persons, although there are other mechanisms by which punishment experience might reduce criminal behavior (Chapter 2). When punishment experience produces a net increase in offending, the effect is commonly interpreted as criminogenic, "deviance amplification," or a "labeling" effect. Both crime-decreasing and crime-increasing effects are possible with any one individual. In practice, nearly all of the studies conducted on this topic actually estimate the total *net* effect of punishment on the punished person, regardless of its direction or how it was produced. The ways in which punishment could increase punishment will be discussed in Chapter 11.

Part I of this chapter reviews research evaluating the overall effect of the personal experience of legal punishment on criminal behavior. Part II more narrowly examines research bearing on the effect of punishment experience on punished offenders' perceptions of legal risk, since this is the intervening variable that must link punishment experiences with subsequent criminal behavior if specific deterrence is actually produced.

## Part I: The Impact of Experienced Punishment on Criminal Behavior

We found 659 findings on the impact of respondents' punishment experience on their criminal behavior. Many studies that obtained findings supporting one of the two classes of possible effects were actually designed to estimate the other one. Thus, a study nominally designed (as indicated by its title and text) to estimate a specific deterrent effect might actually yield evidence of a crime-*increasing* effect that could be attributable to deviance amplification mechanisms such as impairing the offender's prospects for lawful employment. Conversely, a study intended to test a deviance amplification effect might produce results that support a crime-*reducing* effect, possibly due to specific deterrence. We included in our review all individual-level studies that measured the association between subjects' prior punishment experience and either their self-reported crime, self-predicted future crime, or officially measured recidivism, regardless of whether the study's authors intended to assess specific deterrence, deviance amplification, or some other hypothesized effect of the personal experience of legal punishment.

**TABLE 6.1** Overall findings on the effect of experienced punishment on crime

| *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|
| | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

Table 6.1 reports the distribution of 659 findings on the effect of experienced punishment on criminal behavior. The findings are nearly twice as likely to support a significant net crime-increasing effect (+ sig findings) than a significant net crime-reducing effect due to specific deterrence or any other effects of punishment (– sig findings). The results suggest that the experience of punishment often affects criminal behavior, in both good and bad directions, but the net effect is more likely to increase the punished offender's criminal conduct than to reduce it. Only 14.6 percent of the experienced punishment findings were significant and negative, indicating a net crime-reducing effect on those punished. Thus, the evidence provides even less support for specific deterrence than it did for perceptual deterrence (Chapter 5).

## *Findings by Dimension of Experienced Punishment*

As is true of the punishment of other people, one's own experience of punishment is characterized by particular levels of certainty, severity, and swiftness. Some offenders have experienced more certain or likely punishment than other offenders in the sense that a higher share of their prior offenses resulted in some kind of legal punishment. Likewise, the legal punishments that were imposed on some offenders were more severe than those imposed on others, so experienced severity varies across criminals. Finally, some offenders were punished a shorter time after their crimes than others, so experienced swiftness or celerity of punishment varies across offenders. Some studies test the impact of these dimensions of punishments experienced while others test the effect of specific punishment experiences or events such as arrests, convictions, or incarcerations.

Table 6.2a summarizes the findings regarding these different dimensions or types of experienced punishment. Among the minority of studies that do suggest a significant net crime-reducing effect of being punished, the findings indicate that experienced *certainty* of punishment is more effective in reducing subsequent crime than is experienced *severity* of punishment. There are, however, far fewer findings on the effect of experienced certainty of punishment than there were concerning perceived certainty of punishment in Chapter 5.

The summary of findings in Table 6.2a suggests that severity of experienced punishment does not generally reduce subsequent criminal behavior. Only 13 percent of findings indicated a significant negative severity-crime association, and there were twice as many significant positive findings, supporting a crime-increasing effect of more severe experienced punishment. This result confirms the conclusions of a previous review of the impact of more severe sentencing on crime (Doob and Webster 2003).

Swiftness of punishment is given slightly more attention in experienced punishment studies than it has been given in perceptual deterrence studies, but there are still very few relevant findings. Besides certainty, none of the specific types of experienced punishments show much evidence of a net specific deterrent or other crime-reducing effect of punishment experience on crime, as none of them are supported by more than a quarter of relevant findings in the form of a significant negative association with criminal behavior.

Table 6.2b shows the breakdown of findings specifically pertaining to the effects of experience with incarceration. While there is a large total volume of research on this topic, no single measure

**TABLE 6.2a** Findings on the effect of experienced punishment findings by dimension of punishment

| *Dimension of Experienced Punishment* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Experienced Certainty | 62 | 40.3 | 32.3 | 0.0 | 17.7 | 9.7 | 0.0 | 0.0 |
| Experienced Severity | 276 | 13.0 | 21.7 | 9.1 | 27.9 | 25.7 | 0.4 | 2.2 |
| Experienced Swiftness | 21 | 19.0 | 23.8 | 0.0 | 23.8 | 28.6 | 4.8 | 0.0 |
| Arrested or Caught | 132 | 8.3 | 20.5 | 1.5 | 23.5 | 43.9 | 0.8 | 1.5 |
| Conviction | 56 | 7.1 | 14.3 | 0.0 | 32.1 | 39.3 | 0.0 | 7.1 |
| Incarceration[a] | 38 | 10.5 | 26.3 | 5.3 | 31.6 | 21.1 | 0.0 | 5.3 |
| Probation | 31 | 22.6 | 25.8 | 0.0 | 45.2 | 6.5 | 0.0 | 0.0 |
| Multiple Punishments/Others | 43 | 11.6 | 23.3 | 0.0 | 32.6 | 30.2 | 2.3 | 0.0 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

*Note*:
[a]This category is limited to studies in which incarceration was measured as either "incarcerated in the past or not" or "number of past incarcerations." It does not encompass the incarceration measures that are discussed in Table 6.2b.

**TABLE 6.2b** Findings on the effect of experienced incarceration by measure of incarceration

| *Type of Experienced Incarceration Measure* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Imposed Sentence Length | 44 | 22.7 | 27.3 | 13.6 | 25.0 | 9.1 | 0.0 | 2.3 |
| Time Served | 64 | 9.4 | 28.1 | 7.8 | 18.8 | 32.8 | 0.0 | 3.1 |
| Prison vs. Probation | 35 | 0.0 | 5.7 | 5.7 | 37.1 | 51.4 | 0.0 | 0.0 |
| Prison vs. Nonprison Penalty | 52 | 25.0 | 11.5 | 15.4 | 13.5 | 30.8 | 1.9 | 1.9 |
| Jail vs. Non-Jail Penalty | 18 | 5.6 | 38.9 | 0.0 | 33.3 | 22.2 | 0.0 | 0.0 |
| Incarcerated or Not, Lifetime | 23 | 17.4 | 21.7 | 0.0 | 30.4 | 30.4 | 0.0 | 0.0 |
| # of Prior Incarcerations | 21 | 4.8 | 23.8 | 4.8 | 38.1 | 19.0 | 0.0 | 9.5 |
| Total | 257 | 13.6 | 21.4 | 8.6 | 24.9 | 28.8 | 1.6 | 1.2 |

of incarceration experience has been tested frequently. None of the studies employing a given measure of incarceration experience show much evidence of a significant crime-reducing effect on crime. Indeed, all but one of the measures are more likely to indicate a net crime-*increasing* effect than a net crime-reducing effect, with sentence length being the lone exception. Longer time *served* appears to be even less effective in reducing subsequent crime than longer *initially imposed* sentence lengths, indicating that mandatory minimum sentencing policies and other strategies aimed at increasing time spent in prison are more likely to increase crime among released inmates than they are to reduce crime.

Our results are similar to those of an extensive review of research on the effect of imprisonment on reoffending by Nagin, Cullen, and Jonson (2009). They did not report a tabulation of overall findings, but we have computed our own tabulations based on their tables. The distribution of findings is shown in Table 6.2c.

**TABLE 6.2c** Findings on Effects of Incarceration in Nagin et al. (2009) Review

| *# of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|
| | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 460 | 9.3 | 24.6 | 0.4 | 34.6 | 30.7 | 0.2 | 0.2 |

We differ from Nagin and his colleagues, however, in our interpretation of these results. Our own review found 2.1 times as many significant positive findings as significant negative findings, indicating far more support for a criminogenic effect of incarceration than a crime preventative effect. Nagin et al. found the same pattern, in even stronger form: 3.3 times as many significant positive findings as significant negative findings. Their verbal summary of their review, however, was that "compared with noncustodial sanctions, incarceration appears to have a null or mildly criminogenic effect on future criminal behavior" (115). While not exactly inaccurate, we regard the emphasis of their summary to be uninformative at best, misleading at worst. A more informative summary of the literature, regardless of which tabulation of findings one relies on, would be that there is far more research evidence supporting a crime-increasing effect of incarceration than there is supporting a crime-reducing effect. While most findings indicate no significant net effect of incarceration one way or another, when the evidence does point to a significant net effect, it is two or three times more likely to indicate a crime-increasing effect than a crime-decreasing effect.

## *Experienced Punishment Effects by Crime Type*

Recall that perceptual deterrence effects appear to differ substantially across different types of crime (Chapter 5). No such pattern is found among the findings of experienced punishment research. This is in part due to the widespread use of generic "any crime" measure of offending as outcome variables in the experienced punishment literature. With the exception of findings concerning DUI, speeding, white collar crime, domestic violence, or drug crimes, the dependent variable in most analyses of experienced punishment was simply recidivism, measured in various ways as the repetition of any crime rather than a specific type of crime. In particular, the categories of acquisitive and violent crime were not separately tested as often as nonpredatory crimes or mixed/all crime measures. Keeping that caveat in mind, the summary in Table 6.3 reveals no distinct pattern in the effects of experienced punishment by type of crime.

Research on the effects of experiencing legal punishment on crime grew substantially after the 1980s, with nearly 80 percent of the findings generated by studies published since 1990 (Table 6.4).

**TABLE 6.3** Experienced punishment findings by general type of crime tested

| *General Crime Type* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Acquisitive | 65 | 16.9 | 24.6 | 3.1 | 36.9 | 18.5 | 0.0 | 0.0 |
| Violent | 62 | 12.9 | 30.6 | 0.0 | 32.2 | 24.2 | 0.0 | 0.0 |
| Nonpredatory | 172 | 16.3 | 28.5 | 2.9 | 27.9 | 23.3 | 0.6 | 0.6 |
| Mixed/All Types | 360 | 13.6 | 17.8 | 6.1 | 25.0 | 33.1 | 0.8 | 3.6 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

**TABLE 6.4** Experienced punishment findings by decade of analyses

| *Decade of Publication* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 1960s & 1970s | 38 | 7.9 | 7.9 | 26.3 | 15.8 | 28.9 | 0.0 | 13.2 |
| 1980s | 77 | 19.5 | 24.7 | 3.9 | 18.2 | 29.9 | 2.6 | 1.3 |
| 1990s | 271 | 12.5 | 24.4 | 3.7 | 33.2 | 22.9 | 0.4 | 3.0 |
| 2000s | 273 | 16.1 | 22.0 | 2.2 | 26.4 | 33.0 | 0.4 | 0.0 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

**TABLE 6.5** Experienced punishment findings by academic discipline of publication outlet

| *Publication Field* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Criminology/CJ | 372 | 14.8 | 19.6 | 4.3 | 29.3 | 30.6 | 0.3 | 1.1 |
| Economics | 64 | 18.8 | 43.8 | 0.0 | 10.9 | 26.6 | 0.0 | 0.0 |
| Sociology | 40 | 20.0 | 10.0 | 0.0 | 32.5 | 37.5 | 0.0 | 0.0 |
| Working Paper | 62 | 11.3 | 6.5 | 0.0 | 29.0 | 37.1 | 0.0 | 16.1 |
| Other | 121 | 11.6 | 32.2 | 10.7 | 28.9 | 14.0 | 2.5 | 0.0 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

There was little support for a net crime-reducing effect in pre-1980 research on experienced punishment, increased support in 1980s, then a slight decline in support for such an effect in more recent work. Findings that support a net crime-increasing effect have outnumbered those supporting a net crime-reducing effect throughout the decades.

Most findings on the effect of experienced punishment on crime were published in criminology/criminal justice journals, though economic journals and working papers have also contributed significant shares of the findings (Table 6.5). Sociology journals have published fewer findings on experienced punishment (n = 40) than on perceptual deterrence (n = 117—Chapter 5). Sociologists may have faced greater hurdles in acquiring data on experienced punishment, since some scholars have noted difficulties among researchers who did not work in the field in gaining access to data controlled by criminal justice agencies (Sever and Reisner 2008; Sever, Reisner, and King 2001). This could be relevant to the findings of research on experienced punishment because researchers publishing in sociology journals have been more likely to obtain findings indicating significant effects, either crime-decreasing *or* crime-increasing, than scholars publishing in economics or criminology/criminal justice journals. The few individual-level studies of punishment experience published in economics journals are much more likely to report negative associations between experienced punishment and crime (63 percent) than work published in any other kind of outlets, though most of these findings were not statistically significant, and even economists' findings were more likely to indicate significant crime-increasing effects of punishment than crime-decreasing effects.

### *Findings on Experienced Punishment Effects by Age of Offender*

Many scholars have argued that deterrent effects of punishment may differ depending on the age of the prospective offender (Chapter 2). On the one hand, punishment could have a greater deterrent

**TABLE 6.6** Experienced punishment findings by predominant ages of sample members

| *Ages of Sample Members* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Juveniles | 104 | 9.6 | 14.4 | 1.0 | 28.8 | 43.3 | 1.9 | 1.0 |
| Adults | 352 | 15.1 | 21.9 | 6.8 | 25.9 | 26.4 | 0.3 | 3.7 |
| Mixed Ages | 203 | 16.3 | 27.6 | 2.0 | 30.0 | 23.6 | 0.5 | 0.0 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

effect on younger people because they are more malleable and able to change their ways. On the other hand, it may be harder to deter offending among young people if their offenses are committed more impulsively, with less forethought given to potential consequences (Gottfredson and Hirshi 1990; Kindlon, Mezzacappa, and Earls 1995; Vitacco and Rogers 2001; White, Moffit, Caspi, Bartusch, Needles, and Stouthamer-Loeber 1994). Further, juveniles are subject to less severe legal punishments (Bursik 1983; Williams and Hawkins 1986). Even when juveniles are sentenced to be institutionalized, they stay for shorter periods of time and in less harsh environments than adult prison inmates do. Adults may also have greater stakes in conformity than juveniles and, therefore, stand to lose more if they commit crimes and get caught, so a given legal punishment experience carries with it more ancillary costs (Williams and Hawkins 1986). We therefore classified findings with regard to the predominant ages of subjects in the study samples generating the findings (Table 6.6).

Across the 659 findings on experienced punishment effects, we found that those produced by samples of juveniles have been less likely to support a net crime-reducing effect than those produced by samples of adults. This finding contrasts with the research reviewed in Chapter 5 that showed *greater* support for perceptual deterrence among juveniles than among adults. The findings are also more likely to support a criminogenic effect of the experience of punishment among juveniles than among adults, consistent with the proposition that younger people are more changeable for the worse. In sum, punishment appears to be more likely to have, on net, detrimental effects on younger people than on adults. This confirms one of the key ideas that motivated the creation of a separate, less punitive, juvenile justice system—that youthful offenders needed to be shielded from the harmful effects of legal punishments.

Finally, there is some research on the effect of transferring juvenile offenders to adult court, where they are subject to more severe punishment. One rationale for the practice is that the more severe penalties it produces will have a special deterrent effect on the transferred juvenile offenders. A meta-analysis of nine studies found that "transfer may in fact increase offending" (Zane, Welsh, and Mears 2016).

## *Experienced Punishment Findings by Gender*

As we did with perceptual deterrence findings, we explored whether findings on the effects of experienced punishment might differ by gender. To do so, we examined only findings that were clearly based on samples or sub-samples composed of just a single gender. We found only 83 findings based on such samples, although many of the analyses for which we could not determine sample gender composition probably were based on samples composed entirely or primarily of males.

In both male-only and female-only samples, there were fewer findings supporting specific deterrence and other crime-reducing effects than there were findings indicating criminogenic effects.

**TABLE 6.7** Experienced punishment findings by gender of sample subjects

| *Gender of Sample Members* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Sample of Males | 56 | 10.7 | 19.6 | 14.3 | 12.5 | 39.3 | 0.0 | 3.6 |
| Sample of Females | 27 | 14.8 | 14.8 | 11.1 | 18.5 | 33.3 | 0.0 | 7.4 |
| Total | 83 | 12.0 | 18.1 | 13.3 | 14.5 | 37.3 | 0.0 | 4.8 |

This suggests that the experience of punishment, when it has any effect at all, tends to increase both male and female crime, though perhaps somewhat more for males. This conclusion must, however, be tempered by the fact that there were only 27 findings based on female-only samples.

### *Methodological Artifacts—Experienced Punishment Effects by Number of Control Variables*

As with other research topics, variation in research findings on the effects of experienced punishment may be partly attributable to differing flaws in the research methods used to generate the findings. For example, if punishment experience is correlated with other variables that affect criminal behavior, researchers who do a better job of controlling for those variables should obtain better estimates of the effects of experienced punishment. We therefore subdivided findings by the number of variables controlled in the analyses generating the finding. Table 6.8 indicates that there is somewhat more support for a net crime-reducing effect when more variables are controlled, though even within this subset of findings there are far more pointing to significant crime-increasing effects than crime-decreasing effects.

**TABLE 6.8** Experienced punishment findings by number of control variables

| *Number of Independent Variables* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 0–5 | 190 | 10.5 | 11.1 | 11.1 | 28.9 | 30.0 | 2.1 | 6.3 |
| 6 or More | 469 | 16.2 | 27.1 | 1.7 | 27.1 | 27.5 | 0.0 | 0.4 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

### *Experienced Punishment Findings by Controls for Informal Sanctions*

In Chapter 5, it was shown that support for perceptual deterrence was substantially lower in analyses that controlled for informal sanctions (27.5 percent significant negative) than in analyses not controlling for these sanctions (44.4 percent), leading to the suspicion that there might be a similar pattern among findings regarding experienced punishment. Table 6.9, however, indicates that there is little difference in the findings generated by analyses that do or do not control for informal sanctions. Both kinds of studies rarely find support for a net crime-reducing effect of punishment experience, and both are more likely to support a crime-increasing effect than a crime-reducing effect.

**TABLE 6.9** Experienced punishment findings by controls for informal sanctions

| *Controlled for Informal Sanctions?* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Yes | 68 | 11.8 | 29.4 | 0.0 | 30.9 | 26.5 | 0.0 | 1.5 |
| No | 591 | 14.9 | 21.7 | 4.9 | 27.2 | 28.4 | 0.7 | 2.2 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

This conclusion, however, is tentative, since so few experienced punishment findings were generated by analyses that controlled for informal sanctions. Table 6.9 therefore highlights an area in which research on experienced punishment could be improved. Changes in criminal behavior following the experience of legal punishment could be due to changes in the informal sanctions to which the punished persons are subject, rather than to the punishment itself. For example, if prosocial others cut their ties to legally punished persons, thereby weakening the informal sanctions for criminal behavior to which they are subject, this could increase criminal behavior, obscuring or cancelling out any crime-reducing effects of the punishment.

The omission of controls for informal sanction variables, or other potential confounders, in this area of deterrence research is not surprising. Most studies of experienced punishment have used existing or "found" criminal justice data sets that are largely confined to variables of interest to criminal justice administrators and less likely to include the kinds of variables that scholars might need to isolate the causal effects of punishment. Researchers using preexisting criminal justice datasets but hoping to improve controls for potential confounders in the future will need to supplement those datasets with additional information gathered from external sources such as surveys of the research subjects.

## *Experienced Punishment Findings by Study Location*

Legal punishments differ across jurisdictions and regions, so one might expect differences in the effects of those punishments across different areas. Nevertheless, findings on the effects of experienced punishment do not generally differ much across regions of the U.S. or between the U.S. and other countries, notwithstanding a few modest differences (Table 6.10). Findings based on data from

**TABLE 6.10** Experienced punishment findings by study location

| *Location* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Northeast U.S. | 90 | 5.6 | 17.8 | 2.2 | 40.0 | 32.2 | 0.0 | 2.2 |
| Midwest U.S. | 87 | 18.4 | 31.0 | 8.0 | 24.1 | 31.0 | 2.3 | 0.0 |
| South U.S. | 158 | 15.2 | 30.4 | 0.0 | 32.9 | 20.9 | 0.0 | 0.6 |
| West U.S. | 59 | 15.3 | 25.4 | 0.0 | 28.8 | 30.5 | 0.0 | 0.0 |
| National Sample U.S. | 51 | 19.6 | 17.6 | 9.8 | 21.6 | 27.5 | 0.0 | 3.9 |
| Unstated Location in U.S. | 58 | 15.5 | 22.4 | 0.0 | 29.3 | 24.1 | 1.7 | 6.9 |
| Multiple Regions of U.S. | 9 | 33.3 | 22.2 | 0.0 | 11.1 | 33.3 | 0.0 | 0.0 |
| Canada | 43 | 11.6 | 37.2 | 14.0 | 25.6 | 9.3 | 0.0 | 2.3 |
| Other Countries | 104 | 14.4 | 14.4 | 8.7 | 15.4 | 42.3 | 1.0 | 3.8 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

the Northeastern U.S. are less likely to indicate a deterrent and slightly more likely to indicate a criminogenic effect of punishment than those from other parts of the country, and findings from the Midwest are somewhat more likely to support a deterrent effect. Findings from countries outside the U.S. show the same general lack of support for specific deterrence as those obtained in the U.S.

### *Experienced Punishment Findings by Method for Measuring Offending*

There might also be different findings based on different ways of measuring offending. Critics of the self-report method might contend that it relies too heavily on the respondents' honesty in reporting their criminal acts. Respondents who have been legally punished in the past are likely to be the more serious offenders and may be correspondingly more likely to deceive researchers by understating the amount of crime they have committed in the past year. This could either artificially increase the frequency of findings indicating a crime-reducing effect or reduce the chances of obtaining findings that support labeling or other criminogenic effects by understating crime among those who had been punished.

Further, survey-based studies have generally done little to measure serious crimes, instead measuring mostly fairly trivial criminal behavior (Klepper and Nagin 1989). If minor crimes are more weakly motivated, and thus more easily deterred, this emphasis would contribute to an exaggerated impression of deterrent effects. Further, in survey studies using a panel design, there is a potential for "testing" effects (Campbell and Stanley 1966), whereby the respondent becomes aware, in earlier waves, of the rationale behind the study (testing the crime-reducing effect of punishment), and this influences their answers to the questions about offending asked in later waves. Thus, cooperative respondents who had been punished might understate their post-punishment criminal behavior so as to support the researchers' hypothesis. On the other hand, survey methods allow researchers to measure and control for many potentially confounding variables, giving them a better ability to isolate the effect of punishment compared to analyses that can only make use of existing criminal justice administrative data. Although we found little difference in findings based on sheer number of variables controlled, survey-based studies may be better at controlling specific, especially crucial, confounders.

Table 6.11 displays the distribution of findings classified by the type of data collection method used in the studies. Over two-thirds of the findings were generated by analyses using existing criminal justice datasets as the sole source of data. Analyses using existing data for both punishment and crime data provided the lowest support for a net crime-reducing effect and the second highest support for a net crime-increasing effect. Conversely, analyses based on in-person interview data were more likely to support a net deterrent effect and less likely to support a net criminogenic effect.

**TABLE 6.11** Experienced punishment findings by method of measuring offending

| *Measurement Method* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Questionnaire | 71 | 15.5 | 28.2 | 4.2 | 26.8 | 23.9 | 0.0 | 1.4 |
| In-person Interview | 103 | 21.4 | 27.2 | 3.9 | 27.2 | 19.4 | 0.0 | 1.0 |
| Telephone Interview | 17 | 17.6 | 23.5 | 0.0 | 17.6 | 41.2 | 0.0 | 0.0 |
| Existing Criminal Justice Data | 468 | 12.8 | 20.5 | 4.7 | 28.2 | 30.3 | 0.9 | 2.6 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

## *Experienced Punishment Findings by Type of Sample*

Studies of experienced punishment have been heavily reliant on nonprobability samples. In most studies there is no formal basis for generalizing the results concerning the effects of experienced punishment to anyone beyond the people who happened to be included as research subjects. As it happens, the findings based on non-probability samples are no more likely to support a net crime-reducing effect of punishment than those based on probability samples or complete enumerations (Table 6.12). Thus, there does not appear to be any systematic bias either in favor of, or opposed to, specific deterrent effects of punishment. Nevertheless, the field would yield more persuasive evidence if scholars in the future studied probability samples or complete enumerations of large population.

**TABLE 6.12** Experienced punishment findings by sample type

| *Sample Type* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Complete Enumeration | 81 | 14.8 | 24.7 | 12.3 | 13.6 | 25.9 | 1.2 | 7.4 |
| Probability Sample | 100 | 15.0 | 17.0 | 0.0 | 34.0 | 33.0 | 0.0 | 1.0 |
| Non-probability Sample | 470 | 14.3 | 23.0 | 4.0 | 28.7 | 27.9 | 0.6 | 1.5 |
| Unclear | 8 | 25.0 | 37.5 | 0.0 | 25.0 | 12.5 | 0.0 | 0.0 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

## *Experienced Punishment Findings and Length of Follow-Up Period*

The follow-up period in this field is the period for which criminal behavior is measured following an offender's experience of punishment. Scholars differ as to what the optimal follow-up period is for purposes of detecting effects of punishment. One school of thought is that most offenders who will recidivate will do so within the first year after release, making a long follow-up unnecessary (Beck and Shipley 1997; Carter, Glazer, and Wilkins 1984; Whiteacre 2008). Others note that it may take years for recidivism to occur among some offenders, which a short follow-up period would miss (Sampson and Laub 1993; Ventura and Davis 2005; Wheeler and Hissong 1998). In particular, the impact of labeling can take considerable time before it dampens one's legitimate opportunities and eventually leads to increased criminal behavior, particularly among adolescents (Bernburg and Krohn 2003; Sampson and Laub 1993; Ventura and Davis 2005). The diminished opportunity for education, employment, and other legitimate avenues to success that punishment can produce may take years to develop, making longer follow-up periods especially important for detecting deviance amplification effects of experienced punishment. If deterrent effects occur soon after punishment but most criminogenic effects show up later, a short follow-up period will bias results in favor of a net crime-reducing effect.

Some studies reported findings for multiple follow-up periods—as many as five. For those studies, we classified findings for the longest follow-up period, based on the reasoning that this would provide the most complete information on recidivism. Other studies used "time to reoffend" measures rather than recidivism rates as their outcome variables (e.g., DeYoung 1997; McGrath 2009; Spohn and Holleran 2002; Wheeler and Hissong 1988). Findings from these studies were not included in Table 6.13 because they do not have any specific follow-up period and cannot be compared with studies that do.

**TABLE 6.13** Experienced punishment findings by length of follow-up period

| *Length of Follow-Up Period* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 0–6 Months | 33 | 30.3 | 21.2 | 0.0 | 33.3 | 15.2 | 0.0 | 0.0 |
| 6.1–11.9 Months | 21 | 42.9 | 9.5 | 0.0 | 9.5 | 38.1 | 0.0 | 0.0 |
| 1 Year | 109 | 13.8 | 22.0 | 7.3 | 24.8 | 31.2 | 0.0 | 0.9 |
| 1.1–2 Years | 92 | 12.0 | 16.3 | 0.0 | 37.0 | 34.8 | 0.0 | 0.0 |
| 2.1–3 Years | 126 | 14.3 | 22.2 | 1.6 | 30.2 | 30.2 | 0.0 | 1.6 |
| 3.1–5 Years | 93 | 5.4 | 29.0 | 8.6 | 23.7 | 25.8 | 1.1 | 6.5 |
| Over 5 Years | 59 | 11.9 | 22.0 | 8.5 | 20.3 | 28.8 | 1.7 | 6.8 |
| Unclear | 41 | 29.3 | 22.0 | 0.0 | 19.5 | 24.4 | 2.4 | 2.4 |
| Total | 574 | 15.2 | 21.8 | 4.0 | 26.8 | 29.3 | 0.5 | 2.4 |

The pattern of findings indicates that analyses using the shortest follow up period are, as expected, most likely to find a net crime-reducing effect of punishment. Findings supporting a net crime-reducing effect are largely confined to studies with a follow-up period of one year or less. The results are consistent with the proposition that criminogenic effects of punishment take longer to manifest themselves than specific deterrent effects, as well as Ross's (1984) hypothesis that penalties deter offenders soon after punishment but then the deterrent effect weakens. Conversely, findings supportive of a net crime-increasing effect are fairly common with all follow-up periods of six months or longer.

Among the findings for which the follow-up period was known, two-thirds used a fairly short one, of three years or less. If crime-increasing effects of legal punishment largely become evident only after a longer time has passed, the widespread use of short follow-up periods biases findings in favor of finding a net crime-reducing effect of punishment. This proposition should be tested in the future by researchers using multiple follow-up periods of different lengths, including some long ones.

### *Experienced Punishment Findings by Type of Recidivism Measure*

A longstanding criticism of recidivism analysis in deterrence research has been its reliance on official data (Greenberg and Larkin 1998; Klemke 1978). Many researchers rely only on reoffending that has come to the attention of the criminal justice system, so much of reoffending goes undetected. The understating of recidivism is especially severe if the recidivism measures reflect only later stages of the criminal justice process, such as reconviction or reincarceration rather than re-arrest. As a result, offenders who commit crime but elude capture or processing are wrongly classified as punishment successes (Klemke 1978). If prisons serve as "schools for crime" that refine criminals' skills in avoiding arrest, incarceration might appear to reduce recidivism even if it does not, but merely reduces the offender's likelihood of being caught for the crimes he continues committing. Consequently, research using official data on criminal behavior to measure recidivism may be more likely to support a crime-reducing effect than research using self-report measures. Therefore, we cross-tabulated findings to determine if they are related to the recidivism measure used. Since the concept of recidivism implies measurement of criminal behavior during a period after a punishment experience, this analysis was limited to longitudinal studies.

**TABLE 6.14** Experienced punishment findings by measure of recidivism (longitudinal studies only)

| *Recidivism Measure* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Re-Arrest | 263 | 16.0 | 22.4 | 3.8 | 29.3 | 25.5 | 1.1 | 1.9 |
| Recharged | 15 | 0.0 | 6.7 | 6.7 | 53.3 | 33.3 | 0.0 | 0.0 |
| Reconviction | 99 | 8.1 | 19.2 | 3.0 | 23.2 | 41.4 | 1.0 | 4.0 |
| Probation/Parole Outcome | 43 | 4.7 | 25.6 | 9.3 | 27.9 | 32.6 | 0.0 | 0.0 |
| Reincarceration | 32 | 21.9 | 9.4 | 12.5 | 18.8 | 28.1 | 0.0 | 9.4 |
| Court Referrals | 7 | 28.6 | 28.6 | 0.0 | 28.6 | 14.3 | 0.0 | 0.0 |
| Self-Report of Crime | 95 | 17.9 | 23.2 | 5.3 | 26.3 | 26.3 | 0.0 | 1.1 |
| Unclear | 5 | 0.0 | 20.0 | 0.0 | 60.0 | 20.0 | 0.0 | 0.0 |
| Total | 559 | 14.0 | 21.1 | 4.8 | 27.9 | 29.2 | 0.7 | 2.3 |

Table 6.14 indicates that self-report methods to measure criminal behavior are not popular among students of specific deterrence, and that those who use official measures of recidivism use a wide variety of them. Re-arrest is by far the most common measure of official recidivism in this literature. Consistent with expectations, findings based on self-report measures of recidivism are less likely to support a net crime-reducing effect than those based on some kinds of official measures, like reincarceration, but (contrary to expectations) more likely to indicate a net crime-reducing effect than some other official measures, especially compared with recidivism measured as reconviction.

### *Experienced Punishment Findings by Dichotomous vs. Incidence Measures of Recidivism*

Some recidivism studies use dichotomous recidivism measures (reoffended or not), while others measure the frequency or incidence of recidivism (number of post-punishment offenses). Proponents of the use of dichotomous measures contend that abstinence from crime is the true goal of criminal justice punishment, so the measure of recidivism should contrast those who had not reoffended at all with those who had done so. Those preferring to measure the incidence of recidivism assert that the dichotomous measure misses partial successes, in which crime is reduced but not eliminated. These two types of variables could also be seen as measuring the two types of deterrence that Gibbs (1975) termed *absolute* and *restrictive* deterrence, the dichotomous measures being suited to estimating the former kind of deterrence and the incidence measures being suited to detecting the latter kind.

Table 6.15 indicates that there is little difference in the findings of experienced punishment studies with regard to these two types of recidivism measures, suggesting that punishment may

**TABLE 6.15** Experienced punishment findings by Binary vs. Incidence Measure of Recidivism

| *Measure of Recidivism* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Binary | 293 | 16.0 | 20.5 | 6.1 | 27.3 | 29.4 | 0.0 | 0.7 |
| Incidence | 366 | 13.4 | 24.0 | 3.0 | 27.9 | 27.3 | 1.1 | 3.3 |
| Total | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

have the same effects on frequency of reoffending as it does on whether the punished individual reoffends at all.

## Part II: The Impact of Experienced Punishment on Perceptions of Future Punishment Risk

If the personal experience of being punished deters later criminal behavior, this must occur as a result of the punished person's perception of future risk of punishment being increased. This is true by definition—if the experience of punishment had any effect through other mechanisms, such as rehabilitation or moral reformation, the effect could not be defined as a deterrent effect. Conversely, if the experience of punishment does *not* cause an increase in perceived risk of future legal punishment, it means either that there was no crime-reducing effect of the experience of punishment at all or, if there was, it was not the product of deterrence.

Some might regard it as self-evident that criminals who experience punishment increase their perception of the risk of punishment in the future. In particular, it is commonly hypothesized that perceptions of the *certainty* of punishment are especially likely to be increased. Thus, offenders who are arrested will presumably increase their perceptions of the probability that they would be arrested if they committed crimes in the future. Likewise, persons who were convicted would increase their perceived certainty of conviction, and those sentenced to prison would subsequently have higher perceived levels of the likelihood of imprisonment. Although severity of punishment is rarely discussed in this context, experience of more severe punishment might cause offenders to anticipate more severe punishment if they committed further crimes in the future.

Deterrence scholars call this sort of response to the experience of punishment the "updating" of risk perceptions (e.g. Anwar and Loughran 2011). Based on Bayesian learning theory, prior subjective beliefs about punishment risks are hypothesized to be revised in accordance with new information learned from the experience of punishment (Piquero, Paternoster, Pogarsky, and Loughran 2011, 355).

It is crucial to distinguish two very different types of updating of risk perceptions. First, a person might alter their perception of punishment risk because they committed a crime and experienced some kind of legal punishment for it. The conventional expectation is that such a punishment experience will cause an *upward* updating of perceived risk from its previous level (e.g., Apel 2013; Nagin 2013). There is, however, considerable evidence that some offenders respond to arrest in a seemingly perverse or irrational way, concluding that they have "used up their bad luck," and therefore are unlikely to be caught after committing crimes in the future. This has been variously referred to as the "gambler's fallacy" or the "resetting effect" (e.g., Pogarsky and Piquero 2003).

Second, offenders can also update their risk perceptions in response to committing crimes and *not* being punished for them, i.e. the experience of punishment avoidance. This is what deterrence scholars have long referred to as the "experiential effect" (Greenberg 1981; Paternoster 1987). The distinction between these two fundamental types of updating is crucial because it turns out that there is ample evidence of *downward* updating in response to punishment avoidance (Chapter 5) while there is very little evidence of *upward* updating in response to punishment experiences. Further, the handful of studies supporting upward updating are based on an extremely narrow foundation, all of them sharing common methodological problems that are likely to generate misleading positive punishment/perception associations.

Unfortunately, previous reviewers of the updating literature do not always stress this distinction, drawing favorable conclusions about updating *in general* that, in effect, "coast" on the numerous studies supporting downward updating in response to punishment avoidance, while failing to note how limited the support for upward updating is (e.g., Apel 2013; Nagin 2013). For example, Pogarsky, Roche, and Pickett (2017, 88) flatly state, regarding past research, that "the results of these studies reveal that sanction risk perceptions *are* responsive to whether an actor has been punished for past offending experiences." This is only half true, since most studies do *not* find higher risk perceptions among those who have been punished for past offenses (Table 6.16). As we explain later, offenders appear to only be responsive to the *avoidance* of punishment.

Likewise, Lochner (2007) summarized his evidence like this: "all specifications show strong evidence of belief updating in response to the respondent's own criminal history" (2007, 455), i.e. the more crime people do, the more they update their perceptions of risk *downward*. As to whether punishment causes any upward updating, Lochner only stated that "those arrested for a theft increased their perceived probability of arrest (significantly so in most specifications)" (455), failing to mention that the association was *not* significant in the most sophisticated specification (see his Table 5, column [C] [1], 454). A more meaningful summary would have been that "based on the technically strongest test of the hypothesis, arrest does not significantly increase perceived risk."

The evidence bearing on the effect of prior offending on risk perceptions was already reviewed in Chapter 5. It indicated that committing more offenses (and usually avoiding punishment) causes perceived sanction risks to decrease, i.e., to produce downward updating. Here we review only evidence that directly tests the effect of punishment experiences on the punished person's perceptions of the risk of legal punishment. We located 171 relevant empirical findings on the association between punishment experiences and perceptions of punishment risk. If specific deterrence theory or the orthodox upward updating hypothesis are correct, the experience of punishment in the past should have a positive effect on current perceived risks, i.e. cause an upward updating. Conversely, a negative association would suggest that the experience of punishment reduces perceived risk of legal punishment, possibly due to the operation of the "gambler's fallacy" or "resetting."

Table 6.16 shows the distribution of all findings on the impact of experienced punishment on perceptions of punishment. The table indicates that only 26.3 percent of findings were significant and positive, indicating that experience with punishment increases perceived punishment risk, while 9.9 percent were negative and significant, supporting a gambler's fallacy response to punishment. Just over half of the findings indicated positive associations, and less than half of these were statistically significant. The full body of evidence, then, indicates that being punished generally does *not* cause any significant upward updating of the perceived punishment risk. This casts doubt on whether the minority of the findings summarized in Table 6.1 that seemed to support a crime-reducing effect of the experience of punishment could be interpreted as evidence of specific deterrence, since such an effect, by definition, operates by elevating the offender's perception of the risks of legal punishment.

The vast majority of tests (74 percent) have not supported the upward updating hypothesis, finding either no significant association between prior punishment experience and later risk perceptions or a significant *negative* association (e.g., Apospori, Alpert, and Paternoster 1992; Bridges and Stone 1986; Jensen 1969; Kleck, Sever, Li, and Gertz 2005; Pogarsky and Piquero 2003; Richards

**TABLE 6.16** Overall findings on the effect of experienced punishment on perceptions of risk

| *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|
| | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 171 | 9.9 | 25.7 | 2.9 | 29.2 | 26.3 | 3.5 | 2.3 |

and Tittle 1981; Schneider and Ervin 1990; Thomas and Bishop 1984; Watling, Palk, Freeman, and Davey 2010). Therefore, Nagin (2013) was wrong when, in the context of his claim that arrest causes an upward updating of the perceived risk of arrest, he claimed that "the evidence is overwhelmingly consistent with the Bayesian updating model" (250). As it pertains to the prediction that punishment experiences generally cause an increase in perceived risk, the evidence is overwhelmingly *contrary* to the Bayesian updating model as it is usually interpreted by deterrence scholars.

Nagin was able to sustain his extraordinary claim only because he cited a very small, unrepresentative sample of the relevant research. There have been only a handful of studies that generally support the thesis of upward updating following punishment, the majority of them cited by Nagin (2013)—those done by Pogarsky, Piquero, and Paternoster (2004), Matsueda, Kreager, and Huizinga (2006), and Anwar and Loughran (2011), along with another one by Horney and Marshall (1992) that yielded only partially supportive findings. (The dataset used by Anwar and Loughran was also reanalyzed by Thomas, Loughran, and Piquero 2013 with similar results.) There are, however, strong reasons to question the validity of the handful of deviant findings cited by Nagin.

Addressing the Horney and Marshall (1992) study first, its authors put a very pro-updating spin on results that were mixed at best. They measured prior punishment experiences in their prison inmate sample in three ways: (1) crime-specific arrests (whether the inmate had been arrested for the specific offense being analyzed), (2) total lifetime number of arrests, and (3) crime-specific arrest ratios (self-reported arrests divided by self-reported number of offenses). These measures differ with regard to how much they are likely to be distorted by survey response error. There is little reason for inmates to understate their arrests since the authorities already know about these, but they have powerful reasons to underreport offenses that the authorities did not know they had committed, as well as the ability to do so without the researchers discovering that they had concealed the crimes. Experienced arrest ratios are conceptually attractive because of the way they attempt to capture experienced certainty of punishment, but their empirical validity is wholly dependent on the willingness of incarcerated felons and other criminals to confess to crimes that the authorities did not already know about. As noted in Chapter 4, there is ample evidence that survey respondents grossly underreport their offenses in self-report surveys. Thus, the tests of updating based on experienced arrest ratios are probably the least reliable of the three measures of punishment risk used by Horney and Marshall.

Horney and Marshall found (1) no significant positive associations between perceived arrest risks and offense-specific arrests, and (2) no significant positive associations between perceived arrest risks and lifetime arrests. Their only support for upward updating were (3) the findings based on the dubious arrest ratio measures (1992, 575, 589). A conclusion more closely attuned to their most reliable findings, and reflecting the full set of relevant findings, would be that the research as a whole failed to support upward updating of risk perceptions following arrest.

In their study of Denver youth residing in high crime neighborhoods, Matsueda and his colleagues (2006) relied entirely on an experienced arrest ratio variable as their sole measure of punishment experience, obtaining the same experienced arrest ratio results as Horney and Marshall (1992) did. Anwar and Loughran (2011) likewise relied on the experienced arrest ratio as their sole measure of punishment experience in their study of adolescent offenders in two urban counties. Since Horney and Marshall obtained very different results when self-reported experienced arrest ratios were used than when they used measures of experienced punishment less vulnerable to offender response error, this suggests that Matsueda et al. and Anwar and Loughran would likewise have obtained results that were similarly unsupportive of upward updating had they used measures of punishment experience that were less dependent on the honesty of criminals in reporting crimes unknown to the authorities.

The experienced arrest ratio also has a serious problem having to do with its ratio character. It is measured as a ratio of self-reported arrests (A) over self-reported crimes (C). Horney and Marshall (1992), Anwar and Loughran (2011), and Matsueda et al. (2006) all based their support for the upward updating thesis on the positive association between A/C and the perceived probability of arrest (PPA). It is widely accepted that committing more crimes (and avoiding punishment for most of them) causes a reduction in PPA (Chapter 5); that is, more C will cause a lower PPA via the experiential effect. However, a larger number of prior offences (C) also will necessarily produce a lower A/C simply because C is the denominator of this ratio. Since C has a negative effect on both A/C and PPA, this creates a spurious positive association between A/C and PPA, and thus a false impression of support for the upward updating hypothesis. Horney and Marshall (1992) handled this problem by controlling for self-reported prior offending, but Matsueda et al. (2006) and Anwar and Loughran (2011) apparently did not, suggesting that the apparently supportive results of the latter studies were at least partly an artifact of their use of the ratio variable.

Finally, all three of the more clearly supportive studies (Pogarsky et al. 2004; Matsueda et al. 2006; Anwar and Loughran 2011) were based exclusively on samples of juveniles. No studies have generated consistent evidence of upward updating occurring among adults (recall that Horney and Marshall's adult findings were mostly unsupportive), and adults commit the vast majority of crimes and claim the vast majority of persons punished. Anwar and Loughran (2011) found that updating occurs more among less experienced offenders than among those who are more experienced. Assuming that juvenile offenders are generally less experienced in crime than adult offenders, this suggests that upward updating, if it exists at all, may be largely confined to juveniles and adults who have done little crime. Directly supporting this interpretation, Piliavin, Thornton, Gartner, and Matsueda (1986) found evidence of updating within their sample of adolescent dropouts but not within their samples of adult offenders or adult drug users. Pogarsky and Piquero (2003) found that "resetting" of risk perceptions *downward* in response to arrest was more common among people who had done little crime. These findings suggest that both downward and upward updating may only occur among minor offenders or among serious offenders early in their careers. The implication for future research on this topic is that as long as researchers continue to confine their updating/resetting studies to samples of juveniles or minor offenders, they will bias the overall set of findings in favor of support for the updating hypothesis.

### *Findings by Dimension of Punishment Perceptions Treated as the Dependent Variable*

Empirical support for updating is extremely slender in other ways as well. All of the supportive studies previously mentioned addressed only arrests and other contacts with police as the punishment experience examined, and all of them concerned only the impact on perceptions of the risk of being arrested or caught by police. Indeed, all but 20 of the total set of 171 findings summarized in Table 6.16 treated perceived certainty of punishment (mostly certainty of arrest) as the dependent variable, so we know very little about the impact of punishment experiences on perceived severity or swiftness of punishment.

Nevertheless, we begin by first discussing the handful of studies that examined the impact of experienced punishment on perceived severity. Based on their multivariate analysis, Schneider and Ervin (1990) found that past experiences of punishment had no effect on delinquents' estimates of the severity of punishment. Wood (2007) found a statistically significant positive association of experienced severity of punishment with perceived severity among a sample of inmates in the Southeast, but it cannot be given much weight as an estimate of a causal effect since Wood did not control for any potential confounders. McClelland and Alpert (1985) found that, in a sample of felony offenders in the Midwest, the more prior convictions the offenders had, the *lower* their perceptions of the severity of imprisonment. Similarly, Watling et al. (2010) found that more severe punishment of peers was associated with *lower* perceived severity for DUI among respondents, although the association was not statistically significant. These four studies, then, fail to yield any consistent evidence of an effect of experienced punishment on perception of punishment severity.

### *The Effect of Experienced Punishment on Perceptions of Punishment Certainty by Measure of Perceived Certainty of Punishment*

Even among studies of perceived certainty of punishment, prior research has been narrowly focused. Nearly all of the tests of updating effects on perceived certainty have specifically addressed the effects of experienced punishment on perceptions of the risk of *arrest* or "*being caught*." We know almost nothing about the effects of punishment experiences on perceptions of the certainty of non-arrest events such as conviction or being sentenced to prison. Regarding the evidence pertaining to perceived certainty, only 23.2 percent of the findings were significant and in the predicted positive direction (Table 6.17). Studies using the chances of "being caught" as their measure of perceived certainty—a measure that encompasses many nonlegal consequences—were more likely than those measuring the perceived chance of being arrested to support the upward updating hypothesis.

We also explored whether the vicarious experience of other people known to the subject being arrested might affect their own perceived arrest risk. Table 6.18 shows that there has been little evidence of an effect of others' arrests or of one's own arrest on perceived arrest risk. Thus, neither the offender's own arrest nor the arrests of others known to him appear to significantly increase his perception of the risk of arrest.

**TABLE 6.17** Findings on the effect of experienced punishment on perceptions of punishment certainty by measure of perceived certainty

| *Measure of Perceived Certainty* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Being Caught | 66 | 6.1 | 24.2 | 3.0 | 33.3 | 31.8 | 1.5 | 0.0 |
| Being Arrested | 66 | 13.6 | 25.8 | 3.0 | 25.8 | 18.2 | 7.6 | 6.1 |
| Other | 19 | 15.8 | 42.1 | 0.0 | 31.6 | 10.5 | 0.0 | 0.0 |
| Total | 151 | 10.6 | 27.2 | 2.6 | 29.8 | 23.2 | 4.0 | 2.6 |

**TABLE 6.18** Findings on the effect of arrest on perception of arrest risk, Personal vs. Vicarious Experience

| *Type of Arrest Experience* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Arrested or Caught—Self | 46 | 13.0 | 39.1 | 0.0 | 32.6 | 15.2 | 0.0 | 0.0 |
| Arrested or Caught—Vicarious | 18 | 5.6 | 33.3 | 0.0 | 38.9 | 22.2 | 0.0 | 0.0 |
| Total | 64 | 10.9 | 37.5 | 0.0 | 34.4 | 17.1 | 0.0 | 0.0 |

### *The Effect of Punishment Experience on Punishment Perceptions by Dimension of Punishment Experience*

Some kinds of punishment experiences may affect perceptions of punishment risk more than others. The findings in Table 6.19 suggest that a higher experienced *certainty* of punishment is more likely to affect perceptions of legal risk than experienced *severity* of punishment. Caution should be exercised here because the certainty of punishment findings are highly inflated by the results of studies using arrest ratios. There have been only a handful of findings bearing on the effect of experienced severity on perceived risk of punishment, most of which do not support a significant effect on perceived legal risks.

The most common kind of test of the impact of punishment experiences on risk perceptions concerns the effects of being arrested or "caught." These findings as a whole do not support the idea that being arrested raises the arrestee's perceptions of the risk of future punishment. A substantial minority of offenders even reduce their perceived punishment risks, perhaps acting on the basis of the "gambler's fallacy"—the belief that one's bad luck in the past is bound to even out in the future with better luck.

To be sure, *some* offenders may increase their perceptions of the risk of arrest after being arrested, even if most do not. Further, arrestees could be influenced *both* by updating and the gambler's fallacy, the former tending to increase perceived risk and the latter tending to decrease it. The full body of empirical evidence, however, suggests that the *net* effect of arrest on perceived punishment risk across all studied offenders appears to be either close to zero or negative. That is, being arrested either has no effect on perceived certainty of punishment or actually reduces it (see, for example, Kleck et al. 2005; Piquero and Paternoster 1998; Richards and Tittle 1982). Table 6.19 shows that there is some support for the gambler's fallacy phenomenon, given that experience of having been arrested in the past was negatively associated with perceived certainty of arrest in 48 percent of the tests, and was significantly negative in 10.8 percent of them, but the full body of findings suggests that, on average across all offenders, there is no net effect one way or the other of arrest experience on perceptions of arrest certainty.

**TABLE 6.19** Findings on the effect of experienced punishment on perceptions of punishment risk by type of punishment experience

| *Type of Punishment Experience* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *− sig* | *− ns* | *? ns* | *+ ns* | *+ sig* | *− p = ?* | *+ p = ?* |
| Experienced Certainty | 43 | 2.3 | 11.6 | 4.7 | 27.9 | 46.5 | 7.0 | 0.0 |
| Experienced Severity | 6 | 16.7 | 0.0 | 16.7 | 33.3 | 33.3 | 0.0 | 0.0 |
| Experienced Swiftness | 1 | 0.0 | 0.0 | 100.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| Certainty × Severity | 3 | 0.0 | 33.3 | 0.0 | 66.7 | 0.0 | 0.0 | 0.0 |
| Arrested or Caught | 93 | 10.8 | 35.5 | 0.0 | 30.1 | 17.2 | 2.2 | 4.3 |
| Convicted | 7 | 12.5 | 0.0 | 0.0 | 37.5 | 37.5 | 12.5 | 0.0 |
| Incarcerated | 6 | 16.7 | 16.7 | 0.0 | 16.7 | 50.0 | 0.0 | 0.0 |
| Multiple | 8 | 37.5 | 25.0 | 12.5 | 0.0 | 25.0 | 0.0 | 0.0 |
| Other | 4 | 0.0 | 50.0 | 0.0 | 50.0 | 0.0 | 0.0 | 0.0 |
| Total | 171 | 9.9 | 25.7 | 2.9 | 29.2 | 26.3 | 3.5 | 2.3 |

The summary of findings reported in Table 6.19 also shows that there is some evidence that being convicted (Piliavin et al. 1986) or incarcerated (Snortum, Berger, and Hauge 1988) increases perceptions of punishment risk, though there are too few findings to draw any firm conclusions. Table 6.19 also shows how little research has been conducted on the effects of experienced certainty and swiftness of punishment on risk perceptions; most findings pertain to respondents' experience with specific punishment experiences, such as arrest or conviction. And as usual, the most neglected punishment dimension is swiftness (celerity) of punishment. We located only one relevant finding, which indicated there was no significant relationship between experienced swiftness of punishment and perceived certainty or severity of punishment (Schneider and Ervin 1990; there was no test reported for an effect on perceived swiftness).

### *Effects of Experienced Punishment of Self vs. Others on Perceived Certainty of Punishment*

Philip Cook (1980) argued for a theory of deterrence that focused largely on offenders' changes in perceived risk in response to their own punishment experiences and those of offenders they personally know. That is, he was arguing for specific deterrence based on upward updating of risk perceptions. Using robbers as an example, he proposed that

> each arrest and disposition has a relatively large effect on the perceptions of a small number of potential criminals (including the arrestee himself) and goes essentially unnoticed by all others . . . An increase in the true effectiveness of the system results in a corresponding increase in the mean of robbers' perceptions of effectiveness, and an increase in the number of robbers who are deterred. These changes do not occur because the robbers observe that the system has become more effective, but rather because the likelihood that a robber will observe one or more friends apprehended is increased when the overall effectiveness of the system increases. *(225)*

Thus, Cook assumed that offenders would upwardly update their perceived risks of arrest and punishment in response to their own punishment experiences and those of other offenders they knew. We have already seen that offenders do *not* generally increase their perceptions of punishment risk after experiencing punishment themselves, but is it possible that they respond to the punishment experiences of other offenders whom they know?

Perhaps the punishment experiences of one's *associates*, rather than one's own experiences, vicariously influence one's perceptions of punishment risk. Certainly the findings reported in Table 6.16 did not support this idea with respect to arrest. Regarding the broader set of all punishment experiences, Table 6.20 shows that there are few findings testing for an effect of others' punishment

**TABLE 6.20** Effects of experienced punishment of Self vs. Others on perceived certainty of punishment

| *Punishment Experience of Self or Others?* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Self | 118 | 11.0 | 26.3 | 3.4 | 32.2 | 23.7 | 3.4 | 0.0 |
| Others | 25 | 8.0 | 40.0 | 0.0 | 24.0 | 28.0 | 0.0 | 0.0 |
| Other Category | 8 | 0.0 | 0.0 | 0.0 | 12.5 | 12.5 | 25.0 | 50.0 |
| Total | 151 | 10.6 | 27.2 | 2.6 | 29.8 | 23.2 | 4.0 | 2.6 |

experiences and that most of them show no effect on punishment perceptions. For example, Lochner (2007) studied a nationally representative sample of juveniles, testing for the effect of respondents' siblings being arrested in the previous year on the respondent's current perceptions of the risk of arrest. He found not only that there was no significant association, but that the association was in the wrong direction—youth whose siblings had recently been arrested perceived (nonsignificantly) *lower* risks of arrest (454).

### *Effect of Punishment Experience on Risk Perceptions by Crime Type Punished*

The full set of findings indicates that being punished for crimes does not generally affect, on net, the punished person's perceptions of the risk of legal punishment. Given the mixed character of findings on this issue, however, one could speculate that there are such effects for the punishment of some specific types of crime. Table 6.21 shows the distribution of findings categorized by type of crime punished. A larger minority of findings support a significant positive effect of punishment on perceived risk when nonpredatory crimes are punished than when other types of crime are punished. Offenders who commit "victimless" crimes, like illicit drug use or prostitution, may be more likely to increase their perceptions of risk after being punished. On the other hand, there is little evidence that offenders update their perceptions of risk when punished for the serious predatory crimes that the public worries about the most.

DUI and theft, which have been tested in 45 and 19 analyses, respectively, are the only specific crime types that have been frequently studied in analyses testing the impact of punishment on perceptions of punishment risk. Some researchers have found evidence that experience with DUI punishment increases respondent perceptions of punishment risk for DUI (Sitren and Applegate 2006; Snortum et al. 1988), while studies of the punishment of *theft* have had contrasting findings (e.g., Horney and Marshall 1992; Schneider and Ervin 1990). The rest of the findings are spread over a couple of dozen crime types, each tested in only a few analyses.

The effect of experienced punishment of many specific crime types on perceptions of punishment risk remains unstudied. While fraud (Bridges and Stone 1986: Horney and Marshall 1992) and tax evasion (Richards and Tittle 1981; Sheffrin and Triest 1992) have been occasionally studied, we could not find any tests of the impact on risk perceptions of being punished for other white collar crimes, computer crimes, or sexual offenses. Likewise, drug dealing has been largely ignored; only Horney and Marshall (1992) addressed this crime. They found that prison inmates' number of prior arrests, either for drug dealing or for all offenses, showed no significant relationship with their current perceptions of the certainty of getting arrested for drug dealing, though there was a weak positive relationship between experienced certainty of arrest (arrests/crimes committed) for drug dealing and perceived certainty of getting arrested for drug dealing.

**TABLE 6.21** Findings on the effect of punishment experience on risk perceptions by crime type punished

| *Crime Type Punished* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Acquisitive | 60 | 13.3 | 25.0 | 3.3 | 28.3 | 21.7 | 6.7 | 1.7 |
| Violent | 36 | 11.1 | 30.6 | 0.0 | 36.1 | 16.7 | 2.8 | 2.8 |
| Nonpredatory | 57 | 3.5 | 26.3 | 5.3 | 22.8 | 36.8 | 1.8 | 3.5 |
| Mixed/All Types | 18 | 16.7 | 16.7 | 0.0 | 38.9 | 27.8 | 0.0 | 0.0 |
| Total | 171 | 9.9 | 25.7 | 2.9 | 29.2 | 26.3 | 3.5 | 2.3 |

### *Effects of Punishment Experience on Risk Perceptions by Ages of Study Subjects*

Young people are thought to be more malleable than older people, so one might expect that their beliefs would be more profoundly affected by the experience of legal punishment. Our review does not support this expectation. Table 6.22 shows that there is little variation in the findings when broken down by age, though samples of mixed ages show somewhat more support for the updating hypothesis than samples exclusively made up of juveniles. The unsupportive findings for samples of juveniles may be partially explained by the fact that only a small percentage of the tests of this general hypothesis concerned experienced certainty of punishment, which is the type of punishment experience most likely to be associated with risk perceptions. Finally, Schneider and Ervin (1990) found that in a sample of juveniles there was no significant association of experienced swiftness of punishment with either perceived certainty of being caught or perceived severity, while experiencing longer periods of incarceration (experienced severity) apparently *decreased* perceptions of certainty.

Studies performing direct tests of a conditioning effect of age have yielded very mixed findings. Some studies of juveniles found evidence that being punished reduced their perceptions of risk (Paternoster and Piquero 1995; Thomas and Bishop 1984), while others found that punishment increases perceived risk (Anwar and Loughran 2011; Pogarsky et al. 2004). The inconsistency of findings suggests that (a) any effects that punishment has on juveniles' risk perceptions may be contingent on other factors, and (b) that we cannot conclude that there is any consistent effect of punishment on juveniles' risk perceptions in one direction or the other.

### *Findings on the Effect of Punishment on Risk Perceptions by Gender*

We also explored whether gender might condition the impact of punishment experience on perceived risk by cross-tabulating findings by the gender of single-sex samples. The results shown in Table 6.23 suggest that a larger minority of findings support an effect of punishment on risk perceptions among females than among males. If we assume that females are more risk averse than

**TABLE 6.22** Findings on the effect of punishment experience on perceptions of risk by ages of subjects

| *Ages of Subjects* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Juveniles | 44 | 11.4 | 27.3 | 11.4 | 25.0 | 25.0 | 0.0 | 0.0 |
| Adults | 54 | 14.8 | 24.1 | 0.0 | 31.5 | 22.2 | 7.4 | 0.0 |
| Mixed Ages | 73 | 5.5 | 26.0 | 0.0 | 30.1 | 30.1 | 2.7 | 5.5 |
| Total | 171 | 9.9 | 25.7 | 2.9 | 29.2 | 26.3 | 3.5 | 2.3 |

**TABLE 6.23** Findings on the effect of punishment experience on risk perceptions by gender (based on male-only or female-only samples)

| *Gender of Sample* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Sample of Males | 32 | 15.6 | 43.8 | 0.0 | 25.0 | 15.6 | 0.0 | 0.0 |
| Sample of Females | 6 | 0.0 | 16.7 | 0.0 | 50.0 | 33.3 | 0.0 | 0.0 |
| Total | 38 | 13.2 | 39.5 | 0.0 | 28.9 | 18.4 | 0.0 | 0.0 |

males, this accords with the research of Schulz (2014), who found that only risk-averse youth increased their perception of the risk of arrest after being arrested. The pattern, however, is fragile, as it is based on only six findings for female-only samples, two of which supported the hypothesis.

## *Methodological Artifacts—Findings by Variables Controlled*

Analyses that control for more potential confounders, other things being equal, should do a better job of isolating the effect of a variable of interest. Thus, findings in this body of research might differ because some researchers controlled for more variables than other researchers. We cross-tabulated findings based on whether they were generated by analyses that controlled for many or few potential confounders. The pattern displayed in Table 6.24 shows that studies that controlled for more variables were much less likely to find that punishment experiences increased perceptions of punishment risk. This suggests that there were indeed confounding variables that distorted the relevant association in a pro-deterrence direction when they were not statistically controlled. Thus, many of the associations that seemed to show an upward updating effect of punishment on risk perceptions may have been completely spurious, since most of the supportive findings disappear once researchers control for more than a handful of potential confounding variables.

It is particularly important to control for a subject's prior criminal behavior, since this affects both the likelihood that he will be legally punished and his perceptions of punishment risk. Doing more crimes obviously increases the chances a person will eventually suffer a legal punishment such as arrest, but it also reduces perceived certainty of punishment via the experiential effect. These opposite-sign effects would contribute to a negative association between punishment experience and perceived certainty and could thereby suppress any positive effect that punishment had on perceived risk, if prior criminal behavior was not controlled.

The pattern of findings shown in Table 6.25 does not support the hypothesis that there is a positive effect of punishment experience on perceived risk that is suppressed when analysts fail

**TABLE 6.24** Findings on the effect of punishment experience on perceptions by the number of independent variables

| *# of Independent Variables* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 0–5 | 35 | 8.6 | 14.3 | 0.0 | 20.0 | 45.7 | 11.4 | 0.0 |
| 6 or More | 136 | 10.3 | 28.7 | 3.7 | 31.6 | 21.3 | 1.5 | 2.9 |
| Total | 171 | 9.9 | 25.7 | 2.9 | 29.2 | 26.3 | 3.5 | 2.3 |

**TABLE 6.25** Findings on the effect of experienced punishment on risk perceptions by controls for prior crime

| *Control for Prior Crime?* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Yes | 143 | 10.5 | 27.3 | 3.5 | 32.2 | 19.6 | 4.2 | 2.8 |
| No | 28 | 7.1 | 17.9 | 0.0 | 14.3 | 60.7 | 0.0 | 0.0 |
| Total | 171 | 9.9 | 25.7 | 2.9 | 29.2 | 26.3 | 3.5 | 2.3 |

to control for the confounding influence of prior criminal behavior. In fact, studies that failed to control for prior criminal behavior were far *more* likely to find punishment experience positively associated with perceived punishment risk. This could indicate that all updating of risk perceptions is due to punishment avoidance, i.e. committing crimes without being punished. If that is true, once one controlled for prior offending, there would be no more updating effect to be detected. We address this possibility later in the chapter. Alternatively, these seemingly supportive findings may simply reflect the poorer overall quality of the studies that generated them.

There is a flaw common to most of the studies in this area—a failure to match the type of punishment experience to the type of punishment risk measured. One would expect experienced *certainty* of punishment to have its strongest effect on perceptions of the *certainty* of future punishment, experienced *severity* to have its strongest effect on perceptions of the likely *severity* of future punishments, and so forth. Likewise, one would expect a stronger effect of experienced *arrest* on perceived *arrest* risk than on the perceived risk of conviction or incarceration. Most of the findings in this area, however, do not relate types of punishment experiences to perceptions of the corresponding risk, a problem that is likely to weaken the observed association between prior punishment experiences and perceptions about future risk.

Why has there been such a diversity of findings on this issue? Some researchers find that the experience of punishment increases perceived risk, some find it decreases perceived risk, and most find no significant net effect. Rather than attributing this entirely to methodological differences, we believe that effects of punishment experiences on perceived risk may genuinely vary across offenders, and are highly contingent on other factors, few of which have been investigated at this time. For example, Thomas and his colleagues (2013) found that upward updating in response to an arrest occurred only among persons with below-average intelligence. Schulz (2014) found updating in response to an arrest only among more risk-averse persons while Anwar and Loughran (2011) found that there was more updating among less experienced offenders. This is potentially important to an overall assessment of the cumulated research because most of the studies that support updating have been done on juveniles. Nevertheless, the full body of research, across the full set of offenders studied, has failed to support the proposition that the experience of punishment causes a net increase in perceived punishment risk among those punished.

### *Reconciling Findings on the Experiential Effect With Findings on Upward Updating—Asymmetrical Updating*

There is a great deal of evidence that committing crimes without being punished reduces perceived legal risks (Chapter 5), i.e., that punishment avoidance causes *downward* updating of perceived risks of punishment. In sharp contrast, the vast majority of findings indicate that punishment experiences do not generally cause *upward* updating of perceived risks. These may seem to be inconsistent conclusions. If *avoidance* of punishment *reduces* perceived risks, then it is natural to expect that *experience* of punishment should *increase* perceived risks. We believe that these two bodies of findings are more compatible than they may initially appear, once one takes into account what a small share of crimes result in punishment (Table 3.1).

The key lies in the fact that crime without punishment is a common experience, whereas crime with punishment is a rare one. Initially naïve offenders who persist in committing crimes gradually learn the reality that the actual risks of punishment are less than they had initially thought, and consequently "update" their risk perceptions downward—the phenomenon that deterrence researchers have traditionally referred to as the "experiential effect." Thus, avoidance of punishment is a repeated experience, likely to be perceived as part of a persisting pattern. On the other hand, offenders can easily come to regard the rare occasions on which their crimes resulted in punishment as flukes, the product of random chance ("a cop car just happened to go around the corner just as I was breaking into the house"), rather than the result of either the authorities' enduring skill in detecting and punishing crime, or the offender's persisting lack of skill in avoiding punishment.

An offender who perceived punishment as a random event would have no reason to update their risk perceptions upward, since the punishment experience would effectively be regarded as "noise" rather than "signal," that is, as irrelevant input that can be ignored or discounted because it is useless for predicting future risks of punishment. If this view of punishment is widespread, one would not expect punishment experiences to generally result in upward updating of risk perceptions, and thus would not expect the imposition of punishment on more criminals to significantly increase specific deterrent effects.

Some deterrence scholars (Loughran, Paternoster, Piquero, and Fagan 2012; Schulz 2014) attribute this reaction to punishment experiences to "self-serving attributional bias" (Fiske and Taylor 2007), a tendency to attribute one's successes to one's own efforts, and to attribute failures to random chance or bad luck. This bias is believed to be most common among impulsive persons (Schulz 2014). Most criminals, and thus most people subject to legal punishment experiences, are impulsive (Gottfredson and Hirschi 1990), implying that this bias is likely to be widespread within the punished criminal population, and that discounting the significance of rare punishment experiences is correspondingly common.

In sum, updating of risk perceptions appears to be distinctly asymmetrical within the legally punished population. In the aggregate, the downward updating effect of committing many unpunished crimes on perceived risk is widespread, while the upward updating effect of punishment experiences is rare. Consequently, punishing criminals has little special deterrent effect because the experience of punishment generally does not, on net, increase the perceived risk of future punishment among those punished.

## Chapter 6 Summary

In this chapter, we reviewed research that tested the impact of experienced punishment on criminal behavior among those punished and on their perceptions of future punishment risk. Most research findings indicate that criminals who are punished are, on net, slightly more likely to increase their offending than criminals who were not punished. That is, the full body of evidence supports a net crime-*increasing* effect of being punished on those who are punished somewhat more than it supports a net crime-reducing effect (Table 6.1). This accords with the conclusions of an earlier review of the impact of incarceration on recidivism (Gendreau, Goggin, and Cullen 1999). Further, the minority of findings that do suggest a net crime-reducing effect of punishment experience may be an artifact of the unduly short follow-up periods used by most researchers, since specific deterrent and other crime-reducing effects of punishment appear to be strongest less than a year after punishment (Table 6.13), while crime-increasing effects may not become evident until well after the punishment experience.

Another part of the reason why a specific deterrent effect of punishment is not generally supported by the evidence is that punishing criminals does not seem to generally increase their perception of the risk of future punishment (Table 6.16). That is, it does not seem to "teach them a lesson"—or at least not the lesson intended by those imposing the punishments. Further, when the experience of punishment does seem to affect perceived risk, it often *reduces* it, presumably because some criminals fall prey to the "gambler's fallacy." The effects of punishment experience on perceived risk appear to be highly variable, contingent on factors that currently are largely unknown. In the aggregate, the personal experience of punishment within the offender population as a whole does not appear to cause a net increase in perceived risk of punishment.

Finally, on the occasions when punishment does increase the punished offender's perception of risk, any resulting specific deterrent or other crime-reducing effects that are produced are counterbalanced to some degree by crime-increasing effects of punishment, discussed in detail in Chapter 11.

## References

Anwar, Shamena, and Thomas Loughran. 2011. Testing a Bayesian learning theory of deterrence among serious juvenile offenders. *Criminology* 49:667–698.

Apel, Robert. 2013. Sanctions, perceptions, and crime: Implications for criminal deterrence. *Journal of Quantitative Criminology* 29:67–101.

Apospori, Eleni, Geoffrey Alpert, and Raymond Paternoster. 1992. The effect of involvement with the criminal justice system: A neglected dimension of the experience-perceptions relationship. *Justice Quarterly* 9:379–392.

Beck, Allen, and Bernard Shipley. 1997. *Recidivism of Prisoners Released in 1983.* Washington, DC: Bureau of Justice Statistics.

Bernburg, John, and Marvin Krohn. 2003. Labeling, life chances, and adult crime: The direct and indirect effects of official intervention in adolescence on crime in early adulthood. *Criminology* 41:1287–1317.

Bridges, George, and James Stone. 1986. Effects of criminal punishment on perceived threat of punishment: Toward an understanding of specific deterrence. *Journal of Research in Crime and Delinquency* 23:207–239.

Bursik, Robert. 1983. Community context and the deterrent effect of sanctions. In *Evaluating Performance in Criminal Justice Agencies*, eds. Gordon Whitaker and Charles Phillips, 165–181. Beverly Hills: Sage.

Cambell, Donald, and Julian Stanley. 1966. *Experimental and Quasi-Experimental Designs for Research.* Chicago, IL: Rand McNally.

Carter, Robert, Daniel Glazer, and Leslie Wilkins. 1984. *Probation, Parole and Community Corrections* (3rd ed.). New York: Wiley.

Cook, Phillip. 1980. Research in criminal deterrence: Laying the groundwork for the second decade. *Crime and Justice* 2:211–268.

DeYoung, David. 1997. An evaluation of the effectiveness of alcohol treatment, driver license actions and jail terms in reducing drunk driving recidivism in California. *Addiction* 92:989–997.

Doob, Anthony, and Cheryl Webster. 2003. Sentence severity and crime: Accepting the null hypothesis. *Crime and Justice* 30:143–195.

Fiske, Susan E., and Shelley E. Taylor. 2007. *Social Cognition.* New York: McGraw-Hill.

Gendreau, Paul, Claire Goggin, and Francis Cullen. 1999. *The Effects of Prison Sentences on Recidivism.* Ottawa: Solicitor General of Canada.

Gibbs, Jack. 1975. *Crime, Punishment and Deterrence.* New York: Elsevier.

Gottfredson, Michael, and Travis Hirschi. 1990. *A General Theory of Crime.* Stanford, CA: Stanford University Press.

Greenberg, David. 1981. Methodological issues in survey research on the inhibition of crime: Comment on Grasmick and Green. *The Journal of Criminal Law and Criminology* 72:1094–1101.

Greenberg, David, and Nancy Larkin. 1998. The incapacitation of criminal opiate users. *Crime and Criminology* 44:205–228.

Horney, Julie, and Ineke H. Marshall. 1992. Risk perceptions among serious offenders: The role of crime and punishment. *Criminology* 30:575–592.

Jensen, Gary. 1969. Crime doesn't pay: Correlates of a shared misunderstanding. *Social Problems* 17:189–201.

Kindlon, Daniel, Erico Mezzacappa, and Felton Earls. 1995. Psychometric properties of impulsivity measures: Temporal stability, validity and factor structure. *Journal of Child Psychology and Psychiatry* 36:645–661.

Kleck, Gary, Brion Sever, Spencer Li, and Marc Gertz. 2005. The missing link in general deterrence research. *Criminology* 43:623–660.

Klemke, Lloyd. 1978. Does apprehension for shoplifting amplify or terminate shoplifting activity. *Law and Society Review* 12:391–403.

Klepper, Steven, and Daniel Nagin. 1989. The deterrent effect of perceived certainty and severity of punishment revisited. *Criminology* 27:721–746.

Lochner, Lance. 2007. Individual perceptions of the criminal justice system. *American Economic Review* 97:446–460.

Loughran, Thomas, Raymond Paternoster, Alex Piquero, and Jeffrey Fagan. 2012. 'A good man always knows his limitations': The role of overconfidence in criminal offending. *Journal of Research in Crime and Delinquency* 50:327–358.

Matsueda, Ross, Derek Kreager, and David Huizinga. 2006. Deterring delinquents: A rationale choice model of theft and violence. *American Sociological Review* 71:95–122.

McClelland, Kent, and Geoffrey Alpert. 1985. Factor analysis applied to magnitude estimates of punishment seriousness: Patterns of individual differences. *Journal of Quantitative Criminology* 1:307–318.

McGrath, Andrew. 2009. Offenders' perceptions of the sentencing process: A study of deterrence and stigmatization in the New South Wales children court. *Australian and New England Journal of Criminology* 42:24–46.

Nagin, Daniel. 2013. Deterrence: A review of the evidence by a criminologist for economists. *A Review of Economics* 5:83–104.

Nagin, Daniel, Francis Cullen, and Cheryl Jonson. 2009. Imprisonment and reoffending. *Crime and Justice* 38:115–200.

Paternoster, Raymond. 1987. The deterrent effect of the perceived certainty and severity of punishment: A review of the evidence and issues. *Justice Quarterly* 4:173–215.

Paternoster, Raymond, and Alex Piquero. 1995. Reconceptualizing deterrence: An empirical test of personal and vicarious experiences. *Journal of Research in Crime and Delinquency* 32:251–286.

Piliavin, Irving, Craig Thornton, Rosemary Gartner, and Ross Matsueda. 1986. Crime, deterrence, and rational choice. *American Sociological Review* 51:101–119.

Piquero, Alex, and Raymond Paternoster. 1998. An application of Stafford and Warr's reconceptualization of deterrence to drinking and driving. *Journal of Research in Crime and Delinquency* 35:3–39.

Piquero, Alex, Raymond Paternoster, Greg Pogarsky, and Thomas Loughran. 2011. Elaborating the individual difference component in deterrence theory. *Annual Review of Law and Social Science* 7:335–360.

Pogarsky, Greg, and Alex Piquero. 2003. Can punishment encourage offending? Investigating the resetting effect. *Journal of Research in Crime and Delinquency* 40:95–120.

Pogarsky, Greg, Alex Piquero, and Ray Paternoster. 2004. Modeling change in perceptions about sanction threats: The neglected linkage in deterrence theory. *Journal of Quantitative Criminology* 20:343–369.

Pogarsky, Greg, Sean Patrick Roche, and Justin T. Pickett. 2017. Heuristics and biases, rational choice, and sanction perceptions. *Criminology* 55:85–111.

Richards, Pamela, and Charles Tittle. 1981. Gender and perceived chances of arrest. *Social Forces* 59:1182–1199.

Richards, Pamela, and Charles Tittle. 1982. Socioeconomic status and perceptions of personal arrest probabilities. *Criminology* 20:329–346.

Ross, Laurence. 1984. Social control through deterrence: Drinking and driving law. *Annual Review of Sociology* 10:21–35.

Sampson, Robert, and John Laub. 1993. *Crime in the Making.* Cambridge, MA: Harvard University Press.

Schneider, Anne, and Laurie Ervin. 1990. Specific deterrence, rational choice and decision heuristics: Applications in juvenile justice. *Social Science Quarterly* 71:585–601.

Schulz, Sonja. 2014. Individual differences in the deterrence process: Which individuals learn most from their offending experiences? *Journal of Quantitative Criminology* 30:215–236.

Sever, Brion, and Ronald Reisner. 2008. Collecting data from the criminal courts: Perspectives of court staff members: A research note. *Criminal Justice Policy Review* 19:103–116.

Sever, Brion, Ronald Reisner, and Ryan King. 2001. Successfully acquiring data from the criminal courts: Is it what you know, who you know, or what you don't tell them? *The Justice System Journal* 22:315–331.

Sheffrin, Steven, and Robert Triest. 1992. Can brute deterrence backfire? Perceptions and attitudes in taxpayer compliance. In *Why People Pay Taxes*, ed. Joel Slemrod. Ann Arbor, MI: University of Michigan Press.

Sitren, Alicia, and Brandon Applegate. 2006. Intentions to offend: Examining the effects of personal and vicarious experiences with punishment and punishment avoidance. *Journal of Crime and Justice* 29:25–50.

Snortum, John, Dale Berger, and Ragnar Hauge. 1988. Legal knowledge and compliance: Drinking and driving in Norway and the United States. *Alcohol, Drugs and Driving* 4:251–264.

Spohn, Cassia, and David Holleran. 2002. The effect of imprisonment on recidivism of felony offenders: A focus on drug offenders. *Criminology* 40:329–357.

Thomas, Charles, and Donna Bishop. 1984. The effect of formal and informal sanctions on delinquency: A longitudinal comparison of labeling and deterrence theories. *The Journal of Criminal Law and Criminology* 75:1222–1245.

Thomas, Kyle, Thomas Loughran, and Alex Piquero. 2013. Do individual characteristics explain variation in sanction risk updating among serious juvenile offenders? Advancing the logic of differential deterrence. *Law and Human Behavior* 37:10–21.

Ventura, Lois, and Gabrielle Davis. 2005. Domestic violence: Court case conviction and recidivism. *Violence against Women* 11:255–277.

Vitacco, Michael, and Richard Rogers. 2001. Predictors of adolescent psychopathy: The role of impulsivity, hyperactivity, and sensation seeking. *Journal of the American Academy of Psychiatry and the Law* 29:374–382.

Watling, Christopher, Gavan Palk, James Freeman, and Jeremy Davey. 2010. Applying Stafford and Warr's reconceptualization of deterrence theory to drug driving: Can it predict those likely to offend? *Accident Analysis and Prevention* 42:452–458.

Wheeler, Gerald, and Rodney Hissong. 1988. Effects of criminal justice sanctions on drunk drivers: Beyond incarceration. *Crime and Delinquency* 34:29–42.

White, Jennifer, Terrie Moffitt, Avshalom Caspi, Dawn Bartusch, Douglas Needles, and Magda Stouthamer-Loeber. 1994. Measuring impulsivity and examining its relationship to delinquency. *Journal of Abnormal Psychology* 103:192–205.

Whiteacre, Kevin. 2008. Celerity matters: The deterrent effects of disciplinary infraction of disposition times in a community corrections center. *Corrections Compendium* 33:7–11.

Williams, Kirk, and Richard Hawkins. 1986. Perceptual research on general deterrence: A critical review. *Law and Society Review* 20:545–572.

Wood, Peter. 2007. Exploring the positive punishment effect among incarcerated adult offenders. *American Journal of Criminal Justice* 31:8–22.

Zane, Stephen, Brandon Welsh, and Daniel Mears. 2016. Juvenile transfer and the specific deterrence hypothesis. *Criminology and Public Policy* 15:1–25.

# 7

# MACRO-LEVEL RESEARCH ON THE EFFECT OF PUNISHMENT LEVELS

This chapter addresses macro-level research on the overall effects of legal punishment levels on crime rates—effects that have commonly been interpreted as reflecting general deterrent effects. While individual-level studies test hypotheses with data concerning individual persons, macro-level deterrence studies analyze aggregate units of analysis such as nations, regions, states, metropolitan areas, counties, cities, neighborhoods, Census tracts, police beats, or blocks. When tested with macro-level data, the deterrence hypothesis states that higher punishment levels in aggregates (such as the populations of areas) cause lower crime rates in those aggregates, other things being equal, due to prospective offenders' fear of punishment.

In this chapter we review tests of the potential deterrent effects of aggregate levels of certainty, severity and swiftness of *non*capital punishment on crime rates, including tests of the impact of the number or rate of arrests, convictions, or prosecutions, and possible interactions between levels of certainty and severity. Chapter 8 will separately review macro-level findings on the impact of capital punishment while Chapter 10 will cover studies of the incapacitative impact of incarceration levels on crime rates.

Some categories of macro-level crime research are excluded from our review. We do not cover the enormous research literature on the impact of specific laws or policies on crime rates, since these studies do not directly test deterrent or incapacitative effects of punishment, and often do not even specifically assess the effects of punishment per se. For example, we do not cover studies testing the impact of Hot Spots policing policies, community policing, drug courts, gun control laws, "Three Strikes" laws, and other specific policies aimed at reducing crime. We likewise do not cover the substantial literature on the impact of police force size or police expenditures on crime rates since a greater number of police officers or greater expenditures does not necessarily imply increased levels of punishment (Kleck and Barnes 2014). Readers may consult the online Appendix A for more detail on the macro-level analyses that were excluded from our review.

## Overall Macro-Level Findings

Table 7.1 summarizes the findings of all macro-level deterrence analyses that were included in our review. This table reveals that macro-level research is more likely to yield apparent support for the deterrence hypotheses than individual-level research pertaining to either the impact of perceived risks of punishment on crime (Chapter 5) or to the effects of experienced punishment on

**TABLE 7.1** Comparison of overall macro-level and individual-level findings

| *Category of Research* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Macro-Level Findings | 1270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |
| Perceptual Deterrence (Ch. 5) | 569 | 38.0 | 32.0 | 7.0 | 17.2 | 2.1 | 3.3 | 0.4 |
| Experienced Punishment (Ch. 6) | 659 | 14.6 | 22.5 | 4.4 | 27.6 | 28.2 | 0.6 | 2.1 |

**TABLE 7.2** Overall macro-level findings omitting studies with over 10 findings

| *Total Number of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|
| | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p=?* |
| 882 | 46.5 | 23.8 | 4.3 | 11.2 | 1.7 | 9.9 | 2.6 |

crime (Chapter 6). Nevertheless, only a minority of macro-level findings are significant and negative, supporting a deterrent effect. While individual-level findings often suggested that individuals' experience with punishment can increase their criminal activity, there was virtually no macro-level evidence that punishment levels, on net, increase crime rates. This combination of results is not necessarily contradictory; it may simply indicate that punishment increases the criminal behavior of some of those individuals who are punished, yet also deters other (unpunished) people from committing crime.

We carried out a sensitivity test for whether macro-level results are skewed by studies that produced very large numbers of findings. Table 7.2 shows that omission of studies with more than ten findings has little impact on the distribution of findings, since the distribution of findings is essentially identical to that shown in the first row of Table 7.1.

## Macro-Level Findings by Decade of Publication

Contrary to the results of our reviews of individual-level research, we found that the distribution of macro-level deterrence findings have remained relatively stable over the last four decades. Table 7.3 shows that the share of findings supporting the deterrence hypothesis fluctuated between 40 percent and 50 percent over the duration of this study. There was no pronounced trend in support for punishment effects, though there was somewhat more support after 1980 than before.

**TABLE 7.3** Macro-level findings by decade of publication

| *Decade of Publication* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 1960s & 1970s | 530 | 40.8 | 22.5 | 0.2 | 11.9 | 1.1 | 15.7 | 7.9 |
| 1980s | 212 | 49.1 | 19.8 | 6.1 | 8.0 | 1.9 | 10.4 | 4.7 |
| 1990s | 221 | 46.2 | 27.6 | 15.8 | 4.1 | 1.4 | 5.0 | 0.0 |
| 2000s | 307 | 50.5 | 27.4 | 4.2 | 15.6 | 1.6 | 0.3 | 0.3 |
| Total | 1270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |

## Macro-Level Findings by Location

Is punishment more effective in some places than others? Some populations may be more deterrable than others just as some individuals are more deterrable (Chapter 2), so macro-level findings could systematically differ depending on the geographical areas to which the data pertain. We cross-tabulated findings by U.S. regions and, for non-U.S. studies, by nation. Table 7.4 displays the distribution of macro-level deterrence findings by study area. Research in foreign nations accounts for only 21.7 percent of macro-level findings available in English; not surprisingly, most of the English-language macro-level deterrence research pertains to the U.S. And among the U.S. studies, macro-level findings have been most frequently generated using data pertaining to the Southern and Northeastern regions, while substantially fewer findings have been based on areas in the Midwest and Western regions.

Recall that individual-level studies of the impact of punishment on those punished revealed little variation in support for deterrence across U.S. regions (Table 6.10). In sharp contrast, among macro-level studies there appears to be considerable variation in support across regions. Findings are considerably more likely to support a deterrent effect in studies of the Northeast (39.6 percent) and South (39.8 percent) than in studies of the West (30.8 percent) or Midwest (23.5 percent). Thus, the relative frequency of research based on the Northeast or South has had the effect of increasing the number of findings supporting the deterrence hypothesis. Support for deterrence has also been less likely in studies based on Canada than in those based on the U.S., while it has been more likely in studies of England/Wales and in the miscellaneous nations grouped into the "Other Countries" categories.

**TABLE 7.4** Macro-level findings categorized by location to which the data pertain

| *Location* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| National Sample U.S. | 68 | 51.5 | 36.8 | 1.5 | 7.4 | 0.0 | 2.9 | 0.0 |
| Multiple U.S. States | 420 | 43.3 | 20.2 | 0.0 | 7.9 | 0.5 | 18.9 | 9.3 |
| Multiple U.S. Cities | 169 | 45.0 | 28.4 | 8.3 | 17.2 | 0.6 | 0.6 | 0.0 |
| Multiple U.S. Counties | 32 | 68.8 | 9.4 | 0.0 | 21.9 | 0.0 | 0.0 | 0.0 |
| Other Areas Within U.S. | 42 | 59.5 | 16.7 | 7.1 | 14.3 | 0.0 | 2.4 | 0.0 |
| Northeast U.S. | 101 | 39.6 | 15.8 | 25.7 | 8.9 | 2.0 | 5.0 | 3.0 |
| Midwest U.S. | 34 | 23.5 | 20.6 | 23.5 | 14.7 | 5.9 | 8.8 | 2.9 |
| South U.S. | 103 | 39.8 | 30.1 | 4.9 | 12.6 | 4.9 | 5.8 | 1.9 |
| West U.S. | 26 | 30.8 | 26.9 | 3.8 | 3.8 | 0.0 | 23.1 | 11.5 |
| Multiple Countries | 48 | 50.0 | 27.1 | 0.0 | 6.3 | 0.0 | 16.7 | 0.0 |
| England/Wales | 91 | 57.1 | 27.5 | 4.4 | 6.6 | 3.3 | 0.0 | 1.1 |
| Canada | 35 | 34.3 | 34.3 | 0.0 | 5.7 | 0.0 | 14.3 | 11.4 |
| Other Countries | 101 | 51.5 | 26.7 | 0.0 | 17.8 | 3.0 | 1.0 | 0.0 |
| Total | 1,270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |

## Variations in Macro-Level Findings Based on Methodological Variations

### *Macro-Level Findings by Unit of Analysis*

We were interested in whether the macro-level findings are sensitive to various aspects of the methodology used to generate the findings. For instance, support for deterrence findings may differ

**TABLE 7.5** Macro-level findings categorized by unit of analysis

| *Unit of Analysis* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Police Beats | 34 | 55.9 | 20.6 | 2.9 | 17.6 | 2.9 | 0.0 | 0.0 |
| City | 290 | 44.8 | 24.5 | 10.0 | 11.4 | 2.4 | 3.4 | 3.4 |
| County | 98 | 59.2 | 24.5 | 0.0 | 15.3 | 1.0 | 0.0 | 0.0 |
| State | 531 | 40.1 | 20.0 | 4.5 | 10.4 | 0.9 | 16.8 | 7.3 |
| SMSA | 56 | 50.0 | 25.0 | 0.0 | 7.1 | 0.0 | 16.1 | 1.8 |
| Nation | 180 | 45.6 | 35.6 | 2.8 | 8.3 | 1.1 | 5.0 | 1.7 |
| Other Area | 81 | 58.0 | 24.7 | 3.7 | 11.1 | 2.5 | 0.0 | 0.0 |
| Total | 1,270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |

depending on which macro-level unit of analysis that is used in a study. Some researchers have asserted that larger aggregate units such as states and cities may be less appropriate for deterrence research than neighborhoods or even smaller areas (Chamlin, Grasmick, Bursik, and Cochran 1992; Greenberg 1981). Therefore, we categorized the results by the unit of analysis used in each study.

The results in Table 7.5 show that the state was the most commonly tested unit of analysis, largely due to the numerous cross-sectional and panel studies of states. There are only slight differences in findings relating to the units of analysis studied, though studies of counties and police beats have been somewhat more likely to be supportive of deterrence, while studies of states have been somewhat less likely to be supportive. There was not a clear relationship between findings and the size of the aggregates studied. Although studies of some smaller areas such as police beats were more likely than average to support deterrence, studies of some larger aggregates, like counties or SMSAs, were also more likely than average to support deterrence.

### *Macro-Level Findings by Methods for Addressing Causal Order*

Punishment levels may affect crime rates, but crime rates can also affect punishment levels. It has long been recognized that high crime volumes may overwhelm the criminal justice system's ability to deliver punishment, reducing the share of crimes solved by police, the share of arrests resulting in convictions, and the share of persons convicted who can be sent to prison (Fisher and Nagin 1978). Consequently, negative associations between crime rates and punishment levels may reflect the effect of crime rates on punishment levels rather than the reverse. The findings of macro-level deterrence studies may therefore differ depending on whether the analysts properly addressed this causal order issue (Kane 2006: Marvell and Moody 1996; Nagin 1978; Wilson and Boland 1982).

We categorized findings by the method used to take account of causal order in the study generating the finding. One common procedure used has been to simply lag the punishment variable, relating punishment in earlier year t-1 to crime in later year t. Although this method appears to assure the proper time order between punishment and crime variables, it does not correct for the potential simultaneity between the two variables. Since crime rates remain stable over short time intervals, crime rates for the year after the punishment are likely to be highly similar to the crime the year or two before the punishment variables (Fisher and Nagin 1978). Consequently, a negative correlation between crime in year t and punishment in year t-1 may actually capture the impact of a previous years' crime rate (if stable) on later punishment levels. Merely lagging of the punishment variable does not correct for this possibility.

A potentially stronger method for addressing the simultaneity between crime and punishment, if applied properly, is some variant of instrumental variables (IV) methods (Levitt 1997; Listoken 2003). We grouped all analyses that used procedures such as two-stage least squares (2SLS), three-stage least-squares (3SLS), instrumental variables (IV), or similar methods into the IV category. We classified findings according to whether they were based on analyses using (1) IV methods, (2) lagging the punishment variable, or (3) neither. Findings based on analyses using both lags *and* the IV method were classified as IV-based findings.

Unfortunately, most deterrence studies that use IV methods provide too little information to judge the adequacy of the instruments used in the IV studies. In particular, they rarely report tests of whether their instrumental variables were exogenous or valid. In Chapter 4 we explained in detail what these essential properties of instruments are and noted that the few studies that do report tests of these properties rarely yield convincing evidence that the instruments were indeed valid and exogenous. Thus, readers are cautioned that IV-based studies did not necessarily do a better job of addressing causal order issues, since their instrumental variables were not necessarily exogenous, relevant, and valid.

Table 7.6 shows the breakdown of findings categorized by the method used to address the causal order between punishment and crime variables. The most common method employed in the analyses was the application of a simple lag of the crime variable. The use of simple lags was nearly twice as common as the use of instrumental variables methods. The table shows that analyses using instrumental variables are substantially more likely to find support for deterrence than are analyses using lags or no method to assure proper time order. This pattern could be due to better methods leading to more accurate pro-deterrence findings, but this seems unlikely given the general lack of support for deterrence among the individual-level studies examined in Chapters 5 and 6. Another possible explanation could be the related to the academic discipline of researchers who have been most likely to use instrumental variable methods, i.e. economists, whose theories make them generally more favorably disposed to pro-deterrence findings. This idea will be analyzed later in the chapter.

A possible explanation of the lower level of support in studies only using lagged punishment variables to address causal order would be that lags in the effects of punishment on crime rates may differ over time such that the appropriate time lags may differ from one year to the next (Chamlin 1988). Some punishment events may have immediate effects, while others may have effects that become evident only after long lags because it takes time for word of penalties to spread to prospective offenders before punishments can effectively deter criminal behavior. If lags are incorrectly specified, estimates of the deterrence effect could be too low because they were not based on correct assumptions about how much time passes between punishment events or changes in punishment levels and resulting changes in crime rates.

**TABLE 7.6** Macro-level findings categorized by causal order methods

| *Causal Order Method* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| None Reported | 268 | 38.4 | 17.2 | 0.7 | 9.3 | 1.1 | 18.7 | 14.6 |
| Lagged Punishment Variable | 651 | 41.3 | 27.6 | 8.3 | 11.4 | 1.2 | 8.3 | 1.8 |
| Instrumental Variables | 351 | 58.4 | 22.8 | 1.7 | 10.8 | 2.0 | 3.7 | 0.6 |
| Total | 1,270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |

### Macro-Level Findings by General Research Design

Much criticism has been directed at the use of cross-sectional research designs because of limits on their ability to establish causal order between punishment rates and crime rates (Chamlin et al. 1992; Fisher and Nagin 1978; Marvell and Moody 1996; Wilson and Boland 1978), as well as their inability to account for dynamic changes in this relationship over time (Marvell and Moody 1996; Nagin 1978). On the other hand, cross-sectional designs often study larger samples of areas, increasing the generalizability of their findings, and are able to control for more confounding variables due to greater data availability compared to time series and panel studies (Rubin and Babbie 2009; Valente 2002). We classified research designs used in macro-level deterrence research into four basic types: time series, panel (including pooled cross-sections designs), cross-sectional, and percentage change analyses. This last design relates percent changes in punishment variables over brief periods of time to percent changes in crime rates over the same time period or a slightly later period. Table 7.7 displays the distribution of findings by research design. Many scholars in this field would argue that panel designs are superior to pure cross-sectional designs, so one might expect different findings to be generated by the two approaches. Our review, however, finds that studies using the two designs have been about equally likely to support deterrence—panel designs yielded significant negative associations in 50.4 percent of the hypothesis tests, and cross-sectional designs yielded such results in 47.4 percent of the tests. Time series designs were less likely to support deterrence and percent change analyses were by far the least likely to support deterrence. We give little weight to the percent change findings because they are based on only 41 findings and because they are the weakest of the commonly used research designs. Researchers using this approach commonly measure percentage changes in punishments and crime over time spans as long as five or ten years, making it impossible to determine just when punishment variables or crime rates changed within those long time periods and whether changes in the punishment variables preceded changes in the crime rates.

**TABLE 7.7** Macro-level findings categorized by research design

| *Research Design* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Cross-Sectional (CX) | 557 | 47.4 | 21.4 | 2.0 | 8.4 | 1.1 | 12.7 | 7.0 |
| Panel, Pooled CX | 389 | 50.4 | 25.2 | 1.8 | 13.9 | 1.5 | 7.2 | 0.0 |
| Time-Series | 283 | 38.5 | 27.9 | 15.5 | 5.7 | 1.8 | 6.0 | 4.6 |
| Percent Change | 41 | 19.5 | 24.4 | 0.0 | 48.8 | 2.4 | 2.4 | 2.4 |
| Total | 1,270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |

### Macro-Level Findings by Number of Independent Variables Controlled

Table 7.8 shows that macro-level findings are more likely to support deterrence when a larger number of variables are controlled. Inclusion of a greater number of control variables does not, of course, insure that the *correct* variables were controlled, or that all important confounders were controlled, but other things being equal one would generally expect that these goals were more likely to be met if the sheer number of controls is greater.

We previously speculated that the number of independent variables may have less impact on the outcomes of studies than the quality of these variables. Similar to the macro-level findings

**TABLE 7.8** Macro-level findings categorized by the number of control variables

| *Number of Control Variables* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 0–5 | 513 | 32.4 | 16.0 | 7.8 | 10.5 | 1.2 | 21.8 | 10.3 |
| 6 or More | 757 | 54.3 | 29.6 | 2.9 | 11.0 | 1.6 | 0.7 | 0.0 |
| Total | 1,270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |

summarized above, individual-level analyses of experienced punishment on crime also were more likely to support deterrence when they controlled for more variables. Both of these types of research rely more heavily on existing official data than do the survey-based perceptual studies. Using official data limits the number and character of variables that researchers can analyze. This implies that researchers doing macro-level deterrence research are less able to measure the most important confounding variables that they need to control, compared to researchers doing individual-level perceptual deterrence research. Thus, even though researchers using existing official data may include many control variables in their statistical models of crime rates, few of them may be relevant to the task of isolating the effect of punishment levels because few are confounders. That is, they are not variables that both affect crime rates and are associated with punishment levels.

### *Macro-Level Findings by Controls for Incapacitation Effects*

One of the more important specific confounding variables that should be controlled in macro-level deterrence analyses is the size of the incarcerated population in each studied area. Negative macro-level associations between punishment variables like arrest rates and crime rates could be attributable to incapacitative effects of incarcerating many criminals rather than deterrent effects (Kessler and Levitt 1999). Both are crime-reducing effects of legal punishment, but deterrence relies on prospective offenders' perceptions of legal risk while incapacitation does not. For this reason, we broke down the macro-level findings by whether they were generated by analyses that included a control for the size of the prison population.

Table 7.9 shows that only 10.3 percent of macro-level studies controlled for prison population, a critical flaw in this body of research. Out of 1,270 total macro-level findings, only 66 supported the deterrence hypothesis *and* were based on analyses that controlled for prison population—just 5 percent of all macro-level findings. Thus, macro-level support for deterrence is far more fragile than a cursory review of the literature might suggest.

Contrary to expectation, the few analyses that controlled for the incapacitative effects of larger prison population were actually more likely to support deterrence. Given the considerable evidence

**TABLE 7.9** Macro-level findings by controls for incapacitative effects of prison population

| *Control for Prison Population?* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| No | 1,139 | 44.9 | 23.4 | 5.4 | 11.3 | 1.1 | 9.4 | 4.6 |
| Yes | 131 | 50.4 | 30.5 | 0.8 | 6.1 | 3.8 | 7.6 | 0.8 |
| Total | 1,270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |

that prison population size has a negative effect on crime rates (Chapter 10), and the strong logical basis for believing that times and places with more criminals locked up are also likely to have higher levels of deterrence variables like arrest rates, it is highly unlikely that controlling for prison population actually makes it more likely an analyst will find support for deterrence. Instead, it is more likely that this result is attributable to other features of the studies that controlled for prison population. The proposition that controlling for prison population reduces the estimated deterrent effect of other punishment levels was directly demonstrated by Brandt and Kovandzic (2015). They showed that the estimated effects of executions on monthly homicide counts in Texas was sharply reduced when the state's prison population was controlled (13–14).

As a somewhat more refined analysis, we examined the findings by whether researchers employed all of three desirable methodological features: (1) used panel designs to address causal order, (2) controlled for the size of the prison population, and (3) controlled for five or more control variables. Table 7.10 indicates that studies that adopted all three desirable methodological features were more likely to support a deterrent effect. Since higher crime rates have a negative effect on punishment levels, it is hard to see how controlling for this negative effect could *strengthen* estimates of the deterrent effect of punishment levels. And, as noted, it is highly unlikely that controlling for prison population would increase estimates of deterrent effects. Thus, only controlling for more variables among these three features seems plausible as a reason why estimates of deterrent effects would strengthen. Perhaps studies controlling for more variables control for confounders that have same-sign effects on both punishment levels and crime rates, tending to suppress a negative effect of punishment levels on crime rates.

**TABLE 7.10** Macro-level findings on effects of certainty of punishment by methodological strength

| *Employed All 3 Features?* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| No | 771 | 54.5 | 21.3 | 5.6 | 7.9 | 1.3 | 8.8 | 0.6 |
| Yes | 48 | 66.7 | 33.3 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| Total | 819 | 55.2 | 22.0 | 5.3 | 7.4 | 1.2 | 8.3 | 0.6 |

## *Macro-Level Findings by General Type of Crime*

Our reviews of individual-level deterrence research explored the claim of Andenaes (1975) that *mala prohibita* crimes are more likely to be deterred by threats of legal punishment than *mala in se* crimes. He and others argued that violent crimes are less likely to be deterred by legal penalties than property crimes, perhaps due to the impulsiveness and limited rationality that is commonly associated with violence (Bachman, Paternoster, and Ward 1992; Jacob 1980). Conversely, we expect more deterrence of property crimes because they involve more deliberation and calculation (Chambliss 1967; Jacob 1980; Speckart, Angelia, and Deschenes 1989). Patterns of findings of the individual-level research partially supported this idea. Recall that, in Chapter 5, we did find that tests of the impact of perceptions of punishment on acquisitive crime have been, as a group, more likely to support a deterrent effect than tests of the effect of perceptions of punishment on violent crime (Table 5.7). On the other hand, when we down broke the individual-level findings by specific types of crimes, we found that there was very little difference between the estimated impact of perception of punishment on property crimes and the impact on crimes such as marijuana use and drunk driving. Moreover, our Chapter 6 review of research on the effects of experienced

**TABLE 7.11** Macro-level findings categorized by general type of crime

| *General Type of Crime* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Acquisitive | 458 | 45.6 | 24.5 | 8.3 | 9.0 | 1.3 | 6.3 | 5.0 |
| Violent | 667 | 44.7 | 23.4 | 3.3 | 12.1 | 1.2 | 10.8 | 4.5 |
| Nonpredatory | 19 | 31.6 | 26.3 | 0.0 | 21.1 | 10.5 | 10.5 | 0.0 |
| Mixed/All Index Crimes, Etc. | 126 | 50.8 | 26.2 | 1.6 | 8.7 | 1.6 | 11.1 | 0.0 |
| Total | 1,270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |

punishment found little difference between findings on the estimated impact of experienced punishment on acquisitive crimes and findings on the impact on other types of crimes.

Macro-level researchers have likewise theorized that some crime types are more deterrable than others (Bursik, Grasmick, and Chamlin 1990; Chamlin et al. 1992; Sherman and Weisburd 1995), typically asserting that property crimes are more deterrable than are other types of crimes (Bursik et al. 1990). Some have specifically hypothesized that robbery is more deterrable than other types of violent crime because it involves a more calculated motivation to obtain property (Chamlin et al. 1992) and because the public location of most robberies makes them more likely to be affected by the efforts of police to increase the certainty of punishment that are focused on public places (Sherman and Weisburd 1995).

Macro-level findings were therefore categorized by general crime type. Table 7.11 shows that support for deterrence did not substantially differ between violent and acquisitive crimes. Contrary to theoretical expectations, macro-level research has been just as likely to support deterrence with regard to violent crime as acquisitive crime.

## *Macro-Level Findings by Specific Type of Crime*

To further explore the seemingly counterintuitive patterns in Table 7.11, we categorized macro-level findings according to the specific types of crimes studied. Table 7.12 shows that levels of support for deterrence are still not sharply different across crime types, but there is some modest support for the view that homicide/murder and assault/battery are less deterrable than most specific types of acquisitive crimes. On the other hand, the low level of support for deterrence of motor vehicle theft is contrary to the general proposition that property crimes are more deterrable, perhaps because many motor vehicle thefts are unplanned and committed for reasons other than material gain. Certainly many adolescents steal vehicles for the transitory thrills of a joyride in a stolen car rather than for more rational economic reasons (Light, Nee, and Ingham 1993; Miethe, McCorkle, and Listwan 2006).

Findings concerning rape and robbery have been more likely to support deterrence than those pertaining to any of the other commonly studied crime types. Given its acquisitive motives, supportive findings for robbery are not surprising, but the relatively high level of support for deterrent effects on rape is less expected. Rape is the only specific crime for which a majority of macro-level deterrence findings have been negative and significant. On the one hand, this is surprising because many rapes are compulsive, suggesting that they are less deterrable. On the other hand, rape is subject to more severe penalties, and some rapes are premeditated. Some sexual predators stalk victims and wait for favorable opportunities to attack them, suggesting that they might be more likely to think about the risks of punishment.

**TABLE 7.12** Macro-level findings by specific crime type

| *Specific Crime Type* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *− sig* | *− ns* | *? ns* | *+ ns* | *+ sig* | *− p = ?* | *+ p = ?* |
| Murder | 132 | 42.4 | 27.3 | 0.0 | 10.6 | 3.0 | 12.1 | 4.5 |
| Homicide | 98 | 38.8 | 34.7 | 3.1 | 15.3 | 1.0 | 6.1 | 1.0 |
| Rape | 82 | 51.2 | 13.4 | 6.1 | 14.6 | 1.2 | 11.0 | 2.4 |
| Robbery | 208 | 49.5 | 13.5 | 4.3 | 10.1 | 0.5 | 15.4 | 6.7 |
| Assault/Battery | 102 | 35.3 | 31.4 | 2.0 | 13.7 | 2.0 | 8.8 | 6.9 |
| General Violent Crime | 45 | 48.9 | 24.4 | 8.9 | 8.9 | 6.7 | 2.2 | 0.0 |
| Motor Vehicle Theft | 95 | 37.9 | 28.4 | 9.5 | 7.4 | 0.0 | 9.5 | 7.4 |
| Burglary | 148 | 45.9 | 25.0 | 8.1 | 8.1 | 0.7 | 6.8 | 5.4 |
| Theft/Larceny | 140 | 46.4 | 22.1 | 7.1 | 11.4 | 1.4 | 5.7 | 5.7 |
| General Property Crime | 33 | 45.5 | 27.3 | 9.1 | 18.2 | 0.0 | 0.0 | 0.0 |
| Mixed/All Crime Types | 123 | 52.0 | 26.0 | 1.6 | 8.1 | 0.8 | 11.4 | 0.0 |
| Other | 64 | 50.0 | 28.1 | 4.7 | 9.4 | 3.1 | 4.7 | 0.0 |
| Total | 1,270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |

Macro-level researchers have done more testing of serious crimes than individual-level researchers have, but there have been some serious crime types that even macro-level researchers have not addressed. Much of this neglect stems from the limited availability of official data on these neglected crime types. For instance, we located no macro-level analyses that tested the impact of punishment on computer crime, which is not surprising given the current lack of macro-level data on the frequency of computer crime.

Terrorism and illegal immigration are among the more notable of crimes that have yet to be appropriately explored in macro-level deterrence research. We found a few studies that tested the impact of metal detectors and general policies on hijacking behavior (Chauncey 1975; Cauley and Im 1988; Dugan, Lafree, and Piquero 2005; Enders and Sandler 2000; Enders, Sandler, and Cauley 1990; Minor 1975), but only one macro-level analysis of the effects of legal punishments on terrorism that matched our criteria for inclusion in this review. Landes (1978) found that certainty and severity of punishment had mixed effects on U.S. and world hijackings.

We likewise could not locate any macro-level studies of the impact of punishment levels on illegal immigration, despite the considerable attention that this phenomenon has received in recent years. There have instead been only a handful of tests of the impact of general policies and police size on illegal immigration (Espenshad 1994; Gathmann 2008; Hoekstra and Orozco-Aleman 2014; Woodland and Yoshida 2006).

## *Macro-Level Findings by Discipline of Journal Publishing Results*

As we previously noted, the academic discipline of researchers may influence their findings on deterrence, which implies that the distribution of findings should be related to the discipline of the journal publishing the research, Table 7.13 shows that economic journals are the most common publishing outlet for macro-level deterrence research—40.3 percent of these findings appeared in economics journals, while only 21.3 percent were reported in criminology/criminal justice journals. Macro-level findings published in economic journals are far more likely than those reported

**TABLE 7.13** Macro-level findings by academic discipline of publication outlet

| *Discipline of Publication* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Criminology/CJ | 271 | 20.3 | 20.3 | 11.8 | 9.6 | 3.3 | 22.5 | 12.2 |
| Economics | 513 | 60.0 | 24.2 | 3.7 | 8.8 | 1.2 | 1.9 | 0.2 |
| Sociology | 215 | 39.5 | 26.0 | 0.5 | 22.3 | 0.9 | 10.7 | 0.0 |
| Book Chapter/ Working Paper | 94 | 44.7 | 37.2 | 6.4 | 3.2 | 0.0 | 5.3 | 3.2 |
| Other Journal/ Books | 177 | 49.2 | 20.3 | 2.3 | 8.5 | 0.6 | 10.2 | 2.0 |
| Total | 1,270 | 45.4 | 24.1 | 4.9 | 10.8 | 1.4 | 9.2 | 4.2 |

in any other kind of outlet to support the deterrence hypothesis. It is hypothetically possible the deterrence hypothesis is generally correct and that studies published in economics journals use more sophisticated methods likely to reveal this truth than research published in other outlets. One indirect indicator of the often primitive character of methods used in many studies published in criminology/criminal justice journals is that no significance level was reported for over one third of the findings reported in such journals (shown in the –p = ? and +p = ? columns of Table 7.13), an omission commonly characteristic of unsophisticated analyses using very basic descriptive statistics. Even with these columns excluded, however, only 31 percent of the macro-level findings reported in criminological journals supported deterrence.

We also tested the possibility that findings published in economic journals were based on larger sample sizes, thus making them more likely to be statistically significant. We found that the findings published in economic journals were indeed based on larger samples (average n = 150.3), but that findings published in "other journals and books" were based on still larger samples (average n = 195). Moreover, the samples used in macro-level studies published in criminological journals was also substantial (average n = 125), too slight a difference compared to findings published in economics journals to make much difference in the distribution of findings.

A different explanation is more compatible with the generally unsupportive findings of individual-level research (Chapters 5 and 6). That is, economists may be so strongly influenced by their disciplinary expectations to believe that crime rates decline as the costs of crime (including legal risks) increase that they revise their research procedures until they obtain results that appear to them to be reasonable. Many different statistical procedures, ways of measuring key variables, and specifications of control variables may arguably be similarly appropriate, but the methodological decisions yielding the "reasonable" (i.e. pro-deterrence) findings may be the ones that are implemented. It is common to read, in economics journal articles, the author describing results as "reasonable" or "plausible" when they support a crime-reducing effect of punishment (e.g., see Baltagi 2006, 543, 546), as if it were implausible that punishment levels could have null or positive effects on crime rates. It is easy enough for researchers to make a series of arguably legitimate technical decisions as to how deterrent effects should be estimated and choose to rely on those procedures that yield the more theoretically "plausible" estimates, i.e. those that are negative and significant. This could help explain why the findings of macro-level deterrence research by economists stands in stark contrast to both the macro-level findings of scholars in other academic disciplines and most individual-level research. It is also possible that findings failing to support deterrent effects are less likely to be accepted for publication or even submitted to economics journals based on the expectation that such findings would be perceived as the product of methodological shortcomings.

### *Macro-Level Findings by Punishment Dimension*

Table 7.14 shows how the results of macro-level deterrence researchers differ by the dimension of punishment risk that was tested—that is, whether the punishment variables used in the research measured certainty, severity, or swiftness of punishment. Scholars have tested the certainty of punishment far more often than severity of punishment. As was true of individual-level research, the only macro-level findings that generally support the deterrence hypothesis are those pertaining to certainty of punishment. The less numerous findings pertaining to severity and swiftness of punishment overwhelmingly fail to support deterrence. Likewise, there has been little macro-level support for an interactive effect of certainty and severity of punishment on crime rates. Whether aggregate levels of severity of punishment are assessed by themselves or as part of an interaction with certainty of punishment, they do not appear to significantly reduce crime rates.

Table 7.14 also shows that the support for deterrence in the macro-level certainty findings (55.2 percent significant and negative) was considerably higher than was found among individual-level findings in Chapters 5 and 6. Only 40.5 percent of individual-level findings found a significant negative association between perceived certainty of punishment and criminal behavior (Table 5.9), and only 40.3 percent of findings on the effect of experienced certainty of criminal behavior were significant and negative (Table 6.2a). Likewise, macro-level findings regarding severity of punishment were more likely to support deterrence (28.7 percent supportive) than individual-level findings regarding perceived severity (26.8 percent of those findings were significant and negative—Table 5.9) or experienced punishment severity (13.0 percent of findings were negative and significant—Table 6.2a). The full set of results, then, suggest that the macro-level findings may reflect aggregation bias—aggregate-level estimates do not reflect what is happening at the level of individual persons. Possible reasons for this disconnect between individual-level and macro-level deterrence findings will be discussed at length in Chapter 9.

The higher level of support for the deterrence doctrine seen in macro-level research might be seen as validating the deterrence doctrine and justifying greater use of punitive crime control policies. These macro-level analyses do not warrant such conclusions because (a) most macro-level findings do not support deterrence (i.e., are not significant and negative), (b) the findings only support a deterrent effect of punishment certainty and not severity or swiftness, (c) their findings are inconsistent with most individual-level deterrence research, and (d) macro-level research provides only very indirect tests of deterrent effects.

**TABLE 7.14** Macro-level findings categorized by dimension of punishment

| *Dimension of Punishment* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Certainty (C) | 819 | 55.2 | 22.0 | 5.3 | 7.4 | 1.2 | 8.3 | 0.6 |
| Severity (S) | 279 | 28.7 | 31.9 | 1.1 | 20.4 | 1.4 | 9.7 | 6.8 |
| Swiftness | 2 | 0.0 | 100.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| C × S | 38 | 2.6 | 2.6 | 0.0 | 0.0 | 0.0 | 42.1 | 52.6 |
| Total | 1,138 | 46.8 | 23.9 | 4.0 | 10.4 | 1.2 | 9.6 | 3.9 |

Regarding this last point, aggregate-level studies, with one exception, do not actually measure the perceptions of punishment risk that are, according to the deterrence doctrine, supposed to produce reductions in crime. The single macro-level study that measured aggregate levels of perceived punishment risk found no significant association of those perceptions with actual punishment rates (Kleck and Barnes 2013). The assumption that actual punishment levels can serve as adequate proxies for perceived punishment risks has proven to be unwarranted (Chapter 9).

Further, macro-level researchers depend almost entirely on existing or "found" data, mostly generated by criminal justice agencies or the U.S. Census Bureau. As a result, they can only control for potentially confounding variables for which there happens to be existing data rather than all the likely confounders that theory and prior research indicate need to be controlled. Research on perceptual deterrence, on the other hand, is based largely on surveys that allow analysts to measure not only macro-level variables describing the areas in which respondents live but also any variables that can be measured by asking people questions.

Finally, Table 7.14 shows that swiftness of punishment has been almost entirely neglected in macro-level deterrence research. While there were over 30 tests of the effect of punishment swiftness among individual-level studies (Chapters 5 and 6), there have been only two macro-level tests of whether swifter punishment deters crime better, both of them generated by a single study. Selke (1983) found that both the amount of time elapsed from crime to arrest and from arrest to sentencing had negative but insignificant associations with burglary.

### *Macro-Level Certainty of Punishment Findings by Measurement of Certainty*

Even strong defenders of the deterrence doctrine like Daniel Nagin have conceded that evidence supporting deterrence "is strongest for the certainty of punishment" (Nagin, Solow, and Lum 2015, 75). Our review shows that this generalization needs to be narrowed even further. Support for the deterrence doctrine is largely found among *macro-level* tests of the effect of certainty of punishment, since most individual-level research on perceptual deterrence and special deterrence fails to find any significant negative effect of perceived or experienced certainty of punishment (Chapters 5 and 6). The seemingly anomalous macro-level findings regarding punishment certainty therefore deserve more detailed attention.

We begin by examining whether the distribution of findings differs by how aggregate levels of punishment certainty were measured. Macro-level researchers have used a variety of measures of the probability of crimes resulting in arrests of the perpetrators, including clearance rates, the ratio of arrests per 100 offenses, or similar measures (Bursik et al. 1990; Chilton 1982; Decker and Kohfeld 1985; Greenberg, Kessler, and Logan 1979; Tittle and Rowe 1974). Scholars have likewise used a variety of measures of the certainty of conviction or imprisonment (Dusek 2012; Ehrlich and Liu 1999; Mendes 2004; Sampson 1986). In Table 7.14, findings regarding the certainty of punishment are broken down by the specific measure of certainty used. The findings are categorized with respect to five of the most commonly used certainty measures, plus a residual category encompassing all of the other rarely used measures.

The most commonly used type of macro-level certainty measure was certainty of arrest, which is typically computed as either the percentage of crimes known to the police that resulted in arrest or the ratio of arrests over crimes (A/C). This is unfortunate, since deterrence scholars have noted that this quantity does not actually measure police effectiveness in generating crime deterrence because it only measures the share of crimes that *were committed* that resulted in arrest, but takes no account of crimes that were *not* committed in the first place because prospective offenders feared arrest (Cook 1979; Nagin et al. 2015, 84). Consequently, there is good reason to doubt whether the 45 percent of macro-level findings regarding punishment certainty that were generated by analyses using clearance rates and similar variables to measure risk of arrest are relevant to the deterrence doctrine. Yet, if we excluded the A/C findings, 48 percent of the macro-level tests of punishment certainty that supported deterrence (215 of 452 total) would disappear. In sum, much of the support for deterrence is found in macro-level research using clearance rates or arrest ratios as measures of police effectiveness in deterring crime, yet even strong supporters of the deterrence doctrine, like Philip Cook and Daniel Nagin, reject the suitability of these measures for use in deterrence research.

The next most commonly used type of certainty measure was the certainty of prison, commonly computed as the ratio of prison sentences imposed in a given period over crimes known to the police (P/C). These two measures, A/C and P/C, account for 74 percent of the macro-level tests of the effect of certainty of punishment. Among macro-level findings based on the more commonly used measures listed in Table 7.15, there were only modest differences across the types of punishment certainty measures. Roughly similar shares (often a majority) of the certainty findings were significant and negative, apparently supporting a crime-reducing effect of more certain punishment, regardless of how certainty was measured, though there was somewhat more apparent support for the influence of arrest certainty—58.6 percent of the A/C findings were significant and negative compared to 49.4 percent for P/C.

Unfortunately, there is another reason why some measures of punishment certainty would be particularly likely to be negatively related to crime rates. In 85 percent of the macro-level tests of the impact of punishment certainty, the variables measuring certainty contain the number of crimes in the denominator of the ratio (see the first three rows of Table 7.15). In any analysis in which the dependent variable is either a crime rate, i.e. the ratio of crimes over population, or a simple count of crimes, there would be an artifactual negative association between the measure of punishment certainty and the rate or count of crimes simply because the number of crimes appears in the denominator of the punishment variable and the numerator of the crime rate or count. The same problem affects analyses in which the punishment variable is the ratio of arrest, convictions, sentencings to prison, or admissions to prison over crimes.

It is widely recognized that official counts of crimes are subject to substantial measurement error. The number of crimes known to the police not only seriously understates the total number of crimes committed but also does so to a degree that can vary over time and differ across areas (Gove, Hughes, and Geerken 1985). Whatever errors cause crime counts to be too low in a given place at a given time will always push the measured punishment certainty ratio up and the measured crime rate down, thereby contributing to a negative association between the two variables

**TABLE 7.15** Macro-level findings by certainty measures (not including death penalty studies)

| *Certainty Measure* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Arrests/Crimes | 367 | 58.6 | 19.1 | 9.3 | 6.5 | 0.8 | 5.2 | 0.5 |
| Prison Sentences/Crimes | 239 | 49.4 | 23.8 | 0.8 | 8.8 | 1.3 | 15.5 | 0.5 |
| Convictions/Crimes | 89 | 47.2 | 27.0 | 6.7 | 7.9 | 3.4 | 6.7 | 1.1 |
| Convictions/Arrests | 51 | 54.9 | 27.5 | 0.0 | 13.7 | 0.0 | 3.9 | 0.0 |
| Prison Sentences/ Convictions | 45 | 66.7 | 20.0 | 0.0 | 2.2 | 2.2 | 8.9 | 0.0 |
| Other Measures | 28 | 67.9 | 21.4 | 3.6 | 3.6 | 0.0 | 0.0 | 3.6 |
| Total | 819 | 55.2 | 22.0 | 5.3 | 7.4 | 1.2 | 8.3 | 0.6 |

that may be entirely an artifact of error in measuring crimes rather than a reflection of deterrent or other crime-reducing effects. This helps explain why there is a higher level of macro-level support for the deterrent effect of punishment *certainty* than of punishment severity—an explanation that has nothing to do with actual deterrent effects of certainty being stronger than the effects of severity. Most tests of the effect of punishment *certainty* are artificially helped by the biasing effect of this ratio variable artifact, while most tests of the deterrent effect of punishment severity do not enjoy this benefit.

If we consider the less commonly used measures of punishment certainty, the measure most likely to have a significant negative relationship with crime rates was prison sentences/convictions. This is arguably more a measure of punishment severity than of certainty, since it reflects how likely criminal courts judges are to sentence convicted persons to prison. The individual-level evidence reviewed in Chapters 5 and 6, however, did not generally support the view that there was greater deterrent effect of more severe sentencing, undercutting our confidence that these macro-level findings reflect actual deterrent effects. Finally, a large majority of the small number of findings in the residual "other measures" category were consistent with a deterrent effect of certainty, but this is not very informative since there were few findings based on any one of these miscellaneous measures to confidently establish which of these types of punishment certainty is most likely to deter.

## Macro-Level Certainty of Punishment Findings by Crime Type

Recall that the conventional theoretical expectation is that acquisitive crimes are more deterrable than violent offenses. Table 7.16 displays the findings of the macro-level research on the effects of punishment certainty, categorized by the type of crime tested. Contrary to theoretical expectations, findings regarding acquisitive crimes are only slightly more likely to support a deterrent effect than those regarding violent crimes. There has been too little research on nonpredatory crimes to draw any firm conclusions, but the few findings we do have weakly suggest that nonpredatory crimes are less likely to be deterred by more certain punishment than predatory crimes.

**TABLE 7.16** Macro-level certainty findings by crime type

| *General Crime Type* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Acquisitive | 286 | 57.3 | 21.7 | 8.4 | 5.2 | 1.0 | 5.9 | 0.3 |
| Violent | 431 | 52.7 | 22.7 | 4.2 | 9.3 | 1.2 | 9.0 | 0.9 |
| Nonpredatory | 9 | 44.4 | 11.1 | 0.0 | 22.2 | 11.1 | 11.1 | 0.0 |
| Mixed/All Index Crimes | 93 | 61.3 | 20.4 | 1.1 | 4.3 | 1.1 | 11.8 | 0.0 |
| Total | 819 | 55.2 | 22.0 | 5.3 | 7.4 | 1.2 | 8.3 | 0.6 |

## Macro-Level Certainty of Punishment Findings by Methodology

As with the reviews of the individual-level deterrence research, we explored whether findings concerning certainty of punishment might vary depending on the methodology used to generate the findings. In Table 7.17, the findings are cross-classified by the number of variables controlled in the analyses. The results show that macro-level models with more control variables have been more likely to find support for a deterrent effect of punishment certainty. Unfortunately, the number of

**TABLE 7.17** Macro-level certainty findings by number of independent variables

| *Number of Independent Variables* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 0–5 | 297 | 39.7 | 18.9 | 8.1 | 8.4 | 1.3 | 21.9 | 1.7 |
| 6 or More | 522 | 64.0 | 23.8 | 3.6 | 6.9 | 1.1 | 0.6 | 0.0 |
| Total | 819 | 55.2 | 22.0 | 5.3 | 7.4 | 1.2 | 8.3 | 0.6 |

control variables can tell us nothing about whether the *correct* variables, i.e. confounding variables, were controlled. Since macro-level deterrence researchers have been largely confined to using whatever variables happen to be measurable using existing data, it is less likely that they can measure and control all the variables that are likely to be confounders, compared to researchers who generate their own original data using surveys.

One specific likely confounder that many macro-level researchers, especially those using states as their unit of analysis, should have measured and controlled is the size of the prison population. Controlling for this variable would at least partly control for the incapacitative effects of those legal punishments that involve incarceration. Unfortunately, only 10.3 percent of macro-level findings were based on analyses that controlled for this variable (Table 7.9), making it impossible to distinguish deterrent effects of legal punishment from incapacitative effects. Given the stress that some scholars have placed on the importance of distinguishing deterrent and incapacitative effects of punishment (e.g., Kessler and Levitt 1999; Owens 2009; Shavell 2014), it is surprising that so few macro-level researchers have addressed the issue.

As we will see in Chapter 10, there is consistent evidence that larger prison populations reduce crime rates, presumably due to the incapacitative effects of incarceration. Since the prison population has a negative effect on crimes but is also positively correlated with measures of punishment certainty and severity, a failure to control for prison population biases the certainty/crime association in a negative direction—incapacitation effects of imprisoning large numbers of criminals are wrongly attributed to deterrence. Recall that we found (Table 7.9), somewhat anomalously, that the few analyses that controlled for prison population were actually slightly *more* likely to produce negative and significant certainty/crime associations (58.4 percent) than those that did not (54.9 percent), but there are too few findings of the former type to place much reliance on this result.

Another specific confounder that needs to be controlled if a macro-level researcher is to isolate the deterrent effect of higher punishment levels is the average level of social disapproval or condemnation of criminal behavior prevailing within a population. This almost certainly has a negative effect on crime rates but is also positively correlated with punishment levels because punishment is one way populations express their degree of condemnation of crime. Failure to control this confounder will therefore tend to bias the crime/punishment association in a negative direction, overstating apparent deterrent effects. We would have liked to compare deterrence findings of research that controlled for this variable with the findings of research that did not, but we could not find a single macro-level deterrence study that controlled for levels of social condemnation. Thus, the credibility of all macro-level deterrence research is undercut by this failure.

We also investigated whether certainty of punishment findings differ depending on the general research design used to generate them. Table 7.18 shows that analyses using cross sectional or panel designs have been more likely to support the deterrence hypothesis than those using times series designs. Analyses using pooled cross-sections and panel designs are the ones most likely to support

**TABLE 7.18** Macro-level certainty findings by research design

| *Research Design* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Cross Sectional (CX) | 340 | 58.5 | 17.1 | 2.6 | 6.5 | 1.5 | 13.5 | 0.3 |
| Panel, Pooled CX | 263 | 62.4 | 22.8 | 2.3 | 8.4 | 0.8 | 3.4 | 0.0 |
| Time Series | 189 | 42.9 | 29.1 | 14.8 | 3.2 | 1.1 | 6.9 | 2.1 |
| % Change | 27 | 29.6 | 25.9 | 0.0 | 40.7 | 3.7 | 0.0 | 0.0 |
| Total | 819 | 55.2 | 22.0 | 5.3 | 7.4 | 1.2 | 8.3 | 0.6 |

**TABLE 7.19** Macro-level certainty findings by method to address causal order

| *Causal Order Method* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| None Identified | 171 | 51.5 | 18.1 | 0.0 | 8.8 | 1.2 | 18.1 | 2.3 |
| Lags | 403 | 48.9 | 25.6 | 9.4 | 7.4 | 1.5 | 6.9 | 0.2 |
| Instrumental Variables | 245 | 68.2 | 18.8 | 2.0 | 6.5 | 0.8 | 3.7 | 0.0 |
| Total | 819 | 55.2 | 22.0 | 5.3 | 7.4 | 1.2 | 8.3 | 0.6 |

the deterrence hypothesis, while percent change designs were least likely to yield support, as was true of the full set of 1,270 macro-level results.

Punishment certainty levels may affect crime rates, but crime rates may also affect punishment certainty levels, as higher volumes of crime leads to a reduction in the share of crimes that result in arrest. Each variable is theorized to have a negative effect on the other, regardless of which is regarded as the cause and which one the effect. Thus, a negative association could reflect a crime-reducing effect of punishment levels, but it could also reflect a punishment-reducing effect of higher crimes. One would therefore expect that if appropriate methods, such as properly applied IV methods, are used to deal with this possible two-way causation, the estimated negative effect of punishment levels on crime rates should decrease, since such methods would allow analysts to account for the negative effect of crime rates on punishment levels, and thereby isolate the effect of punishment levels on crime rates (Klein, Forst, and Filatov 1978).

This is not, however, what is indicated by the broad pattern of certainty findings categorized by the method used to address the causal order issue. Table 7.19 shows that certainty of punishment analyses using instrumental variables (IV) methods are somewhat more likely to yield a significant negative estimate of the effect of punishment certainty on crime rates than analyses using the cruder method of merely lagging the punishment variables. Since use of better causal order methods should not produce such a pattern, this seeming anomaly calls for some explanation. In Chapter 4 we found that the macro-level studies we reviewed that used IV methods did not apply them properly because the instrumental variables used by analysts lacked the statistical properties they should have possessed, i.e. exogeneity, relevance, and validity (see Chapter 4 for explanations of these terms). Studies using poorly executed IV methods may happen to share other methodological flaws that favor finding deterrent effects, such as failures to control crucial confounding variables like prison rates. Finally, if most of the researchers using IV methods are economists who are strongly disposed by their training to expect crime-reducing effects of more certain punishment,

**TABLE 7.20** Macro-level certainty findings by academic discipline of journal

| *Discipline of Journal* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Economics | 351 | 69.9 | 19.1 | 2.6 | 19.1 | 0.9 | 2.3 | 0.0 |
| Criminology/CJ | 161 | 26.7 | 16.8 | 14.9 | 9.9 | 3.1 | 28.0 | 0.6 |
| Sociology | 139 | 52.5 | 30.2 | 0.0 | 13.7 | 0.7 | 2.9 | 0.0 |
| Book Chapter | 68 | 48.5 | 32.4 | 8.8 | 2.9 | 0.0 | 4.4 | 2.9 |
| Other Journal / Books | 100 | 58.0 | 22.0 | 4.0 | 5.0 | 1.0 | 8.0 | 2.0 |
| Total | 819 | 55.2 | 22.0 | 5.3 | 7.4 | 1.2 | 8.3 | 0.6 |

this might influence the sorts of findings that they considered to be "reasonable" and thus worthy of publication. We therefore cross-tabulated these findings based on the academic discipline with which publications outlets were affiliated.

The breakdown of findings shown in Table 7.20 indicates that certainty results are strongly related to the academic discipline of the journal publishing the results, and thus presumably the discipline of the scholars publishing in those journals. There is a radical divergence between the results published in economics journals and those published in criminology/criminal justice journals. The vast majority (70 percent) of macro-level findings published in economics journals support a significant deterrent effect of punishment certainty, while less than 27 percent of those published in CCJ journals are supportive. Findings published in sociology journals are also less supportive of a deterrent effect than those published in economics journals, but more supportive than those published in criminology/criminal justice journals.

We have now isolated the body of research that most consistently supports the deterrence doctrine. The very core of support for deterrence lies within the set of macro-level findings on the impact of certainty of punishment generated by economists. While most individual-level findings have failed to support either general deterrence or specific deterrence (Chapters 5 and 6) and most macro-level findings on severity of punishment likewise fail to find support for a deterrent effect (Table 7.14), most macro-level analyses of the impact of punishment certainty do support the deterrence doctrine, especially when conducted by economists.

### *Macro-Level Severity of Punishment Findings by Severity Measure*

Researchers have used a host of measures to test macro-level certainty of punishment, but only two have been frequently used to test the effects of severity of punishment. "Sentence length" is computed as the average number of years, months, or days to which criminal defendants were sentenced in court, regardless of how much time they served, while "time served" is usually measured as the average number of years, months, or days that released inmates actually served in prison. The differences in these two measures may seem subtle, but scholars have disputed over which measure is more suitable for deterrence research. One could argue that time served better reflects the severity of punishment actually inflicted, which is what is assumed to produce specific deterrent effects on the criminals who are punished. On the other hand, sentence lengths imposed on defendants in court are more likely to be publicized than are the times that defendants eventually serve in prison, so prospective offenders in general should be more aware of, and more likely to be deterred by, longer sentences initially imposed (Erickson and Gibbs 1975, 1976; Farrington, Langan and Wikstrom 1994; Mustard 2003; Tittle 1969; Wolpin 1978). The average time served may function as the better aggregate index of punishment experienced and thus is the quantity more relevant to any special deterrent effects of the experience of punishment among punished criminals, while severity of sentences imposed by judges, and sometimes widely reported in news outlets, is the quantity more relevant to any general deterrent effects that punishment severity may have on the population as a whole.

Table 7.21 indicates that there has been somewhat greater support for a deterrent effect of punishment severity levels when analysts used the time served severity measure, though no commonly used severity measure had a significant negative association with crime rates in more than a small minority of tests. Supportive findings based on analyses using the time served measure could reflect the aggregate effects of specific deterrence if longer prison sentences deterred better than short ones. This is not, however, what most individual-level research on the effects of experienced punishment has found (Chapter 6). Macro-level research has provided even less support for an effect of imposed sentence lengths, undercutting the hypothesis of a general deterrent effect of punishment severity.

Table 7.22 shows that findings on the effect of severity of punishment follow the same pattern regarding academic discipline as did the findings on certainty of punishment. Analyses published in economics journals are far more likely to support a deterrent effect of punishment severity than those published in other disciplines' journals. Indeed, such findings are rare outside the pages of economics journals, and the disciplinary differences are even more pronounced for severity findings than for certainty findings (compare with Table 7.20). Because of these sharp differences between disciplines, the impression that scholars with only a casual interest in deterrence are likely to formulate about empirical support for the effects of punishment on crime could be heavily determined

**TABLE 7.21** Macro-level severity findings by type of severity measure

| *Severity Measure* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Time Served | 188 | 31.4 | 26.6 | 0.5 | 20.2 | 0.5 | 13.8 | 6.9 |
| Imposed Sentence Length | 85 | 21.2 | 44.7 | 0.0 | 22.4 | 3.5 | 1.2 | 7.1 |
| Other Measures | 6 | 50.0 | 16.7 | 33.3 | 0.0 | 0.0 | 0.0 | 0.0 |
| Total | 279 | 28.7 | 31.9 | 1.1 | 20.4 | 1.4 | 9.7 | 6.8 |

**TABLE 7.22** Macro-level severity findings by academic discipline of journal

| *Academic Discipline* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Criminology/CJ | 54 | 3.7 | 38.9 | 0.0 | 5.6 | 1.9 | 27.8 | 22.2 |
| Economics | 103 | 39.8 | 34.0 | 1.9 | 20.4 | 1.9 | 1.0 | 1.0 |
| Sociology | 67 | 17.9 | 20.9 | 1.5 | 43.3 | 1.5 | 14.9 | 0.0 |
| Book Chapter | 23 | 39.1 | 56.5 | 0.0 | 4.3 | 0.0 | 0.0 | 0.0 |
| Other Journal/Books | 32 | 50.0 | 18.8 | 0.0 | 9.4 | 0.0 | 3.1 | 18.8 |
| Total | 279 | 28.7 | 31.9 | 1.1 | 20.4 | 1.4 | 9.7 | 6.8 |

by which journals they read. It would be entirely understandable for an economist who read only economics journals to get the impression that there is overwhelming support for an effect of punishment certainty, and even considerable support for an effect of punishment severity, on crime and thus little reason to question their orthodox expectation that criminal behavior should decrease, other things being equal, if the cost of crime increases.

### The Impact of Arrest Rates on Crime

In addition to assessing the effects of levels of certainty or severity of punishment, macro-level researchers have also tested the impact of miscellaneous measures of punishment frequency that do not necessarily reflect either certainty or severity. For example, although the fraction of crimes resulting in arrest would be a measure of punishment certainty (and was accordingly classified in our previous reviews), the rates of arrests *per capita* has also been studied by some deterrence scholars, even though having more arrests per population does not necessarily mean that there are more arrests per crime and thus greater certainty of punishment. Some scholars, such as Decker and Kohfeld (1985), have hypothesized that the sheer frequency of arrests is more likely to affect the amount of deterrence than clearance rates or ratios of arrests over crimes, supposedly because criminals are more likely to be aware of general levels of arrests.

The patterns in Table 7.23, however, support the view that measures of the sheer per capita frequency of arrests are less likely to show deterrent effects than measures of the certainty of arrest. Only 35.2 percent of findings regarding arrest rates per capita were negative and significant (Table 7.23), compared to 58.6 percent of findings regarding the certainty of arrest (Table 7.15).

**TABLE 7.23** Macro-level findings on arrest rates and other punishment measures

| *Punishment Type* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Arrest Rates | 122 | 35.2 | 23.8 | 13.1 | 15.6 | 3.3 | 2.5 | 6.6 |
| Other Miscellaneous Measures | 10 | 10.0 | 50.0 | 0.0 | 0.0 | 0.0 | 30.0 | 10.0 |

## Summary of Review of Macro-Level Research

We observed the following patterns in our review of the macro-level research reviewed in this chapter:

1. Most macro-level findings do not support the deterrence hypothesis. That is, they do not show a significant negative association between aggregate punishment levels and crime rates (Tables 7.1 and 7.2). On the other hand, macro-level studies have been considerably more likely to appear to support the deterrence hypothesis than individual-level studies of the impact of either perceptions of punishment risk (Chapter 5) or the personal experience of being punished (Chapter 6). This is true regarding both the certainty and severity of punishment.
2. Macro-level support for deterrence is higher in the Northeast and Southern U.S. than in the West or Midwest and higher in other countries, with the exception of Canada, than in the U.S. (Table 7.4).
3. There have been only modest differences in findings relating to the units of analysis studied. Studies of metro areas were somewhat more likely to be supportive of crime-reducing

effects of punishment levels, while studies of states were somewhat less likely to be supportive (Table 7.5).

4. Simply lagging the punishment variable has been the most common procedure used to address the possible simultaneous relationship between macro-level crime and punishment levels. Few researchers have used the potentially superior instrumental variables (IV) methods, and those who used IV methods have been more likely to obtain results supporting the deterrence hypothesis (Table 7.6). Given that most individual-level findings fail to support deterrence, however, it is unlikely that it was the use of IV methods that produced stronger support for deterrence but rather some other feature of studies than happen to use IV methods.
5. Macro-level studies are dominated by cross-sectional and panel designs, both of which are more likely to support the deterrence hypothesis than time-series analyses (Table 7.7).
6. Analyses controlling for more variables have been more likely to support the deterrence hypothesis, but we cannot say whether the researchers who control for *more* variables control for the *right* variables, i.e. confounders (Table 7.8).
7. Our doubts about whether the correct variables were controlled for were supported by the fact that we found that only a small share (10.3 percent) of the macro-level findings were generated by analyses that controlled for the size of the incarcerated population. Thus, few studies allowed researchers to distinguish deterrent effects of punishment from its incapacitative effect. The majority of macro-level findings seemingly supporting deterrent effects of punishment may actually have been reflecting only the incapacitative effects of locking up many criminals. So far, however, analyses that controlled for incapacitation have been slightly more likely to yield results favorable to deterrence. Overall, only 5 percent of all macro-level findings support the deterrence hypothesis *and* controlled for the size of the incarcerated population (Table 7.9). Further, *no* macro-level studies have controlled for the level of public intolerance for crime, which almost certainly causes less crime and more punishment, contributing to a spurious negative association between punishment levels and crime rates.
8. Contrary to theory-based expectations, macro-level research is not substantially more likely to support deterrent effects of punishment levels on acquisitive crimes than on violent offenses (Table 7.11).
9. Of specific crime types studied, findings regarding rape and robbery were the most likely to support the deterrence hypothesis, while those regarding assault/battery, homicide, and motor vehicle theft were the least likely to support it. Thus, although there has been no pronounced difference in deterrence findings between acquisitive crimes in general and violent crimes in general, the evidence does suggest that homicide is less deterrable than other crimes. This is a pattern bearing strongly on the research we review in Chapter 8 regarding the deterrent effect of capital punishment on homicide (Table 7.12).
10. The academic discipline of the journal publishing research, and thus presumably the discipline of the scholars conducting the research, was strongly related to the findings of macro-level deterrence research. Findings reported in economic journals usually support the deterrence hypothesis, while those reported in all other publication outlets generally do not support it (Table 7.13). The theoretical commitment of economists to the general proposition that higher costs of crime should cause less crime, and specifically to rational choice theory and the principle of utility maximization, may have made it harder for them to accept evidence contradicting the deterrence doctrine (Chapter 3).
11. Most macro-level findings indicate that greater punishment severity does not deter more crime. This accords with previous reviews by Doob and Webster (2003) and the meta-analysis of studies of get-tough policies by Pratt and his colleagues (2006). There has been far more macro-level support for deterrent effects of certainty of punishment than of severity of

punishment. Swiftness of punishment has been almost completely ignored in the macro-level literature (Table 7.14).

12. Within the set of macro-level findings bearing on the effects of certainty of punishment, measures of the certainty of crime resulting in arrest have been most often tested and are the certainty measures most likely to show support for the deterrence hypothesis (Table 7.15). These findings, however, are at least partly an artifact of the use of ratio variables, which are probably unsuitable for assessing the deterrent effect of police activities.
13. As was true of macro-level findings as a whole, findings regarding certainty of punishment are not substantially more likely to support deterrence of acquisitive crime than of violent crimes. Although rarely studied, nonpredatory crimes like illicit drug use appear to be the offense types least deterrable by more certain punishment (Table 7.16).
14. The macro-level severity findings are characterized by patterns much like the certainty findings—studies are more likely to obtain significant negative associations between severity levels and crime rates when they control more variables, use panel or pooled cross-sections designs, employ instrumental variables methods, and are published in economics journals (Tables 17–20).
15. The most commonly tested measure of severity is average time served, and these analyses are the ones most likely to generate support for the hypothesis that more severe punishment causes more deterrence. Nevertheless, the vast majority of macro-level tests do not support the proposition that higher levels of punishment severity cause lower crime rates (Table 7.21).

## Conclusions

The evidence reviewed in Chapters 7 through 9 as a whole strongly indicates that greater severity of punishment does not deter crime any better than less severe punishment. Daniel Nagin (2013b) has summarized deterrence research on this point in a potentially misleading way, stating only that "the certainty of apprehension, not the severity of ensuing consequences is the more effective deterrent" (199). This wording leaves open the possibility that the evidence supports the view the greater severity of punishment *does* deter, but just not as much as certainty. In fact, the vast majority of tests indicate that greater severity of punishment has *no* deterrent effect, either by itself or when combined with more certain punishment.

Regarding *certainty* of punishment, however, there is a pronounced contrast between two large bodies of evidence. Most individual-level findings indicate that greater perceived certainty of legal punishment and more certain experienced punishment do not, on net, reduce criminal behavior (Chapters 5 and 6), yet most (55 percent) macro-level tests of the impact of punishment certainty levels on crime rates have yielded significant negative associations, consistent with a deterrent effect (Table 7.14).

Why have so many macro-level studies obtained significant negative associations between punishment certainty and crime, despite the fact that research on individual persons generally indicates that people are not responsive to perceived or experienced punishment certainty? We have identified at least four major methodological problems that are likely to contribute to misleading macro-level negative certainty/crime associations.

First, 90 percent of macro-level findings have been based on analyses that did not control for the size of the incarcerated population and thus could not separate deterrent effects of higher aggregate punishment levels from their incapacitative effects. The same areas and time periods characterized by higher arrest, conviction, and prison sentencing rates are also likely to have larger prison populations, and there is sound evidence that incarcerating more criminals reduces crime rates, albeit with sharply diminishing returns as the prison population grows (Chapter 10). Consequently, in the

90 percent of macro-level studies that do not control for incarceration populations, much of the macro-level certainty/crime association, and possibly all of it, could reflect an incapacitative effects of incarcerating many criminals rather than a deterrent effect of greater certainty of punishment.

Second, as discussed in Chapter 4, there is a problem with using the ratio variables commonly employed in macro-level studies of the impact of punishment certainty levels on crime rates. Punishment certainty, and especially the certainty of arrest, is usually measured as a ratio of punishment events (arrests, convictions, sentencings to prison, admissions to prison) over crimes known to the police, while the crime rate is measured as crimes divided by population. Errors in measuring crimes will therefore push the punishment certainty measures and crime rates in opposite directions, artificially contributing to a negative association. The same problem afflicts studies in which the dependent variable is a crime count rather than a per capita rate. This explains why certainty findings are more likely to be negative than severity findings—the latter, typically measured as average length of sentence imposed or served, does not enjoy the artificial help of this ratio artifact.

Third, no macro-level deterrence studies have controlled for the level of social condemnation of criminal behavior prevailing in populations of the areas and time periods studied. The level of moral disapproval of crimes almost certainly reduces criminal behavior through informal social control mechanisms (Chapter 4) and is positively correlated with levels of legal punishment because punishment is one of the ways that societies express their disapproval of crime. Thus, as far as we can tell so far, all negative punishment/crime associations observed in macro-level research could be entirely a product of the negative bias attributable to the universal failure to control this factor.

Finally, perhaps the most serious weakness of macro-level deterrence studies of all types is their failure to actually measure perceptions of the risk of punishment among prospective offenders. The deterrence doctrine is an essentially perceptual theory—deterrence is entirely due to prospective offenders perceiving the risks of being punished for committing crimes. The essential assumption of macro-level deterrence research is that aggregate measures of actual risks of legal punishment as measured by official statistics are adequate, albeit imperfect, proxies for the perceptions of legal risk prevailing in the population. The evidence to be presented in Chapter 9 strongly indicates that this assumption is almost certainly wrong. Indeed, virtually the entire body of macro-level research can be said to have no direct relevance to testing deterrence effects; at best it tests the overall net effect of punishment levels without respect to how they might be produced. This point is an old one (see, e.g. Jacob 1978), but it has evidently not been made forcefully enough to disabuse macro-level researchers of the belief that their research tests the deterrent effect of punishment.

These problems are so serious, and so pervasive in macro-level research, that they are probably sufficient all by themselves to account for nearly all of the apparent support for the proposition that higher levels of punishment cause lower crime rates. Until a substantial body of macro-level research avoiding these vital flaws is carried out, it would be imprudent to even weakly infer anything about the deterrent effects of punishment from extant work. Leaving aside the question of how higher punishment levels might influence crime rates, the conclusion that is most consistent with the full body of macro-level research findings is that neither greater certainty nor greater severity of actual punishment causes, on net, reductions in rates of the types of "street crime" that are examined in nearly all macro-level "deterrence" studies.

## References

Andenaes, Johannes. 1975. General prevention revisited: Research policy and implications. *Journal of Criminal Law and Criminology and Police Science* 66:338–365.

Bachman, Ronet, Raymond Paternoster, and Sally Ward. 1992. The rationality of sexual offending: Testing a deterrence/rational choice conception of sexual assault. *Law and Society Review* 26:343–372.

Baltagi, Badi. 2006. Estimating an economic model of crime using panel data from North Carolina. *Journal of Applied Econometrics* 21:543–547.

Brandt, Patrick, and Tomislav Kovandzic. 2015. Messing up in Texas? A re-analysis of the effects of executions on homicides. *PLOS One* 10 (9):e0138143. doi:10.1371/journal.pone.0138143.

Bursik, Robert, Harold Grasmick, and Mitchell Chamlin. 1990. The effect of longitudinal arrest patterns on the development of robbery trends at the neighborhood level. *Criminology* 28:431–450.

Cauley, Jon, and Eric Im. 1988. Intervention policy analysis of skyjackings and other terrorist incidents. *American Economic Review* 78:27–31.

Chambliss, William. 1967. Types of deviance and the effectiveness of legal sanctions. *Wisconsin Law Review* 3:703–719.

Chamlin, Mitchell. 1988. Crime and arrests: An autoregressive integrated moving average (ARIMA) approach. *Journal of Quantitative Criminology* 4:247–258.

Chamlin, Mitchell, Harold Grasmick, Robert Bursik, and John Cochran. 1992. Time aggregation and time lag in macro level deterrence research. *Criminology* 30:377–395.

Chauncey, Robert. 1975. Deterrence: Certainty, severity, and skyjacking. *Criminology* 12:447–473.

Chilton, Roland. 1982. Analyzing urban crime data: Deterrence and the limitations of arrests per offense ratios. *Criminology* 19:590–607.

Cook, Phillip. 1979. The clearance rate as a measure of criminal justice system effectiveness. *Journal of Public Economics* 11:135–142.

Decker, Scott, and Carol Kohfeld. 1985. Crimes, crime rates, arrests and arrest ratios: Implications for deterrence research. *Criminology* 23:437–450.

Doob, Anthony, and Cheryl Webster. 2003. Sentence severity and crime: Accepting the null hypothesis. *Crime and Justice* 30:143–195.

Dugan, Laura, Gary LaFree, and Alex Piquero. 2005. Testing a rational choice model of airline hijackings. *Criminology* 43:1031–1065.

Dusek, Libor. 2012. Crime, deterrence, and democracy. *German Economic Review* 13:447–469.

Ehrlich, Isaac, and Zhiqiang Liu. 1999. Sensitivity analyses of the deterrence hypothesis: Let's keep the econ in econometrics. *The Journal of Law and Economics* 42:455–487.

Enders, Walter, and Todd Sandler. 2000. Is transnational terrorism becoming more threatening? A time-series investigation. *Journal of Conflict Resolution* 44:307–332.

Enders, Walter, Todd Sandler, and Jon Cauley. 1990. UN conventions, technology and retaliation in the fight against terrorism: An econometric evaluation. *Terrorism and Political Violence* 2:83–105.

Erickson, Maynard, and Jack Gibbs. 1975. Specific versus general properties of legal punishments and deterrence. *Social Science Quarterly* 56:390–397.

Erickson, Maynard, and Jack Gibbs. 1976. Further findings on the deterrence question and strategies for further research. *Journal of Criminal Justice* 4:175–189.

Espenshade, Thomas. 1994. Does the threat of border apprehension deter undocumented U.S. immigration? *Population and Development Review* 2:871–892.

Farrington David, Patrick Langan, and Per-Olof Wikstrom. 1994. Changes in crime and punishment in America, England and Sweden between the 1980s and the 1990s. *Studies on Crime and Crime Prevention* 3:104–131.

Fisher, Franklin, and Daniel Nagin. 1978. On the feasibility of identifying the crime function in a simultaneous model of crime rates and legal sanction levels. In *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, eds. Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin. Washington, DC: National Academy of Sciences.

Gathmann, Christina. 2008. Effects of enforcement on illegal markets: Evidence from migrant smuggling along the Southwest border. *Journal of Public Economics* 92:1926–1941.

Gove, Walter, Michael Hughes, and Michael Geerken. 1985. Are uniform crime reports a valid indicator of the Index crimes? *Criminology* 23:451–502.

Greenberg, David. 1981. Methodological issues in survey research on the inhibition of crime: Comment on Grasmick and Green. *The Journal of Criminal Law and Criminology* 72:1094–1101.

Greenberg, David, Ronald Kessler, and Charles Logan. 1979. A panel model of crime rates and arrest rates. *American Sociological Review* 44:843–850.

Hoekstra, Mark, and Sandra Orozco-Aleman. 2014. *Illegal Immigration, State Law, and Deterrence.* Cambridge, MA: National Bureau of Economic Research.

Jacob, Herbert. 1978. Rationality and criminality. *Social Science Quarterly* 59:584-585.

Jacob, Herbert. 1980. Deterrent effects of formal and informal sanctions. *Law and Policy Quarterly* 2:61–80.

Kane, Robert. 2006. On the limits of social control: Structural deterrence and the policing of "suppressible" crimes. *Justice Quarterly* 23:186–212.

Kessler, Daniel, and Steven Levitt. 1999. Using sentence enhancements to distinguish between deterrence and incapacitation. *The Journal of Law and Economics* 42:343–363.

Kleck, Gary, and J. C. Barnes. 2013. Deterrence and macro-level perceptions of punishment risks: Is there a collective wisdom? *Crime and Delinquency* 59:1006–1035.

Kleck, Gary, and J. C. Barnes. 2014. Do more police generate more crime deterrence? *Crime and Delinquency* 60:716–738.

Klein, Lawrence, Brian Forst, and Victor Filatov. 1978. The deterrent effect of capital punishment: An assessment of the estimates. In *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, eds. Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin. Washington, DC: National Academy of Sciences.

Landes, William. 1978. An economic study of U.S. aircraft hijacking 1961–1976. *Journal of Law and Economics* 21:1–31.

Levitt, Steven. 1997. Using electoral cycles in police hiring to estimate the effect of police on crime. *American Economic Review* 87:270–290.

Light, Roy, Claire Nee, and Helen Ingham. 1993. *Car Theft: The Offender's Perspective*. Home Office Research Study No. 130. London: Her Majesty's Stationery Office.

Listoken, Yair. 2003. Does more crime mean more prisoners? An instrumental variables approach. *Journal of Law and Economics* 46:181–206.

Marvell, Thomas, and Carlisle Moody. 1996. Specification problems, police levels and crime rates. *Criminology* 34:609–646.

Mendes, Silvia. 2004. Certainty, severity, and their relative deterrent effects: Questioning the role of risk. *Policy Studies Journal* 32:59–74.

Miethe, Terance, Richard McCorkle, and Shelley Listwan. 2006. *Crime Profiles: The Anatomy of Dangerous Places, Persons, and Situations*. New York: Oxford University Press.

Minor, William. 1975. Skyjacking crime control models. *Journal of Criminal Law and Criminology* 66:94–105.

Mustard, David. 2003. Reexamining criminal behavior: The importance of omitted variable bias. *The Review of Economics and Statistics* 85:205–211.

Nagin, Daniel. 1978. General deterrence: A review of the empirical evidence. In *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, eds. Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin. Washington, DC: National Academy Press.

Nagin, Daniel. 2013. Deterrence in the twenty-first century. *Crime and Justice* 42:199–263.

Nagin, Daniel, Robert M. Solow, and Cynthia Lum. 2015. Deterrence, criminal opportunities, and police. *Criminology* 53:74–100.

Owens, Emily. 2009. More time, less crime? Estimating the incapacitative effect of sentence enhancements. *Journal of Law and Economics* 52:551–579.

Pratt, Travis, Francis Cullen, Kristie Blevins, Leah Daigle, and Tamara Madensen. 2006. In *Taking Stock: The Status of Criminological Theory*, eds. Francis T. Cullen, John Paul Wright, and Kristie R. Blevins, 367–395. New Brunswick, NJ: Transaction.

Rubin, Allen, and Earl Babbie. 2009. *Essential Research Methods for Social Work* (2nd ed.). Boston: Centage Learning.

Sampson, Robert. 1986. Crime in cities: The effects of formal and informal social control. *Crime and Justice* 8:271–311.

Selke, William. 1983. Celerity: The ignored variable in deterrence research. *Journal of Police Science and Administration* 11:31–37.

Shavell, Steven. 2014. *A Simple Model of Optimal Deterrence and Incapacitation*. Working Paper Series, National Bureau of Economic Research, No. 20747.

Sherman, Lawrence, and David Weisburd. 1995. General deterrent effects of police patrol in crime hot spots: A randomized controlled trial. *Justice Quarterly* 12:625–648.

Speckart, George, Douglas Angelia, and Elizabeth Deschenes. 1989. Modeling the longitudinal impact of legal sanctions on narcotics use and property crime. *Journal of Quantitative Criminology* 5:33–56.

Tittle, Charles. 1969. Crime rates and legal sanctions. *Social Problems* 16:409–423.

Tittle, Charles, and Alan Rowe. 1974. Certainty of arrest and crime rates: A further test of the deterrence hypothesis. *Social Forces* 52:455–462.

Valente, Thomas. 2002. *Evaluating Health Promotion Programs*. Oxford: Oxford University Press.

Wilson, James, and Barbara Boland. 1978. The effect of the police on crime. *Law and Society Review* 12:367–390.

Wilson, James, and Barbara Boland. 1982. The effect of police on crime: A response to Jacob and Rich. *Law and Society Review* 16:163–169.

Wolpin, Kenneth. 1978. An economic analysis of crime and punishment in England and Wales, 1894–1967. *Journal of Political Economy* 86:815–840.

Woodland, Alan, and Chisato Yoshida. 2006. Risk preference, illegal immigration and immigration policy. *Journal of Development Economics* 81:500–513.

# 8

# THE IMPACT OF CAPITAL PUNISHMENT ON MURDER RATES

## The Issues

The most severe of all legal punishments is the death penalty. Although the laws of many nations technically provide this as a possible punishment for a wide variety of crimes, such as treason, espionage, rape, and other extremely serious crimes, capital punishment in the U.S., and most other nations that provide for the death penalty, is actually imposed almost exclusively for murders of some sort. And even among murders, only a small subset are realistically eligible for the death penalty—primarily those that are in some sense aggravated, by virtue of the offender's premeditation or unusual cruelty, or the nature of the victim or victims (especially a child victim or multiple victims). Thus, the debate over the impact of this specific punishment on crime almost entirely revolves around its effect on murder. More specifically, because the research exclusively studies aggregates like the populations of states or cities it is almost entirely concerned with the death penalty's impact on murder *rates*.

Murder will not go unpunished in the absence of executions. Without capital punishment, convicted murderers who otherwise would have qualified for a death sentence instead receive a long prison sentence. Thus, the key issue regarding its impact on murder is not whether the death penalty has any absolute deterrent effect, i.e. any effect compared to no punishment, but rather whether any additional deterrence is achieved with capital punishment than in its absence. The effect that capital punishment exerts above and beyond whatever is generated by virtue of long prison sentences is referred to as its "unique" deterrent effect, and it should be tested with the deterrent effects of imprisonment being somehow controlled, e. g., by controlling for (a) the probability of a convicted murder being sentenced to prison and (b) the average length of prison sentences imposed or served for murder. Many death penalty deterrence studies fail to include such controls, which means their estimates of impact are likely to reflect a combination of the deterrent effects, if any, of both long sentences and executions, making it impossible to conclude anything about the unique deterrent effect of executions.

Further, the vast majority of studies fail to control for the size of the prison population and thus fail to separate the incapacitative effects of imprisonment from the deterrent effect of the death penalty. The same times and places that have a death penalty statute, a higher risk of death sentences or executions, or more executions also tend to imprison more criminals, and the size of the prison population has its own homicide-reducing effect (Chapter 10). Therefore, studies that fail to control for the prison population will confound the deterrent effect of the death penalty with the greater collective incapacitative effects of larger prison populations. There are virtually no death penalty studies that control for both the probability and length of prison sentences for homicide and for the size of the prison population. Consequently, we believe that the vast majority of death penalty studies have little of a credible nature to say on the question of whether capital punishment exerts any unique deterrent effect on homicide.

There is, however, an even more fundamental problem that afflicts *all* of these studies. They purportedly test for deterrent effects, which are wholly dependent on perceptions of the risk of execution, yet not one of the studies directly measures these perceptions. The studies are all macro-level studies of homicide frequency in large aggregates such as states. Perceptions of risk are merely proxied by the actual, objective risks of execution. As we will see in Chapter 9, however, the best available evidence indicates that there is no association between the actual certainty, severity, and swiftness of legal punishments, such as imprisonment, and perceptions of those risks. While the death penalty could prove to be an exception, at this point there is no sound empirical basis for believing that actual risks of execution for aggregates like states can serve as even approximately accurate proxies of perceived risks of execution risk. Thus, it is fair to say that none of the studies in this area have directly tested for the unique deterrent effect of the death penalty, and it is questionable whether any have even indirectly tested for it.

A National Research Council review published in 2012 yielded a similar conclusion. After reviewing hundreds of empirical studies and detailing their flaws, the Committee on Deterrence and the Death Penalty concluded that "research to date on the effect of capital punishment on homicide in not informative about whether capital punishment decreases, increases, or has no effect on homicide rates." The Committee laid special stress on two flaws in the research that led them to this skeptical position: (1) the failure to control for other aspects of the "sanction regime for homicide," and (2) the "failure to pose a credible model of the sanction risk perceptions of potential murderers and the behavioral response to such perceptions." We agree with the Committee that these two deficiencies alone are "sufficient to make existing studies uninformative about the effect of capital punishment" (101). The authors went on to note that both deficiencies were potentially correctable. We later summarize a recent study, not available to the Committee, that goes a long way towards reducing both deficiencies.

## Findings on the Deterrent Effect of Capital Punishment

Keeping in mind these serious deficiencies, Table 8.1 summarizes the findings of capital punishment deterrence studies, including findings regarding capital punishment publicity As in previous chapters, each "finding" is a statistically independent or substantively distinct test of the hypothesis. This table includes findings for which the authors did not report levels of statistical significance. The findings as a whole show little support for a unique deterrent effect of the death penalty—only 16.8 percent of the tests of death penalty deterrence yielded the significant negative associations that are supportive of a deterrent effect. On the other hand, even fewer findings support a

**TABLE 8.1** Summary of all capital punishment deterrence findings

| *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|
| | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 518 | 16.8 | 34.6 | 2.5 | 25.5 | 5.2 | 5.4 | 10.0 |

"brutalizing" or other homicide-increasing effect of capital punishment, as only 5.2 percent of findings were positive and significant. Most findings have been consistent with the conclusion that the death penalty exerts no detectable net effect on homicide rates.

We tested whether this distribution of findings was sensitive to the inclusion of studies that contributed disproportionately large numbers of findings, thereby exerting undue influence on the overall results. Table 8.2 excludes findings from studies that contributed over ten findings; 188 findings were omitted on this basis. These exclusions caused support for death penalty deterrence to increase slightly. This shift reflects the influence of the work of Bailey (1991, 1998), Lempert (1983), and Peterson and Bailey (1988) who all reported very large numbers of tests in their deterrence studies, and who generally did not find evidence of deterrence. Although not documented in table form, we also tabulated findings when studies that contributed 15 or more findings were excluded. The results were very similar to those based on all capital punishment findings. With or without studies reporting many findings, the whole body of evidence generally does not find a significant negative association between capital punishment and crime.

For the sake of completeness, we also cross-classified capital punishment findings with regard to the crimes they examined. The vast majority of findings concern either homicide or murder, but there are a few findings bearing on other violent crimes and even property crime. Table 8.3 shows that findings were more likely to support a deterrent effect on murder than other crimes, consistent with the commonsensical expectation that the death penalty should affect the crimes for which perpetrators could actually receive a death sentence (Bailey 1980a).

While murder as a whole might be affected more by the threat of capital punishment than the more general measure of homicide, only *first degree* (or "capital") murders are actually eligible for the death penalty. Thus, one reasonable expectation would be that first degree murders are the type of killings most likely to be deterred by capital punishment. Unfortunately, few studies have separately tested for effects on different types of homicide. For instance, only Bailey (1974, 1975, 1976, 1983a) compared the effects of executions on first degree murder, homicide, and second degree

**TABLE 8.2** Capital punishment findings when studies with 10+ findings are excluded

| *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|
| | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 330 | 20.3 | 39.4 | 3.9 | 24.2 | 5.5 | 2.7 | 3.9 |

**TABLE 8.3** Capital punishment findings categorized by crime type

| *Crime Type* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Murder | 130 | 30.0 | 36.2 | 2.3 | 20.8 | 5.4 | 0.0 | 5.4 |
| Homicide | 304 | 14.1 | 30.9 | 1.3 | 26.3 | 5.3 | 7.2 | 14.8 |
| Violent | 58 | 5.2 | 48.3 | 8.6 | 25.9 | 6.9 | 5.2 | 0.0 |
| Property | 25 | 8.0 | 36.0 | 4.0 | 40.0 | 0.0 | 12.0 | 0.0 |
| General | 1 | 0.0 | 100.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| Total | 518 | 16.8 | 34.6 | 2.5 | 25.5 | 5.2 | 5.4 | 10.0 |

murder, finding executions to have no significant association with any of these three homicide types. Past researchers have noted this weakness in the literature while acknowledging the paucity of available data to distinguish types of homicide subject to the death penalty from types not eligible for death sentences (e.g., Sellin 1959). The data limitation has forced most capital punishment researchers to simply assume that rates of general murder or homicide are valid proxies for rates of first degree or capital murder (Bailey 1980a).

A few researchers have tested capital punishment effects on different types of murder distinguished by other, nonlegal dimensions. Shepherd (2004) asserted that the certainty of receiving a death sentence has greater deterrent effects on intimate and acquaintance murders than on stranger murders, finding significant negative associations of death sentences with passion murders and insignificant negative associations with felony murders (which result from premeditated crimes like robbery, even if the killing itself is not likely to have been planned). This is a puzzling set of results from one theoretical standpoint—deterrence is supposed to work more strongly with premeditated crimes, since premeditation increases the likelihood of prospective offenders considering the risks of crime, while "passion" murders among intimates are usually unpremeditated. Bailey (1998), on the other hand, found no difference between the impact of executions on stranger and nonstranger killings and did not find consistent significant impacts for either category. Peterson and Bailey (1991) obtained findings similar to those of Bailey, while Thomson (1997) obtained findings suggesting a brutalization effect of an Arizona execution on argument-instigated homicides (which are generally unpremeditated) but no effect on felony homicides.

## Findings by Execution Risk Measures Used

Including capital punishment publicity, there been five basic measures of capital punishment employed in capital punishment studies, shown in Table 8.4. Some analysts measured execution certainty by calculating ratios of executions to homicides (Avio 1979; Bailey 1976, 1977; 1979b, 1979c, 1980a, 1983b; Black and Orsagh 1978; Boyes and McPheters 1977; Cloninger 1987; Cover and Thistle 1988; Lester 1993; Lott and Landes 1999; Yunker 2001), executions to murders (Bailey 1979a, 1980b, 1982, 1984), executions to arrests (Bailey 1990; Zimmerman 2004), executions to convictions (Boyes and McPheters 1977; Bowers and Pierce 1975: Cantor and Cohen 1980; Cloninger 1994; Ehrlich 1975, 1977; Veal 1992; Wolpin 1978), executions to prison admissions (Bailey 1983a), executions to death penalty sentences (Dezhbakhsh, Rubin, and Shepherd 2003; Jarrell and Howsen 1990; Narayan and Smyth 2006), and even different methods of execution (Ekelund,

**TABLE 8.4** Capital punishment findings categorized by measure of death penalty risk

| *Execution Measure* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Execution Certainty | 150 | 24.0 | 50.0 | 0.0 | 22.7 | 2.0 | 0.7 | 0.7 |
| Number of Executions | 126 | 8.7 | 41.3 | 1.6 | 34.9 | 4.0 | 6.3 | 3.2 |
| Death Sentence Certainty | 50 | 34.0 | 26.0 | 6.0 | 16.0 | 2.0 | 0.0 | 16.0 |
| Swiftness of Execution | 16 | 31.3 | 12.5 | 0.0 | 31.3 | 25.0 | 0.0 | 0.0 |
| Execution Publicity | 63 | 11.1 | 23.8 | 3.2 | 44.4 | 17.5 | 0.0 | 0.0 |
| Other Measures | 113 | 9.7 | 19.5 | 5.3 | 11.5 | 2.7 | 16.8 | 34.5 |
| Total | 518 | 16.8 | 34.6 | 2.5 | 25.5 | 5.2 | 5.4 | 10.0 |

Jackson, Ressler, and Tollison 2006; Zimmerman 2006). There were similar ratio measures of death *sentence* certainty. Other scholars measured the sheer number of executions or per capita rate of executions. Swiftness of execution has typically been measured by time elapsed from death sentence to execution (Shepherd 2004). Finally, some researchers have related the volume of news coverage of executions to homicide rates.

Table 8.4 shows that execution certainty has been the most commonly used measure of death penalty risk, followed by number of executions, execution publicity, and death sentence certainty. Tests of the effect of death sentence certainty and execution certainty were most likely to support the deterrence hypothesis, although most of these tests did not yield a significant negative association with homicide. Among certainty findings, those pertaining to the certainty of *death sentence* were somewhat more likely to support deterrence than those pertaining to certainty of *executions*. This is theoretically reasonable if one assumes that people are more aware of, and influenced by, the sentences handed down for murderers than they are regarding the actual carrying out of the executions.

## Publication Discipline and Capital Punishment Deterrence Findings

The impact of the discipline of the publishing journal was analyzed in previous chapters. Academic discipline appeared to have little relationship to the individual-level findings on deterrence reviewed in Chapters 5 and 6, but was strongly related to the findings of the macro-level deterrence research reviewed in Chapter 7. Therefore, it would not be surprising if macro-level death penalty findings likewise differ substantially across disciplines. Table 8.5 shows that findings published in economics and law journals have been substantially more likely to support capital punishment deterrence than those published in criminology/criminal justice or sociology journals. Studies reported in criminology/criminal justice journals also find more support for a possible brutalization effect, whereby the death penalty increases homicide, than studies published in economics or law journals.

Differences in findings across disciplines deserve more attention than they have heretofore received, and they raise questions about the methodologies used and the objectivity of those on both sides of the debate. Many researchers have been highly critical of the economists' findings, particularly those of Ehrlich (1975). Indeed, even some economists have been critical of the econometric approach to death penalty research (Black 1982; Hendry 1980; Leamer 1983; Pratt and Schlaifer 1979), with economist Edward Leamer going so far as to urge, "Let's take the con out of econometrics," criticizing

**TABLE 8.5** Capital punishment findings categorized by publication field

| *Publication Field/ Type* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *− sig* | *− ns* | *? ns* | *+ ns* | *+ sig* | *− p = ?* | *+ p = ?* |
| Criminology/CJ | 217 | 6.0 | 22.6 | 4.6 | 27.6 | 6.9 | 8.8 | 23.5 |
| Economics | 105 | 35.2 | 35.2 | 2.9 | 17.1 | 2.9 | 6.7 | 0.0 |
| Sociology | 69 | 8.7 | 58.0 | 0.0 | 26.1 | 5.8 | 1.4 | 0.0 |
| Law | 51 | 37.3 | 45.1 | 0.0 | 15.7 | 2.0 | 0.0 | 0.0 |
| Book Chapter | 32 | 0.0 | 37.5 | 0.0 | 56.3 | 6.3 | 0.0 | 0.0 |
| Working Paper/ Book | 22 | 27.3 | 22.7 | 0.0 | 40.9 | 9.1 | 0.0 | 0.0 |
| Other Journal | 22 | 27.3 | 59.1 | 0.0 | 4.5 | 0.0 | 4.5 | 4.5 |
| Total | 518 | 16.8 | 34.6 | 2.5 | 25.5 | 5.2 | 5.4 | 10.0 |

econometrics as too reliant on incomplete data and based more on fragile assumptions than on reality (Leamer 1983). Some criminologists and sociologists have likewise been critical of the techniques of econometrics that yielded support for death penalty deterrence, but then relied themselves on similar statistical techniques in finding support for a brutalization effect.

## Death Penalty Deterrence Findings by Unit of Analysis and Region

Table 8.6 shows that the majority of findings on the deterrent effect of capital punishment were based on research in which the units of analysis were either states (typically all, or nearly all, of the states) or the entire U.S. in time series analyses. Findings based on U.S. states or the entire nation have been more likely to support the deterrence hypothesis than those based on data pertaining to specific regions. It is debatable whether it is appropriate to use entire nations as the unit of analysis in death penalty studies since it is questionable whether capital punishment in one state or region would affect murder in all or most other states or regions in the nation. Further, although some state-level studies separate abolitionist and death penalty states in the U.S. (Bailey 1974, 1975, 1990; Choe 2009; Lin 2008; Manski and Pepper 2013; Peterson and Bailey 1991), studies of the entire nation obviously cannot make this distinction.

Examining only studies focused on particular regions, we find greater support for death penalty deterrence in analyses based on the Southern U.S. than those based on the Midwest. Indeed, there is virtually no support for deterrence in the Midwest and some support for a brutalization effect in both the Midwest and the South. Due to the rare use of the death penalty in the Northeast U.S., it is not surprising that there have been few deterrence studies limited to that region. We reviewed only a few studies of foreign nations, largely due to our limitation of the review to studies written in English.

**TABLE 8.6** Capital punishment findings categorized by unit of analysis and region

| *Region of Analysis* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| National Sample U.S. | 90 | 17.8 | 35.6 | 6.7 | 26.7 | 2.2 | 8.9 | 2.2 |
| Multiple States, U.S. | 207 | 20.3 | 43.0 | 1.0 | 26.1 | 4.8 | 0.5 | 4.3 |
| Multiple Other Areas, U.S. | 14 | 57.1 | 0.0 | 14.3 | 21.4 | 7.1 | 0.0 | 0.0 |
| Northeast U.S. | 12 | 25.0 | 16.7 | 8.3 | 41.7 | 0.0 | 0.0 | 8.3 |
| Midwest U.S. | 53 | 1.9 | 37.7 | 3.8 | 41.5 | 15.1 | 0.0 | 0.0 |
| South U.S. | 49 | 18.4 | 36.7 | 0.0 | 34.7 | 10.2 | 0.0 | 0.0 |
| West U.S. | 53 | 5.7 | 20.8 | 0.0 | 9.4 | 0.0 | 15.1 | 49.1 |
| Other Countries | 38 | 13.2 | 15.8 | 0.0 | 2.6 | 2.6 | 28.9 | 36.8 |
| Multiple Countries | 2 | 0.0 | 50.0 | 0.0 | 50.0 | 0.0 | 0.0 | 0.0 |
| Total | 518 | 16.8 | 34.6 | 2.5 | 25.5 | 5.2 | 5.4 | 10.0 |

## Research Design

Scholars disagree on the likely timing and persistence of any deterrent effects the death penalty might have (Mocan and Gittings 2001; Shepherd 2005). They have debated whether executions will deter homicide shortly after they are carried out (Land, Teske, and Zheng 2009; Lester 1980; Shepherd 2004; Phillips 1980) or whether they will have a lagged effect that may take weeks, months, or even years to become evident (Land et al. 2009; Lester 1980; McFarland 1983). Likewise, they have disputed how long the effect will last (Archer, Gartner, and Beittel 1983; Bailey 1990; Peterson and Bailey 1991). If deterrent effects are fairly immediate and last only for short periods, only longitudinal designs using small time units like days or perhaps weeks may be able to detect them. Studies analyzing one-year units of time may miss short-lived deterrent effects. To test these possibilities, we classified findings by whether they were generated by research using a cross-sectional or longitudinal design, and by the length of temporal unit used.

Table 8.7 shows that longitudinal analyses have been more likely to find support for deterrent effects cross-sectional analyses. This analysis does not include publicity studies, due to the fundamentally different nature of their longitudinal analyses.

One caveat about our review should be mentioned at this point. Because we focused on the "overall" findings of each analysis, we did not separately count every single slightly different finding in each study. This has implications for our counts of findings, particularly regarding longitudinal studies and disputes over the timing of deterrent effects of executions. For instance, if analysts estimated models testing for effects in different time periods and one model showed support for deterrence between three and six months after executions but the other models showed no support for periods zero to three months, six to nine months, or nine to twelve months after executions, we classified the overall effect as no deterrent effect for that study. We acknowledge, however, that the authors of such a study may have interpreted that set of findings to mean that capital punishment does deter murder or homicide, but only within a certain period of time. The timing of possible deterrent effects of capital punishment is not addressed in our review since there was such a wide variety of methods for addressing the issue in prior research and so few findings based on each approach. This is clearly an issue that needs greater systematic attention in future research.

We can, however, break down findings by the temporal unit of analysis used to generate each finding. Table 8.8 shows that one-year time periods have been by far the most common temporal unit used in the death penalty research, with much less use of weeks or months. Findings based on

**TABLE 8.7** Capital punishment findings (excluding publicity) categorized by research design

| *General Research Design* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Longitudinal | 334 | 20.1 | 33.8 | 2.1 | 19.8 | 2.7 | 8.4 | 13.2 |
| Cross-Sectional | 121 | 10.7 | 42.1 | 3.3 | 31.4 | 5.8 | 0.0 | 6.6 |
| Total | 455 | 17.6 | 36.0 | 2.4 | 23.9 | 3.5 | 6.2 | 11.4 |

**TABLE 8.8** Capital punishment findings categorized by temporal unit of analysis

| *Temporal Unit of Analysis* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Over One Year | 11 | 0.0 | 9.1 | 0.0 | 0.0 | 0.0 | 45.5 | 45.5 |
| Year | 397 | 18.9 | 34.0 | 2.5 | 23.2 | 4.3 | 5.5 | 11.5 |
| Month | 45 | 13.3 | 35.6 | 0.0 | 42.2 | 4.4 | 2.2 | 2.2 |
| Week | 52 | 5.8 | 38.5 | 5.8 | 34.6 | 15.4 | 0.0 | 0.0 |
| Day | 13 | 23.1 | 53.8 | 0.0 | 23.1 | 0.0 | 0.0 | 0.0 |
| Total | 518 | 16.8 | 34.6 | 2.5 | 25.5 | 5.2 | 5.4 | 10.0 |

**TABLE 8.9** Capital punishment findings categorized by number of independent variables

| *Number of Independent Variables* | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| 0–5 | 188 | 9.6 | 25.5 | 5.3 | 14.9 | 3.2 | 14.4 | 27.1 |
| 6 or More | 330 | 20.9 | 39.7 | 0.9 | 31.5 | 6.4 | 0.3 | 0.3 |
| Total | 518 | 16.8 | 34.6 | 2.5 | 25.5 | 5.2 | 5.4 | 10.0 |

daily observations were the most likely to support the deterrence hypothesis, those based on months were somewhat less likely to support it, and those based on weekly data rarely supported it. It is possible that executions have very short-term effects, lasting no more than a few days. These might not be detected in studies using longer time units because the deterred murders would be such small fractions of the total homicide counts for months or years. Nevertheless, these conclusions are fragile because of the fairly small numbers of findings based on the shorter temporal units. Clearly, more research using shorter time periods is called for.

One of the main criticisms of capital punishment deterrence research is that the existing data are insufficient to allow analysts to estimate realistic models of homicide and thereby isolate the deterrent effects of capital punishment. We therefore have cross-classified findings based on the number of other independent variables controlled. Table 8.9 shows that findings based on analyses with six or more independent variables in their models were more likely to support the death penalty deterrence hypothesis. Much of this difference is due to the large number of bivariate correlation coefficients within the five-or-fewer category that did not provide significance scores. If findings of unknown significance are eliminated, findings based on tests with six or more variables are only slightly more likely (21.0 percent vs. 16.4 percent) to support the deterrence hypothesis. This suggests that if the deterrence hypothesis is being hampered by a lack of data for control variables, it is more likely due to problems as to which variables were controlled rather than their sheer quantity.

As in other realms of inquiry, it is logically impossible to "prove a negative" with regard to the deterrent effect of the death penalty. There is, however, a specific reason to make this point in connection with this particular issue. It is possible that the existence of death penalty statutes, and thus the mere theoretical possibility of execution following murder, deters some prospective killers from killing. There could be some baseline unique deterrent effect that does not covary with the frequency, certainty, or level of publicity surrounding executions. Indeed, this effect theoretically could even operate in jurisdictions that do not actually have a death penalty, and thus no actual possibility of an execution, based entirely on the misperceptions of some prospective killers that there is an operative death penalty statute in their area. Given the ample evidence of substantial ignorance about punishment of crime (Chapter 9), it would scarcely be surprising that large segments of the population could be ignorant of even this basic fact. In this light, one could never completely rule out the possibility of the death penalty exerting some unique deterrent effect above and beyond that generated by long prison sentences.

## The Effects of Publicity About Executions

Research on the deterrent effect of the death penalty is perhaps most noteworthy for how indirect it is. Perceptions of execution risk are never directly measured. Murder is an extremely rare crime, so killers, and people likely to kill, are correspondingly rare. Consequently, it is not feasible to use standard sample surveys to measure both (a) individual perceptions of the risk of being executed for murder and (b) homicidal behavior. Even if respondents were willing to admit to murder, there would be virtually no variation on the dependent variable (murder behavior) since few or no killers would be included in a standard probability sample of the general population. It is therefore understandable that all death penalty deterrence research is macro-level research, in which the existence of death penalty statutes or the occurrence or rate of executions is related to rates or counts of homicides, and a perceptual connection between higher objective execution risk and lower homicide is simply assumed. This is highly problematical since, as we will see in Chapter 9, there is serious reason to doubt whether there in fact is any relationship between actual risks of legal punishment and perceptions of those risks.

At present, there is no empirical evidence whatsoever indicating that perceptions of execution risk are strongly related to its actual probability or frequency. There is no direct evidence that people are aware of variations in execution risk, as distinct from merely being aware of the existence of the death penalty or the fact that executions have occurred at some time in the past. For example, no one has established with surveys whether people in states with more executions perceive a higher likelihood of execution for murderers or how accurate people are if asked whether any executions had been carried out in their state in the past 30 days. It would be perfectly feasible to carry out such tests, but no one has yet done so. Nevertheless, it is possible that variation in actual death penalty risk is more likely to shift perceptions of that risk than is the case with noncapital punishment because executions are more highly publicized than lesser punishments, and thus more likely to be perceived by prospective killers.

The closest researchers have come to measuring perceived execution risk is to measure the amount of publicity surrounding executions, based on the assumption that the more news coverage there is of executions, the more likely it is that prospective killers will think about the possibility of suffering the death penalty when considering a killing. Actual risks of execution are a dubious proxy for perceived risks, but the amount of publicity about executions is arguably a reasonable proxy for the share of prospective killers who had been made aware of particular executions, which should in turn be positively correlated with prospective killers' perceived risk of being executed themselves should they commit a homicide.

One review of ten execution publicity studies found that half of the findings supported a negative effect on homicide rates while the other half supported a positive effect, leading the authors to conclude that execution publicity on net did not affect murder rates (Yang and Lester 2008). Some researchers tested execution publicity effects on different types of murder or homicides. Peterson and Bailey (1991) found newspaper coverage to have no impact on total murders and various types of felony murders. Bailey (1998) supported this finding but also found that newspaper articles on executions were positively associated with the number of stranger killers, stranger murders, and stranger nonfelony killings. Finally, Cochran and Chamlin's (2000) research indicated that highly publicized executions do not consistently impact felony or stranger homicides, with the exception of stranger argument homicides, but appear to significantly reduce nonstranger homicides.

We have summarized the research on the effects of capital punishment publicity in Table 8.10. There have been at least 18 studies that either measured the amount of news media publicity about executions or studied individual executions that were shown to be highly publicized. In order to be included in this review, studies that assessed a single highly publicized execution had to report

**TABLE 8.10** Execution publicity deterrence findings

| | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| Execution Publicity | 63 | 11.1 | 23.8 | 3.2 | 44.4 | 17.5 | 0.0 | 0.0 |

evidence that the execution was indeed publicized more than other executions. Three studies of supposedly publicized executions —Lester (1980), Cochran and Chamlin (2000), and Thomson (1999)—were excluded because they did not actually measure the amount of publicity. Of the 14 studies that directly measured media coverage, nine measured newspaper coverage and five measured television coverage.

The full set of 18 studies generated 63 distinct associations between publicity, or publicized executions, and homicide. Of the 63 findings, only seven were negative and significant, supporting the hypothesis that publicized executions or the amount of execution publicity reduce homicide. These were counterbalanced by 11 findings that were positive and significant, while the remaining 47 findings were not significantly different from zero. Of 61 findings where the sign of the association was reported, 39 were positive and only 22 were negative, so even the general pattern of the sign of associations did not support the idea that execution publicity reduces homicide. This research as a whole indicates that there is no significant effect of publicity about executions on homicide. In sum, the research that comes closest to measuring perceived risk of execution indicates that it has no unique deterrent effect on homicide.

We also categorized execution publicity findings by the type of publicity measure, temporal unit, geographical unit, crime type, and number of variables that were controlled. Table 8.11 shows the results. These subsets of findings generally show little evidence of a deterrent effect of publicity

**TABLE 8.11** Execution publicity findings categorized by methodology

| | *Total # of Findings* | *Percent of Findings* | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | *– sig* | *– ns* | *? ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
| *Publicity Measure* | | | | | | | | |
| Number of Newspaper Stories | 26 | 7.7 | 19.2 | 0.0 | 42.3 | 30.8 | 0.0 | 0.0 |
| Number of Television Stories | 10 | 10.0 | 30.0 | 20.0 | 30.0 | 10.0 | 0.0 | 0.0 |
| Publicized Executions | 27 | 14.8 | 25.9 | 0.0 | 51.9 | 7.4 | 0.0 | 0.0 |
| *Temporal Unit* | | | | | | | | |
| Year | 11 | 18.2 | 36.4 | 0.0 | 36.4 | 9.1 | 0.0 | 0.0 |
| Month | 16 | 18.8 | 31.3 | 0.0 | 37.5 | 12.5 | 0.0 | 0.0 |
| Week | 34 | 0.0 | 17.6 | 5.9 | 52.9 | 23.5 | 0.0 | 0.0 |
| Day | 2 | 100.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| *Geographical Unit to Which Homicide Data Pertained* | | | | | | | | |
| State | 30 | 3.3 | 6.7 | 6.7 | 50.0 | 33.3 | 0.0 | 0.0 |
| City | 5 | 0.0 | 60.0 | 0.0 | 50.0 | 0.0 | 0.0 | 0.0 |
| Entire Nation | 22 | 27.3 | 36.4 | 0.0 | 31.8 | 4.5 | 0.0 | 0.0 |
| Multiple States | 6 | 0.0 | 33.3 | 0.0 | 66.7 | 0.0 | 0.0 | 0.0 |
| *Crime Type* | | | | | | | | |
| Homicide | 51 | 13.7 | 21.6 | 3.9 | 43.1 | 17.6 | 0.0 | 0.0 |
| Murder | 12 | 0.0 | 33.3 | 0.0 | 50.0 | 16.7 | 0.0 | 0.0 |
| *# of Independent Variables* | | | | | | | | |
| 0–5 | 22 | 9.1 | 31.8 | 9.1 | 40.9 | 9.1 | 0.0 | 0.0 |
| 6 or More | 41 | 12.2 | 19.5 | 0.0 | 46.3 | 22.0 | 0.0 | 0.0 |
| *All Publicity Findings* | 63 | 11.1 | 23.8 | 3.2 | 44.4 | 17.5 | 0.0 | 0.0 |

about executions. The only subset in which any substantial minority of the findings supported a deterrent effect were those based on the nation as a whole, i.e. studies in which an impact on national homicide rates was tested. This is contrary to what would logically be expected if deterrent effects of execution publicity existed, since publicity about a given execution is largely concentrated in the state in which it occurred, while awareness of the execution is likely to be negligible in the rest of the nation. Thus, we would expect the impact of any given execution to be slight when spread across the entire country and correspondingly difficult to detect.

Another pattern contrary to expectations concerned types of killings supposedly affected. Murders should be more likely to be deterred by the death penalty than homicides in general because a larger percentage of murders are eligible for the death penalty, yet the pattern of findings shows the exact opposite—findings based on analyses of total homicide are more likely to support a deterrent effect of execution publicity than findings based specifically on murder. This anomalous pattern could be due to the greater room for error in measuring murders, which requires court data on how homicides were legally classified and not just counts of the number of killings.

Finally, if publicity about executions generated a deterrent effect, daily or weekly counts of killings should show more impact of publicity than monthly or yearly counts, since publicity about executions is largely confined to the days preceding and following executions. The findings, however, show the exact opposite. These patterns undercut an interpretation of the few negative associations between publicity and killings as reflections of deterrent effects.

Unfortunately, there is no direct evidence that publicity is in fact a strong proxy for perceived execution risk. Therefore the absence of any association between publicity and homicide frequency could be entirely attributable to random measurement error. Thus, one might find no publicity/homicide correlation even if perceived execution risk actually did exert a unique deterrent effect on homicide. Consequently, this body of research, while arguably providing more relevant tests of deterrent effects than other death penalty studies, cannot definitively refute the hypothesis that executions exert some unique deterrent effect on homicide.

It is also worth noting one policy implication if, in future research, it turns out that deterrent effects of executions depend on publicity. If executions became more common, they would become less novel and thus less newsworthy. Thus, if courts sentenced more people to death, the marginal effect of each execution or death sentencing could be expected to decline as the volume of publicity of each execution declined. This would parallel patterns of effects of incarcerating more criminals. As we shall see in Chapter 10 concerning the impact of mass incarceration, the frequency with which a given punishment is inflicted is subject to a distinct diminishing returns pattern—the more the policy is used, the less an additional unit of that policy will yield in crime reduction.

## The Use of Inappropriately Large Units of Analysis

It is doubtful whether the body of exclusively macro-level evidence on capital punishment, enormous though it is, has much to say about deterrence. The most common research designs employed in recent years seem to have been developed in complete indifference as to how perceptions of risk are likely to be influenced by the existence or use of the death penalty. The research tends to be highly aggregated, studying large areas, and using long time units. The most commonly used research design in recent decades has been a panel design studying states or counties for yearly periods (e.g., Dezhbakhsh et al. 2003; Katz, Levitt, and Shustorovich 2003; Zimmerman 2004). These are but a few of the more serious flaws in the panel research, among many documented so far. On the whole, we agree with the assessment of Chalfin, Haviland, and Raphael (2013), who concluded that panel research using state-years as the unit of analysis was "inconclusive as a whole and in many cases uninformative" (5).

Studies using time units of years, or even months, have little to say about whether executions have unique deterrent effects, if perceived risks of execution are elevated for only a few days or weeks around the time of an execution (Phillips and Hensley 1984). Grogger (1990) found that 85 percent of the newspaper stories concerning California executions appeared between two weeks prior to the event and three days after it. Outside of this very brief time, there is very little news to trigger thoughts of executions among potential killers. If perceptions of execution risk follow even an approximately similar temporal pattern, prospective offenders should experience heightened awareness of the risk of execution within no more than the two weeks before and after an execution.

## Public Intolerance for Violence as a Confounding Variable

Many of these highly aggregated death penalty studies obtain findings that were interpreted as evidence of deterrent effects (e.g., Dezhbakhsh et al. 2003; Shepherd 2004), but it is doubtful whether these interpretations are valid. If a given state or county in a given year has more executions and fewer homicides, the pattern may reflect nothing more than the fact that public intolerance for deviant behavior in general, and violence in particular, varies across time and space, reduces violence through informal social mechanisms, and also expresses itself in greater legal punitiveness, including use of capital punishment (Chapter 7). Executions may be little more than an inconsequential by-product of the popular outrage over violence that is really reducing homicide. Assuming that these mechanisms do in fact operate, as seems likely, the negative association between executions and homicide often observed in highly aggregated studies is likely to be at least partly spurious and could easily be entirely spurious. No macro-level deterrence study of noncapital punishment has eliminated this possibility by measuring and controlling for levels of public disapproval of crime or violence (Chapter 7) and no death penalty study has done so either.

It is unlikely that social disapproval of violence changes substantially over periods as short as days or weeks, but it does measurably change over periods as short as a year, if survey data on the punitiveness of preferred responses to crime can be regarded as indicators of the intensity of public disapproval of crime. The public opinion poll data reviewed in Chapter 1 indicated that there were substantial changes in support for punitive measures from year to year, well in excess of what random sampling error would be likely to produce. For example, the percent favoring the death penalty itself increased from 42 in 1966 to 54 in 1967 and from 70 in 1984 to 76 in 1985, while it decreased from 80 in 1994 to 75.5 in 1995 (Table 1.5). Analyses of homicide counts for shorter time intervals minimize this problem because public disapproval of violence probably changes very little over a span of just a few days or weeks.

Just as studies with long time intervals are especially vulnerable to this spurious association problem, there are also sound reasons to expect that, if there *are* deterrent effects of executions, they are likely to be missed in studies using time units of months or years. This is at least partly due to the absolute rarity of executions. From 1990 through 2009, there were an average of only 53.25 executions per year in the United States, a number that shows no signs of increasing substantially in the foreseeable future (Table 1.4). Thus, in the 37 states with an active death penalty statute as of 2008, there were just 1.44 executions per year per death penalty state. Let us accept, for the sake of argument, Isaac Ehrlich's (1975) widely-cited estimate that the average execution deters eight homicides. This would imply that in an average state, 11.5 homicides ($1.44 \times 8 = 11.5$) would be prevented each year as a result of those executions. In the middle of this period, in 2000, there were 14,317 murders and nonnegligent manslaughters in those 37 states, an average of 387 per death penalty state (U.S. Federal Bureau of Investigation 2001, 76–84). Thus, even if Ehrlich's estimate, widely regarded as a generous one, were accurate, the actual levels of death penalty usage in recent decades could only have reduced murders in a given state by only about three percent over the course of a year ($11.5/387=.03$).

Even homicide statistics, the most accurate of crime statistics, are probably not accurate to within 3 percent, so a deterrent effect of this magnitude would not be statistically detectable for reasons of measurement error alone. And even if homicides were measured perfectly, the many uncertainties in specification and estimation of the statistical models underlying macro-level death penalty research make it highly unlikely that one can be confident that estimates of the number of homicides deterred by executions are accurate to within 3 percent (Berk 2005). The scale of implementation of this particular policy is simply too small to be observable within time units as large as one year because any likely effects are swallowed up by the effects of all other forces influencing homicide during so long a period. Thus, when researchers analyzing highly aggregated data fail to detect a significant effect of executions or execution rates on monthly, quarterly, or annual homicide counts, this is not very strong evidence that executions exert no unique deterrent effect.

Another way to view the results of these highly aggregated studies is to ask the question: When a negative association is found between execution risk and homicide frequency in a study using annual data for areas like states or counties, which is the more plausible interpretation—that executions deterred a huge (i.e., large enough to be detectable) number of homicides or that the association was spurious, attributable to (uncontrolled) variation in public disapproval of violence, the severity of prison sentences, or the size of the prison population? Given the complete absence of any direct evidence that variations in execution risk cause variations in perceived risk of execution, the latter interpretation would at present seem to be more plausible than the former.

We are not claiming that there can be no effects of executions that persist beyond a few days or weeks. Certainly there may be lagged effects and perhaps even long-term effects. Rather, we merely assert that short-term effects are likely to be stronger and thus more easily detected than effects dispersed over months or a year. This is the standard assumption underlying studies using time units shorter than years (e.g., Grogger 1990; Phillips and Hensley 1984; Stack 1987). Consequently, the implication for research is that the search for deterrent effects should begin with short-term effects, because if deterrent effects operate at all, they are most likely to be evident in the short run. Conversely, if there is no evidence of short-term effects of executions, it is highly unlikely that we will ever be able to detect any long-term effects either, and thus there is little empirically based crime-control rationale for persisting in, or increasing the frequency of, executions of murderers.

In contrast, if shorter time units are studied, the problem of the scale of effects is considerably reduced, since the average death penalty state experiences only 7.44 homicides per week, or 1.06 per day. If the bulk of a hypothetical "eight fewer homicides per execution" deterrent impact (or even an effect half as large) occurred within days or weeks of an execution, the proportional reduction in homicides would be quite pronounced and correspondingly easier to detect. Unfortunately, only a handful of studies have used daily or weekly data on homicides, and most of these used very primitive statistical procedures that failed to adequately test alternative explanations of changes in homicide counts (e.g., Dann 1935; Savitz 1958). Further, many of these studies examined only a handful of executions, or even a single execution, casting doubt on whether the results are reliable or can be generalized to executions as a whole. For example, Lester (1980), Cochran, Chamlin, and Seth (1994), and Cochran and Chamlin (2000) each studied a single execution, while Phillips and Hensley (1984) studied just three publicized executions.

## The Grogger Study of Daily Homicide Counts in California

There appears to be only one published death penalty study that applied sophisticated statistical analysis to homicide frequencies measured for time units smaller than months, and that also examined a large number of executions. Grogger (1990) used Poisson and negative binomial regression methods to estimate models of daily homicide counts for California in 1960–1963, a period during which there was 29 executions. He tested for effects that could have occurred as much as 14 days before or 14 days after an execution, controlling for seasonal (monthly) and day-of-the week patterns in homicide. He also analyzed patterns of news coverage of executions to test the hypothesis that apparent deterrent effects occurred on the days when execution stories were most numerous. He found that the number of newspaper stories about executions in the *San Francisco* Chronicle (the only newspaper for which Grogger had data) was not negatively correlated with homicide counts and that peaks in news accounts did not correspond with the days when estimated deterrent effects were strongest.

Grogger's study was powerful because it used the smallest time period for which homicide data could be obtained, thereby maximizing his ability to detect deterrent effects that persisted for only brief periods of time. He also used sophisticated statistical methods to control for day-of-the-week and seasonal periodicity in homicide. The use of daily data also reduced the likelihood of a spurious homicide/execution association because it is unlikely that other factors that influence homicide frequency are correlated with the exact dates when executions are carried out. Grogger concluded that "the analyses conducted consistently indicate that these data provide no support for the hypothesis that executions deter murder in the short term" (302).

This study provided what was, until recently, probably the strongest test of the deterrent impact of the death penalty. It was limited, however, to a single state (California) in a single brief period of time (1960–1963) during which only a few executions (29) had been carried out. Thus, the data provided little foundation for generalization to other places and times and were vulnerable to the peculiarities of when these few executions occurred.

## The Hong-Kleck National Study of Daily Homicide Counts

Findings based solely on data for California from 1960–1963 might not be generalizable to other places or times. Until recently, this unique study had never been replicated. Moonki Hong and Gary Kleck recently carried out an ambitious replication of Grogger's work, using the same basic research design, but expanding it to cover the entire United States, for every day in the 20-year period from January 1, 1979 to December 31, 1998, during which there were 499 executions for murder (Hong and Kleck 2017). Thus, this study had all the strengths of Grogger's work—the daily unit of analysis and sophisticated multivariate statistical analysis—but applied to a far larger body of data (n=372,555 state-days) providing both greater statistical power and a much stronger foundation for generalizing the results. Further, the authors controlled for the estimated daily prison population in each state, thereby partially controlling for the "noncapital punishment regime," as the National Research Council panel put it.

Hong and Kleck used negative binomial regression methods to estimate a fixed effects panel model of the daily homicide counts of all states for all of the days in the study period. Thus, the unit of analysis was the state-day. They controlled for day of the week, month (and thus seasonality), year (using year fixed effect dummy variables), state (using state fixed effect dummy variables), the occurrence of holidays, and for whether the state had an active statute providing the death penalty for murder at the time. As Grogger had done, they tested for effects on daily homicide counts for each day from 14 days before an execution to 14 days after the execution.

The authors obtained even weaker estimates of deterrent effects than Grogger did, though more of these estimates were statistically significant, since their much larger sample size enabled them to detect smaller effects of executions. Of the 29 days around the time of an execution, six showed statistically significant reductions in the number of homicides, none showed significant increases, and the other 19 showed no significant evidence of any effect. Leaving aside statistical significance, the coefficients implied very small effects on each state's daily homicide count (more on this later). Further, the temporal patterns of supposed effects of executions, with a single exception, did not correspond with what a deterrence model would predict. Deterrent effects of executions should be greatest for days closest to the execution and/or the days when news coverage of executions peaked. The days when executions were carried out did show modest drops in homicide counts, but the rest of the other estimated daily effects did not correspond with days when media coverage was higher. Taken at face value, the estimates appeared to indicate deterrent effects 13 days before an execution, but not 12 or 14 days before. They also suggested that there were deterrent effects four days before an execution, but not three or five days before. Finally, there seemed to be lagged deterrent effects appearing five or six days after executions but not one to four days after an execution, when memory of the executions should have been fresher and exerted more deterrent effect on prospective killers and when there were more news stories about the execution.

The authors summed the coefficients (some positive, some negative) of the 29 dummy variables for days near executions as a measure of the net short-term impact of the occurrence of an execution. The sum was –0.57, indicating that, on net, an average execution prevented 0.57 killings in the four-week period around the execution within the state where the execution was carried out. This is a far cry from Isaac Ehrlich's (1975) estimate of eight homicides deterred per execution. In any case, it is debatable whether even this modest figure represented a deterrent effect, since the temporal pattern of estimated effects did not correspond to what a deterrence hypothesis would predict.

The authors also examined the temporal pattern of newspaper and television stories about executions, compiling the count of stories about executions that appeared in the execution state's newspapers or that were broadcast by the three main television networks, on each day within two weeks of an execution (there were very few stories published outside this period). Like Grogger, they found that nearly all news coverage occurred within a few days, before or after, of an execution. Beyond that point, coverage dropped to virtually nothing. If temporal patterns of newspaper coverage correspond reasonably well with temporal patterns of when prospective killers think more about the prospect of execution, deterrence should peak on these same days. Aside from the day of the execution, however, the pattern of estimated effects did not correspond with this expectation. There was no significant deterrent effect at all one to three days before executions and, most surprisingly, no significant effect at all one to four days following executions. Conversely, there appeared to be significant drops in homicides five to seven days following executions, even though there typically are virtually no news stories about executions by that time.

The homicide decreases occurring the day of an execution may well reflect genuine, very short-term deterrent effects, but it is less likely that the decreases on other days can be so regarded. Even if accepted as a genuine effect, the aggregate impact of executions implied by these estimates is slight. In the period 2000–2014, the entire nation averaged 53 executions per year. If each execution prevented 0.57 homicides via short-term deterrence, this would mean about 30 lives saved due to short-term effects of executions in the U.S. in an average year. Over that same period, the U.S. averaged 16,949 homicides per year, so 30 would be less than a fifth of one percent of the homicides. While saving any life is worthwhile, executions clearly cannot be regarded as an important tool for reducing homicide, at least not based on the short-term effects of executions.

## Conclusions

These findings do not necessarily imply that the death penalty is of no significance in preventing murder. While it is often said that laws that go unenforced can have no effect, it is nevertheless possible that the mere existence of a death penalty provision for murder—and thus the theoretical possibility, however slight, of death for a murderer—could exert an impact on some prospective murderers, independent of the actual carrying out of executions. One could save the pro-deterrence position by speculating that there are deterrent effects of capital punishment that do not covary with the frequency of executions, but rather are attributable to the mere fact that some killers in some places at some times have been executed, and that prospective killers know that the same might befall anyone else who murders. Taking into account human misperception of reality, deterrent effects posited by such a theory need not even be limited to legal jurisdictions that actually have statutes providing for the death penalty, since people might refrain from murder because they wrongly believed that they might be subject to capital punishment.

This sort of unique deterrent effect of capital punishment could occur anywhere, at any time, independent of the occurrence or risk of execution, and for that reason would be effectively undetectable. It is hard to imagine any way to empirically test for it in any convincing way. Consequently, the hypothesis that the death penalty has this sort of deterrent effect is probably nonfalsifiable—even if it were false, there would be no feasible research methods that could *show* it to be false.

Richard Berk, one of the most statistically sophisticated criminologists in the world, summarized his own extremely skeptical assessment of the statistical research on the impact of capital punishment on crime thusly: "First, no credible evidence exists that the death penalty, as implemented in the United States since 1979, has any deterrent value. Second, no credible evidence exists to rule out any deterrent effects" (2009, 848). While other scholars may not be quite as skeptical as Berk about the evidence, many would endorse the general notion that the extant deterrence research has little to say of a convincing nature on the question, and what little there is provides only minimally persuasive evidence. Research studying shorter time periods, and the stronger studies that proxied perceptions of execution risk using publicity about executions probably provide the best evidence we currently have, and this body of research suggests that there is very little measurable unique deterrent impact of executions on the frequency of homicide.

## References


Archer, Dane, Rosemary Gartner, and Marc Beittel. 1983. Homicide and the death penalty: A cross-national test of a deterrence hypothesis. *Journal of Criminal Law and Criminology* 74:991–1013.

Avio, Kenneth. 1979. Capital punishment in Canada: A time-series analysis of the deterrent hypothesis. *Canadian Journal of Economics* 12:647–676.

Bailey, William. 1974. Murder and the death penalty. *Journal of Criminal Law and Criminology* 65:416–423.

Bailey, William. 1975. Murder and capital punishment: Some further evidence. *American Journal of Orthopsychiatry* 45:669–688.

Bailey, William. 1976. Deterrence data reanalyzed. *American Journal of Orthopsychiatry* 46:568–570.

Bailey, William. 1977. Deterrence and the violent sex offender: Imprisonment versus the death penalty. *Journal of Behavioral Economics* 6:107–144.

Bailey, William. 1979a. An analysis of the deterrence effect of the death penalty in North Carolina. *North Carolina Central Law Journal* 10:29–51.

Bailey, William. 1979b. Deterrence and the death penalty for murder in Oregon. *Willamette Law Review* 16:67–85.

Bailey, William. 1979c. The deterrent effect of the death penalty for murder in California. *Southern California Law Review* 52:743–764.

Bailey, William. 1980a. A multivariate cross-sectional analysis of the deterrent effect of the death penalty. *Sociology and Social Research* 64:183–207.

Bailey, William. 1980b. Deterrence and the celerity of the death penalty: A neglected question in deterrence research. *Social Forces* 58:1308–1333.

Bailey, William. 1982. Capital punishment and lethal assaults against police. *Criminology* 19:608–625.

Bailey, William. 1983a. Disaggregation, in deterrence and the death penalty research: The case of murder in Chicago. *Journal of Criminal Law and Criminology* 74:827–859.

Bailey, William. 1983b. The deterrent effect of capital punishment during the 1950s. *Suicide* 13:95–107.

Bailey, William. 1984. Murder and capital punishment in the nation's capital. *Justice Quarterly* 1:211–233.

Bailey, William. 1990. Murder, capital punishment, and television: Execution publicity and homicide rates. *American Sociological Review* 55:628–633.

Bailey, William. 1991. The general prevention effect of capital punishment for non-capital felonies. In *The Death Penalty in America: Current Research*, ed. Robert M. Bohm. Cincinnati, OH: Anderson.

Bailey, William. 1998. Deterrence, brutalization and the death penalty: Another examination of Oklahoma's return to capital punishment. *Criminology* 36:711–733.

Berk, Richard. 2005. New claims about executions and general deterrence: De ja vu all over again. *Journal of Empirical Legal Studies* 2:303–330.

Berk, Richard. 2009. Can't tell: Comments on Does the death penalty save lives? *Criminology & Public Policy* 8:845–851.

Black, Fischer. 1982. The trouble with econometric models. *Financial Analysts Journal* 35:29–37.

Black, Theodore, and Thomas Orsagh. 1978. New evidence on the efficacy of sanctions as a deterrent to homicide. *Social Science Quarterly* 58:616–631.

Bowers, William, and Glenn Pierce. 1975. The illusion of deterrence in Isaac Ehrlich's research on capital punishment. *The Yale Law Journal* 85:187–208.

Boyes, William, and Lee McPheters. 1977. Capital punishment as a deterrent to violent crime: Cross-section evidence. *Journal of Behavioral Economics* 6:67–86.

Cantor, David, and Lawrence Cohen. 1980. Comparing measures of homicide trends: Methodological and substantive differences in the vital statistics and Uniform Crime Report time series: 1933–1975. *Social Science Research* 9:121–145.

Chalfin, Aaron, Amelia Haviland, and Steven Raphael. 2013. What do panel studies tell us about a deterrent effect of capital punishment? A critique of the literature. *Journal of Quantitative Criminology* 29:5–43.

Choe, Jongmook. 2009. Another look at the deterrent effect of the death penalty. *Journal of Advanced Research in Law and Economics* 1:12–15.

Cloninger, Dale. 1987. Capital punishment and deterrence: A revision. *Journal of Behavioral Economics* 16:55–57.

Cloninger, Dale. 1994. Enforcement risk and deterrence: A re-examination. *The Journal of Socio-Economics* 23:273–285.

Cochran, John, and Mitchell Chamlin. 2000. Deterrence and brutalization: The dual effects of executions. *Justice Quarterly* 17:685–706.

Cochran, John, Mitchell Chamlin, and Mark Seth. 1994. Deterrence or brutalization? An impact assessment of Oklahoma's return to capital punishment. *Criminology* 32:107–134.

Cover, James, and Paul Thistle. 1988. Time series, homicide, and the deterrent effect of capital punishment. *Southern Economic Journal* 54:615–622.

Dann, Robert. 1935. *The Deterrent Effect of Capital Punishment*, Bulletin 29, Friends Social Service Series, Committee on Philanthropic Labor and Philadelphia Yearly Meeting of Friends.

Dezhbakhsh, Hashem, Paul Rubin, and Joanna Shepherd. 2003. Does capital punishment have a deterrent effect? New evidence from postmoratorium panel data. *American Law and Economics Review* 5:344–376.

Ehrlich, Isaac. 1975. The deterrent effect of capital punishment: A question of life and death. *American Economic Review* 65:397–417.

Ehrlich, Isaac. 1977. The deterrent effect of capital punishment: Reply. *The American Economic Review* 67:452–458.

Ekelund, Robert B., John D. Jackson, Rand W. Ressler, and Robert D. Tollison. 2006. Marginal deterrence and multiple murders. *Southern Economic Journal* 72:521–541.

Grogger, Jeffrey. 1990. The deterrent effect of capital punishment: An Analysis of daily homicide counts. *Journal of American Statistical Association* 85:295–303.

Hendry, David. 1980. Econometrics-alchemy or science? *Economica*, 47:387–406.

Hong, Moonki, and Gary Kleck. 2017. The short-term deterrent effect of executions: An analysis of daily homicide counts. Forthcoming in *Crime & Delinquency*.

Jarrell, Stephen, and Roy Howsen. 1990. Transient crowding and crime: The more strangers in an area, the more crime except for murder, assault and rape. *American Journal of Economics and Sociology* 49:483–494.

Katz, Lawrence, Steven Levitt, and Ellen Shustorovic. 2003. Prison conditions, capital punishment, and deterrence. *American Law and Economics Review* 5:318–343.

Land, Kenneth, Raymond Teske, and Hui Zheng. 2009. The short-term effects of executions on homicides: Deterrence, displacement or both? *Criminology* 47:1009–1043.

Leamer, Edward. 1983. Let's take the con out of econometrics. *The American Economic Review* 73:31–43.

Lempert, Richard. 1983. The effect of executions on homicide: A new look in an old light. *Crime and Delinquency* 29:88–115.

Lester, David. 1980. Effect of Gary Gilmore's execution on homicidal behavior. *Psychological Reports* 47:1262.

Lester, David. 1993. The deterrent effect of the death penalty in Canada. *Perceptual and Motor Skills* 77:186.

Lin, Ming-Jen. 2008. Does unemployment increase crime? Evidence from U.S. data 1974–2000. *Journal of Human Resources* 43:413–436.

Lott, John Jr., and William Landes. 1999. *Multiple Victim Public Shootings, Bombings, and Right-to-Carry Concealed Handgun Laws: Contrasting Private and Public Law Enforcement*. University of Chicago Law School, John M. Olin Law & Economics Working Paper No. 73.

Manski, Charles, and John Pepper. 2013. Deterrence and the death penalty: Partial identification analysis using repeated cross sections. *Journal of Quantitative Criminology* 29:123–141.

McFarland, Sam. 1983. Is capital punishment a short-term deterrent to homicide? A study of the effects of four recent American executions. *Journal of Criminal Law and Criminology* 74:1014–1032.

Mocan, Naci, and Kaj Gittings. 2001. Pardons, Executions and Homicide. NBER Working Paper 8639. Cambridge, MA: National Bureau of Economic Research.

Narayan, Paresh, and Russell Smyth. 2006. Dead man walking: An empirical reassessment of the deterrent effect of capital punishment using the bounds testing approach to cointegration. *Applied Economics* 38:1975–1989.

National Research Council, Committee on Deterrence and the Death Penalty. 2012. *Deterrence and the Death Penalty*. Washington, DC: National Academies Press.

Peterson, Ruth, and William Bailey. 1988. Murder and capital punishment in the evolving context of the post-Furman era. *Social Forces* 66:774–807.

Peterson, Ruth, and William Bailey. 1991. Felony murder and capital punishment: An examination of the deterrence question. *Criminology* 29:367–395.

Phillips, David. 1980. The deterrent effect of capital punishment: New evidence on an old controversy. *American Journal of Sociology* 86:139–148.

Phillips, David, and John Hensley. 1984. When violence is rewarded or punished: The impact of mass media stories on homicide. *Journal of Communication* 34:101–116.

Pratt, John, and Robert Schlaifer. 1979. On the nature and discovery of structure. *Journal of the American Statistical Association* 79:9–21.

Savitz, Leonard. 1958. A study in capital punishment. *Journal of Criminal Law, Criminology, and Police Science* 49:338–341.

Sellin, Thorsten. 1959. *The Death Penalty*. Philadelphia, PA: American Law Institute.

Shepherd, Joanna. 2004. Murders of passion, execution delays, and the deterrence of capital punishment. *Journal of Legal Studies* 33:283–322.

Shepherd, Joanna. 2005. Deterrence versus brutalization: Capital punishment's differing impacts among states. *Michigan Law Review* 104:203–255.

Stack, Steven. 1987. Publicized executions and homicide 1950–1980. *American Sociological Review* 52:532–540.

Thomson, Ernie. 1997. Deterrence versus brutalization: The case of Arizona. *Homicide Studies* 1:110–128.

Thomson, Ernie. 1999. Effects of an Execution on Homicides in California. *Homicide Studies* 3:129–150.

U.S. Federal Bureau of Investigation. 2001. *Crime in the United States—2000*. Washington, DC: U.S. Government Printing Office.

Veal, Michael. 1992. Bootstrapping the process of model selection: An economic example. *Journal of Applied Econometrics* 7:93–99.

Wolpin, Kenneth. 1978. An economic analysis of crime and punishment in England and Wales, 1894–1967. *Journal of Political Economy* 86:815–840.

Yang, Bijou, and David Lester. 2008. The deterrent effect of executions: A meta-analysis thirty years after Ehrlich. *Journal of Criminal Justice* 36:453–460.

Yunker, James. 2001. A new statistical analysis of capital punishment incorporating U.S. postmoratorium data. *Social Science Quarterly* 82:297–311.

Zimmerman, Paul. 2004. State executions, deterrence, and the incidence of murder. *Journal of Applied Economics* 1:163–193.

Zimmerman, Paul. 2006. Estimates of the deterrent effect of alternative execution methods in the United States: 1978–2000. *American Journal of Economics and Sociology* 65:909–941.

# 9

# DO ACTUAL LEVELS OF PUNISHMENT AFFECT PERCEPTIONS OF LEGAL RISK?

A substantial minority of the individual-level evidence reviewed in Chapter 5 indicated that some perceptions of punishment risk—specifically perceived certainty of punishment—may reduce the criminal behavior of some offenders to some degree. On the other hand, most of the macro-level evidence reviewed in Chapters 7 and 8 indicated that higher objective risks of punishment, including the death penalty, do *not* generally reduce crime through any mechanism that could reasonably be interpreted as deterrence. How can these seemingly contradictory or inconsistent sets of conclusions be reconciled?

The most straightforward explanation is that, even though perceptions of legal risk (especially its certainty) sometimes affect criminal behavior, changes in the actual legal risks do not generally affect prospective criminals' perceptions of those risks in the first place. Yet, the idea that actual punishment levels will affect perceived punishment levels is one of the core assumptions of deterrence theory. Indeed, faith in this link is so profound among economists that they take it for granted and rarely even address it as a proposition that needs to be demonstrated (e.g., Avio and Clark 1978; Kessler and Levitt 1999; Levitt 1997; Shepherd 2002). Nevertheless, the assumption that this link exists is so fundamental to deterrence theory that Raymond Paternoster (2010, 786), one of the leading deterrence theorists, stated the following as one of three propositions of the deterrence doctrine:

> H1: Other things being equal, there should be a positive relationship between the objective properties of punishment (certainty, severity, celerity) and the perceptual properties.

That is, in order for deterrence of criminal behavior due to punishment to occur, a positive association must exist between actual levels of punishment (objective properties of punishment) and perceived levels of punishment (perceptual properties of punishment). While this link may well exist, this is scarcely self-evident. This chapter is devoted to empirically assessing whether individual perceptions of legal risk are affected by the actual punishment levels prevailing in the individual's environment.

The deterrence doctrine asserts that some people will refrain from some criminal acts because they perceive a risk of punishment for committing such acts. To review, research on the general deterrent effect of punishment on criminal behavior largely falls within two broad categories: (1) macro-level research using official crime statistics to assess the links between objective levels of punishment prevailing in aggregates like the populations of states or cities, such as the ratio of arrests to offenses or the average length of prison sentences, to crime rates pertaining to those aggregates, and (2) individual-level research using survey methods to assess the links between perceptions of punishment (e.g. its certainty or severity) and self-reported criminal behavior.

Macro-level research (reviewed in Chapter 7) simply assumes, but does not demonstrate, links between actual punishment levels and perceptions of punishment. If there were no such link, more punishment could not produce more deterrence. Thus, the underlying deterrence model proposes that increases in actual punishment levels cause increases in perceptions of punishment, which in turn cause reduced rates of criminal behavior.

Individual-level research (reviewed in Chapters 5 and 6) typically uses survey research to assess the effect of perceptions of punishment on criminal behavior. It therefore addresses the scientific question of whether the former affects the latter but does not resolve the policy issue that lurks behind the scientific debate: do higher levels of punishment, such as higher arrest, conviction, or imprisonment rates, produce lower rates of criminal behavior via increased deterrence? It is possible that, even if the individual-level research suggests an effect of punishment perceptions on criminal behavior, higher levels of punishment may still not increase the deterrence of criminal behavior because punitive policy efforts fail to increase perceptions of risk in the first place. This chapter presents evidence on the missing link between these two bodies of research, exploring the effect of actual punishment levels on individuals' perceptions of punishment.

## Deterrence and Perceptions of Punishment

The abstract possibility of punishment for crime is perceived by virtually everyone beyond early childhood. Few people beyond infancy are oblivious to the fact that at least some criminals suffer legal punishments for committing crimes. Some may believe that these risks are low, or that they are themselves unlikely to be caught and punished for any crimes they might commit, but almost everyone recognizes at least the theoretical possibility that they might suffer legal punishment if they violated the law. Nevertheless, this does not imply that increases in punishment levels (i.e., increases in actual certainty, severity, or swiftness of legal punishment) will increase deterrent effects and thereby reduce crime, since variations in actual punishment levels may or may not cause variations in the average perceived level of punishment among prospective offenders. Indeed, critics of deterrence as crime control, such as Leslie Wilkins, have long pointed to this reality-perception gap as a key weakness in deterrence-based crime control policies (cited in Zimring and Hawkins 1973, 45). One widely cited review of the deterrence literature identified research on the policy-perception link as one of the three top priorities for future deterrence research (Nagin 1998).

Because punishment for crime is to a great degree justified on moralistic grounds, some level of punishment will persist regardless of its utility for controlling crime. Thus, policy-makers are not concerned with the question: "Should we punish crime?"—the answer is clearly "yes." Further, the answer to the simple yes/no question "Can punishment deter crime?" is almost certainly "yes," since at least a few people almost certainly refrain from some crimes due to a fear of legal punishment (Chapter 5). This is, however, an irrelevant question from a policy standpoint, since those who make policy are not concerned with the binary question of whether there will be punishment, but only the quantitative question whether there will be more punishment or less than there is currently.

It is not obvious that more punishment produces more deterrent effect. Even if one were willing to assume that there are no deviance-amplifying effects of punishment (e.g., labeling or stigmatizing effects) to counterbalance deviance-suppressing effects, it would still be unclear whether more punishment would reduce crime via deterrence because it is uncertain whether increased punishment levels would cause increases in the perceived risk of punishment. Even if one could assume that there is some prevailing "baseline" deterrent effect attributable to the mere existence of legal punishment, this would still not resolve the policy question of whether higher punishment levels would *increase* this deterrent effect of punishment beyond its current baseline level.

This way of framing the issue is important because so much of the debate over crime control in policy-making circles is confined to variations on the theme of increasing legal punishments, and much of this debate is even more narrowly confined to methods of increasing the severity and certainty of punishment. Thus, legislators debate bills that would mandate minimum sentences for certain crimes, "third strike" penalties for repeat offenders, enhanced penalties for crimes committed with guns or in connection with drug trafficking, and many other strategies for increasing the severity of punishment. Law enforcement officials lobby for increased budgets and enforcement authority so that they can arrest more criminals, thereby increasing the certainty of arrest for crime, and thus the likelihood of legal punishment. Prosecutors make similar appeals for resources that would enable them to increase the certainty of conviction and thus the certainty of punishment. And many advocates argue for building more prisons so that both the probability and severity (length) of prison sentences can be increased. In sum, advocates strive to increase punishment levels above existing levels.

These policy proposals are frequently justified at least partly on the grounds that they will reduce crime through increased deterrence, by "sending a message," "getting the word out" that crime will not be tolerated, that criminals will be "taught a lesson," and that punishment will surely follow crime. Thus, it is asserted that policy changes producing increases in punishment levels will reduce crime by means of an increased perception of legal risk among prospective offenders.

## Perception-Reality Correspondence and Theories of Criminal Behavior

Leaving public policy aside, many scientific explanations of criminal behavior, and indeed human behavior in general, rely heavily on assumptions of a reality-perception correspondence. The validity of the rational choice model (Chapter 3) does not require that there is a *perfect* correspondence between contingencies and perceptions of contingencies, but it does certainly require *some* correspondence if the theories are to have any explanatory or predictive power, and the greater this correspondence, the greater the predictive and explanatory power of the model. All theories of general deterrence assume that the average effect of actual punishment levels on perceived levels, however much it may vary across individuals, is a significant positive one. Nagin (1998) was quite unambiguous as to the central significance of whether punitive policies affect perceptions of risk: "The conclusion that crime decisions are affected by sanction risk perceptions is not a sufficient condition for concluding that policy can deter crime. Unless the perceptions themselves are manipulable by policy, the desired deterrent effect will not be achieved" (5). In an earlier review of deterrence research, Cook (1980) defended the deterrence doctrine by asserting that even though "public perceptions [of legal sanction threat] are not accurate, [they] do tend to be systematically related to criminal justice activities" (222)—that is, public perceptions of legal risk are positively correlated with actual punishment activities. Likewise, Raymond Paternoster (2010, 786) stated that "whether or not the certainty of enforcement or punishment increases with the use of incarceration, it cannot have a deterrent effect unless it affects the perceptions of those who are contemplating offending." In sum, any version of general deterrence theory that asserts a deterrence-based impact of punitive policies on crime rates assumes there is a net positive effect of punitive activities on average perceptions of legal risk among prospective offenders.

No matter how inclined and able people may be to rationally process and weigh information, and to consider potential costs and benefits of various courses of action, they cannot actually *decide and act* rationally unless there is at least some accuracy to their perceptions of those costs and benefits and thus some correspondence between reality and their perceptions of reality. In some realms of human activity, it is perfectly reasonable to assume a fairly close correspondence between perceptions and the realities of costs and benefits. In the sphere of economic behavior, narrowly construed, the assumption is particularly plausible, mainly because there is an unusually large volume of relevant information easily available to actors and a relatively high degree of accuracy to that information. Consumers generally know the exact price of different brands of goods, and investors know the exact price of a share of stock in any given business firm.

Thus, relevant information in the sphere of market behavior is unusually voluminous, accurate, and easily obtained. Rational behavior, and predictable responses to changes in costs and benefits, are not surprising in such information-rich environments. Shaped by research experiences in this context, some economists appear to consider it self-evident that there must be at minimum a significant positive, albeit imperfect, correlation between actual risks and perceived risks (Becker 1968; Cook 1980; Ehrlich 1973). It is so self-evident to economists that they typically do not even make this assumption of a strong perception/reality link explicit. Mendes and McDonald (2001), however, offer a nicely overt example of the assumption. Arguing that more severe sentences reduce crime rates, they stated that if effective sentences were not imposed on convicted criminals, "potential criminals . . . would soon perceive that criminal behavior [was] going unpunished" (591).

Making decisions regarding the commission of crimes may be radically different from economic decision making, especially with regard to one of the main risks associated with it—punishment. If information about legal risks is limited, hard to obtain, and often inaccurate, the correlation between actual risks and perceptions of those risks will be far weaker than reality/perception correlations in the realm of market behavior. And if prospective offenders' perceptions of punishment risk bear *no* systematic relationship to punishment reality at all, variations in that reality would have no effect on deterrence of criminal behavior. People might well be deterred by the possibility of punishment, but they would be no more likely to be deterred in settings where actual risks were higher than in places where they were lower. Under such circumstances, investment in policies increasing punishment levels would be wasted, to the extent that they relied for their benefits on an increase in deterrent effects.

## The Relevance of These Issues to Prior Research on Crime and Deterrence

The main concepts addressed by macro-level and individual-level deterrence studies can be represented in simple diagrammatic form as illustrated in Figure 9.1.

The perceptual deterrence studies using survey data on individuals that we reviewed in Chapter 5 address the *scientific* issue of whether perception of legal risks affects criminal behavior but do not address the *policy* issue of whether policies that change actual punishment levels, such as increasing the share of convicted persons who are sentenced to prison, affect crime rates via deterrence. It is the first causal link in each of the Figure 9.1 diagrams that is the focus of the research presented in

| <u>Body of Research</u> | | | | | |
|---|---|---|---|---|---|
| Macro-Level: | Punishment Levels | → | [Perception of Punishment] | → | Crime Rates |
| Individual-Level: | [Punishment Levels] | → | Perception of Punishment | → | Criminal Behavior |

**FIGURE 9.1** The Place of Punishment Perceptions in Two Types of Deterrence Research
*Note*: Variables shown in brackets are unmeasured in the indicated body of research.

this chapter, i.e. the link between actual punishment levels (levels of certainty, severity, or swiftness of punishment) and perceptions of those levels.

While a positive association between actual and perceived levels of punishment might seem self-evident to some, there is good reason to question the linkage, and a fair amount of research on related topics that casts doubt on the assumption that the link is strong. Few people, whether criminals or noncriminals, are consumers of criminal justice statistics, and even active criminals have only limited direct personal experience with crime and punishment. Depending on hearsay and selective recall among their criminal associates is not likely to be a reliable basis for forming even approximately accurate estimates of levels or trends in CJS punishment activities.

Nevertheless, persons already actively involved in crime could draw on their own experiences and those of close associates to formulate their perceptions of punishment risk, though research reviewed in Chapter 6 indicated that punishment experiences of a person's associates has no significant effect on the person's own perceptions of legal risk. To the extent that offenders accurately stored away these experiences when they occurred, and then accurately retrieved the information later, these experiences could theoretically improve both perceptions of past legal risks and forecasts of future risks. The research on personality traits common among known offenders (Vold, Bernard, and Snipes 2002, 77–81), however, does not encourage a view of criminals as disciplined and careful processors of information, likely to systematically recall and assess such past experiences. Indeed, within a population of persons who already evince tendencies towards risk-taking, past experience of punishment often appears to lead to a variant of the gambler's fallacy: "My string of past bad luck in getting caught is due to end; my chances of avoiding arrest are bound to improve because I've exhausted my share of bad luck." The evidence reviewed in Chapter 6 indicates that prior experience of punishment usually has no significant effect on perceptions of future risk, and when it does, it often *decreases* perceived risk. In sum, personal experience with punishment does not typically lead to the upward adjustments in perceived risk predicted by a conventional rational choice model of decision-making.

The information situation is far worse for noncriminals, the people that deterrence-based policies are supposed to keep law-abiding. Noncriminals have no personal experience of criminal behavior leading to either punishment or avoidance of punishment, and thus no individual experience-based pool of information at all to use in formulating perceptions of punishment risk.

The news media do not provide either criminals or noncriminals with much reliable information on levels of either crime or punishment. At the macro-level, the amount of news coverage of legal *punishment* is unlikely to bear a strong relation to the general level of actual punishment. There is a great deal more news about crimes than about punishment, yet research on the relationship between the volume of news coverage of crime and actual rates of crime has found the relationship to be close to nonexistent (Davis 1952, 327–329; Jones 1976, 241–242; Garofalo 1981; Marsh 1989; Barlow, Barlow, and Chiricos 1995, 7–8; McClellan 1997). Since common punishment events such as court sentencings or admissions to prison receive even less publicity than the crimes that gave rise to them, it is unlikely that people could formulate even minimally accurate perceptions of punishment risks from the frequency of news media coverage of punishment events.

Indeed, various documented news media biases in coverage of crime and punishment could cause, in an irregular fashion, either overstated or understated perceptions of punishment risk. For example, some scholars have found that newspapers exaggerate the certainty of arrest by over-reporting solved crimes (Parker and Grasmick 1979, 371; Roshier 1973, 37). On the other hand, studies reviewed by Roberts (1992) indicate that news stories about suspects who "got off on a technicality" or who got a "slap on the wrist" sentence from a judge lead to the public perceiving the certainty of imposing a prison sentence or average severity of sentences to be lower than it really is. As a result, people may get an exaggerated notion of the efficiency of the police, and an understated notion of the punitiveness of the courts. Further, since estimates of the certainty of punishment necessarily reflect perceptions of the volume of criminal acts relative to punishments, the lack of correspondence between the volume of news media coverage of crime and actual crime rates makes it unlikely people will form perceptions of the certainty of punishment that closely correspond with reality.

The salient implication of Figure 9.1 is that neither macro-level nor individual research has addressed the main issue considered in this chapter—the effect of actual punishment levels on perceived punishment levels. The individual-level studies address whether perceptions of punishment, however arrived at, influence criminal behavior, but say nothing about whether actual macro-level punishment levels and crime control policies influence the formation of those perceptions in the first place. On the other hand, macro-level studies address whether actual punishment levels somehow affect crime rates but do not address intervening causal mechanisms and thus can say little of a persuasive nature about whether any alleged effects involved deterrence.

Some self-report studies have treated perceptions of punishment as a dependent variable (e.g., Cohen 1978; Horney and Marshall 1992; Parker and Grasmick 1978; Paternoster, Saltzman, Waldo, and Chiricos 1985; Richards and Tittle 1981, 1982; see Chapter 6 for an overview), but there has been virtually no research on the impact of actual punishment levels prevailing in the person's area on those perceptions. Coming closest to doing so, Erickson and Gibbs (1978) surveyed a random sample of Phoenix residents, asking them to estimate the probability of arrest for ten different offenses. Comparing their collective estimates with police statistics on arrest probabilities, they found a 0.55 Pearson correlation (rho=0.39) between objective and perceived certainty of arrest, across ten offenses (259).

This study addressed variation in perceptions only across offense types rather than across individuals, populations, areas, or time periods. The authors stated that a study examining variation across areas would be desirable but asserted that it would be prohibitively expensive (255, fn. 6). Using offense types as the unit of analysis was problematic because perceived certainty of punishment, while it varied considerably across individuals, showed very little variation across offenses within the Phoenix population as a whole (260). Using this unit of analysis also meant that the study was limited to a sample of just ten "cases" (offense types), potentially producing unstable findings. Further, the study was limited to a single jurisdiction, addressed only certainty of punishment, and examined only the police contribution to punishment certainty.

## The Kleck, Sever, Li, and Gertz Study

This section is based on the research conducted by Kleck, Sever, Li, and Gertz (2005). That study was designed to address perceived and objective levels of severity and swiftness of punishment as well as its certainty, and to consider the contributions of courts and correctional institutions to perceptions of those aspects of punishment levels. The general strategy was to interview a large nationally representative probability sample of urban residents, measure their perceptions of punishment risks prevailing in their area, and then relate these perceptions to actual punishment risks as measured in official criminal justice system data.

### *Methodology*

To properly address this topic, it was necessary to identify aspects of punishment for which we could measure both actual and perceived levels. Since it would have been ideal to be able to generalize our findings to the entire population of the U.S., we would have preferred to study objective measures of actual punishment-related CJS activities that are available across the entire nation.

Unfortunately, there are no systematic national data on the swiftness of punishment, while data on severity of punishment, such as mean lengths of prison sentences imposed or served, are available only for varying small subsets of states or selected local areas.

A much richer set of measures of actual punishment risk, however, is available for a smaller set of local jurisdictions. As of 2000, approximately 300 counties participated in the Bureau of Justice Statistics' National Judicial Reporting Program (NJRP). The counties were selected to be representative of the entire U.S. but disproportionately cover larger urban counties (U.S. Bureau of Justice Statistics 1999). Within each sampled county, NJRP staff selects a representative sample of criminal convictions from court records. NJRP data permit analysts to estimate, for each county, the number of convictions (which in turn allows the computation of conviction rates, i.e. adults convicted or who plead guilty per 100 persons charged or arrested), the number of convicted adults who received prison sentences (thus allowing computation of prison sentencing rates, i.e., adults sentenced to prison per 100 adults convicted), the average maximum sentence imposed, and the average number of days between arrest and sentencing. Thus, objective measures can be obtained for the actual levels of certainty, severity, and swiftness of punishment in these counties.

The number of felony convictions is too low in the smaller of the counties participating in the NJRP to yield stable estimates of sentencing-related parameters for specific crime types in individual counties, even when aggregated across multiple years. Therefore, we used data only from the 54 largest NJRP counties. Because these counties were selected to be representative of the 75 most populous counties in the U.S., results based on our urban sample are generalizable to the nation's 75 biggest urban counties. In 1998, these 75 counties accounted for 50.2 percent of the nation's murders, 61.9 percent of robberies, and 51.4 percent of all violent crimes known to the police (analysis of U.S. Federal Bureau of Investigation 2000). In sum, our results can be generalized to the large urban counties that account for most of the nation's crime.

Combining county-level Uniform Crime Reports (UCR) data on crimes and arrests with these NJRP data on convictions and sentences allowed us to measure the following kinds of punishment levels, for each of four offense types. Each actual punishment variable pertains to the county in which the respondent resides.

**Certainty of Punishment**

—Total arrests per 100 offenses known to the police (the arrest rate)
—Adults convicted per 100 adults arrested (the conviction rate)

**Severity of Punishment**

—Adults sentenced to prison per 100 adults convicted (the imprisonment rate)
—Average maximum sentence imposed

**Swiftness of Punishment**

—Average number of days from arrest to sentencing

## *Measurement of Perceptions of Legal Risk*

To measure perceptions of punishment levels, we interviewed representative samples of adults (age 18 and over) in each of the 54 urban counties. For each county-level measure of actual punishment levels (whether a measure of certainty, severity or swiftness of punishment), we devised closely matching questions for the survey, which measured respondents' perceptions of the levels of these risks.

These perceptions were measured by asking questions concerning what the respondent thought the average certainty, severity, or swiftness of punishment was in their county. In each case, to put all respondents (Rs) on an equal footing, interviewers provided a simple nontechnical definition of the offense type and punishment risk measure being asked about. Rs were asked about the preceding ten year period (1988–1998). This was done to ensure a reasonably close correspondence between the period to which perceptions pertained and the period for which reliable data on actual punishment levels could be obtained.

The exact wording of questions measuring perceived punishment levels are shown below. These examples pertain to robbery, but identically worded questions were also asked about criminal homicides, aggravated assaults, and burglaries. Each set of questions concerning a measure of punishment was preceded by a reminder to respondents that they were being asked about their county for the preceding ten years and were provided with a brief nontechnical explanation of the punishment measure being asked about. For example, before asking about the average maximum sentence length, interviewers told respondents that judges sometimes impose a single flat sentence but at other times impose sentences in the form of a range such as one to five years in prison.

*Arrest Certainty*: "In the past ten years in your county, out of every 100 *robberies* known to the police, about how many do you think resulted in the arrest of the robber?"

*Conviction Certainty*: "In your county, out of every 100 persons arrested for *robbery*, about how many do you think are convicted of that crime?"

*Percent Sentenced to Prison*: "Out of every 100 persons convicted of this crime in your county, about how many do you think are given a jail or prison sentence?"

*Average Maximum Sentence Length*: "How about the average person given a prison or jail sentence for committing a *robbery*? What do you think is the average *maximum* sentence length imposed by judges in your county?"

*Swiftness of Punishment*: "For persons convicted of *robbery*, what do you think is the average amount of time that passes between the day the offender is arrested and the day they were sentenced in court?"

A complete copy of the survey instrument is available from the senior author.

It was necessary to go back as far as 1988 to insure enough sample convictions to have reliable estimates of actual punishment levels in individual counties for each offense type. The NJRP data are gathered only for even-numbered years, and the most recent NJRP data available at the time data were gathered were for 1996. Thus, we used NJRP data on representative samples of convictions obtained in 1988, 1990, 1992, 1994, and 1996.

We obtained data on actual punishment levels for as much of the 1988–1998 period as were available. Data on crimes and arrests for counties were available for all five years from 1994 through 1998; pre-1994 county-level UCR data are unusable because they have not been adjusted for non-reporting agencies (U.S. Federal Bureau of Investigation [2000, and preceding years]). Due to this limit on availability of UCR county arrest data, conviction rates could likewise be measured only for 1994 and 1996. Data for all other actual punishment measures were available for even-numbered years from 1988 through 1996 (U.S. Bureau of Justice Statistics 1999, and preceding years).

We asked respondents about punishment of criminal homicide, robbery, aggravated assault, and burglary. These four offenses are all serious crimes and include both violent and property crimes. They encompass most of the offenses that are likely to be publicized, and respondents are probably more likely to hear about punishment of these offenses than almost any other crime types. We could only study the seven traditional FBI/UCR Crime Index offenses because these were the only offenses for which data were available on crimes known to the police. The only two serious Index crimes we could not study were rape and motor vehicle theft. We could not include rape because sexual assault statutes are so different from one state to another that NJRP data on actual punishment levels for rape were not likely to be comparable across states. And we could not cover motor vehicle theft because some states do not have a separate statutory category for this crime, which is lumped in with other grand larcenies. Thus, there are no separate data on punishment of this crime from the NJRP for some states.

### Sampling

The survey sample consisted of 1,500 adult respondents (Rs) selected using random digit dialing procedures. This sampling method allows access to the 95 percent of the U.S. households that have a telephone, including those with unlisted numbers. The sample was drawn exclusively from the 54 largest NJRP counties, with sample sizes for each county proportionate to the population size of the county. Numbers were randomly generated for each county, using the area code and residential prefixes operative in each county. Within each household contacted, an adult respondent was randomly selected by the interviewer, by asking to speak to the resident age 18 or older who had most recently celebrated a birthday.

Telephone interviews were conducted by Research Network, Inc. of Tallahassee, Florida, a professional polling firm that has conducted hundreds of telephone surveys, including many concerning crime. The interviews were conducted in April and May of 1998, a time when nearly all telephone subscribers had landline phones.

### Control Variables

We controlled for a number of individual-level attributes that we thought might affect perceptions of punishment. These are listed in Table 9.1. First, we speculated that people who believe in the crime-control effectiveness of punishment may infer low punishment levels from crime rates that they perceive as high or increasing, based on the assumption that low punishment levels must be at least partly to blame. Thus, we controlled for whether the respondent (R) believed that crime in his or her county was higher than the national average, whether the R considered it to be their community's most important problem, and whether they thought that crime rates in their county were increasing. Following a similar line of reasoning, we hypothesized that persons who had been personally victimized would be more likely to think that punishment levels were inadequately low. Thus, we controlled for whether the R had been a victim of robbery, assault, or burglary. For the same reasons, we also controlled for whether the R personally knew someone who had recently been a victim of crime, i.e. vicarious victimization.

The effects of exposure to news media were difficult to predict. Certainly the media may exaggerate the impression of high crime rates and give an impression that too few criminals are being punished. Yet the news media and entertainment outlets may also exaggerate the effectiveness of criminal justice personnel, especially police, by reporting morally satisfying tales of criminals being brought to justice. Nevertheless, given the potential for news media effects in either direction, we controlled for how often Rs watched local and national television news.

We hypothesized that employees of criminal justice system (CJS) agencies, as well as members of their families, would be especially cynical about system effectiveness and perceive lower punishment levels than other people. Therefore, we controlled for whether the R or a member of the R's family worked in the CJS.

Many scholars have hypothesized that engaging in criminal behavior could itself affect perceptions of punishment, as well as the reverse (e.g., Saltzman, Paternoster, Waldo, and Chiricos 1982).

**TABLE 9.1** Variables in the analysis of the impact of actual punishment levels on individual perceptions of punishment risk[a]

| *Name* | *Description* | *Mean* | *Std. Dev.* |
|---|---|---|---|
| PPAHOM | Perceived probability of arrest, homicide (%) | 51.92 | 25.78 |
| PPAROB | Perceived probability of arrest, robbery (%) | 43.30 | 23.77 |
| PPAASLT | Perceived probability of arrest, aggravated assault (%) | 48.55 | 24.67 |
| PPABURG | Perceived probability of arrest, burglary (%) | 37.97 | 23.74 |
| PPCHOM | Perceived probability of conviction, homicide (%) | 53.47 | 26.42 |
| PPCROB | Perceived probability of conviction, robbery (%) | 50.73 | 25.82 |
| PPCASLT | Perceived probability of conviction, aggravated assault (%) | 50.19 | 26.23 |
| PPCBURG | Perceived probability of conviction, burglary (%) | 46.21 | 26.54 |
| PPPHOM | Perceived probability of prison/jail sentence, homicide (%) | 63.95 | 28.87 |
| PPPROB | Perceived probability of prison/jail sentence, robbery (%) | 52.33 | 26.63 |
| PPPASLT | Perceived probability of prison/jail sentence, agg. assault (%) | 47.69 | 27.03 |
| PPPBURG | Perceived probability of prison/jail sentence, burglary (%) | 44.12 | 26.85 |
| PMSLHOM | Perceived average maximum sentence, homicide (months) | 318.32 | 191.80 |
| PMSLROB | Perceived average maximum sentence, robbery (months) | 84.97 | 85.05 |
| PMSLASLT | Perceived average maximum sentence, aggravated assault (months) | 69.58 | 76.84 |
| PMSLBURG | Perceived average maximum sentence, burglary (months) | 57.14 | 74.23 |
| PSWHOM | Perceived average days from arrest to sentencing, homicide | 491.57 | 597.35 |
| PSWROB | Perceived average days from arrest to sentencing, robbery | 303.63 | 391.76 |
| PSWASLT | Perceived average days from arrest to sentencing, agg. assault | 288.19 | 448.59 |
| PSWBURG | Perceived average days from arrest to sentencing, burglary | 269.76 | 462.46 |
| ARMURD | Total arrests per 100 reported homicides | 90.12 | 36.42 |
| ARROB | Total arrests per 100 reported robberies | 27.04 | 9.89 |
| ARASLT | Total arrests per 100 reported aggravated assaults | 53.94 | 25.52 |
| ARBURG | Total arrests per 100 reported burglaries | 14.17 | 5.52 |
| CRMURD | Adult convictions per 100 adult arrests, homicide | 88.02 | 46.39 |
| CRROB | Adult convictions per 100 adult arrests, robbery | 49.06 | 27.21 |
| CRASLT | Adult convictions per 100 adult arrests, aggravated assault | 19.79 | 14.52 |
| CRBURG | Adult convictions per 100 adult arrests, burglary | 46.23 | 29.70 |
| PRMURD | Prison/jail sentences per 100 adults convicted, homicide | 98.13 | 11.42 |
| PRROB | Prison/jail sentences per 100 adults convicted, robbery | 93.15 | 7.65 |
| PRASLT | Prison/jail sentences per 100 adults convicted, aggravated assault | 82.14 | 13.67 |
| PRBURG | Prison/jail sentences per 100 adults convicted, burglary | 86.77 | 11.72 |
| PSLMURD | Average maximum prison sentence imposed, homicide | 288.12 | 50.62 |
| PSLROB | Average maximum prison sentence imposed, robbery | 90.42 | 44.44 |
| PSLASLT | Average maximum prison sentence imposed, aggravated assault | 51.52 | 24.91 |
| PSLBURG | Average maximum prison sentence imposed, burglary | 51.54 | 31.54 |
| DAYSMURD | Average days from arrest to sentencing, homicide | 417.38 | 119.27 |
| DAYSROB | Average days from arrest to sentencing, robbery | 223.50 | 268.39 |
| DAYSASLT | Average days from arrest to sentencing, aggravated assault | 231.19 | 83.75 |
| DAYSBURG | Average days from arrest to sentencing, burglary | 180.55 | 78.49 |
| INDXRATE | County rate of UCR Index crimes per 100,000 population | 6108.06 | 2019.5 |
| CRIMPROB | R regards crime as community's most important problem (1/2) | 1.51 | 0.50 |

| *Name* | *Description* | *Mean* | *Std. Dev.* |
|---|---|---|---|
| CRIMRELA | R believes crime in county is lower than/same as/higher than national average (1–5) | 2.96 | 1.10 |
| CRIMTRND | R believes county crime is decreasing/stable/increasing (1–3) | 2.17 | 0.77 |
| ROBVICT | Robbery victim in past year (1/2) | 1.04 | 0.20 |
| ASLVICT | Assault victim in adult life (1/2) | 1.25 | 0.43 |
| BURGVICT | Burglary victim in past year (1/2) | 1.06 | 0.24 |
| KNOWVICT | Personally knows victim of serious crime in past year (1/2) | 1.35 | 0.48 |
| NEWSLOCL | Number times watch local news in average week | 5.97 | 4.12 |
| NEWSNATL | Number of times watch national news in average week | 4.57 | 3.44 |
| CJSWORK | R or family member has ever worked in CJS (1/2) | 1.22 | 0.41 |
| AGE | R's age in years | 44.28 | 17.42 |
| MALE | R is male (1/2) | 1.46 | 0.50 |
| BLACK | R is African-American (1/2) | 1.12 | 0.33 |
| SOUTH | R resides in South (1/2) | 1.25 | 0.43 |
| EDUC | Schooling completed (1–7) | 4.72 | 1.60 |
| ARREST | R has been arrested (1/2) | 1.12 | 0.33 |

*Note*: [a] Homicide=murder and nonnegligent manslaughter. Perceptions of punishment pertain to preceding ten years in the respondent's (R's) county. Variables with "(1/2)"at the end of the description are binary, where 2 indicates that R possesses trait, and 1 indicates the absence of the trait.

Criminals who have escaped punishment many times in the past are more likely to come to perceive punishment as unlikely. Conversely, being arrested (and possibly punished) might encourage perceptions of CJS effectiveness, especially regarding law enforcement agencies (though evidence reviewed in Chapter 6 casts doubt on this proposition). We did not ask a battery of questions asking the R to self-report various criminal acts that they had committed, but instead asked simply whether they had ever been arrested. This is a mixed measure in that it is both an indicator of past criminal behavior and of punishment experiences.

Finally, we controlled for the usual background variables of age, sex, race, education, and region. Because this research does not focus on them, they will not be discussed further.

### *Differing Sensitivity to Punishment Levels Among Criminals vs. Noncriminals*

It might be argued that deterrence is not relevant to the bulk of the general population because most people would remain largely noncriminal regardless of perceived or actual punishment levels. According to this view, a more relevant subset of the population for present purposes would be criminals, because they are the people who most need to be deterred to reduce crime. This argument is illogical, since the deterrence doctrine asserts that the prospect of punishment is precisely why many noncriminal persons remain noncriminal. Thus, it is high perceptions of punishment risk among *non*criminals that would provide the most crucial support for the deterrence doctrine.

Alternatively, it might be argued that perceptions of punishment should correspond more closely to reality among criminals, because they are the ones most likely to be knowledgeable about actual punishment levels. Already-active criminals have a larger store of information derived from their own crime-punishment experiences and those of associates and also have the strongest incentives to acquire and retain information on punishment risks.

On the other hand, research on the personality of known offenders portrays them as impulsive, impatient, easily distracted, narrowly focused on the short-term consequences of their actions, and biased towards behaviors that are immediately gratifying, regardless of long-term risks (summarized in Vold et al. 2002, 77–81). Since the risks of punishment for crime are fairly low in the short-term, and substantial only in the long-term, such persons should be especially unresponsive to legal risks because they would give the risks relatively little thought and consideration. Further, despite possessing strong *incentives* to acquire and retain accurate information about legal risks, people with such personality traits may nevertheless be especially unlikely to exercise the patience and forethought to actually do so.

If this view of criminals is accurate, to focus solely on already-active criminals would bias results against finding a reality-perception association by examining only those least inclined and able to acquire reasonably accurate information about punishment levels. Nevertheless, it was possible to empirically address this issue. We roughly distinguished criminals from noncriminals by asking Rs whether they had ever been arrested for a non-traffic offense (using a question asked in seven national General Social Surveys between 1973 and 1984). This allowed us to estimate the associations between actual and perceived punishment levels separately for self-reported arrestees and for non-arrestees. Our sample of the general urban population included 182 admitted arrestees, and thus included many significantly criminal individuals, as well as noncriminals.

### Hypotheses

The validity of the deterrence doctrine does not depend on an assumption that people are able to accurately estimate the *absolute* levels of actual punishment levels. Rather, the doctrine depends only on the existence of a positive association between perceived and actual punishment levels, such that higher actual levels lead to relatively higher perceived levels. People might, on average, misperceive the average prison sentence for robbery to be only half of what it really is, but the deterrence doctrine would still be supported if people in areas with relatively longer sentences provided *relatively* higher estimates of sentence length, however inaccurate in absolute terms they might be, than people in areas with shorter actual sentences.

Thus, the null hypothesis was that there is no statistically significant association between actual and perceived punishment levels. This basic hypothesis was tested with respect to each of four specific types of crime for which punishment data are gathered, for each of the five measures of certainty, severity, and swiftness previously described. We further hypothesized that criminals' perceptions of punishment would not correspond any more closely to reality than those of noncriminals.

### Model Estimation Procedures

We sought to estimate the effects of actual local punishment levels on the perceptions of punishment levels of the residents of large U.S. urban counties. We therefore needed a multivariate estimation technique that would allow us to separate the effects of punishment levels from other factors that might affect perceptions of punishment. Using ordinary least squares regression (OLS), however, would have a potential drawback. The individuals included in this study were clustered by county, so it is likely that errors in predicting their perceptions of punishment would not be independent, violating one of the assumptions of OLS. We therefore instead used hierarchical linear modeling (HLM) techniques (Raudenbush and Bryk 2002). The regression coefficients in the following tables are estimated using a two-level, intercepts-as-outcomes model with individuals as the level-1 units and counties as the level-2 units.

The first step in the regression analysis was to test whether there is sufficient level-2 variance that warrants a hierarchical model. The test was performed using a two-level ANOVA model, which partitions the total variance in the dependent variable, $Y_{ij}$, into two separate components: variance component at level 1 ($\sigma^2$) and variance component at level 2 ($\tau_{00}$). Intraclass correlation ($\rho$) was computed to measure the extent to which differences in the responses exist between level-2 units (counties). The intraclass correlation is used to assess the existence or nonexistence of meaningful differences in responses between the level-2 units—differences that determine the degree to which the data are hierarchically differentiated. The variance components and interclass correlations are listed in Appendix 9.1. As shown in the table, the intraclass correlations for the models are all quite small, indicating that only a small portion (less than 10 percent in any case) of the total variance in the dependent variables is associated with counties as opposed to individuals. However, chi-square tests of these correlations are all statistically significant. On the basis of these results, we can reject the null hypothesis that the mean response scores for all counties are equal and conclude that significant variability in means exists across counties. The results demonstrated that there is sufficient variability to proceed with the multilevel analysis.

The independent variables entered in the level-1 equation are listed in Table 9.1. All of these variables are grand-mean centered when entered into the equation. The level-2 model is specified for a coefficient in the level-1 model that varies across counties. In this study, only the intercept is assumed to vary across the level-2 units. The effects of all of the individual-level independent variables are assumed to be fixed. The level-2 independent variables used to predict the level-1 intercept include actual punishment for the offense in the perceived punishment category and county-level index crime rate. These two variables were entered into the equation as uncentered variables.

The proportional reduction in level-2 variance associated with the HLM models was assessed by comparing variance computed from the fitted model with variance computed from a base model. The base model and the fitted model differ with regard to inclusion of the two level-2 independent variables. The two variables were included in the fitted model but were not entered into the base model. The numbers in the last row of each of the HLM tables show the amount of reduction in level-2 between the two models (see Tables 9.3–9.7). The following formula is used to compute the proportion of reduction.

Proportion variance explained $(R^2) = 1 - \dfrac{\text{var}(\bar{Y}_{.j} - \bar{Y}_j\beta)}{\text{var}(\bar{Y}_{.j})}$, which represents the proportional reduction in mean squared prediction error for the prediction of $\bar{Y}_{.j}$ for a randomly drawn level-2 unit J. Some reduction in level-2 variance is expected when the level-2 variables are entered. However, as shown in the last rows of the tables, some of the reductions carry negative signs, meaning that level-2 variance *increased* after the level-2 variables were introduced. This is likely a statistical artifact. Raudenbush and Bryk (2002, 150) pointed out that "it is mathematically possible under maximum likelihood estimation for the residual variance to increase slightly if a truly nonsignificant predictor is entered into the equation."

## *Findings*

The means of the various actual and perceived punishment variables are of some substantive interest in their own right, since some scholars believe that Americans favor more severe sentences because they misperceive the sentences imposed by their local courts as less severe than they really are (Roberts 1992, 112–113; Tonry 2004, 158). This belief is contradicted by our evidence on average maximum sentence length but supported by our data on the share of convicted persons sentenced to prison. The urban public's estimates of the average sentence length are surprisingly accurate in the aggregate, and to the extent they deviate from reality, they are slightly more severe than actual sentence lengths, for three of four offense types (robbery being the weak exception). On the other hand, the public greatly underestimates the percent of convicted criminals who are given a jail or prison sentence.

These observations about means, however, tell us nothing about whether individuals in areas with higher actual punishment levels perceive higher levels of punishment. The simple bivariate correlations between perceived and actual punishment levels are shown in Table 9.2. They are all

**TABLE 9.2** Correlations of perceived and actual punishment levels

| | *Pearson's r, 1 Tailed Significance (Correlations that are significant at 0.05 level, one tailed are shown in* ***bold****)* | | | | | |
|---|---|---|---|---|---|---|
| | *Total Sample (n = 1142–1330)* | | *Arrestees (n = 150–182)* | | *Non-arrestees (n = 910–1251)* | |
| *Certainty* | *r* | *p* | *r* | *p* | *r* | *p* |
| Arrest Rates[a] | | | | | | |
| Murder | **0.048** | 0.04 | –0.030 | 0.35 | **0.071** | 0.01 |
| Robbery | 0.011 | 0.34 | –0.043 | 0.29 | 0.004 | 0.45 |
| Aggravated Assault | 0.006 | 0.42 | –0.120 | 0.06 | 0.028 | 0.18 |
| Burglary | 0.015 | 0.29 | –0.145 | 0.03 | 0.047 | 0.06 |
| Conviction Rates[b] | | | | | | |
| Murder | –0.030 | 0.14 | 0.072 | 0.18 | –0.042 | 0.08 |
| Robbery | 0.020 | 0.24 | 0.107 | 0.08 | 0.019 | 0.26 |
| Aggravated Assault | 0.040 | 0.07 | **0.130** | 0.05 | 0.019 | 0.27 |
| Burglary | **0.053** | 0.03 | 0.084 | 0.14 | 0.046 | 0.06 |
| *Severity* | | | | | | |
| Prison Rates (%)[c] | | | | | | |
| Murder | –0.027 | 0.15 | –0.074 | 0.16 | –0.017 | 0.27 |
| Robbery | 0.026 | 0.16 | –0.014 | 0.43 | 0.041 | 0.07 |
| Aggravated Assault | 0.026 | 0.16 | 0.028 | 0.35 | 0.028 | 0.16 |
| Burglary | **0.058** | 0.01 | –0.068 | 0.18 | **0.079** | 0.00 |
| Average Max. Sentence | | | | | | |
| Murder | –0.000 | 0.50 | –0.023 | 0.38 | 0.008 | 0.40 |
| Robbery | 0.009 | 0.37 | –0.008 | 0.46 | 0.020 | 0.26 |
| Aggravated Assault | –0.011 | 0.35 | –0.027 | 0.37 | –0.006 | 0.42 |
| Burglary | 0.006 | 0.41 | 0.095 | 0.12 | –0.002 | 0.48 |
| *Swiftness* | | | | | | |
| Average Time, Arrest | | | | | | |
| to Sentencing Murder | 0.026 | 0.21 | –0.013 | 0.44 | 0.039 | 0.14 |
| Robbery | **0.122** | 0.00 | 0.016 | 0.44 | **0.144** | 0.00 |
| Aggravated. Assault | 0.025 | 0.23 | –0.045 | 0.32 | 0.037 | 0.16 |
| Burglary | –0.013 | 0.35 | 0.001 | 0.50 | –0.015 | 0.35 |
| Average Correlation: | 0.020 | | –0.004 | | 0.027 | |

*Notes*:

[a] Number of persons arrested per 100 offenses known to police.

[b] Percent of adults arrested who were convicted.

[c] Percent of adults convicted who received a prison or jail sentence.

extremely weak to nonexistent. None of the 20 perception/reality correlations reached 0.13 in the full sample, only two exceeded 0.05, and they averaged a negligible 0.02. Many were even negative. Only four of twenty correlations were even statistically significant, despite the fairly large sample sizes, which ranged from 1,142 to 1,330, depending on the number of cases with missing data.

Even the largest correlation of 0.122, pertaining to swiftness of punishment for robbery, implies that variation in actual punishment levels accounts for only 1.7 percent of the variation in perception of punishment levels ($0.122^2 = 0.017$). Further, there was no clear pattern, regarding either crime type or dimension of punishment, among the four correlations that were statistically significant. Since the hypothesis of a correlation between actual and perceived punishment risks was tested 20 times (five measures of punishment risk times four crime types), one nontrivial correlation could easily be obtained by chance alone, as a result of the large number of hypothesis tests. Neglecting this one correlation, it is fair to say that *there is virtually no association between actual and perceived risks of legal punishment.*

It is possible that while criminals might not directly perceive overall punishment levels prevailing in their local areas, they could be indirectly influenced by those levels via their own personal experiences with crime and punishment and the experiences of their close associates. Criminals are familiar with their own rate of past criminal behavior and their own experiences with legal punishment and have at least some knowledge of such experiences among associates. Thus, if the personal experiences of any one criminal tend to reflect, on average, the aggregate experiences of all criminals in an area, perceptions of punishment risk might correlate well with actual punishment risk. This presupposes, however, that criminals reasonably accurately recall their own criminal behavior and punishment experiences, have reasonably accurate perceptions of the experiences of associates, and take account of these experiences when deciding whether to commit crimes. The evidence reviewed in Chapter 6, however, generally indicated that criminals' perceptions of legal risks had no consistent relationship with their own punishment experiences or those of associates.

In any case, the hypothesis that criminals are more closely attuned to actual punishment risks than noncriminals is clearly not supported by the evidence in Table 9.2, which shows that correlations between perceived and actual punishment levels were even weaker among arrestees (middle two columns) than among non-arrestees (last two columns). Indeed the average perception-reality correlation among arrestees is slightly negative, though not significantly different from zero. Evidently, urban criminals' perceptions of punishment risks prevailing in their areas have virtually no systematic correspondence with reality. Only one of the 20 arrestee correlations was statistically significant at the 0.05 level (one-tailed). Thus, the notion that perceptions correspond to reality more strongly among criminals than among noncriminals is clearly not supported by the evidence.

The multivariate HLM estimates are presented in Tables 9.3–9.7, each table being devoted to a different measure of punishment. The predictors are individual-level attributes of the survey respondents (Rs), measured through interviewing, and the county-level punishment variable whose effect is being estimated.

Tables 9.3 and 9.4 concern measures of the certainty of punishment of criminal behavior. In Table 9.3, the dependent variable is the perceived arrest rate. The focus is on the effect of the actual county arrest rate for a given crime type on the arrest rate for that crime type as perceived by county residents. One seemingly counterintuitive finding is that persons who have personally been arrested perceive a *lower* probability of being arrested than non-arrestees, perhaps because this variable also serves as an indicator of frequent past criminal behavior, little of which resulted in arrest. It is also consistent with the "resetting" or "gambler's fallacy" hypothesis that offenders respond to being arrested by concluding that they have used up their bad luck, and so are less likely to be arrested for future crimes. The finding is a common one in the literature (e.g., see Horney and Marshall 1992 and similar studies summarized in Chapter 6). The result, in any case, does not

**TABLE 9.3** Effects of actual arrest rates on perceived arrest rates (HLM estimates)

| | *Dependent Variable: Perceived Arrests per 100 Known Offenses for:* | | | |
|---|---|---|---|---|
| | *Homicide* | *Robbery* | *Assault* | *Burglary* |
| *Level-1 Predictors* | | | | |
| Crime Most Important Problem | –1.94 | –0.89 | –2.00 | –0.13 |
| Crime in County Relative to Nation | 0.78 | –0.63 | –0.75 | –0.70 |
| Crime Trend in County | –3.76** | –1.49* | –1.81** | –2.46** |
| Robbery Victim in Past Year | –1.77 | 1.07 | –1.64 | 0.34 |
| Assault Victim as Adult | –0.05 | –2.42 | 0.84 | –0.91 |
| Burglary Victim in Past Year | –2.49 | –4.79 | –2.16 | –3.05 |
| Knows Victim of Serious Crime | –1.38 | –1.01 | –0.98 | –1.47 |
| Times per Week Watching Local News | –0.13 | 0.29 | 0.02 | 0.16 |
| Times per Week Watching Natl. News | 0.11 | –0.13 | 0.01 | 0.15 |
| R or R's Family Worked in CJS | 2.14 | 1.94 | 0.50 | 0.94 |
| Age | –0.03 | 0.01 | –0.04 | –0.04 |
| Male | 2.02 | –0.77 | 0.07 | –2.38* |
| Black | –6.78 | –3.01 | –2.41 | –3.00 |
| South | –2.48** | –1.59 | –0.88 | –0.80 |
| Education | 0.86* | –0.68* | 0.10 | –1.33** |
| Non-Traffic arrest | –3.43 | –4.87** | –5.09** | –3.97* |
| *Level-1 Modeled Variance ($R^2$)* | 0.07 | 0.05 | 0.04 | 0.05 |
| *Level-2 Predictors* | | | | |
| Total Arrests/100 Reported Murders | 0.02 | | | |
| Total Arrests/100 Reported Robberies | | –0.08 | | |
| Total Arrests/100 Reported agg Assaults | | | –0.02 | |
| Total Arrests/100 Reported Burglary | | | | –0.11 |
| County-Level Index Crime Rate | 0.00 | –0.00 | –0.00 | –0.00* |
| *Level-2 Modeled Variance ($R^2$)* | –0.02 | –0.05 | –0.03 | 0.01 |

*Notes*:
* Significant at 0.05 confidence level;
** Significant at 0.01 confidence level

support a special deterrent effect. The findings also indicate that those who believed that crime was increasing tended to perceive arrest rates to be low, perhaps because they believed that low arrest rates were partly responsible for the rising crime problem.

The main findings concern the effects of actual arrest rates on perceptions of arrest rates. Estimates can be found in the lower section of the table labeled Level-2 Predictors (referring to county-level variables). The actual likelihood of being arrested for a crime appears to have no effect on perceptions of this likelihood. Whether for homicide, robbery, aggravated assault, or burglary, actual arrest rates show no evidence of an effect on perceived arrest rates. Lochner (2007) later confirmed these findings for perceptions of the risk of arrest for an offense we did not address, motor vehicle theft. Using an independent national body of data on youth, he found (in his more complete models) that actual county arrest rates for auto theft had no significant association with the youths' perceived probability of arrest for auto theft (449–450).

**TABLE 9.4** Effects of actual conviction rates on perceptions of conviction rates (HLM estimates)

| | *Dependent Variable: Perceived Conviction Rates for:* | | | |
|---|---|---|---|---|
| | *Homicide* | *Robbery* | *Assault* | *Burglary* |
| *Level-1 Predictors* | | | | |
| Crime Most Important Problem | –0.17 | –0.50 | –0.84 | –1.38 |
| Crime in County Relative to Nation | 0.34 | 0.56 | 0.15 | –0.01 |
| Crime Trend in County | –2.76** | –2.28** | –1.69* | –1.44 |
| Robbery Victim in Past Year | –4.76 | –5.51 | –5.87 | –5.63 |
| Assault Victim as Adult | 0.90 | 1.23 | 1.04 | 0.34 |
| Burglary Victim in Past Year | –2.93 | –1.63 | –0.44 | –3.63 |
| Knows Victim of Serious Crime | –0.34 | –0.63 | –0.77 | –1.21 |
| Times per Week Watching Local News | 0.02 | –0.08 | –0.03 | –0.16 |
| Times per Week Watching Natl. News | –0.19 | 0.12 | –0.15 | –0.20 |
| R or R's Family Worked in CJS | –0.59 | –0.32 | –0.84 | –1.66 |
| Age | –0.07 | –0.06 | –0.07 | –0.03 |
| Male | 2.01 | 4.35** | 3.99** | 3.18** |
| Black | –0.99 | –0.47 | –2.83 | –4.88 |
| South | –4.64** | –5.29* | –3.33 | –2.31 |
| Education | 1.04* | 0.35 | 0.21 | –0.17 |
| Non-Traffic Arrest | –0.05 | –6.21** | –1.65 | –2.86 |
| *Level-1 Modeled Variance ($R^2$)* | 0.05 | 0.04 | 0.04 | 0.04 |
| *Level-2 Predictors* | | | | |
| Adult Convictions/100 Murder Arrests | –0.01 | | | |
| Adult Convictions/100 Robbery Arrests | | 0.03 | | |
| Adult Convictions/100 agg. Assault Arrests | | | 0.08* | |
| Adult Convictions/100 Burglary Arrests | | | | –0.01 |
| County-Level Index Crime Rate | –0.00 | –0.00 | –0.00* | –0.00 |
| *Level-2 Modeled Variance ($R^2$)* | –0.02 | –0.01 | 0.14 | –0.01 |

*Notes*:
* Significant at 0.05 confidence level;
** Significant at 0.01 confidence level

Table 9.4 presents findings concerning the effects of actual conviction rates on perceived conviction rates. Paralleling the arrest rate results, these findings generally indicate that actual conviction rates have no effect on perceived conviction rates. The sole exception is a slight, albeit statistically significant, positive effect for assault: one more conviction per 100 arrests is associated with a perceived 0.08 more convictions per 100 arrests.

Tables 9.5 and 9.6 address perceptions of the *severity* of punishment for crime. Table 9.5 presents findings concerning the perceived percent of convicted offenders who received a prison sentence in the R's county. The results for all four offenses indicate no positive effect of actual prison sentence rates on individual perceptions of those rates. The results for murder actually suggest a perverse, and significant, negative effect on the perceived rate. This could, however, be nothing more than a chance finding attributable to the large number of multivariate hypothesis tests performed.

**TABLE 9.5** Effect of actual prison sentence rates on perceptions of prison sentence rates (HLM estimates)

| | *Dependent Variable: Perceived Prison Sentences per 100 Adults Convicted for:* | | | |
|---|---|---|---|---|
| | *Homicide* | *Robbery* | *Assault* | *Burglary* |
| *Level-1 Predictors* | | | | |
| Crime Most Important Problem | –1.05 | 0.51 | 0.20 | 0.15 |
| Crime in County Relative to Nation | 0.08 | –0.32 | –0.80 | –0.43 |
| Crime Trend in County | –1.63* | –1.80** | –1.40 | –1.07 |
| Robbery Victim in Past Year | –4.31 | –4.79 | –4.53 | –1.80 |
| Assault Victim as Adult | 2.89 | 0.10 | –1.61 | –1.99 |
| Burglary Victim in Past Year | –3.41 | –1.16 | –1.06 | –3.30 |
| Knows Victim of Serious Crime | –1.86 | –1.34 | –1.32 | –2.28 |
| Times per Week Watching Local News | –0.03 | –0.10 | –0.21 | –0.23 |
| Times per Week Watching Natl. News | –0.36 | –0.18 | 0.14 | 0.04 |
| R or R's Family Worked in CJS | 3.78* | 2.28 | 3.23* | 2.46 |
| Age | –0.04 | 0.01 | –0.02 | –0.01 |
| Male | 3.02** | 2.84** | 0.68 | 2.86* |
| Black | –3.72 | –2.29 | –1.69 | –3.16* |
| South | –4.42 | –7.69** | –3.74* | –3.33 |
| Education | 2.34** | 1.18** | 0.65 | 0.35 |
| Non-Traffic Arrest | 2.39 | –0.38 | –3.42 | –3.03 |
| *Level-1 Modeled Variance ($R^2$)* | 0.08 | 0.05 | 0.04 | 0.04 |
| *Level-2 Predictors* | | | | |
| Prison Sentences for Murders/100 Convictions | –0.08* | | | |
| Prison Sentences for Robberies/100 Convictions | | 0.08 | | |
| Prison Sentences for Assaults/100 Convictions | | | 0.03 | |
| Prison Sentences for Burglaries/100 Convictions | | | | 0.02 |
| County-Level Index Crime Rate | –0.00* | –0.00 | –0.00 | –0.00 |
| *Level-2 Modeled Variance ($R^2$)* | 0.05 | –0.02 | 0.03 | 0.05 |

*Notes*:
* Significant at 0.05 confidence level;
** Significant at 0.01 confidence level

Table 9.6 presents the multivariate findings regarding the average length of prison sentences imposed in the R's county. These estimates uniformly indicate no significant effect of actual sentence lengths on the sentence lengths that residents thought were being imposed in their counties.

Finally, Table 9.7 reports multivariate findings concerning the swiftness of punishment, measured as the average number of days from arrest to sentencing. For three of four offense types, the association between actual and perceived swiftness was not significantly different from zero, the exception being robbery.

To summarize, none of the five measures of punishment, whether measures of certainty, severity, or swiftness of punishment, showed consistent indications of an effect of actual punishment levels on perceived punishment levels. Across four crime types, there were a total of 20 estimates of this effect. Two of these were positive and significant, supporting the deterrence doctrine, while one

**TABLE 9.6** Effect of actual sentence lengths on perceptions of sentence length (HLM estimates)

| | *Dependent Variable: Perceived Average Maximum Sentence for:* | | | |
|---|---|---|---|---|
| | *Homicide* | *Robbery* | *Assault* | *Burglary* |
| *Level-1 Predictors* | | | | |
| Crime Most Important Problem | 6.07 | –0.15 | –0.65 | 2.71 |
| Crime in County Relative to Nation | –5.02 | –0.03 | 2.56 | 1.46 |
| Crime Trend in County | –1.41 | –5.13 | –3.54 | –4.07 |
| Robbery Victim in Past Year | 11.19 | –8.13 | –6.78 | –3.80 |
| Assault Victim as Adult | –14.97 | –8.10 | –11.57** | –7.05 |
| Burglary Victim in Past Year | 6.14 | 1.32 | 6.52 | –3.46 |
| Knows Victim of Serious Crime | 3.03 | –2.55 | 0.96 | –2.11 |
| Times per Week Watching Local News | –1.02 | –1.47** | –0.48 | –0.34 |
| Times per Week Watching Natl. News | 0.42 | 0.79 | –0.09 | 0.15 |
| R or R's Family Worked in CJS | 6.72 | –5.42 | –1.36 | 1.98 |
| Age | 0.02 | 0.17 | –0.07 | 0.15 |
| Male | 3.27 | –2.65 | –4.40 | 1.78 |
| Black | –45.92** | 9.51 | –6.09 | 5.44 |
| South | –8.20 | 0.34 | 6.32 | –1.12 |
| Education | 3.71 | –0.72 | 0.82 | –0.72 |
| Non-Traffic Arrest | 17.84 | –0.21 | –2.20 | –3.91 |
| *Level-1 Modeled Variance ($R^2$)* | 0.03 | 0.04 | 0.03 | 0.03 |
| *Level-2 Predictors* | | | | |
| Mean max Prison Sentence, Murder | 0.02 | | | |
| Mean max Prison Sentence, Robbery | | 0.02 | | |
| Mean max Prison Sentence, agg. Assault | | | –0.04 | |
| Mean max Prison Sentence, Burglary | | | | –0.02 |
| County-Level Index Crime Rate | 0.00 | 0.00 | 0.00 | 0.00 |
| *Level-2 Modeled Variance ($R^2$)* | 0.06 | –0.03 | –0.02 | –0.05 |

*Notes*:
*Significant at 0.05 confidence level;
** Significant at 0.01 confidence level

was significant and negative, contradicting the doctrine, and the remaining 17 were not significantly different from zero. With a large number of tests of the same basic hypothesis, one would expect one or two coefficients to be significant by chance alone, suggesting that little importance can be attributed to the two supportive results. This view is strengthened by the lack of any pattern in the signs of the significant coefficients (two positive, one negative), and the lack of any pattern, either by crime type or punishment type, concerning which estimates appeared to support the deterrence doctrine.

More generally, expressed perceptions of punishment appear to have little relationship with any of the variables measured in this study. Tables 9.3–9.7 report the level-1 variance explained in the dependent variables, i.e. the $R^2$ for the individual-level variables, and these figures are uniformly

**TABLE 9.7** Effect of actual swiftness of punishment on perceptions of swiftness (HLM estimates)

| | *Dependent Variable: Perceived Number of Days from Arrest to Sentencing for:* | | | |
|---|---|---|---|---|
| | *Homicide* | *Robbery* | *Assault* | *Burglary* |
| *Level-1 Predictors* | | | | |
| Crime Most Important Problem | 0.02 | 20.43 | 28.32* | 39.25** |
| Crime in County Relative to Nation | –7.88 | –0.27 | 5.75 | –3.46 |
| Crime Trend in County | 10.30 | –7.27 | –12.46 | –22.59 |
| Robbery Victim in Past Year | –98.42* | –0.97 | –9.12 | 39.32 |
| Assault Victim as Adult | 47.71 | –2.26 | –40.67 | –16.40 |
| Burglary Victim in Past Year | –32.86 | 8.64 | –27.54 | –14.69 |
| Knows Victim of Serious Crime | 45.23 | 17.01 | 32.71 | 24.52 |
| Times per Week Watching Local News | –3.42 | 1.25 | 1.95 | 0.34 |
| Times per Week Watching Natl. News | 4.75 | 0.55 | –2.25 | –2.49 |
| R or R's Family Worked in CJS | –20.63 | 14.18 | 59.95* | 33.79 |
| Age | 3.30** | 1.68** | 2.21 | 3.06** |
| Male | –33.97 | –22.70 | –21.96 | –38.78 |
| Black | –29.24 | –41.86* | –48.74* | –33.64 |
| South | –8.95 | 10.39 | –15.56 | –10.02 |
| Education | 18.84* | 12.07* | 16.48** | 13.91** |
| Non-Traffic Arrest | 53.90 | 37.07 | 38.04 | 55.53 |
| *Level-1 Modeled Variance ($R^2$)* | 0.04 | 0.02 | 0.03 | 0.05 |
| *Level-2 Predictors* | | | | |
| Mean Days bet. Arrest and Sentence, Murder | 0.17 | | | |
| Mean Days bet. Arrest and Sentence, Robbery | | 0.15** | | |
| Mean Days bet. Arrest and Sentence, Assault | | | 0.20 | |
| Mean Days bet. Arrest and Sentence, Burglary | | | | 0.14 |
| County-Level Index Crime Rate | 0.01 | –0.00 | 0.03 | 0.03 |
| *Level-2 Modeled Variance ($R^2$)* | 0.00 | 0.14 | –0.06 | –0.06 |

*Notes*:
* Significant at 0.05 confidence level;
** Significant at 0.01 confidence level

low. Either these perceptions are being formed largely at random, or they are produced by factors, perhaps very individualistic and idiosyncratic, that we did not measure.

Tables 9.8 and 9.9 report multivariate HLM results pertaining to the issue of whether the correspondence of perception and reality of punishment is any closer for criminals than for noncriminals. Table 9.8 summarizes the key findings for arrestees while Table 9.9 does so for non-arrestees. Because these multivariate estimates require nonmissing data for a large number of variables, they are based on considerably smaller samples due to missing data. These multivariate results confirm the bivariate findings of Table 9.2. There is no significant association between perceptions of punishment and its reality among either arrestees or non-arrestees, and there is no evidence that the correspondence of perception and reality is any closer among criminals than among noncriminals.

**TABLE 9.8** Effects of actual punishment on perceived punishment among arrestees (HLM estimates)

| *Dependent Variable* | *Level-2 Explanatory Variables* | | *Level-2 Variance & Sample Size* | |
|---|---|---|---|---|
| | *Actual Punishment ($\gamma_{01}$)* | *Index Crime Rate ($\gamma_{02}$)* | *Modeled Variance ($R^2$)* | *Level-2 Sample Size (N)* |
| A. Perceived Arrests per 100 Known Offenses for: | | | | |
| Homicide | 0.03 | –0.00 | –0.16 | 37 |
| Robbery | –0.35 | –0.00 | –0.00 | 37 |
| Assault | –0.21** | –0.00 | 0.12 | 37 |
| Burglary | –0.54 | –0.00 | –0.01 | 37 |
| B. Perceived Conviction Rates for: | | | | |
| Homicide | 0.06 | –0.00 | –0.02 | 37 |
| Robbery | 0.04 | –0.00 | –0.30 | 37 |
| Assault | 0.47** | –0.00* | 0.16 | 37 |
| Burglary | –0.03 | –0.00** | 0.02 | 37 |
| C. Perceived Prison Sentences per 100 Adults Convicted for: | | | | |
| Homicide | –0.05 | –0.00 | –0.08 | 37 |
| Robbery | 0.52* | –0.00* | 0.37 | 37 |
| Assault | –0.09 | –0.00* | 0.13 | 37 |
| Burglary | 0.10 | –0.00* | 0.20 | 37 |
| D. Perceived Average Maximum Sentence for: | | | | |
| Homicide | –0.23 | 0.01 | –0.06 | 37 |
| Robbery | –0.03 | –0.01 | –0.00 | 37 |
| Assault | –0.11 | –0.01 | –0.02 | 37 |
| Burglary | 0.05 | –0.01 | 0.06 | 37 |
| E. Perceived Days From Arrest to Sentencing for: | | | | |
| Homicide | –0.08 | 0.01 | –0.02 | 30 |
| Robbery | –0.35 | 0.03 | –0.34 | 30 |
| Assault | –0.52 | 0.01 | –0.27 | 30 |
| Burglary | –0.01 | 0.01 | –0.26 | 30 |

*Notes*:
* Significant at 0.05 confidence level;
** Significant at 0.01 confidence level

## Caveats

In at least three important ways, these are generous estimates of the reality-perception association. First, in all of our analyses we had to exclude the 15–20 percent of respondents who were not willing to even guess at punishment levels in their area. Thus, by excluding what were presumably less knowledgeable respondents, we biased results in favor of finding a higher correspondence between reality and perception, yet still found virtually no association. Second, we asked Rs for these perceptions in a context favoring accuracy. Rs were in the relative comfort and security of their homes and could calmly reflect on punishment risks. In contrast, prospective offenders, at the moment when they consider committing a criminal act, especially a violent act, are often under considerable emotional stress and thus less likely to make reasonable assessments of legal risks. Third, because samples of the general household population exclude incarcerated criminals, our sample excluded

**TABLE 9.9** Effects of actual punishment on perceived punishment among non-arrestees (HLM estimates)

| *Dependent Variable* | *Level-2 Explanatory Variables* | | *Level-2 Variance & Sample Size* | |
|---|---|---|---|---|
| | *Actual Punishment* ($\gamma_{01}$) | *Index Crime Rate* ($\gamma_{02}$) | *Modeled Variance* ($R^2$) | *Level-2 Sample Size* (*N*) |
| *A. Perceived Arrests per 100 Known Offenses for:* | | | | |
| Homicide | 0.03 | –0.00 | –0.02 | 54 |
| Robbery | –0.10 | –0.00 | –0.03 | 54 |
| Assault | 0.00 | –0.00 | –0.05 | 54 |
| Burglary | 0.02 | –0.00 | –0.04 | 54 |
| *B. Perceived Conviction Rates for:* | | | | |
| Homicide | –0.02 | –0.00 | –0.02 | 54 |
| Robbery | 0.02 | –0.00 | –0.04 | 54 |
| Assault | 0.04 | –0.00 | 0.06 | 54 |
| Burglary | –0.01 | –0.00 | –0.05 | 54 |
| *C. Perceived Prison Sentences per 100 Adults Convicted for:* | | | | |
| Homicide | –0.04 | –0.00 | 0.04 | 54 |
| Robbery | 0.05 | 0.00 | –0.05 | 54 |
| Assault | 0.05 | –0.00 | –0.02 | 54 |
| Burglary | 0.03 | –0.00 | 0.00 | 54 |
| *D. Perceived Average Maximum Sentence for:* | | | | |
| Homicide | 0.04 | 0.01* | 0.09 | 54 |
| Robbery | 0.03 | 0.00 | –0.01 | 54 |
| Assault | –0.03 | 0.00 | –0.05 | 54 |
| Burglary | –0.02 | 0.00 | –0.05 | 54 |
| *E. Perceived Number of Days from Arrest to Sentencing for:* | | | | |
| Homicide | 0.20 | 0.02 | 0.06 | 38 |
| Robbery | 0.17** | –0.01 | 0.23 | 38 |
| Assault | 0.28 | 0.03 | –0.06 | 38 |
| Burglary | 0.17 | 0.03 | –0.04 | 38 |

*Notes*:
* Significant at 0.05 confidence level;
** Significant at 0.01 confidence level

people who were undeterred by, and thus presumably least responsive to, threats of legal punishment. Likewise, if nonincarcerated criminals also tend to be unavailable for survey interviews, the same sample bias would be expected. Thus, one could view our sample as excluding those whose perceptions of punishment risks were the most weakly correlated with actual risks—a belief supported by a comparison of the arrestee correlations with non-arrestee correlations in Table 9.2.

It might be speculated that prospective offenders might be more aware of the statutory penalties "on the books" than the severity of sentences actually imposed, so the former might affect perceptions of severity even if the latter did not. This speculation is, however, undercut by empirical evidence on public knowledge of statutory penalties. Roberts's (1992, 112–113) summary of the evidence indicates that the public is widely ignorant of statutory maxima and minima, even for well-publicized offenses. This ignorance should tend to weaken the link between actual and perceived punishment levels. Further, to the extent that people misestimate penalty severity, they underestimate it, which should weaken deterrent effects of the penalties.

It might be suspected that some of the variables we controlled were intervening variables mediating the effect of actual punishment levels on perceived punishment levels. For example, punishment levels might affect crime rates (whether through deterrent effects or by other means), and thus the likelihood of the individual respondent being victimized or the respondent's perceptions of the relative level or trends in crime in his or her area. If this were true, we might have "controlled away" some of the indirect effects of actual punishment levels on perceived punishment levels. This speculation, however, is contradicted by the fact that introducing the controls into the analysis had virtually no effect on the APL/PPL associations. The simple bivariate associations were essentially zero to begin with (average $r = 0.02$, Table 9.2).

No measurements are perfect, and to the extent that our measurements of actual and perceived punishments are affected by random error, the associations will be attenuated, favoring the null hypothesis. Perhaps perfect measurement of the variables would have resulted in strong associations, but the attenuation due to measurement error would have to be substantial indeed to suppose that the near-zero associations observed between the measured variables are concealing strong associations between the true variables.

More broadly speaking, it is impossible to prove a negative and thus to prove the null hypothesis. The most precise way to summarize these findings is to say that they consistently fail to support the hypothesis that higher actual punishment levels lead to higher perceived punishment levels.

## The Reaction to These Findings by Deterrence Doctrine Advocates

This research was first published in 2005 in the journal *Criminology*. The reaction to it since then among scholars committed to the deterrence doctrine is instructive. The reaction among economists was uniform—they ignored it and continued assuming that official punishment rates can serve as adequate proxies for perceived risk in the macro-level studies they prefer. Based on a search of the Web of Science database, between 2005 and 2015 the article was cited just once in an economics journal, in a footnote, and that one article did not cite the findings (Kleck 2016).

In an extended defense of the deterrence doctrine published in the prestigious *Crime and Justice*, Daniel Nagin (2013, 247–248) discounted the results of the Kleck et al. (2005) research, insisting that the findings were actually irrelevant to the issue of whether punishment-based crime control policies have deterrent effects because most members of the general public "have no intention of committing the types of crimes surveyed in these studies" (248). He began his critique by borrowing the prestige of two of the classical godfathers of rational choice theory, Ceasare Beccaria and Jeremy Bentham, in the service of his claim that it is irrelevant whether members of the general public are ignorant of punishment risks. Nagin does not cite any specific pages where either authors stated anything like this, presumably because neither ever wrote any such thing. Quite the contrary—both believed that it was crucial that noncriminals believe that risks of legal punishment were significant because this was a major reason why they remained noncriminals. They believed that there were always significant numbers of people who did not commit crime under normal circumstances but would be willing to do so if the risks of legal punishment were low enough.

Bentham explained all human behavior with his principle of utility, which stated that a given behavior (such as a criminal act) would be engaged in to the extent that it "appears to . . . augment or diminish the happiness" of the actor and thus was a function of anticipated pleasures and pains—or as we would say today, benefits and costs (Bentham 1789 [1988], 2). It is noteworthy that Bentham's use of the phrase "appears to" indicates that he recognized that it was what people *believed* that affected their behavior, that *perceived* pains and pleasure were what mattered, rather than actual pains and pleasures per se. Even more important, Bentham stated that this principle of balancing perceived pains and pleasures applied to *all* actions, not just those of criminals. Thus, the perceptions of the risk of suffering the "pains" of punishment affect the behaviors of everyone, not just criminals.

Bentham, then, offers no support for Nagin's novel idea that noncriminals' perceptions of punishment risk are irrelevant to the frequency of crime, but the idea is worth exploring for what it would imply for deterrence-based crime control if it were correct. In 1994, Nagin concluded that the people least likely to be deterred by the threat of punishment were those who were present-oriented and self-centered (Nagin and Paternoster 1994, 581). As many have noted, it is criminals who fit this description best and noncriminals who fit it least (e.g., Gottfredson and Hirschi 1990). Thus, threats of punishment work well in deterring noncriminals from becoming criminal but work poorly in deterring present-oriented, self-centered criminals from continuing their criminal actions. The Nagin of 2013 claimed that actual punishment levels affect perceived risks among criminals, while not disputing the finding of Kleck and his colleagues that actual punishment levels have no impact on perceived risk among noncriminals (Nagin 2013). The following would therefore be diagrammatic summaries of Nagin's views as indicated by a combination of his 1994 findings and his 2013 speculations:

| | | | | | |
|---|---|---|---|---|---|
| **Criminals:** | Actual Punishment | ➔ | Perceived Punishment | | Criminal Behavior |
| **Noncriminals:** | Actual Punishment | | Perceived Punishment | ➔ | Criminal Behavior |

Stated verbally, criminals' perceived punishment risks are responsive to actual punishment levels (Nagin 2013), but this does not deter crime because criminals are present-oriented and self-centered and therefore are not affected by their perceptions of future punishment risk (Nagin and Paternoster 1994). On the other hand, perceptions of future punishment risk *do* affect the criminal behavior of noncriminals (Nagin and Paternoster 1994), but this has no crime control value either, since noncriminals' perceptions are not affected by actual punishment levels (Nagin 2013). Either way, if one accepts both Nagin's past research findings and his recent speculations, deterrence-based crime control fails.

Note, however, that there is no empirical support for Nagin's speculation that criminals' risk perceptions are significantly more responsive to actual punishment levels that noncriminals' perceptions are. Although he did not explicitly claim that there *is* a closer perception/reality link among criminals, he strongly hinted that there is such a link by stressing that it is criminals who have a "need to know" about the "sanction regime." Nagin actually knew better. The critical fact that he carefully withheld from his *Crime and Justice* readers was that Kleck and his colleagues had already explicitly tested the hypothesis that the perception/reality link was closer among criminals than among noncriminals and *found no support for it whatsoever.* As discussed earlier in this chapter, we carried out analyses of the reality/perception linkages separately among arrestees and non-arrestees. Not only did the results *not* indicate better awareness of "the sanction regime" among arrestees, they indicated an even *worse* correspondence of perception with reality among arrestees, in that there was actually a weak *negative* correlation of arrestees' perceptions with actual punishment risks. Lochner (2007) (another study with which Nagin was quite familiar) similarly found no closer a correlation between perceived and actual risks of arrest for auto theft among offenders than among nonoffenders (449). While offenders may have more of a "need to know" about punishment risks, this need does not result in them *actually* knowing about them any better than nonoffenders.

Even if Nagin's speculation that criminals' risk perceptions are more accurate than those of noncriminals were true, it would be irrelevant if the sample we used had included many persons willing to commit serious crimes if they thought the risk of punishment risk was low enough. In that case, there should still have been substantial reality/perception correlations in the sample as a whole even if the correspondence was only strong among those willing to offend. Nagin was in effect assuming that our sample of residents of big high-crime urban counties interviewed in 1994 did not include any significant number of people willing to commit the crimes we studied. Nagin cited no empirical support for this remarkable claim, presumably because there is none. Criminal behavior is in fact widespread in the U.S. population and is especially common in large urban places. By the age of 23, between 25 percent and 41 percent of the U.S. population have committed crimes often enough to have been arrested (Brame, Turner, Paternoster, and Bushway 2012; Brame, Bushway, Paternoster, and Turner 2014), a percentage that would obviously be even higher if the data covered arrests after age 23. And of course, a much larger share of the public commits arrestable offenses but avoids arrest. In samples largely composed of residents of high-crime big cities, the proportion would be higher still since urban crime rates are much than in the nation as a whole. Nagin knew that arrested persons were common in the Kleck sample, since the *Criminology* article that he cited reported that 12 percent of the sample *admitted to* a non-traffic arrest (641; see also Table 9.1 herein), which was almost certainly an underestimate of the share that had actually been arrested. Thus, there were ample numbers of respondents willing to commit serious crimes included in our sample, and yet there was still no evidence that actual punishment levels affected perceptions of punishment risk, even among those with a "need to know." In sum, Nagin's argument was not only speculative, but also utterly inconsistent with extant empirical evidence.

Perhaps what is most ironic about Nagin's (2013) "irrelevant sample" argument, intended to help save the deterrence doctrine, is that it required him to adopt an invalid argument routinely made by *opponents* of deterrence-based policies. Opponents of punitive policies often claimed that the death penalty had no effect on murderers, citing as evidence the fact that the prisons were full of killers who obviously had not been deterred by the threat of capital punishment. Zimring and Hawkins (1973) long ago pointed out the logical error underlying this argument, dubbing it the "Warden's Fallacy," after a prison warden who had made this argument. The fallacy was that the successes of deterrence were not to be found among prison inmates or among criminals in general, but rather among those who, as a result of deterrence-based policies working, did *not* commit crimes. No matter how many failures of deterrence that could be found in prisons, there might be far more persons who had been successfully deterred by the threat of punishment. And of course, for deterrence-based policies to reduce crime, the threat must be perceived by prospective offenders and there must be some correspondence between actual risks and perceptions of those risks. The successes of deterrence, and thus those whose perceptions correspond to actual risks, are therefore to be found within the largely *noncriminal* population—the very population that Nagin insisted was irrelevant to tests of the perception/reality connection. By turning logic on its head in this way, he was ironically falling prey to one of the more clearly fallacious arguments employed by opponents of deterrence.

As we noted in Chapter 3, the threat of punishment is most likely to affect the criminal behavior of persons who fall between the two extremes of criminal propensity: (a) hard-core offenders of the type found in prisons who are powerfully motivated to commit crimes and have become inured to threats of punishment, and (b) those so virtuous that they never even consider committing crimes, for whom the potential for legal punishment is simply irrelevant. One could scarcely imagine a population that fits this description better than the noninstitutionalized population of America's biggest urban areas. The high crime rates of these areas indicate that they are characterized by conditions that induce many of the residents to commit crimes. The noninstitutionalized character of the Kleck et al. sample, on the other hand, suggests that its members were not such habitual offenders that their behavior had gotten them sent, at the time of the survey, to jail or prison. In short, this sample was ideal for detecting whether perceptions of legal risk correspond to actual risks among potential offenders.

Nagin also made the curious remark that "the ratios calculated by Kleck and colleagues pertain only to criminal opportunities that have actually been acted on" (2013, 248). Given that punishment obviously can only follow crimes that have actually been committed, this odd comment is true but irrelevant to the reality/perception issue. It is true enough that ratios of arrests to crimes actually committed do not capture criminal opportunities that were not acted upon, but it is also utterly irrelevant to the question at issue—the degree to which perceptions of arrest risk correspondent to actual risks of arrest. Arrest ratios, regardless of whatever limitations they may have as measures of police effectiveness, are clearly measures of the legal risks of the criminal opportunities acted upon, and these *are* the punishment risks that respondents in the survey were asked about.

Nagin attributed to Apel (2013) the claim that measures of arrest per crime calculated at the county level "may be poor indicators of risk at the specific locations where would-be offenders are plying their trade" (Nagin 2013, 248). The key word is "may"—the assertion is totally speculative and without any supportive empirical evidence. Further, if the term "specific locations" refers to very small areas such as those within sight of a prospective offender, the speculation is trivial from the standpoint of crime control policy. Awareness of legal risks that are this extremely local would be more likely to produce mere spatial displacement of crime rather than its deterrence. If offenders were aware only of the risk of arrest within, say, one block of the location where they initially consider committing a crime, it would require only a negligible amount of additional time and effort to shift the crime to a place where no such risk was perceived (Chapter 3). Further, this entire line of speculation was irrelevant to four out of the five categories of punishment risk variables that we studied, since the rest of these measures were inherently county-based. They pertained to risks generated by county and circuit courts—conviction rates, prison sentence rates, average length of maximum sentence, and the time from arrest to sentencing.

Even with respect to the risk of arrest, however, the Nagin-Apel argument is illogical. If they were correct in their speculation that prospective offenders accurately perceive arrest risks at "specific locations" within their county where they "ply their trade," there still should have been a correlation between individual risk perceptions and county-level realities, since a county-level objective arrest risk is nothing more than a weighted average of local objective arrest risks. The actual arrest risks of small local subareas of counties (such as blocks) are necessarily positively correlated with the arrest risks of the counties in which the subareas are located. If criminals were familiar with arrest risks in small subareas of the county in which they reside (the Nagin-Apel speculation), which in turn are positively correlated with the county-level arrest risks, then one should still expect to find individual perceptions of arrest risk to be correlated with county arrest rates. No such correlation was found, among either arrestees or non-arrestees, for any of five offense types.

Given the complete absence of a correlation between individuals' perceived arrest risks and *county-level* actual arrest risks, the only way that individual arrest risk perceptions could be positively correlated with *local* actual arrest risks in some specific locations in the county would be if these positive correlations were counterbalanced by *negative* perception/reality correlations in other local areas within the county. In the absence of the latter eventuality, which certainly was not endorsed by Nagin or Apel, there still should have been a significant positive correlation between individual perceptions of arrest risk and county-level actual arrest risk. Again, no such correlation was observed.

All this raises the question: why would such a sophisticated and knowledgeable scholar as Nagin make such conspicuously weak or irrelevant arguments? His critique did not identify any known flaws in the evidence or analysis but was instead based entirely on implausible speculations about the supposedly irrelevant nature of the sample interviewed and an irrelevant observation about the way that one of the five legal risks was measured. The transparently weak nature of Nagin's critique could be seen as evidence of just how determined he was to minimize the impact of this research. Whereas Nagin had previously stressed the critical need for evidence on the impact of punitive policies on punishment perceptions (1998), once he got the evidence he had called for, he completely discounted it, entirely on the basis of implausible one-sided speculations. The evidence cast grave doubts on what Nagin himself had explicitly acknowledged to be an essential assumption underlying deterrence-based crime control policy: "the conclusion that crime decisions are affected by sanction risk perceptions is not a sufficient condition for concluding that policy can deter crime. *Unless the perceptions themselves are manipulable by policy, the desired deterrent effect will not be achieved*" (Nagin 1998, 5, emphasis added). The best available evidence indicates that under current conditions, sanction perceptions are *not* routinely "manipulable by policy," and thus, according to Nagin's own reasoning, increases in deterrent effects are not achieved. Since Nagin was unlikely to repudiate his own prior beliefs, he had little choice but to try to "speculate away" the evidence undercutting one of the central tenets of deterrence-based crime control, if the case for deterrence-based crime control was to be saved.

In a trivial sense, prospective offenders do accurately perceive some extremely local indicators of risk, if the indicators are perceptually prominent enough. For example, criminals can hardly fail to notice a police patrol car driving past them, and they presumably delay committing crimes they had been contemplating until the car is out of sight. This, however, would be mere temporal displacement of crime, not its prevention, and thus would be neither an instance of deterrence nor a benefit to the public.

The evidence for a correlation of actual legal punishment risks and perceptions of those risks is so uniformly weak that defenders of the orthodox deterrence doctrine have resorted to citing evidence having nothing to do with legal punishment to buttress their views. Apel, Pogarsky, and Bates (2009) claimed support for a "sanctions-perceptions link" entirely on the basis of eighth grade students being aware that "rules for behavior" are stricter in high school than in middle school (208, 210). This finding has no bearing on the perceptions/reality correspondence for risks of legal punishment for crime, but the fact that the authors insisted that it was somehow relevant is diagnostic of how far advocates of the deterrence doctrine are willing to go to salvage their favored position. If one is to interpret evidence of a perceptions/reality correspondence regarding literally *any* kind of risk, no matter how dissimilar to legal punishments of crime, as support for the deterrence doctrine, it virtually renders the theory of criminal deterrence nonfalsifiable.

To summarize, there is generally no significant association between perceptions of punishment levels and actual levels of punishment produced by the criminal justice system, implying that increases in punishment levels do not routinely reduce crime through the increased operation of general deterrence mechanisms. Increases in punishment might reduce crime through increased incapacitative effects, through the effects of treatment programs linked with punishment, or through other mechanisms, but they are not likely to do so in a way that relies on producing changes in perceptions of risk.

These findings do not imply that punishment does not exert any deterrent effect. Rather, they support the view that any deterrent effect, however large or small it may be, does not covary with actual punishment levels to any substantial degree, since the perceptions of risk on which deterrent effects depend generally do not covary with punishment levels. There may well be some baseline level of deterrent effect generated by punishment-generating activities of the criminal justice system, but this level apparently is one that does not consistently increase when actual punishment levels are increased or diminish when they are decreased. Increased punishment levels are not likely to increase deterrent effects, while decreased punishment levels are not likely to decrease deterrent effects.

For those seeking ways to improve existing levels of ability to control crime, these findings suggest a need for either (1) a shift in crime control resources towards strategies whose success does not depend on general deterrence effects, or (2) different, nonroutine methods for generating effective deterrence messages. One approach in the latter category is to more narrowly direct very specific deterrence messages at audiences who are at especially high risk of committing crimes in the near future. This was the main idea behind the Ceasefire program implemented in Boston and aimed at reducing youth gang violence. Rather than using apprehension, prosecution and punishment to send broad "wholesale" deterrence messages aimed at the general population, the program delivered direct and explicit "retail deterrence" messages to a relatively small target audience of gang members and potential members. Unfortunately, there has been no rigorous evaluation of this program or any similar one, so it remains to be seen whether more narrowly targeted delivery of deterrence messages works any better than traditional methods (Kennedy 1997).

## Do Highly Publicized Punishment Events Increase Deterrent Effects?

Deterrence is the result of a communications process—legal punishment is inflicted, information about this punishment is somehow communicated to at least some prospective offenders, and some of those receiving these communications are induced to refrain from crimes they otherwise would have committed. We noted in Chapter 2 that more visible punishments may exert more deterrent effect. Most punishment events are not widely publicized and are known only to the small numbers of people directly involved—victims, offenders, and their families and friends. Because the punishment information is not widely disseminated, few prospective offenders can be influenced. Therefore, one might hypothesize that unusually highly publicized punishment events could generate additional deterrent effects that the routine, largely unpublicized punitive activities of the criminal justice system do not.

On a routine basis, the most highly publicized type of legal punishment is the execution of murderers. Correspondingly, it has been asserted that highly publicized executions exert a deterrent effect, albeit a possibly temporary one, on homicidal behavior (Phillips 1980; Stack 1985). This presumes that there is an effect of executions on perceptions of the risk of being legally punished for murder, but there is no direct evidence bearing on this issue. The most relevant *in*direct evidence on the impact of perceptions of the risk of execution on homicide comes from research on the effect of *publicity* about executions, such as the number of newspaper or televisions stories about executions. This research is indirectly relevant to the extent that the perceived risk of execution in a given time or place is positively correlated with publicity about executions or death sentencings. The findings of these studies, however, do not show any consistent evidence of a deterrent effect of publicized executions (Chapter 8). While five studies found some evidence of deterrent effects of more publicized executions (Phillips 1980; Phillips and Hensley 1984; Stack 1990, 1995, 1998), seven other studies concluded that there was no deterrent effect of execution publicity (Bailey 1990, 1998; Bailey and Peterson 1989; King 1978; Hong 2016; Peterson and Bailey 1991; Stack 1993; see Chapter 8 for more detailed discussion).

Since even punishment events that are as highly publicized as executions do not increase the general deterrent effect of punishment, it is unlikely that less publicized punishments will do so. Further, there is a severe upper limit on how much one could increase publicity-dependent deterrent effects, since the very newsworthiness that is essential for gaining publicity would, in the absence of direct governmental control of the news media, decline as soon as a given type of punishment event became more common, since routine events are not newsworthy.

## Do Policy "Experiments" Establish the Operation of Deterrence?

Most macro-level "experimental" studies of specific policy interventions, such as changes in police patrol practices or the enactment of new laws, have no direct relevance to deterrence because they do not actually measure any perceptions of legal risk (e.g., Corman and Mocan 2005). Thus, supposed "deterrent" effects can only be very indirectly inferred from decreases in crime frequency. The key assumption in studies finding drops in crime following a policy intervention is that changes in actual risk of punishment produced changes in perceived risk. This is mere guesswork, not a fact empirically established by the researchers. In light of this chapter's evidence, it is highly implausible guesswork, since it is unlikely that the changes in actual punishment risk produced by these policy interventions altered prospective offenders' perceptions of those risks.

## Is There a "Collective Wisdom" About Legal Risks?

Phillip Cook (1980) suggested that although individuals may misperceive legal risks, large populations will, on average, perceive risks relatively correctly. He believed that even poorly informed offenders acting with limited rationality would, as a group, perceive more legal risk when legal risk in fact increased. He conjectured that this correspondence between actual and perceived risks would come about because offenders were aware of their own punishment experiences and those of associates, which would in turn reflect, on average, the population-wide actual legal risks of criminal behavior (226–228; see Levitt 2002 for similar speculations). If Cook was right, there should be a substantial correlation between the *average* perceived legal risks prevailing within a population and the actual risks.

Kleck and Barnes (2013) directly tested this hypothesis. Using the same survey data analyzed in this chapter, they computed the average perceived certainty, severity, and swiftness of punishment prevailing among the residents of 54 large urban counties and analyzed the county-level associations of these population-level perceptions with actual legal risks. They found there was *no* association between actual legal risks and the average perceptions of those risks among the county populations. Thus, there is no more evidence that average population-wide perceptions correlate, even roughly, with actual legal risks than there was that individual perceptions correlate with actual risks. There appears to be no "collective wisdom" of populations, and thus no reason to believe that increasing actual punishment levels will increase the general deterrent effect of punishment. Nagin (2016) made similar claims of collective wisdom, which were promptly refuted by Pickett and Roche (2016a, 2016b).

Cook's argument that perceived punishment risks in the aggregate would be correlated with actual risk levels was heavily dependent on the implicit assumption that criminals who are punished adjust their perceptions of punishment risk upward and, conversely, that those who commit crimes and go unpunished adjust those perceptions downward. Decades of research, however, have failed to support the idea that punishment experiences cause the persons punished to adjust their perceptions of future punishment risk upward. Our review of 171 independent tests of the hypothesis (Chapter 6) found that only 26 percent of the findings showed a significant positive association between punishment experience and perceived risk, while 10 percent of the associations were significant and *negative* (Table 6.16). Most findings indicated no significant association one way or the other. Thus, in the aggregate, punishing more criminals does not increase the average perceived risk of future punishment among criminals.

Piquero and Pogarsky (2002) argued that vicariously experienced punishment is negatively related to criminal behavior—that is, people aware of the punishment of others may be deterred from doing crime. Regardless of how true or false this may be, our research implies that increases in actual punishment levels will not increase vicarious deterrence, since that requires an effect of actual punishment on perceptions of legal risk, whether the punishment is of one's self or of others one knows. Thus, from the standpoint of crime control policy, it does not matter whether one's criminal behavior can be influenced vicariously by the punishment of others, since policy does not at present influence perceptions of the risk of punishment in the first place.

## Reinterpretation of Macro-Level Research in Light of the Absence of Any Macro-Level Association Between Perceptions of Punishment Risk and Actual Risk Levels

We can now reassess the enormous body of macro-level deterrence research reviewed in Chapter 7 in light of the current chapter's findings. The Chapter 7 review indicated that most research has found that aggregate measures of the *severity* of punishment do not generally show a significant effect on crime rates but did find that a large minority of macro-level findings indicate that the *certainty* of punishment is significantly and negatively associated with crime rates.

To be sure, many of these associations were the product of researchers' failure to address causal order problems. That is, at least some of these associations reflected the effects of crime rates on the certainty of punishment (high crime volume can overwhelm the ability of criminal justice agencies to apprehend, convict, and punish criminals), rather than the reverse. Nevertheless, there have been a few studies that made serious efforts to model possible two-way causation and still found a negative certainty/crime association.

Scholars obtaining such negative associations have commonly interpreted them as supporting a deterrent effect of punishment on crime, but the more careful among them also acknowledged that part of the association was due to the *incapacitative* effects of incarcerating criminals rather than deterrent effects, since the same places and times with greater certainty and severity of legal punishment usually also have larger inmate populations. Only a handful of scholars (e.g., Kleck 1979; Levitt 1995) also controlled for the size of inmate populations, as well as the possibility of reciprocal causation, and still found some remaining negative association between certainty of punishment and crime rates.

Can these few supportive associations be interpreted as evidence of deterrent effects of the certainty of punishment? In light of the present chapter's evidence, the answer at present is "no." The key assumption underlying macro-level tests of deterrence, sometimes acknowledged explicitly, more usually assumed implicitly, is that macro-level measures of the certainty of punishment serve as reasonably valid proxies of perceived certainty, which was what supposedly deterred criminal behavior. Our findings indicate that this assumption is untenable. Not only are macro-level measures of the certainty (or severity or swiftness) of punishment not perfectly or strongly correlated with their perceptual counterparts, they have no significant association at all. That is, peoples' perceptions of legal risk are, on average, unrelated to the actual macro-level levels of punishment certainty used in virtually all macro-level research.

The assumption of a strong correlation between perceptions of legal risk and aggregate measures of actual legal risk was a necessary condition for macro-level research being capable of testing the deterrence doctrine. Since this assumption is not even approximately valid, this means that the entire body of macro-level research fails to provide any valid tests of deterrence and is largely irrelevant to the question of whether more certain, severe, or swift punishment produces more deterrence of crime.

Given the irrelevance of most macro-level research to the deterrence hypothesis, this leaves mainly the individual-level, survey-based research as relevant. Most of this body of research does not support deterrence (Chapters 5 and 6). The strongest individual-level support for any kind of deterrent effect was from survey studies that purportedly indicated that higher perceived certainty of punishment has some effect on delinquent or criminal behavior. Many of these studies, however, actually related current risk perceptions to *past* criminal behavior and thus did not test the deterrent effects of perceived risk because they had the causal order wrong. The negative associations between punishment perceptions and criminal behavior more likely reflected the experiential effects of criminal behavior on perceptions of punishment risk than a deterrent effect of punishment perceptions on criminal behavior.

We concluded (Chapter 5) that this body of research provides at best an uncertain body of evidence, most of it failing to support a deterrent effect. In any case, in light of this chapter's evidence, this body of research turns out to have little or no established relevance to crime control policy because variations in punishment policy do not routinely produce variations in perceptions of legal risk, in which case there is no sound reason to expect increased punishment to reduce crime via increased general deterrent effects.

## Conclusions

To summarize, if the deterrence doctrine is to serve as a useful guide to crime control policy, it is necessary that the actual risks of punishment for crime that the criminal justice system labors to create actually affect perceptions of punishment risk among prospective offenders. The best available evidence indicates that no such affect occurs. Even among populations that are most strongly relevant to tests of deterrence, such as those currently offending, there is no correspondence between the actual punishment risks prevailing in prospective offender's environments and their perceptions of those risks, regarding either the certainty, severity, or swiftness of punishment. Consequently, at the present time, there is no sound foundation for the belief that increasing the actual certainty, severity, or swiftness of punishment will generate more deterrence of crime.

Prospective offenders' responses to punishment levels appear to be characterized by a severely constricted rationality. While many people are capable of weighing perceived risks and rewards when deciding whether to do crime, they typically possess so little accurate information about key risks and rewards that this capacity for rational decision-making remains to a great extent inoperative (Kleck 2003). Potential offenders' awareness of legal risks may be largely confined to the most conspicuous features of their immediate environments at the time a crime is contemplated. They are aware of the presence of a police officer, patrol car, or bystander who might intervene or summon the police but are not sensitive, directly or indirectly, to the overall likelihood of arrest in their areas. In light of the present study's evidence of even worse reality-perception correlations among arrestees than among non-arrestees, the data support the conclusion that neither news media information, nor personal experiences of the actor and his associates, nor any other sources of information of which we are aware provide an adequate foundation for forming even minimally accurate perceptions of the certainty, severity, or swiftness of punishment.

Punishment-generating activities will be continued regardless of the evidence bearing on general deterrence, either for the sake of justice and retribution or for the sake of crime control via (a) incapacitation, (b) the treatment associated with apprehension and conviction, or (c) whatever continuing, though perhaps hard-to-increase, baseline general deterrent effects that punishment may produce. These present findings nevertheless indicate that no deterrent effects would be lost if punishment levels were reduced from their current levels or would be gained if punishment levels were increased.

## References


Apel, Robert. 2013. Sanctions, perceptions, and crime: Implications for criminal deterrence. *Journal of Quantitative Criminology* 29:67–101.

Apel, Robert, Greg Pogarsky, and Leigh Bates. 2009. The sanctions-perception link in a model of school-based deterrence. *Journal of Quantitative Criminology* 25:201–226.

Avio, Kenneth, and Scott Clark. 1978. The supply of property offenses in Ontario: Evidence on the deterrent effect of punishment. *Canadian Journal of Economics* 11:1–19.

Bailey, William. 1990. Murder, capital punishment, and television: Execution publicity and homicide rates. *American Sociological Review* 55:628–633.

Bailey, William. 1998. Deterrence, brutalization and the death penalty: Another examination of Oklahoma's return to capital punishment. *Criminology* 36:711–733.

Bailey, William, and Ruth Peterson. 1989. Murder and capital punishment: A monthly time series analysis of execution publicity. *American Sociological Review* 54:722–743.

Barlow, Melissa, David Barlow, and Theodore Chiricos. 1995. Economic conditions and ideologies of crime in the media: A content analysis of crime news. *Crime and Delinquency* 44:3–19.

Becker, Gary S. 1968. Crime and punishment: An economic approach. *Journal of Political Economy* 76:169–217.

Bentham, Jeremy. 1988[1789]. *The Principles of Morals and Legislation*. Buffalo, NY: Prometheus Books.

Brame, Robert, Michael Turner, Ray Paternoster, and Shawn Bushway. 2012. Cumulative prevalence of arrest from ages 8 to 23 in a national sample. *Pediatrics* 129:21–27.

Brame, Robert, Shawn Bushway, Ray Paternoster, and Michael Turner. 2014. Demographic patterns of cumulative arrest prevalence by ages 18 and 23. *Crime and Delinquency* 60:471–486.

Cohen, Larry. 1978. Sanction threats and violation behavior: An inquiry into perceptual variation. In *Quantitative Studies in Criminology*, ed. Charles Wellford. Beverly Hills: Sage.

Cook, Philip J. 1980. Research in criminal deterrence. *Crime and Justice* 2:211–268.

Corman, Hope, and Nanci Mocan. 2005. Carrots, sticks and broken windows. *Journal of Law and Economics* 48:235–66.

Corman, Hope, and Nanci Mocan. 2015. Alcohol consumption, deterrence and crime in New York City. *Journal of Labor Research* 36:103–128.

Davis, F. James. 1952. Crime news in Colorado newspapers. *American Journal of Sociology* 57:325–330.

Ehrlich, Isaac. 1973. Participation in illegitimate activities. *Journal of Political Economy* 81:521–565.

Erickson, Maynard L., and Jack Gibbs. 1978. Objective and perceptual properties of legal punishment and the deterrence doctrine. *Social Problems* 25:253–264.

Garofalo, James. 1981. Crime and the mass media. *Journal of Research in Crime and Delinquency* 18:319–350.

Gibbs, Jack. 1975. *Crime, Punishment, and Deterrence.* New York: Elsevier.

Gottfredson, Michael, and Travis Hirschi. 1990. *A General Theory of Crime*. Stanford, CA: Stanford University Press.

Hong, Moonki. 2016. *The Short-Term Deterrent Effect of Executions*. Doctoral dissertation, College of Criminology and Criminal Justice, Florida State University, Tallahassee, FL.

Horney, Julie, and Ineke Haen Marshall. 1992. Risk perceptions among serious offenders: The role of crime and punishment. *Criminology* 30:575–592.

Jones, Terrence. 1976. The press as metropolitan monitor. *Public Opinion Quarterly* 40:239–244.

Kennedy, David. 1997. Pulling levers: Getting deterrence right. *National Institute of Justice Journal* 236:2–8.

Kessler, Daniel, and Steven Levitt. 1999. Using sentence enhancements to distinguish between deterrence and incapacitation. *The Journal of Law and Economics* 42:343–363.

King, David. 1978. The brutalization effect: Execution publicity and the incidence of homicide in South Carolina. *Social Forces* 57:683–687.

Kleck, Gary. 1979. Capital punishment, gun ownership, and homicide. *American Journal of Sociology* 84:882–910.

Kleck, Gary. 2003. Constricted rationality and the limits of general deterrence. In *Punishment and Social Control, Enlarged Second Edition*, eds. Thomas G. Blomberg and Stanley Cohen, 291–310. New York: Aldine de Gruyter.

Kleck, Gary. 2016. Objective risks and individual perceptions of those risks. *Criminology & Public Policy* 15:767–775.

Kleck, Gary, and J. C. Barnes. 2013. Deterrence and macro-level perceptions of punishment risks: is there a collective wisdom? *Crime and Delinquency* 59:1006–1035.

Kleck, Gary, Brion Sever, Spencer Li, and Marc Gertz. 2005. The missing link in general deterrence research. *Criminology* 43:623–660.

Levitt, Steven. 1995. *Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?* Cambridge, MA: National Bureau of Economic Research.

Levitt, Steven. 1997. Using electoral cycles in police hiring to estimate the effect of police on crime. *American Economic Review* 87:270–290.

Levitt, Steven. 2002. Using electoral cycles in police hiring to estimate the effect of police on crime: A reply. *American Economic Review* 92:1244–1250.

Lochner, Lance. 2007. Individual perceptions of the criminal justice system. *American Economic Review* 97:446–460.

Marsh, H. L. 1989. Newspaper crime coverage in the U.S. 1983–1988. *Criminal Justice Abstracts* 21:506–514.

McClellan, Steve. 1997. Crime spree on network news. *Broadcasting & Cable* 127:28, 30.

Mendes, Silvia, and Michael McDonald. 2001. Putting severity of punishment back in the deterrence package. *Policy Studies Journal* 29:588–610.

Nagin, Daniel S. 1998. Criminal deterrence research at the outset of the twenty-first century. *Crime and Justice* 23:1–42.

Nagin, Daniel S. 2013. Deterrence in the twenty-first century. *Crime and Justice* 42:199–263.

Nagin, Daniel S. 2016. What we've got here is a failure to communicate. *Criminology & Public Policy* 15:753–765.

Nagin, Daniel S., and Raymond Paternoster. 1994. Personal capital and social control: The deterrence implications of individual differences in criminal offending. *Criminology* 32:581–606.

Parker, Jerry, and Harold G. Grasmick. 1979. Linking actual and perceived certainty of punishment. *Criminology* 17:366–379.

Paternoster, Raymond. 2010. How much do we really know about criminal deterrence? *Journal of Criminal Law & Criminology* 100:765–824.

Paternoster, Raymond, Linda E. Saltzman, Gordon Waldo, and Theodore G. Chiricos. 1985. Assessments of risk and behavioral experience: An exploratory study of change. *Criminology* 23:417–436.

Peterson, Ruth, and William Bailey. 1988. Murder and capital punishment in the evolving context of the post-Furman era. *Social Forces* 66:774–807.

Peterson, Ruth, and William Bailey. 1991. Felony murder and capital punishment: An examination of the deterrence question. *Criminology* 29:367–395.

Phillips, David. 1980. The deterrent effect of capital punishment: New evidence on an old controversy. *American Journal of Sociology* 86:139–148.

Phillips, David, and John Hensley. 1984. When violence is rewarded or punished: The impact of mass media stories on homicide. *Journal of Communication* 34:101–116.

Pickett, Justin, and Sean Roche. 2016a. Arrested development: Misguided directions in deterrence theory and policy. *Criminology & Public Policy* 15:727–751.

Pickett, Justin, and Sean Roche. 2016b. A few clarifying comments on Picket and Roche. *Criminology & Public Policy* 15:831–836.

Piquero, Alex, and Greg Pogarsky. 2002. Beyond Stafford and Warr's reconceptualization of deterrence: Personal and vicarious experiences, impulsivity, and offending behavior. *Journal of Research in Crime and Delinquency* 39:153–186.

Raudenbush, Stephen, and Anthony Bryk. 2002. *Hierarchical Linear Models: Applications and Data Analysis Methods*. Thousand Oaks, CA: Sage.

Richards, Pamela, and Charles R. Tittle. 1981. Gender and perceived chances of arrest. *Social Forces* 59:1182–1199.

Richards, Pamela, and Charles R. Tittle. 1982. Socioeconomic status and perceptions of personal arrest probabilities. *Criminology* 20:329–346.

Roberts, Julian V. 1992. Public opinion, crime, and criminal justice. *Crime and Justice* 16:99–180.

Roshier, Bob. 1973. The selection of crime news by the press. In *The Manufacture of News*, eds. Stanley Cohen and Jock Young, 28–39. London: Constable.

Saltzman, Linda E., Raymond Paternoster, Gordon Waldo, and Theodore G. Chiricos. 1982. Deterrent and experiential effects: The problem of causal order in perceptual deterrence research. *Journal of Research in Crime and Delinquency* 19:172–189.

Shepherd, Joanna. 2002. Fear of the first strike: The full deterrent effect of California's two- and three-strikes legislation. *Journal of Legal Studies* 31:159–201.

Stack, Steven. 1985. Publicized executions and homicide 1950–1980. *American Sociological Review* 52:532–540.

Stack, Steven. 1990. Execution publicity and homicide in South Carolina: A research note. *Sociological Quarterly* 31:599–611.

Stack, Steven. 1993. Execution publicity and homicide in Georgia. *American Journal of Criminal Justice* 18:25–39.

Stack, Steven. 1995. The impact of publicized executions on homicide. *Criminal Justice and Behavior* 22:172–186.

Stack, Steven. 1998. The effect of publicized executions on homicides in California. *Criminal Justice and Behavior* 22:172–186.

Tonry, Michael. 2004. *Thinking about Crime: Sense and Sensibility in American Penal Culture.* New York: Oxford University Press.

U.S. Bureau of Justice Statistics. 1999. *National Judicial Reporting Program 1996: [United States]* [Computer file]. Compiled by U.S. Department of Commerce, Bureau of the Census. ICPSR ed. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor].

U.S. Bureau of Justice Statistics. 2002a. *Sourcebook of Criminal Justice Statistics*. Available online at www.albany.edu/sourcebook/.

U.S. Federal Bureau of Investigation. 2000. *Uniform Crime Reporting Data [United States]: County-Level Detailed Arrest and Offense Data 1998* [Computer file]. 2nd ICPSR ed. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor].

Vold, George B., Thomas J. Bernard, and Jeffrey B. Snipes. 2002. *Theoretical Criminology* (5th ed.). New York: Oxford.

Zimring, Franklin, and Gordon Hawkins. 1973. *Deterrence: The Legal Threat in Crime Control.* Chicago: University of Chicago Press.

# APPENDIX 9.1

**Table 9A.1** Two-level ANOVA models of perceived punishment

| *Dependent Variable:* | $\sigma^2$ | $\tau_{00}$ | *Intraclass Correlation* | *Chi-square* | *df* |
|---|---|---|---|---|---|
| A. Perceived Arrests per 100 Known Offenses for: | | | | | |
| Homicide | 558.57 | 24.56 | 0.04 | 112.96** | 53 |
| Robbery | 505.96 | 5.69 | 0.01 | 77.26* | 53 |
| Assault | 536.21 | 8.35 | 0.02 | 82.39** | 53 |
| Burglary | 495.88 | 8.99 | 0.02 | 83.35** | 53 |
| B. Perceived Conviction Rates for: | | | | | |
| Homicide | 599.05 | 34.75 | 0.05 | 128.14** | 53 |
| Robbery | 579.90 | 29.49 | 0.05 | 118.08** | 53 |
| Assault | 593.01 | 13.49 | 0.02 | 84.28** | 53 |
| Burglary | 600.23 | 30.31 | 0.05 | 117.50** | 53 |
| C. Perceived Prison Sentences per 100 Adults Convicted for: | | | | | |
| Homicide | 708.83 | 48.56 | 0.06 | 136.32** | 53 |
| Robbery | 598.59 | 44.51 | 0.07 | 146.88** | 53 |
| Assault | 630.81 | 18.78 | 0.03 | 93.20** | 53 |
| Burglary | 610.00 | 32.91 | 0.05 | 127.43** | 53 |
| D. Perceived Average Maximum Sentence for: | | | | | |
| Homicide | 29935.07 | 789.83 | 0.03 | 91.53** | 53 |
| Robbery | 5808.73 | 71.07 | 0.01 | 71.68* | 53 |
| Assault | 4608.78 | 104.70 | 0.02 | 82.77** | 53 |
| Burglary | 4209.12 | 126.50 | 0.03 | 98.56** | 53 |
| E. Perceived Number of Days from Arrest to Sentencing for: | | | | | |
| Homicide | 282801.72 | 1195.33 | 0.00 | 57.48* | 37 |
| Robbery | 108400.93 | 1891.74 | 0.02 | 65.63** | 37 |
| Assault | 151828.61 | 15763.23 | 0.09 | 118.56** | 37 |
| Burglary | 158219.43 | 11501.49 | 0.07 | 108.84** | 37 |

*Notes*:
* Significant at 0.05 confidence level;
** Significant at 0.01 confidence level

# 10

# THE INCAPACITATIVE EFFECTS OF IMPRISONMENT

If America's dominant crime control strategy of recent decades had to be summarized in a single phrase, it would be "lock 'em up" (Walker 2005). That is, the policy has been one of incarcerating an increasingly large number of criminals in prisons and jails. Incarceration might reduce crime in a number of ways (Chapter 2), but the best-researched mechanisms are deterrence and incapacitation. We have already addressed deterrence at length. The individual-level evidence on the effects of personal experience of imprisonment indicates that any special deterrent effect it may have is outweighed by its crime-increasing effects and that the net effect of incarceration on inmates is therefore crime-increasing (Chapter 6). The macro-level evidence indicates that longer prison sentences do not reduce crime via general deterrent effects. Some macro-level research nevertheless suggests that a higher certainty of receiving a prison sentence might have some general deterrent effect. The latter finding, however, is doubtful because, among other deficiencies, supportive research fails to distinguish deterrent effects from the greater amount of incapacitation of criminals that accompanies higher certainty of imprisonment (Chapter 7).

The *incapacitative* effect of punishment refers to crime-reducing effects that are attributable to the punished person being made physically incapable of committing crimes against the general public. Incapacitative effects do not depend on perceptions of punishment risks among criminals or prospective offenders and thus can occur in the complete absence of effective communication of legal threats. Criminal behavior is limited purely as a result of physical restraints, such as being confined to a prison cell. A broad interpretation of "incapacitation" could also encompass executed criminals being rendered incapable of criminal conduct by their death, but the term is more commonly used to describe the restraining effects of incarceration. Those who are locked up cannot commit crimes (or at least those crimes requiring the offender's co-presence with the victim or the victim's property) against persons outside the prison or jail in which they are incarcerated. The term might also be broadened to encompass the effects of electronic monitoring on restriction of offenders' movements, but this effect is probably more properly conceptualized as a variety of special deterrence, since those subject to this monitoring are not physically precluded from committing crimes, but rather are made to believe that their crime-related movements would be detected by the authorities. The vast majority of research on incapacitative effects of punishment has instead exclusively addressed the effects of incarceration.

It was concluded in Chapter 7 that virtually all evidence from macro-level studies that appeared to support a deterrent effect of higher levels of punishment on crime rates may actually have been attributable to the incapacitative effects of imprisonment, since higher arrest or conviction rates or longer prison sentences could contribute to a larger number of criminals being incarcerated at any one time. Given that very few of these macro-level tests controlled for the size of incarcerated populations (Kleck 1979, 1984; Levitt 1995 being among the few exceptions), this means that nearly all findings that seemed to support deterrent effects might actually have reflected, at least partially, incapacitative effects. Indeed, scholars like Marvell and Moody (1994) have concluded that nearly all of the crime-reducing effects of sending people to prison is probably produced by incapacitative effects rather than deterrent or other effects.

Even the few deterrence studies that controlled for prison population, however, measured only the sizes of state and federal prison populations, failing to capture incapacitative effects of incarceration in local jails, juvenile facilities, forensic psychiatric hospitals, and so forth. This is a serious limitation, given that 32 percent of criminals incarcerated in 2014 were in local jails (Table 1.1). At this point, it is fair to say that the whole body of macro-level deterrence research has failed to yield any convincing evidence of deterrent effects of punishment, since few of the studies show any impact of punishment beyond that which could have been produced by incapacitation alone. In combination with the Chapter 6 evidence indicating that any special deterrent effects of being incarcerated on the punished offender are cancelled out by imprisonment's deviance-amplifying effects, this suggests that incapacitation is probably the primary mechanism by which imprisonment reduces crime.

There is little doubt that there is some incapacitative effect of locking criminals up. Incarcerated offenders cannot commit crimes that require direct contact with people in the outside world or their possessions. Further, there is little reason to doubt that locking up more criminals produces some increase in the aggregate incapacitative effect. Unless the courts sentenced only innocent people to jail or prison terms or by sheer coincidence sentenced only criminals for the offense that, even in the absence of arrest, would have been the very last crime of the offender's career, some of the people sent to prison would have committed crimes had they not been incarcerated. Thus, locking up more people prevents at least a little more crime. To be sure, less crime is thereby prevented than may appear to be the case (e.g., due to unincarcerated criminals substituting for those incarcerated and committing crimes that meet a market demand), but some crime is almost certainly prevented by increases in the prison population.

The key policy question, however, is whether we should continue to expand prisons and jails and continue increasing the numbers of criminals incarcerated. The factual issues that must be resolved to answer this question are: (1) whether the impact of prison population size on crime is subject to diminishing returns and has reached a point where further additions to the prison population yield too little crime-reduction benefits to justify its costs, and (2) whether alternative crime control strategies would have more impact on crime or be more cost-effective than prison expansion.

## Simulation Studies

Two broad research strategies have been used to estimate incapacitative effects. First, simulation studies use mathematical models to simulate the number of offenses that incarcerated criminals would have committed had they not been incarcerated. These studies (e.g., Avi-Itzhak and Shinnar 1973; Bernard and Ritti 1991; Clarke 1974; Greenberg 1975; Greenwood 1982; Greenwood and Turner 1987; Shinnar and Shinnar 1975; Spelman 1994; Van Dine, Dinitz, and Conard 1977; Visher 1987; Zedlewski 1987) use mathematical models of varying degrees of complexity to estimate this quantity, and the models are based on a series of assumptions about the frequency and pattern of criminal activity of the offender population.

A simple example serves to illustrate the general approach. Suppose self-report surveys among prison inmates indicated that in their last years before entering prison, the inmates committed an average of 20 serious crimes (say, violent crimes plus burglary) per inmate, per year. One might assume that this is also the rate at which those same offenders would have committed crimes had they not been imprisoned. If there were two million inmates incarcerated at the time of the analysis, one simplistic conclusion would be that incarcerating them prevented 20 times two million, or 40 million serious crimes a year.

Although simulation studies have some possible advantages, including the potential to separate incapacitative effects from deterrent and other effects of punishment, they have lost favor among scholars in recent years. Only one major study of this type has been published in the past 25 years—by Spelman (1994, largely based on his 1988 doctoral dissertation). This development may be partially due, ironically, to the fact that the results of many studies seemed *too* strong. Some simulation-based estimates of the impact of incarceration were so large that they were plainly implausible to all but the most enthusiastic supporters of mass imprisonment. To take our crude example, it would be implausible that locking up two million criminals could prevent 40 million serious crimes (violent crimes plus burglaries) a year, because the U.S. has never had, even prior to the post-1973 prison expansion, more than five million crimes of this type known to the police in any one year (U.S. Federal Bureau of Investigation 1999, 2016). Even assuming three additional unreported serious crimes for each one known to police, it would still be highly unlikely that there were ever 100 million serious crimes to be prevented, by any crime control strategy. Although actual simulation studies are considerably more complex than this example, some of them (e.g., Zedlewski 1987) nevertheless yielded impact estimates that many scholars, including some who had themselves used the simulation approach, found plainly implausible (e.g., Spelman 1994; Zimring and Hawkins 1988).

It is worth noting, however, that these simulation-based estimates cannot be rejected based on the specific *reductio ad absurdum* argument made by Zimring and Hawkins (1988) to discredit Zedlewski's extreme conclusions. Their much-cited article (see Jacobsen 2005 for an example of a scholar who accepted their conclusions) supposedly established that Zedlewski's data and methods implied that *all* crime should have been eliminated by the increases in the prison population of the 1977–1986 period. Zimring and Hawkins' computations of crime reductions did not actually support such a conclusion because their "total crime" figures pertained only to offense types covered in the National Crime Survey (basically the same types covered in the Uniform Crime Reports, minus homicide), whereas Zedlewski's estimates pertained to all crime types covered in a Rand Corporation survey of prison inmates, a much broader set of offenses that included frequently repeated crimes like drug law violations (Chaiken and Chaiken 1982). The latter set of crimes is far more numerous than those counted by Zimring and Hawkins, and the incidence of many of them, such as frauds, cannot be reliably measured by any existing methods. Thus, while Zedlewski's estimates may well be implausibly high, one cannot determine this from the apples-and-oranges comparisons employed by Zimring and Hawkins.

The main scholarly reason for abandoning the simulation approach was that researchers could not obtain reliable information for estimating its most important parameters and had to rely on weakly substantiated assumptions as to their values. They were never able to credibly estimate the key parameters with sufficient precision for the resulting estimates of incapacitative impact to be useful and reliable. Spelman (1994) provided by far the most sophisticated attempt at a simulation study, an attempt that looks heroic in retrospect, given the difficulties he faced.

In particular, credible estimates of "lambda," the average individual offending rate (offenses per year per offender) among criminals, proved elusive. Merely counting crimes that resulted in arrest (e.g., Bernard and Ritti 1991) would obviously grossly understate lambda, but relying on inmate self-reports was at least equally problematic. Low-frequency prison inmates tend to understate their criminal acts, just as respondents in self-report surveys of student and general population samples do. More critically, high-frequency inmates seriously overstate their activity, seeming to boast of their extensive criminal exploits (Marquis and Ebener 1981, 65; Spelman 1994, 47–55). Spelman reanalyzed the Rand prison survey data and found that the self-reported mean number of arrests for personal crimes was about 60 percent higher than the mean number found in official records, while the self-reported mean for property crime arrests was 43 percent higher than what official records indicated (1994, 50). The implication of Spelman's analysis was that past estimates of incapacitation effects that relied on inmate self-reports were, on net, too high. Spelman also found that the distribution of criminal activity, as indexed by arrests, looks far more skewed if one accepts inmate self-reports of arrests at face value, than official arrest records indicate it to be. The share of crime that high-frequency offenders appear to account for, when based on inmate self-reports, is much higher than the share implied by official data. Thus, the benefits to be derived from selectively incarcerating high-frequency offenders will tend to be overestimated if such self-report data are used.

Because high-frequency offenders commit such a large share of crimes, determining exactly how many they commit is critical to judging how much crime is prevented when offenders are incarcerated. Unfortunately, simulation researchers have no practical alternative to using self-reports to estimate absolute levels of criminal behavior among inmates, and this method's flaws seem unavoidable as long as serious criminals will not, or cannot, provide accurate accounts of their criminal activities. One indication of their unreliability is that estimates of lambda are wildly variable across studies. Spelman (1994) noted that estimates of lambda in various studies ranged anywhere from 0.4 to 16.9 crimes per year for violent offenses and anywhere from 0.7 to 125.4 for property offenses.

Similar degrees of uncertainty afflicted estimates of other critical parameters of simulation models such as the average length of criminal careers, the relative probability of arrest for more experienced offenders compared to less experienced ones, the degree to which crime was concentrated among a small subset of offenders, and the level of specialization by crime type prevailing among offenders (Spelman 1994, 16–18). With uncertainty about the key parameters this large, simulations based on them yielded results that were so wildly variable as to be virtually useless. Indeed, the ranges were so great that, depending on which values one accepted, investing in prison expansion could be (a) highly cost-effective, (b) moderately cost-effective, or (c) not at all cost-effective, costing more than it was worth in crime prevented.

Not surprisingly, the conclusions drawn by scholars using simulation techniques were correspondingly divergent. Zedlewski (1987), a Reagan-era Justice Department analyst, concluded that incarceration of a single felon cost $25,000 but produced $430,000 worth of crime prevention, and that prison expansion was therefore well worth the investment. In contrast, Spelman (1994, 227) estimated that a 1 percent increase in the prison population would reduce crime by just 0.12–0.20 percent, and that a cost-benefit analysis indicated that "for most states and the nation as a whole, constructing additional jails and prisons is a risky investment with a very uncertain payoff." Greenberg (1975) concluded that a one-year increase in all prison terms (a huge proportional increase, on the order of 25–30 percent) would reduce crime by only 3 to 4 percent. Even more negatively, Van Dine et al. (1977) described the incapacitative effects of a "stringent" sentencing policy as "minimal" (22), and Clarke (1974) estimated that even doubling the number of juvenile offenders incarcerated would reduce Index crimes by "only 1 to 4 per cent," and that "the benefit is not worth the cost." Some scholars even rejected their own previous simulation-based estimates after reassessing the evidence (compare Greenwood and Turner [1987] with Greenwood [1982]).

The simulation studies that yielded high estimates of crime prevention impact, especially the less sophisticated ones, commonly relied on one or more of the following dubious, usually unacknowledged, simplifying assumptions:

(1) *Offenders who committed X offenses per year in the period just before their admission to prison would have committed crimes at a similar rate during their period of incarceration, had they been free.* (*Equivalently, it is assumed that the offending rate [lambda] is constant across criminal careers, so regardless of when a criminal was imprisoned, one could expect to prevent X crimes per year in prison if the population of all offenders averaged X crimes per year.*) This assumption is seriously wrong because offending rates of typical criminals rapidly decline after their late teens (Blumstein, Cohen, Roth, and Visher 1986; Hirschi and Gottfredson 1983), and few criminals are sent to prison before age 18 (only one half of one percent of state prison inmates in 1997 were under 18 [Strom 2000]). Thus, offending rates would generally have been far lower, had they been free, during the adult ages when inmates are typically imprisoned than they were during the period prior to incarceration. The ages at which criminals are most commonly first sent to prison (c. 21–34 years) are past the peak ages of crime committing, so even if all offenders, including high-frequency younger offenders, averaged X crimes per year, the average among the "older" offenders—say, those past age 25—sent to prison would be less than X, simply because lambda declines throughout adulthood. Maltz and Pollock (1980) also noted that high rates of offending, as indexed by police contacts, for the period prior to inmates' imprisonment could be largely a selection artifact—judges "select" for prison sentences those offenders who had a larger number of recent arrests. Thus, even if actual offending rates did not increase in the period prior to incarceration, this selection process would create the artificial appearance of an offending spurt during the period just before imprisonment. Estimates of incapacitative effects based on arrests for this period, among those selected for imprisonment, would thereby be overstated.

(2) *The offending rate among any additional set of criminals that we might send to prison (such as those currently sentenced to probation rather than prison) will be similar to the rate that would prevail, had they been free, among those already in prison.* This is wrong because criminals sentenced to prison are a select minority of all caught criminals, a subset who were given prison sentences rather than a nonincarceration sentence partly *because* they were higher frequency offenders. Convicted criminals with more prior convictions are more likely to be given an incarceration sentence. Thus, any additional set of criminals sent to prison will have, on average, a lower offending rate than those already in prison, and fewer crimes will be prevented per year of incarceration of the new set of inmates than was the case with those already in prison (Spelman 1995).

(3) *Criminal careers are quite long, so even if you kept criminals in prison for many years, you would continue to prevent each of them from committing significant numbers of crimes, even during the later parts of their sentences.* This assumption is also clearly inaccurate. Although some offenders continue doing crimes well into middle age, even those who were high-rate offenders in their teens and twenties typically are far less active in their later years and many average-frequency offenders stop serious offending altogether by their thirties (Blumstein et al. 1986). Thus, extending the length of prison sentences would not necessarily increase incapacitative effects much and would waste prison space from this standpoint, if existing sentences were already long enough to encompass the relatively active portion of a criminal career of typical length. For example, if the typical active career lasted ten years, and a criminal was incarcerated in his fourth year of that career, any extension of his sentence past the remaining six years of that career would not prevent any crimes and would prevent fewer crimes than if he was released and his place in prison was taken by a more active (and probably younger) offender.

(4) *Incarcerating an offender will prevent any crimes in which he would have been involved, regardless of whether he committed his crimes alone or in groups.* This implicit assumption ignores the fact that many crimes, especially property crimes and offenses committed by younger people, are committed in groups such as gangs or circles of friends and that imprisoning just one member of the group would not prevent many of the group's crimes. Thus, counts of inmates' self-reported crimes will overstate incapacitative effects if it is effectively assumed that all these group crimes will be prevented by the imprisonment of a single co-offender. The potential seriousness of this issue can be judged from Albert Reiss' finding that *half* of burglaries in one sample were committed by offenders in groups (1988, 121). Similarly, data from the National Crime Victimization Survey indicate that at least 44 percent of robberies in 2007 involved groups of offenders (U.S. Bureau of Justice Statistics 2010).

(5) *The offenses committed by any given set of incarcerated criminals could only be committed by those offenders, so incapacitating that set of offenders must reduce the number of crimes committed.* This implicit assumption makes some sense with violent and some property crimes for which there is no "demand," in the sense of a set of customers who want the goods and services provided by the criminal. There is no "demand" for a certain number of predatory crimes (offenses with unwilling victims), such as murders, rapes, robberies, or burglaries, to be committed. In contrast, there *is* a demand for illicit drugs, unlicensed gambling, and the sexual services of prostitutes, a demand that is met by offenders committing the crimes of obtaining, possessing, providing, and selling these goods and services. Thus, for example, locking up a drug dealer who made 200 drug sales per year prior to his imprisonment will not prevent 200 drug sales per year, since one can expect others to take his place—either existing dealers (including associates of the incarcerated dealer) will absorb some of his customers or new dealers will step into the void left by his departure. With unchanged demand, there is little reason to expect much reduction in demand-driven crimes. Falling somewhere in between these two extremes are professional thefts, which may be committed partly to meet the thief's own needs and partly in response to market demand for the items he steals. Professional auto thieves, for example, steal to meet a generic demand for late-model vehicles of any kind and may even steal specific models in response to advance orders from customers. Incapacitating such a thief would prevent some thefts, but not most of his demand-driven thefts, since other, unincarcerated thieves would increase their thefts to take advantage of the imprisoned thief's absence.

The general pattern of these five assumptions is that they served to produce serious overestimates of incapacitative effects in simulation studies. Because the assumptions are false, and substantially so, the simulation-based estimates that rely on them cannot be taken seriously.

## A Cross-Individual Alternative to Simulation Studies

Sweeten and Apel (2007) developed a different individual-level method for estimating the impact of incapacitation that substantially improves on simulation studies. Instead of using rates of self-report offending of inmates *prior to their incarceration* to estimate lambda (the individual offending rate), they compared the self-reported offending of unincarcerated youth with that of similar incarcerated youth *over the same periods in their lives* to approximate the amount of offending in which the incarcerated youth would have engaged had they not been locked up. They worked to insure that the two groups were similar with respect to the factors that influence criminal conduct and the risk of being sent to an institution by using variants of propensity score matching. A propensity score, in this instance, is a number that reflects the likelihood that a given individual would be incarcerated. The number reflects how high or low an individual is on the entire set of factors available to the analysts that influence this outcome. In the Sweeten and Apel research, the propensity score was based on 23 variables that were available in the National Longitudinal Survey of Youth and that were found to be related to the likelihood of incarceration.

Each youth in this survey who had been incarcerated was matched to another surveyed youth with a similar propensity score who had not been incarcerated. In this way the researchers controlled to some degree for the factors that affect offending and that might also differ between incarcerated and unincarcerated youth. As a result, the rate of offending behavior of the matched unincarcerated youth could serve as a reasonable approximation of the rate of offending in which the incarcerated youth would have engaged had they not been locked up. Thus, in contrast to simulation studies that assumed that offending rates of inmates would have been the same (had they not been incarcerated) during incarceration periods as they were in the times just before incarceration, Sweeten and Apel used data pertaining to the *same* periods of time and same stages of life when some youth were incarcerated, but for *another* set of individuals—otherwise similar unincarcerated persons—to estimate the number of offenses that would have been committed by the incarcerated persons had they not been locked up.

Sweeten and Apel (2007) obtained estimates of incapacitation effects that were far lower than had been obtained in the simulation studies. Zedlewski (1987), for example, had claimed that each year an adult offender was imprisoned prevented 187 serious crimes. Sweeten and Apel's data covered assaults, thefts, and other property crimes (roughly the scope of offenses covered in the FBI Crime Index, minus murders and rapes), and they estimated that each year of incarcerating a juvenile aged 16 or 17 prevents 6.2–14.1 offenses of these types, while each year of incarcerating an adult age 18 or 19 prevents 4.9–8.4 offenses (318). Because few juveniles are incarcerated, even in juvenile institutions (see Table 1.1—juveniles claim only 2.4 percent of persons incarcerated), and those few are locked up only for short periods of time, it is primarily the adult estimate that is relevant to assessments of the aggregate impact of incarceration. Further, since offending rapidly declines after age 19, the incapacitative impact of imprisoning the vast majority of inmates—nearly all of whom are older than 19—would be still lower than that applying to persons age 18–19. Thus, these results imply that, as of the 1997–2003 period to which the data pertain, imprisoning the average adult criminal for one year probably prevented fewer than six or seven serious offenses of the type Sweeten and Apel covered.

This research was a considerable improvement on the simulation studies in its ability to credibly approximate the rate of offending that would have prevailed among incarcerated offenders had they not been locked up. In particular, the behavior of the propensity score-matched unincarcerated persons is likely to be a far better approximation than is the behavior of incarcerated persons in the period just prior to their imprisonment. The probability survey sample used by Sweeten and Apel was also far more representative of the national population than the convenience samples of state prison inmates typically used in simulation studies. Further, their data are far more contemporary than those used in simulation studies, reflecting the reality of crime in the 1998–2002 period.

On the other hand, the credibility of the Sweeten and Apel findings depends heavily on how well their propensity scores actually predict incarceration, and thus the degree to which they permit the matching of unincarcerated youth who are genuinely similar to incarcerated youth with regard to the factors that influence the risk of incarceration. While the authors convincingly documented that unincarcerated individuals were successfully *matched* on propensity scores to incarcerated individuals, they did not document how well their propensity scores statistically predict which youth were incarcerated, and thus leave it in doubt how effective their matching on propensity scores was in achieving the goal of creating two groups that were highly similar with regard to all factors that influence incarceration. In an email communication, Sweeten reported that the pseudo R-squared of the logistic regression equations predicting risk of incarceration was 0.27 for both the incarcerated and unincarcerated groups of youth (Sweeten 2010)—not an overwhelmingly high level of predictive ability. Thus, the authors only partially controlled for some of the factors that affect incarceration and only imperfectly matched incarcerated and unincarcerated youth. Nevertheless, these results on the whole probably provide the best individual-level basis currently available for estimating the incapacitative effect of incarceration.

## Can an Effective Selective Incapacitation Sentencing Policy Be Implemented?

The early simulation studies focused mainly on the impact of collective incapacitation, i.e. effects attributable to the sheer size of the imprisoned population, but later scholars focused on sentencing policies based on the principle of selective incapacitation. They asserted that it was possible to increase the incapacitative effect of incarceration by being more selective about which criminals were sentenced to prison, reserving such sentences for more serious, high frequency offenders (Greenwood 1982). While this assumption was eminently reasonable in principle, it proved to be very hard to implement in practice in any thoroughgoing way, for a number of reasons. First, justice-based concerns about the severity of the penalty that an offender deserves often conflict with the principles of selective incapacitation, as when a person with few official indications of prior criminal behavior commits an isolated but heinous crime (Gottfredson and Hirschi 1990, 263–264). The selective incapacitation philosophy implies that this person should be a low-priority candidate for incarceration because he is unlikely to commit many offenses in the future, while a "just deserts," or retribution-based, philosophy implies that he should be incarcerated because a serious crime calls for a severe penalty.

Second, even if some judges were willing to minimize use of just-deserts considerations, they would still be handicapped by the fact that courts have, at the time of sentencing, little information on factors that (a) effectively predict future offending, and (b) are legally permissible to use in sentencing. Sex, race, or employment status might greatly improve our ability to identify those who will be high-rate offenders in future, but using those factors to determine a criminal sentence would violate moral notions of fairness and, in some cases, the legal principle of equal protection before the law. Likewise, the school performance of juveniles undoubtedly improves our ability to predict future criminal conduct, but few are willing impose harsher treatment on youth because they are dyslexic or suffer from other learning disabilities that impair school performance. Other information on predictors of recidivism, such as details on type and frequency of illicit drug use or the juvenile records of adult criminals, is typically not available to judges. Aside from various measures of prior criminal behavior, such as prior convictions, sentencing decision makers possess few measures that are both good predictors of future crime committing and are legally usable. Thus, they have only limited ability to focus prison sentences on the defendants most likely to reoffend.

Third, even prior record indicators of criminal activity levels often are available too late to do much good in maximizing incapacitation effects. Records of delinquent behavior as a juvenile are often unavailable when young adult offenders are sentenced, and by the time criminals have accumulated multiple adult convictions they are usually in the late, declining parts of their careers (i.e., in their late 20s or 30s) when incarceration would prevent few crimes (Gottfredson and Hirschi 1986, 263–264; Greenwood and Turner 1987; Visher 1987).

Finally, many decision-makers are morally uncomfortable with the idea of appearing to punish criminals for crimes that they were predicted to commit at some time in the future, rather than crimes they had actually committed (von Hirsch 1985). This would be an ethical problem in using any predictive scales that relied on factors other than prior criminal convictions to identify future frequent offenders.

Many different prediction scales were developed to identify in advance offenders likely to commit many serious offenses in the future, but none proved much more effective than existing sentencing practice. Since sentencing authorities already made use of a number of the few predictors that are effective, such as number and seriousness of prior convictions, gains from use of the newer predictive scales were modest (Visher 1987). Further, all of the prediction instruments had high rates of false positives—many offenders who were predicted to be seriously criminal in the future actually turned out to be not seriously criminal. This implied that use of the scales to guide sentencing decisions would frequently result in both (a) injustice in the form of inflicting severe punishments on offenders who did not merit such harsh treatment and (b) the relative waste of prison spaces on lower-frequency offenders.

Spelman (1994, 229–288) conducted the most careful analysis of selective incapacitation and concluded that selective *enforcement* efforts by police and prosecutors, focusing more on repeat offenders, were more likely to reduce crime, and less subject to the aforementioned ethical constraints, than selective *sentencing* policies. He estimated that widespread implementation of repeat offender programs that represent the best of current police and prosecutor practice could reduce crime by two to five percent without increasing enforcement costs. While expanding both selective enforcement practices *and* selective sentencing could have still larger total effects, ethical and legal constraints make it less feasible that optimal selective sentencing policies could actually be implemented.

Although the concept of selective sentencing faced seemingly insurmountable ethical and practical problems, this did not prevent legislators from trying to implement it. One clear-cut example of an effort to focus prison sentences more on repeat offenders was "three-strikes" laws and their variants, which imposed harsh sentences (up to life imprisonment) on offenders convicted of qualifying offenses for the third (or, in some states, second) time. These laws came up against the familiar problem of recognizing repeat offenders too late, near the ends of their careers. Further, the laws did not substantially affect the sentences of many offenders, since judges would have sentenced most criminals with two prior convictions relatively harshly even without the laws. Not surprisingly, the laws appear to have had little or no impact on crime rates (Kovandzic, Sloan, and Vieraitis 2004; Males and Macallair 1999; Stolzenberg and D'Allessio 1997; Worrall 2004; Zimring, Hawkins, and Kamin 2001; but see Shepherd 2002 for an exception). Worse still, the laws may have actually *increased* murder rates by providing a new incentive for twice-convicted criminals to kill their victims, as a way of avoiding arrest and conviction for a third-strike offense that was, as a result of these laws, punished almost as harshly as murder (Kovandzic, Sloan, and Vieraitis 2002; Marvell and Moody 2001).

Many of the more serious limitations of the selective sentencing strategy seem impossible, at least under current conditions, to avoid, such as (a) judges basing sentencing decisions partly on factors like the heinousness of the current offense, that may have little utility in predicting future offending or (b) moral constraints on which factors we are willing to use in sentencing decisions. Even though being age 13 to 16 is a strong predictor of increased offending in the near future and locking offenders up before they hit their peak offending years would increase incapacitative effects, it is unlikely that the legal system is going to reverse its current morality-based preference for treating juveniles more leniently than adults.

In light of the foregoing constraints, the impact of further implementation of selective incapacitation policies is likely to be modest at best. Based on his very sophisticated simulation study, Spelman (1994, 312) concluded that "even the most favorable selective policy would reduce crime by no more than seven percent." Such selective policies may well be cost effective, but their aggregate impact is nevertheless likely to be modest. America in recent decades has not, however, seriously pursued a selective incapacitation strategy so much as it has pursued a policy of massively increased *collective incapacitation*—increasing crime control impact through increases in the sheer size of the prison population. We now turn our attention to efforts to assess the impact of the size of the prison population on crime rates.

## Macro-Level Studies of the Impact of Prison Population Size on Crime Rates

The main alternative to the simulation approach to assessing the impact of prison on crime is to apply variants of multiple regression to empirically estimate the real-world association between the size of the prison population and crime rates among macro-level units like states or the nation. These nonexperimental "prison population" studies do not require assumptions about individual offending rates, the length or shape of criminal careers, or any of the other crucial parameters needed to generate meaningful findings from the simulation models. Instead, they simply observe the actual association between crime rates and the number of criminals incapacitated by imprisonment, controlling for other factors that influence crime rates. These studies generally shared a significant disadvantage, relative to simulation studies—the mirror image of a flaw in macro-level tests of deterrence. With few exceptions (e.g., Kleck 1979, 1984), they did not distinguish between (a) the deterrent effects of greater certainty or severity of punishment and (b) the incapacitative effects of large numbers of criminals being locked up. Both certainty and severity of punishment can be expected to covary with the size of the prison population, so deterrent effects of the former could be confused with incapacitative effects of the latter. In two national time series studies, Kleck found that larger prison populations per capita were significantly and negatively associated with homicide rates, controlling for the certainty of arrest and conviction for homicide. To the extent that controls for certainty of punishment partially control for deterrent effects, it was more reasonable to interpret the remaining prison/homicide association as reflecting the incapacitative effect of the size of the prison population.

Studies of the impact of prison population size on crime rates should not be confused with those that estimate the effect of the *probability* of imprisonment. Studies that examine the impact of measures such as prison admissions per 1,000 crimes or prison commitments per 100,000 population (e.g., Cappell and Sykes 1991; Ehrlich 1973; Nagin 1978) are more properly regarded as tests of the macro-level proposition that higher certainty or severity of punishment increases its general deterrent effect on crime. Those kinds of studies were already reviewed in Chapter 7.

### *Do Crime Rates Affect Prison Rates?*

One of the technical problems that is claimed to afflict use of this approach is the possibility that there is a contemporaneous two-way causal relationship between prison population and crime rates, i.e., that crime rates in year **t** could affect the prison population in year **t**, as well as the reverse. The issue is crucial to judging whether locking more people up reduces crime, because researchers have obtained substantially different results depending on whether their models assumed a two-way relationship. Economists such as Levitt (1996) and Spelman (2005) appear to regard it as self-evident that prison rates are affected by crime rates, Levitt flatly stating that "there is also little question that increases in crime will translate into larger prison populations" (1996, 322).

Actually, it is not at all obvious that variations in crime rates have any short-term effect on changes in prison rates, and they may not have much long-term effect either. For example, Langan (1991, 1572) estimated that only 9 percent of the growth in prison admissions from 1974 to 1986 could be attributed to increases in crime rates. Other scholars have concluded that crime rates have no measurable short-term effects on prison populations. This seemingly counterintuitive idea becomes understandable once one appreciates some of the basic facts of the way the criminal justice system actually works in America.

Consider first the simple reality that even at times of the historically lowest crime rates, there has always been more than enough serious crime to fill America's prisons, no matter what their capacity. If we define "imprisonable" crimes as offenses for which significant numbers of criminals are actually sent to prison, as distinct from offenses for which prison is merely a theoretical legal possibility, the number of imprisonable crimes committed each year, and even the number of persons arrested for such crimes, is many times the number of slots for prison admission that open each year, even during times with low crime rates and very high prison capacity. That is, regardless of whether crime rates are high or low, there are always far more persons arrested for imprisonable offenses than the prison system can admit, even in the years when the prison capacity reached its highest levels. For example, the greatly expanded American prison system was able to admit 457,096 new inmates in 2004 (Table 1.2), but, in that same year, 1,204,314 persons were arrested for a violent Index crime, burglary, or drug selling—just a few of the offenses that commonly result in a prison sentence (U.S. Federal Bureau of Investigation 2005). Thus, even in a year with a relatively low crime and a very high prison capacity, there were 2.6 times as many people arrested, for just a handful of the imprisonable types of crime, than the prison system could absorb in the form of new admissions.

Crime rates therefore can only affect the size of this enormous oversupply of caught criminals eligible for a prison sentence, not the number that are actually sent to prison. In this light, it is not at all obvious why crime rates should affect the number of people in prison. Why should the prison population covary with the degree to which the number of imprisonable crimes, and thus the number of people who could be arrested, convicted, and sentenced to prison, *exceeds* the ability of the prison system to absorb new inmates? Regardless of whether crime rates are high or low, we always have far more than enough convictable criminals to fill the prisons.

Furthermore, for the entire span of history for which we have the relevant data, prison systems in America have always operated near, at, or over capacity. Strictly speaking, for prisons to operate efficiently, they should never be at 100 percent capacity, since some slack is needed to deal with repair and maintenance of cells and unexpected temporary upward bumps in admissions. Nevertheless, the nation's prisons on average operated at population levels *over* 100 percent of design capacity in every year from 1984 (the earliest year for which data are available) through 2014 (Table 10.1). This was true despite enormous increases in prison capacity and little change in crime rates. Even in 2008, when state prisons were at 97 percent of capacity (if one uses the higher definitions of capacity), the impression of a slight amount of unused capacity may reflect little more than the fact that courts and correctional authorities cannot always instantaneously replace released prisoners with newly admitted ones. Because of this systemic "friction," there are likely to be a few prison cells that are empty at any one time but only very temporarily so. The same situation applies to local jails: "Based on the peak number of inmates incarcerated on a given day during the year [2006], local jails nationwide operated at 100% of rated capacity" (U.S. Bureau of Justice Statistics 2007, 7). In sum, America's prisons, at least in recent decades, have always been full, regardless of whether crime rates were high or low. Where a prison is concerned, it is safe to assume that "if you build it, they [inmates] will come."

Thus, the only factor that actually affects variation in the size of the prison population is variation in prison capacity—as prison capacity grows, the number of incarcerated criminals grows by the same amount. The ability of police and courts to fill up all available prison beds is effectively a constant—these institutions always have the capacity to fill up all empty prison spaces in all states at all times. Thus, the only way that one can plausibly argue that increases in crime rates cause increases in prison population is if one asserts that higher crime rates cause higher prison capacity, perhaps by triggering popular demand for more punishment in the form of imprisonment. This possibility is discussed later in the chapter.

For a few exceptional prison systems, usually for short periods of time, there is some unused prison capacity available to be filled, so in these rare circumstances increased crime could theoretically lead

**TABLE 10.1** Prison populations relative to prison capacity—the prisons are always full

| | *Prisoners Under Jurisdiction of (Federal/State) Prisons, Dec. 31* | | *Prison Population as % of (Highest/Lowest) Capacity of (Federal/State) Prisons* | | | |
|---|---|---|---|---|---|---|
| | | | *Federal* | | *State* | |
| *Year* | *Federal* | *State* | *Highest* | *Lowest* | *Highest* | *Lowest* |
| 1984 | 24,363 | 427,739 | 110 | 137 | 105 | 116 |
| 1985 | 40,223 | 462,284 | 123 | 154 | 105 | 119 |
| 1986 | 44,408 | 500,564 | 127 | 159 | 106 | 124 |
| 1987 | 48,300 | 536,784 | 137 | 173 | 105 | 120 |
| 1988 | 49,928 | 577,672 | 133 | 172 | 107 | 123 |
| 1989 | 59,171 | 653,193 | 125 | 125 | 107 | 127 |
| 1990 | 65,526 | 708,393 | 151 | 151 | 115 | 127 |
| 1991 | 71,608 | 753,951 | 146 | 146 | 116 | 131 |
| 1992 | 80,259 | 802,241 | 137 | 137 | 118 | 131 |
| 1993 | 89,587 | 880,857 | 136 | 136 | 118 | 131 |
| 1994 | 95,034 | 959,668 | 125 | 125 | 117 | 129 |
| 1995 | 100,250 | 1,025,624 | 126 | 126 | 114 | 125 |
| 1996 | 105,544 | 1,077,824 | 125 | 125 | 116 | 124 |
| 1997 | 112,973 | 1,131,581 | 119 | 119 | 115 | 124 |
| 1998 | 123,041 | 1,178,978 | 127 | 127 | 113 | 122 |
| 1999 | 135,246 | 1,228,455 | 132 | 132 | 101 | 117 |
| 2000 | 145,416 | 1,245,845 | 131 | 131 | 100 | 115 |
| 2001 | 156,993 | 1,247,038 | 131 | 131 | 101 | 116 |
| 2002 | 163,528 | 1,276,616 | 133 | 133 | 101 | 117 |
| 2003 | 173,059 | 1,295,542 | 139 | 139 | 100 | 116 |
| 2004 | 180,328 | 1,316,772 | 140 | 134 | 99 | 115 |
| 2005 | 187,618 | 1,338,292 | 134 | 134 | 99 | 114 |
| 2006 | 196,046 | 1,375,628 | 137 | 137 | 98 | 114 |
| 2007 | 199,618 | 1,397,217 | 136 | 136 | 96 | 113 |
| 2008 | 201,280 | 1,407,002 | 135 | 135 | 97 | 109 |
| 2009 | 208,118 | 1,407,369 | 136 | 136 | | |
| 2010 | 209,771 | 1,404,032 | 136 | 136 | | |
| 2011 | 216,362 | 1,382,606 | 138 | 138 | | |
| 2012 | 217,815 | 1,352,582 | 137 | 137 | | |
| 2013 | 215,866 | 1,361,084 | 133 | 133 | | |
| 2014 | 210,567 | 1,350,958 | 128 | 128 | | |

*Sources*: U.S. Bureau of Justice Statistics, *Prisoners in 2014*, and previous issues in this series (U.S. Bureau of Justice Statistics 2015); 1984 is the earliest year for which national prison capacity data were published. BJS ceased reporting national figures on capacity of state prisons after 2008.

to a few empty prison beds being filled. Even in these rare situations, however, it is unlikely that criminals who committed crimes in a given year would be admitted to prison in the same year and thereby have a same-year effect on the prison population. The courts rarely work quickly enough to process offenders who committed a crime in a given year so that they are admitted to prison that same year. In 2002, 433,959 offenders were sentenced by the courts and admitted to the nation's prisons (new court commitments), contributing to an end-of-year population of 1,380,516 inmates (Table 1.3). The members of the 2002 population who could be in prison for a crime committed in 2002 would have to be some subset of these 433,959 admissions (31.4 percent of the prison population), since those admitted in a year prior to 2002 obviously also committed their crimes in a year prior to 2002. But even among those admitted in 2002, most committed their crimes in *2001*, since the median time from arrest to sentencing among persons convicted in 2002 was 218 days for violent offenses, 196 days for drug selling, and 161 for burglary. The lag time is still longer once one includes the interval between the offense and arrest and between sentencing and admission to prison. While some of those who committed crimes in the early months of 2002 might have been admitted to prison by the end of 2002, less than half of those sentenced for crimes committed in the middle months would be admitted by the end of the year and hardly any of those who committed their crimes in the later months would be admitted by December 31. Among violent offenders convicted in 2002, only 19 percent were sentenced within three months of arrest and only 49 percent within six months; indeed, 26 percent had still not been sentenced after a year had passed (U.S. Bureau of Justice Statistics 2006d). Even if one generously assumed that half of those admitted in 2002 had committed their crime in 2002, this group would claim only half of 31.4 percent, or 15.7 percent, of the end-of-year prison population. Crime rates could at most affect the size of this 15.7 percent component—the portion of the prison population whose size could be immediately affected by crime increases.

To get a sense of the size of such an effect, consider a hypothetical example. Assume a year with an unusually large ten percent increase in the rate of imprisonable crime. This would increase the 15.7 percent "affectable" share of the prison population proportionally by ten percent, an amount equal to just 1.57 percent of the total prison population. This is an upper-limit estimate of the maximum immediate effect that an unusually large rise in crime rates could have on the size of prison population. Given the many complexities of counting inmates, however, it is unlikely that prison population counts can even be measured to within 1.57 percent of the true count. For example, statistics may cover only those under the legal jurisdiction of prisons rather than those in the physical custody of prisons, may or may not count those under state prison jurisdiction but serving terms in local jails due to prison crowding, may fail to exclude escapees from the counts, or may miss those admitted in the days just before the target date to which counts pertain. Given this degree of measurement error, even unusually large crime increases in a given year could not have a statistically detectable effect on the prison population measured at the end of the same year and could have virtually *no* effect on the mid-year prison counts used by many researchers (such as Liedka, Piehl, and Useem 2006; Marvell and Moody 1994).

One might speculate that judges are influenced by current crime levels to alter their sentencing practices quickly enough to affect the number of prison admissions or that parole boards might quickly change their willingness to release inmates in response to recent crime trends, but there is no empirical evidence of such effects. And in any case, the effects of such changes in attitudes among CJS decision-makers would still be constrained by the fact that the prisons are always effectively full. In sum, there is no reason to expect that crime rates in a given year can have a measurable effect on the prison population in that same year, and thus no strong *a priori* basis for expecting simultaneity problems due to such an effect.

The key fact is that prison populations are always and everywhere virtually identical to prison capacity. Regardless of how this is brought about, it seems to be a fixed constraint on changes in prison population. Prison admissions and prison releases are always adjusted so that prison population is roughly equal to prison capacity. Thus, in a sense, there is only one "cause" of increases in prison population—increases in prison capacity—and all other factors can influence prison population only indirectly, by influencing capacity. This year's crime rate, however, cannot affect this year's prison capacity, since it takes years for crime increases to motivate legislators to authorize more prison building and for those prisons to finally start accepting inmates and contributing to total prison capacity—surely at least two or three years. In short, it is simply implausible that crime rates and prison population could be simultaneously related. No doubt crime levels affect the relative sizes of prison populations *across states*, and changes in crime levels may *eventually* influence prison population changes, but an immediate impact (within one year) of crime rates on prison population is plainly implausible.

It is, however, doubtful whether crime rates have even a lagged effect on prison capacity. The biggest prison-building boom in U.S. history occurred between 1973 and 2005 (Table 1.3), at a time when crime rates were (excepting the 1985–1991 crack epidemic period) generally flat or declining. Over this entire period, the murder rate declined by 40 percent, and other serious crimes showed similar trends. Crime rates dropped in more years than they increased. From 1973 through 2005, the U.S. murder rate increased in 14 years, decreased in 16 years, and stayed the same for two years (Table 1.5). Thus, for most of the period in which this enormous increase in prison capacity occurred, legislators who supported building more prisons could not possibly have been responding to actual crime rate increases, because no such increases occurred. It is true that increases in incarceration itself may have been partly responsible for the fact that crime did not increase, but this is totally irrelevant to the question of whether policy makers could have been motivated by crime increases to increase prison capacity or do anything else that increased the number of inmates. Legislators do not know how crime rates would have trended in the absence of some causal force—they only know (at best) how crime actually did trend. And over most of the period of the prison boom *there was no crime increase* that could have motivated prison expansion.

Direct empirical tests have also supported the view that crime rates do not affect prison rates. Marvell and Moody (1994) used Granger methods to explicitly test whether increases in state crime rates tended to precede increases in the prison population. They concluded that they did not, and that crime rates did not contribute to prison increases. They summarized their findings thusly: "there is little evidence of a short-term effect of crime on SPP [state prison populations]" (130). Kovandzic and Viereaitis (2006) performed a Granger analysis of Florida county data for 1980–2000 and independently confirmed this result. Likewise, the elaborate time series analyses of Cappell and Sykes (1991) found no significant contemporaneous effect of crime rates (and only equivocal evidence concerning a lagged effect) even on prison admissions, which should be more quickly responsive to crime rate changes than the prison population. Finally, Smith (2004) used a state panel study to assess competing explanations of the prison boom and concluded that changes in crime rates had no contemporaneous impact on changes in prison populations.

In sum, contrary to Levitt (1996) and Spelman (2000b), there is no *a priori* justification for expecting a contemporaneous two-way relationship between crime rates on prison rates and only weak justification for believing there are even lagged effects. Thus, the simultaneity issue—in the sense of crime having an immediate effect on prison levels as well as the reverse—is a red herring. Nevertheless, some scholars who have reviewed this literature were so convinced that simultaneity was a critical issue that they divided studies into two major categories—those that had explicitly addressed this issue and those that had not, treating the former group as superior (Spelman 2005, 136). This emphasis is not justified. We believe that there may instead be a different variety of endogeneity distorting the results—omitted variables that influence both prison populations and crime rates. This could create a correlation between the prison rate and the error term for the crime rate, and contribute to a spurious negative association between prison rates and crime rates. This problem, however, can only be solved by controlling for these omitted confounding variables, not by methods aimed at addressing simultaneity. We discuss omitted variables later.

The simultaneity issue is critical is assessing the evidence because researchers who have used instrumental variables (IV) methods to "break" the simultaneity supposedly attributable to contemporaneous reciprocal causation have obtained radically different estimates of prison effects from scholars who used ordinary least squares methods assuming one-way causation. Those who insisted there was a simultaneity problem to be solved interpreted this contrast in findings, in a somewhat circular way, as evidence that there was indeed a simultaneity problem to be fixed (e.g. Levitt 1996; Spelman 2005). An alternative interpretation, however, is that an inappropriate "fix" for a nonexistent simultaneity problem distorted estimates that were more correct without the fix.

Levitt's IV methods relied, as do all IV analyses, on the validity of his instrumental variables, but as we noted in Chapter 4 his instruments were not valid. The instruments used by other scholars in attempts to deal with simultaneity were equally implausible. Besci (1999) assumed that police expenditures and the number of police had no effect on crime aside from their effects via prison population, an assumption directly contrary to the findings of work by Kovandzic and Sloan (2002), Levitt (1997), and Marvell and Moody (1996). Worse still, Devine, Sheley, and Smith (1988) did not even report what their instruments were, an omission that critics justifiably interpreted as an indication that they probably were not valid (Spelman 2000b, 481).

In sum, Levitt's unusually large estimates of prison effects, and similar ones obtained by other analysts using IV methods based on the implausible assumption of contemporaneous reciprocal causation between prison and crime rates (Besci 1999; Devine et al. 1988; Spelman 2000a, 2005), may be little more than artifacts of poor instrumentation. That is, these estimates deviate from those obtained by other scholars because the analysts applied inappropriate solutions to a nonexistent simultaneity problem.

## Are There Cross-State Displacement and Free Rider Effects?

It likewise is questionable whether it is important to address the possibility of cross-state effects of prison population on crime rates. Marvell and Moody (1998) concluded that higher prison populations in one state could displace criminals from that state to other states, especially those nearby, as criminals moved to avoid the risks of imprisonment in the more punitive state. Conversely, they argued that some states enjoy "free rider" effects from the larger prison populations of other states because some of their own criminals had been incarcerated in another state where these criminals had committed crimes or because criminals who would have moved to their state were instead locked up in a prison in another state. They concluded that studies of prison population effects on crime rates therefore needed to take account of prison populations in other areas besides the one whose crime rates were being measured.

No doubt there are some such cross-state effects. Critics, however, noted that the magnitude of these effects as estimated by Marvell and Moody (1998) were implausible in light of actual levels of cross-state movement of criminals. Marvell and Moody's estimates implied that prison populations in nearby states actually had *three times* as much effect on crime rates as a state's own prison population. Kovandzic and Viereaitis (2006, 216–217) noted, however, that there is too little cross-state commission of crime and cross-state migration of criminals to account for such enormous cross-state prison effects. Other researchers in this area evidently agree, as the authors of subsequent studies have declined to incorporate cross-state effects into their analyses of prison effects (Liedka et al. 2006; Spelman 2005; Zimmerman 2006; Zimmerman and Benson 2007).

## The Omitted Variables Problem—Failing to Control for Public Intolerance for Crime

Perhaps the most consequential flaw in the prison/crime rate studies is an obvious variant of the omitted variables problem. Analysts have failed to measure and control for a key variable that almost certainly affects both the size of prison populations and crime rates—the level of public intolerance for crime (see Chapter 4 for a fuller discussion). Some individuals are obviously less tolerant, or more disapproving, of crime, and rule-breaking in general, than others. Correspondingly, then, some populations have higher average intolerance levels than others. Social intolerance for crime almost certainly has its own effect on crime rates. It is a sociological commonplace that persons who associate with those who strongly disapprove of crime are less likely to commit crimes themselves (e.g., Akers 1973; Grasmick and Bursik 1990; Matsueda 1988; Nagin and Paternoster 1994; Sutherland 1947; Williams and Hawkins 1992). Yet, one of the principle ways this disapproval is outwardly manifested in the public opinion sphere is with stronger support for punishment of criminals. Thus, one would also expect that a higher average level of disapproval of crime in a population would, other things being equal, lead to greater public support for increased levels of punishment. If higher intolerance or disapproval levels cause (a) higher punishment levels and (b) lower crime rates, then one would expect a spurious (noncausal) negative association between punishment levels (such as the imprisonment rate) and crime rates, unless intolerance levels were measured and controlled by the analyst.

As we shall see, *none* of the prison population studies controlled for public intolerance for crime. Thus, the meaning of the negative prison/crime associations found in these studies is subject to serious doubt, since the patterns could, in every case, reflect little more than spurious associations produced by the impact of public intolerance levels on levels of both crime and punishment. Just how much this accounts for the crime/prison association cannot be determined until someone actually measures and controls for intolerance. Future research needs to measure attitudes towards crime using surveys in multiple populations and/or multiple points in time, statistically control for variation in public intolerance levels, and determine if there is any remaining negative association between prison rates and crime rates.

## Empirical Studies of the Impact of the Size of the Prison Population on Crime Rates

Table 10.2 summarizes 38 studies of the impact of prison population size on crime rates in the United States, listed chronologically in order of publication. To our knowledge, it is the most comprehensive review of the published English-language literature on the subject. It does not cover studies of the impact of the probability of imprisonment (e.g., prison admissions divided by crimes, arrests, convictions, or population), such as those of Cappell and Sykes (1991), Ehrlich (1973), and Wolpin (1980), as these pertain more to deterrent effects and were covered in our review of macro-level studies of general deterrence in Chapter 7.

A number of patterns can be discerned in the findings.

1. There are a surprisingly large number of positive and nonsignificant negative estimates of prison elasticities, given how self-evident it initially seems that increased prison populations should reduce crime rates to some degree. Of 106 total estimates of the effects of prison population on various crime rates, 38 (36 percent) were not significantly different from zero or positive. Thus, the evidence as a whole is mixed and, at best, only weakly supportive of the hypothesis that increases in prison populations cause reductions in crime.
2. More recent studies, analyzing more recent historical periods, find smaller effects of prison population than studies covering large numbers of pre-1980 years or find no significant effect at all. Not surprisingly, the more an estimate is based on data from recent years, when marginal effects of additional prison beds were probably declining, the lower the estimated effect. We will address this matter in detail later.

**TABLE 10.2** Incapacitation studies: the effects of prison population size on crime rates

| *Study* | *Sample* | *# Signif. Deterrence* | | *Inmates Counted*[c] | *Estimation Methods*[d] | *Estimated Elasticities/1-tailed significancee* | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | *Control Variables*[a] | *Variables Controlled*[b] | | | *Total* | *Violent* | *Property* | *Murder* | *Others* |
| Kleck (1979) | US 1947–73 | 6 | AR, CR | S, F | 2SLS | | | | –0.686<br>$p < 0.01$ | |
| Bowker (1981) | US 1941–78 | 0 | none | S, F | Correlation | ($r = 0.13$)<br>$p = 0.28$ | | | | |
| Biles (1983) | | | | | | | | | | |
| McGuire & Sheehan (1983) | US 1969–79 | 0 | none | S, F | Granger | –0.478<br>($p < 0.01$) | | | | |
| Kleck (1984) | US 1947–78 | 4 | AR, CR | S, F | 2SLS | | | | –0.472<br>$p < 0.01$ | |
| Withers (1984) | | | | | | | | | | |
| Galster & Scaturo (1985) | | | | | | | | | | |
| Cohen & Land (1987) | US 1947–84 | 3 | none | S, F | OLS | | | | –0.282<br>$p < 0.01$ | |
| Devine et al. (1988) | US 1948–85 | 3 | none | S, F | 2SLS | | | | –1.88<br>$p < 0.01$ | -2.62 (robbery) $p < 0.01$<br>–1.90 (burglary) $p < 0.01$ |
| Inverarity & McCarthy (1988) | | | | | | | | | | |
| Von Hoffer & Tham (1989) | | | | | | | | | | *–056 (assault)* $p > 0.05$<br>–0.200 (mv theft)<br>$p < 0.0001$ |
| Marvell & Moody (1994) | 49 states,1971–89 | 1 | none | S only | FE, WLS | –0.159<br>$p < 0.001$ | | | –1.31<br>$p < 0.05$ | –0.113 (rape) $p < 0.05$<br>–0.260 (robbery)<br>$p < 0.0001$<br>–0.253 (burglary)<br>$p < 0.0001$<br>–0.138 (larceny)<br>$p < 0.0001$ |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Levitt (1996) | 50 states, DC 1971–93 | 3 | none | S only | FE, 2SLS | | −0.379 $p < 0.05$ | −0.261 $p < 0.05$ | −0.147 *$p > 0.05$* | *−0.246 (rape) $p > 0.05$*<br>−0.703 (robbery) $p < 0.05$<br>−0.410 (assault) $p < 0.05$<br>*−0.259 (mv theft) $p > 0.05$*<br>−0.401 (burglary) $p < 0.05$<br>*−0.277 (larceny) $p > 0.05$* |
| Marvell & Moody (1996a) | 49 states 1973–92 | 2 | # police | S only | GLS | −0.076 $p < 0.05$ | | | | |
| | 56 large cities | 2 | # police | S only | | −0.126 $p < 0.05$ | | | | |
| Marvell & Moody (1997) | US 1930–94 | 6 | none | S only | FE, OLS | | | | −1.31 $p < 0.0001$ | −2.57 (robbery) $p < 0.0001$<br>−0.53 (assault) |
| D'Alessio & Stolzenberg (1998) | Orlando, 184 days | 1 | AR | Local jail | VARMA | *?, n.s.* | | | | |
| Levitt (1998) | 50 states 1978–93 | 3 | none | S only (juveniles) | FE, WLS | | −.024 $p < 0.01$ | −0.013 $p < 0.05$ | $p < 0.05$ | |
| Marvell & Moody (1998) | 48 states 1930–92 | 4 | none | S only | FE, WLS | | | | −0.22 $p < 0.05$ | |
| | 9 regions 1930–92 | 4 | none | S only | FE, WLS | | | | *−0.20 $p > 0.05$* | |
| Besci (1999) | 50 states 1971–94 | 5 | none | S only | FE, 2SLS | *−1.35 $p = 0.103$* | *−0.95 $p = 0.113$* | *−1.475 $p = 0.095$* | *0.94 $p = 0.21$* | *−0.316 (mv theft) $p = 0.47$* |
| Hennessy et al. (1999) | US 1980–96 | 0 | none | S, F prison | correlation | $r = 0.659$ $p < .01$ | | | | |
| | | | | Local jail | | $r = 0.659$ $p < 0.01$ | | | | |
| Lynch (1999) | US 1972–93 | 0 | none | S only | correlation | $r = 0.48$ $p = 0.03$ | | | | |

(*Continued*)

**TABLE 10.2** (Continued)

| *Study* | *Sample* | *# Signif. Deterrence* | | *Inmates Counted*[c] | *Estimation Methods*[d] | *Estimated Elasticities/1-tailed significancee* | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | *Control Variables*[a] | *Variables Controlled*[b] | | | *Total* | *Violent* | *Property* | *Murder* | *Others* |
| Marvell & Moody (1999) | US 1930–95 | 4 | none | S, F | SUR | | | | –1.28<br>$p < 0.05$ | |
| Spelman (2000a) | 50 states, DC, 1971–97 | 2 | none | S only | FE, 2SLS? | –0.401<br>$p < 0.01$ | | | | |
| Witt & Witte (2000) | US 1960–97 | 1 | none | S, F | VAR | *–0.068*<br>$p > 0.05$ | | | | |
| Donohue & Levitt (2001) | 50 states, DC, 1985–97 | 0 | none | S only | FE, WLS | | *–0.027*<br>$p > 0.05$ | –0.159<br>$p < 0.01$ | –0.231<br>$p < 0.01$ | |
| Kovandzic (2001) | 58 FL counties 1980–98 | 0 | none | S only | FE, WLS | | | | *–0.057*<br>$p > 0.05$ | *–0.037 (rape)*<br>$p > 0.05$<br>*–0.064 (robbery)* $p > 0.05$<br>*–0.005(assault)* $p > 0.05$<br>–0.071 (burglary) $p < 0.05$<br>–0.111 (larceny) $p < 0.05$<br>–0.123 (auto) $p < 0.05$ |
| Levitt (2001) | 50 states 1950–90 | 3–5 | executions | S only | FE, WLS | | *–0.72*<br>$p > 0.05$ | –0.142<br>$p < 0.01$ | | |
| Raphael & Winter-Ebmer (2001) | 50 states 1971–97 | 4–5 | none | S only | FE, WLS | | –0.042<br>$p < 0.05$ | –0.108<br>$p < 0.01$ | | |
| DeFina & Arvanites (2002) | 50 states, DC, 1971–98 | 0–6 | none | S only | FE, OLS | | | | *–0.052*<br>$p > 0.05$ | *+ 0.016 (rape)* $p > 0.05$<br>*–0.095 (robbery)* $p > 0.05$<br>*–0.077 (assault)* $p > 0.05$<br>–0.135 (mv theft) $p < 0.01$<br>–0.110 (burglary) $p < 0.01$<br>–0.056 (larceny) $p < 0.01$ |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Kovandzic & Sloan (2002) | 57 FL counties, 1980–98 | 3 | police | S only | FE, WLS | –0.038<br>$p = 0.04$ | | | | |
| Kovandzic et al. (2002) | 188 large cities, 1980–99 | 5 | none | S only | FE, WLS | | | | –0.290<br>$p < 0.05$ | |
| Washington State Institute (2003) | 39 WA counties, 1982–2000 | ? | none | S only | FE, WLS | –0.024<br>$p = 0.03$ | | | | |
| Kovandzic et al. (2004) | 188 large cities, 1980–2000 | 4 | none | S only | FE, WLS | | | | –0.30<br>$p<0.01$ | *–0.06 (rape)* $p>0.10$<br>–0.21 (robbery) $p < 0.01$<br>0.04 (assault) $p > 0.10$<br>*–0.21(burglary)* $p < 0.01$<br>–0.12 (larceny) $p < 0.01$<br>–0.15 (auto), $p < 0.05$ |
| Saridakis (2004) | US 1960–2000 | 2 | none | S, F | GLS | | *–0.29*<br>$p > 0.10$ | | –0.62<br>$p < 0.05$ | –0.65 (rape) $p < 0.05$<br>*–0.25 (assault)* $p > 0.10$ |
| Zimmerman (2004) | 50 states, 1978–1997 | 7 | Pr(a\|c),<br>Pr(c\|a),<br>Pr(e\|c) | S only | 2SLS | –f<br>$p < 0.05$ | | | | |
| Corman & Mocan (2005) | NYC, 312 Months | 2 | Arrests Police | S only | OLS | | | | –0.08<br>$p < 0.05$ | *—? (rape)* $p > 0.10$<br>–0.03 (robbery) $p < 0.05$<br>*–? (assault)* $p > 0.10$<br>–0.06 (burglary) $p < 0.01$<br>–0.03 (auto) $p < 0.01$<br>–0.02 (larceny) $p > 0.05$ |
| Spelman (2005) | 254 TX counties, 1990–2000 | 4/7 | executions (not signif) | S, L | FE, 2SLS | | *–0.0129*<br>$p > 0.47$ | *–0.0261*<br>$p > 0.40$ | | |
| Batton & Wilson (2006) | US 1947–71<br>US 1972–98 | 4 | executions | S, F | OLS | | | | | –f (police murder) $p < 0.01$<br>*? (police murder)* $p > 0.05$ |

(*Continued*)

**TABLE 10.2** (Continued)

| *Study* | *Sample* | *# Signif. Deterrence* | | *Inmates Counted*[c] | *Estimation Methods*[d] | *Estimated Elasticities/1-tailed significancee* | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | *Control Variables*[a] | *Variables Controlled*[b] | | | *Total* | *Violent* | *Property* | *Murder* | *Others* |
| Kovandzic & Viereaitis (2006) | 58 FL counties, 1980–2000 | 1–4 | none | S only | FE, WLS | | | | *−0.06 $p > 0.20$* | *−0.13 (rape) $p = 0.08$*<br>*−0.00 (robbery) $p = 0.48$*<br>*−0.07 (assault) $p = 0.15$*<br>*−0.07 (mv theft) $p=0.24$*<br>*−0.08 (burglary) $p = 0.21$*<br>*−0.08 (larceny) $p = 0.13$* |
| Liedka et al. (2006) | 50 states, DC, 1972–2000 | 1 | none | S only | FE, WLS | −0.072 $p < 0.001$ | | | | |
| Zimmerman (2006) | 50 states, 1978–2000 | 7 | Pr(a\|c), Pr(c\|a), executions | S only | WLS | –f $p < 0.05$ | | | | |
| Zimmerman & Benson (2007) | 50 states 1982–2000 | 5 | Police | S, F | FE, WLS | | | | | +0.146 (rape) $p > 0.01$ |
| Kovandzic et al. (2009) | 50 states 1977–2006 | 3 | Executions, Police | S only | FE, WLS | | | | –f $p < 0.01$ | |

*Notes*:

[a] Does not include multiple versions of the same basic variable (e.g., % age 15–24, % age 25–29), lagged crime rates, or time or place dummy variables in fixed effects models. Variables are counted as significant even if signs of coefficients appear counterintuitive.

[b] AR=arrests/crimes, CR=convictions/arrests

[c] S=state prisoners, F=federal prisoners, L=local jail inmates

[d] 2SLS=two-stage least-squares, OLS=ordinary least squares, FE=fixed effects models, WLS=weighted least squares, VAR=vector autoregressive modeling.

[e] Estimates in italics were *not* significant at 0.05 level, 1-tailed. "Total" crime=all FBI Index crimes combined.

[f] Elasticities unknown—usually because variables were not logged.

3. Few researchers have thought that they had to take two-way causation between prison population and crime into account (Besci 1999; Devine et al. 1988; Levitt 1996; Spelman 2000a, 2005), but those who did so obtained far larger estimates of prison's impact on crime than the majority who did not. The estimates based on an assumption of reciprocal causation between prison and crime are probably misleading, because they are based on the implausible assumption that crime rates have immediate (within a single year) effects on prison populations and rely on the use of dubious instrumental variables (e.g., Levitt's use of instruments that were almost certainly endogenous with respect to prison population) and implausible exclusion restrictions.
4. Models of crime rates in prison population studies are all very simplistic, rarely controlling for more than four significant potential confounder variables and never controlling for more than seven. This means that there is a strong chance that the omission of variables that affect crime and that are also correlated with prison levels has biased estimates of prison effects. The inclusion of time and place dummy variables in fixed effects models helps in this regard but cannot be regarded as a complete substitute for controlling for explicitly measured confounding variables. This is demonstrated by the fact that such variables are often significantly related to crime rates in models that also included time and place dummy variables (fixed effects), proving that the fixed effects did not control for all factors that affect crime rates.
5. To be more specific, none of these studies controlled for levels of public intolerance for crime, and thus all negative prison/crime associations are probably at least partly spurious, leading to an overestimation of crime-reduction effects. More intolerance leads to more punishment, but also reduces crime independent of any punishment effects.
6. None of the early studies controlled for the deterrent effects of legal punishment, other than those done by Kleck (1979, 1984), and only a few of the later studies made any efforts to do so. Spelman (2000b) speculated that it may be impossible to separate deterrent and incapacitative effects of prison, but stressed that it is nevertheless important to try. Consequently, some of the effects attributed to incapacitation may be due to contemporaneous deterrent effects, just as some of the supposed deterrent effect of higher certainty or severity of punishment may actually be due to incapacitative effects in those studies not controlling for the size of incarcerated populations.
7. Only the national time series studies consistently included both state and federal prisoners in the prison population measures, so subnational studies understated the number of criminals incarcerated by not counting those in federal prisons. Both national and subnational studies also generally fail to measure incarceration in local jails. These mismeasurements are not likely to be uniform across states or random. Any state with higher rates of illicit drug use is likely to also have more of its criminals in federal prison, since federal prisons historically have disproportionately housed drug offenders (57 percent of federal prisoners in 2000 had been committed for drug offenses—U.S. Bureau of Justice Statistics 2002, 526). Thus, the more serious an area's drug problem is, the higher its crime rate will be, but also the more its prison rate is underestimated due to the omission of federal inmates. This creates more areas that have high crime rates and misleadingly low prison rates, artificially biasing the prison/crime association in a negative direction and thereby overstating prison effects. Further, only Spelman (2005) took account of local jail populations in addition to state prison populations (a few other studies counted *only* local inmates).
8. Estimates of prison effects, as measured by elasticities, show enormous instability across studies, time periods, and areas. Estimates on the effect of a 1 percent increase in prisoners per capita on the rate of Index crimes range from –0.038 (Kovandzic and Sloan 2002) to –1.35 (Besci 1999), while estimates for total violent crimes range from –0.027 to –0.95 and those for total

property crimes range from –0.108 to –1.475, the latter *fourteen* times larger than the former. Even when one focuses on effects on a single crime type, the same huge variation in estimates is observed. For example, elasticities for murder rates range from a *positive* (though nonsignificant) 0.94 (Besci 1999) down to –1.88 (Devine et al. 1988). In short, the estimated effects varied from negligible and of little or no policy significance to enormous. It is also clear from the figures in Table 10.2 that this variation is not attributable to the deviant estimates of just a few flawed studies but is evident throughout the full body of studies, good and bad.

## Variation in the Effects of Prison Population Size on Crime Rates

To be sure, some of this variation in estimates reflects genuine differences in prison effects. Actual elasticities are likely to be smaller in places and times when the prison rate is high (due to diminishing returns effects) and in places that waste a larger share of their prison spaces on drug dealers (for whom incapacitation effects are negated by the replacement of incarcerated dealers with unincarcerated dealers). Nevertheless, even when elasticities were estimated for virtually identical time periods and sets of areas, estimates still varied enormously, suggesting that much of the variation is attributable to differing methodological flaws. Consequently, once viewed in its entirety, it becomes clear that this body of research provides little reliable foundation for conclusions as to how much effect on crime rates one can expect from changes in the number of criminals incarcerated.

### *Instability of Estimates Across Time Periods*

Early estimates of the effect of prison population on crime rates indicated that the estimates were highly unstable over different time periods. Kleck (1979) investigated the effects on homicide rates using data covering the U.S. for 1947–1973 and estimated the elasticity for homicide rates to be –0.686. When he re-estimated the exact same model with data covering 1947–1978, adding just five later years to the time series, he found the elasticity to be only –0.472, a 31 percent drop. Likewise, DeFina and Arvanites (2002) found that, when based on data for 1971–1992, the estimated elasticity for murder was 2.33 times as large as when the estimate was based on the 1971–1998 sample. The estimate for assault was only one quarter as large when based on 1971–1998 as when it was based on 1971–1992. That is, extending the time series by just six years caused three quarters of the estimated prison effect to disappear. Batton and Wilson (2006) found a significant negative effect of the prison population during the period 1947–1971 but none at all for 1972–1998. These sorts of huge variations were not addressed by scholars like Levitt (1996), who reported only estimates for a single time period, mentioning nothing about the sensitivity of his estimates to the composition of the time series analyzed.

Spelman (2000a) argued that these differences in estimated elasticities reflected real differences in prison impact rather than merely being indications of instability in estimates of those effects, asserting that actual elasticities varied over time. His own estimates, however, did not support this claim. Contrary to his own interpretation, his models that assumed time-varying elasticities showed no better a fit to the data than models assuming constant elasticities (adjusted R-squareds were 0.2864 and 0.2875, respectively), and the estimate of the coefficient for the variable-elasticity prison rate was not substantially more significant than for the constant elasticity measure (p=0.002 vs. 0.004—see his Table 4.3). In any case, even though actual incapacitative effects may well vary somewhat over time, it is unlikely that they vary so greatly that they account for the extreme degree of instability of estimates across time observed in these studies. The substantial instability almost certainly also reflects problems with the models and methods used to generate the estimates.

### *Instability of Estimates Across Areas (Inappropriate Pooling)*

Marvell and Moody (1998) and DeFina and Arvanites (2002) also have documented huge differences in estimated prison effects across states. Some of these differences very likely reflect some actual variation in effectiveness. One would, for example, expect less effect of incarcerating criminals on crime rates if a state wasted a large share of its prison spaces on drug offenders. The differences, however, are much larger than such factors are likely to be able to explain. For example, Marvell and Moody (1998, 525) found that the rate of criminals incarcerated in a state was significantly and negatively related to that state's homicide rate in only four states and appeared to exert *no* significant crime-reducing effect in the other 44 states examined. Similarly, DeFina and Arvanites (2002, 647) found that for the period 1971–1998, there was a significant negative effect of the prison population on homicide rates in only 4 of the 51 states (including D.C.) and in no more than 10 states for any other crime type. The authors concluded that for six of seven crime types they examined it was inappropriate to pool states together because apparent effects of prison populations differed so substantially across states. Another interpretation is that these differing estimates did not reflect actual differences in effects so much as they reflected differences across states in the impact of various methodological flaws such as erroneously omitting relevant variables like the level of public intolerance for crime.

On the other hand, since the impact of marginal increases in prison population declines as prison population grows, some of these cross-state differences may be due to differences in how close each state had gotten to a point of diminishing returns—there will be less marginal impact in states that already incarcerate a large share of their more active criminals. Nevertheless, even taking account of such differences, it seems unlikely that prison population increases have enormous effects in some states and none at all in others. More likely, technical flaws account for some of the variation, suggesting that the estimates of prison effects are not reliable.

## Problems in Quantifying the Crime Prevention Benefits of Incarceration

The estimates of prison effects are nevertheless mostly negative, and it is worth knowing how much crime reduction benefit they produce. The benefits of incarceration are commonly measured as the value of crimes estimated to have been prevented by the incarceration of a set of criminals, an effort that requires knowing not only how many fewer crimes were committed because offenders were incarcerated (the subject addressed in preceding sections), but also the average "value" of each crime prevented. The latter is commonly "monetarized," i.e. quantified by being given a dollar value. Some tangible, out-of-pocket expenses to victims are relatively easy to measure in dollars, such as the value of property stolen or damaged, or medical expenses associated with violent crimes. In contrast, intangible costs such as pain and suffering and the value of lost lives, are harder, and arguably impossible, to quantify.

Some have proposed valuing pain and suffering as the dollar amount one would predict a jury would award in a lawsuit (Cohen 1988), while others have tried to value it based on how much prospective victims questioned in surveys say they would be willing to pay to avoid these harms (Cook and Ludwig 2000). Neither method is satisfactory, partly because it is conceptually unclear what exactly the "value" of pain and suffering is and partly because there is no clear reason to believe that these procedures actually measure the concept.

Cohen's overall estimates of the "cost of crime" were almost entirely attributable to violent crimes (1988, 552) and most of the costs of each of the violent crimes were in turn due to Cohen's estimates of (1) pain and suffering and of (2) the "risk of death," which Cohen used as a proxy for the fear experienced by crime victims. Thus, when Cohen's cost estimates are used as measures of the benefits of preventing crimes by the incarceration of felons, the measures largely stand or fall on the validity of his methods for measuring the pain and suffering, or fear, involved in violent crimes.

Cohen relied on damage awards for pain and suffering made by juries in civil cases concerning accidents of various sorts. While pain and suffering are obviously real, it is doubtful whether they can be objectively measured in any way. Even if it were possible, however, juries have no special expertise to measure these costs, even with the assistance of (conflicting) expert witnesses. Nor does averaging the judgments of hundreds of juries help much if all the judgments are flawed in similar ways. A jury award for "pain and suffering" may actually reflect how much the jury wants to punish the responsible party, above and beyond their compensation of the victim for tangible costs such as medical expenses and lost wages. But the jury's assessment of how much the defendant "deserves" to be punished may in turn be a function of how unlikable the defendant was, or how able he was to pay large damages (was he a "deep pockets" defendant?), not just how much the jury thought the victim's pain and suffering was "worth." While judges in criminal cases can inflict a variety of penalties to show how bad they think the defendant deserves to be hurt, civil juries have only monetary damage awards to use as a way of conveying such sentiments, so these awards reflect a mix of dimensions that probably cannot be disentangled.

Cohen conceded that average jury awards for pain and suffering could not be directly used as measures of pain and suffering in the average crime because the harms in civil cases with jury awards are more serious than the harms in the average crime. He claimed, however, that one could nevertheless create a simple formula that estimated the "functional relationship between . . . (medical costs and lost wages) and the pain and suffering awards" (1988, 542). Unfortunately, this in no way solves the problem of jury award cases involving far more serious harm than the average crime. For example, one formula Cohen used regarding gunshot victims was: Pain and Suffering = $17,957 + $5.20 (medical costs + lost wages). While the formula may well have accurately reflected how pain and suffering awards were "functionally related" to medical costs and lost wages, the very fact that the constant term was as high as $17,959 is itself a reflection of how exceptionally high jury awards for pain and suffering are, and thus of how different the costs of accidents addressed in civil suits are from the average crime. Thus, Cohen's calibration method does not solve the problem of average jury awards for pain and suffering in accidents overstating the value of pain and suffering in the average crime.

If one does not accept that juries can meaningfully measure pain and suffering, then Cohen's estimates of pain and suffering in crimes are meaningless. On the other hand, if one does accept that jury awards can be meaningful in some subjective sense with respect to accident cases resolved in civil courts, they are nevertheless too high to be used with regard to average crimes, which typically involve no injury and only minor property loss (U.S. Bureau of Justice Statistics 2006c). Later efforts by Cohen and various colleagues to estimate the pain and suffering of crimes were also largely based on jury awards and thus suffered from the same conceptual flaws (e.g., Cohen, Miller, and Rossman 1994; Miller, Cohen, and Wiersema 1996).

Cohen called his second measure of an intangible cost of crime the "risk of death," which was intended to measure the fear that victims experience during crimes. It was computed as the fraction of crimes, of the type a victim was involved in, that resulted in death, multiplied times the legally determined "value" of a human life. The measure was implicitly based on the assumption that victim fear is linearly related to the objective risk that a given category of crime will result in death. There is, however, no evidence that victims have even approximate notions of the risk of death linked with various crime types, nor any evidence that subjectively experienced fear bears a linear relationship with this objective probability. The "value" of a human life used in Cohen's studies is also quite artificial, reflecting how much wages workers would be willing to forego to reduce their risk of death on the job. In its defense, Cohen cites the relative consistency of estimates yielded by this method but does not provide any reason to believe it really reflects the value of an average human life or even "the willingness to pay to save a statistical life for most violent crimes" (p. 549), as he asserts.

The term "willingness to pay" is itself a misnomer because there is little evidence that people are actually willing to pay the amounts that they say, in surveys, that they would be willing to pay to avoid some harm. It costs no more to *say* that you would be willing to pay $10,000 than it does to say that you would be willing to pay $100—only hypothetical dollars are being "spent," and there are an unlimited number of those. "Willingness to pay" methods for estimating intangible costs are sometimes called "contingent valuation" (CV) methods and employ surveys with questions in which respondents are asked if they would be willing to pay X dollars (an amount randomly varied across respondents) to reduce a particular harm by Y percent. Analysts then use the survey results to extrapolate the total amount of dollars the entire population would be willing to spend to avoid the harm, and divide by the number of instances of harm avoided, which yields the average "cost" of each instance of harm.

CV methods have been most thoroughly evaluated regarding their use in assessing the costs of harms to the environment, e.g. from industrial pollution. After assessing the evidence on the merits of CV methods in a series of chapters, a blue ribbon panel headed by Nobel-laureate economist Kenneth Arrow summed up their findings as follows: "the basic conclusion of all the papers is that CV should be discarded as a tool for determining economic damages to the environment" (Hausman 1993, 467). The panel based this conclusion on evidence that the method produces estimates that are illogical, internally inconsistent, and highly inconsistent across studies.

The harm deriving from crime victimization is an especially unsuitable candidate for CV methods for two reasons. First, understanding how much benefit is implied by an X percent reduction in some harm requires at least an approximate notion of how much total harm there was to be reduced—a 10 percent reduction from a starting point of 10 million crimes is very different from a 10 percent reduction from a starting point of one million crimes. Americans do not have the slightest idea how much crime, or crime-related harm, is inflicted each year, so their notions of the magnitude of a given percentage reduction in crime are likely to be both inaccurate and wildly variable. Second, responses in CV surveys are likely to be especially artificial when the harm in question is a rare one that few people have directly experienced. Violent crime victimization is just such a harm. Since CV methods are primarily relevant to assessing the harms of violent crime, this is an especially crippling limitation (Kleck 2001).

In sum, neither jury awards nor willingness to pay methods are very credible ways of estimating the intangible costs of crime, which are the largest component of the benefit of preventing crimes. Pain and suffering clearly are real harms of crime, so conscientious analysts want very much to have good estimates of them. We suspect that the real reason so many scholars make use of these dubious methods is not so much that they are actually adequate but rather because the task to which they are applied is a vital one, and no better alternatives have been identified. Thus, some analysts may prefer dubious estimates of important quantities to no estimates at all. Another alternative in policy analysis, however, would be to candidly acknowledge that there are some costs and benefits we cannot meaningfully quantify and leave it at that. This "just say no" alternative, in effect, implies simply leaving out of cost/benefit calculations all of the unmeasurable costs and benefits, regardless of how important they may be. This was precisely the approach taken in earlier, more cautious efforts to measure the costs of crime (e.g., Gray 1979; Klaus 1994).

The cost-of-crime estimates that include intangible costs obviously are far larger than those that cover only tangible costs such as lost wages and the cost of medical care for physical and psychological injuries. Not surprisingly, those who argue that incarceration produces very large crime prevention benefits prefer the higher estimates (Piehl and DiIulio 1995; Reynolds 1990; Zedlewski

1987). Indeed, Cohen himself applied his cost-of-crime estimates to this very question, concluding that longer terms of incarceration would be highly cost effective (1988, 549–552). These same scholars, however, are all conspicuously one-sided in their efforts to assess intangible consequences of incarceration, since none of them have turned their talents to estimating any of the intangible *costs* of expanding incarceration, such as the costs of mistakenly incarcerating the innocent or the pain and suffering of the families of those incarcerated. Nor do they address the arguably more tangible and measurable criminogenic effects of incarceration due to the blocking of formal education and legitimate employment opportunities, the stigmatization of the imprisoned in the eyes of the law-abiding, or the sharpening of the inmate's criminal skills while incarcerated.

To be sure, any one analyst can only do so much, and one might consider one's self a specialist in estimating some costs but not others. This does not, however, justify performing cost-benefit analyses and drawing conclusions based solely on an exhaustive assessment of the benefits of a preferred policy, while ignoring or discounting most of the credible, very serious, costs. Since there appears to be little prospect for developing credible estimates of the intangible costs of either crime or incarceration, the only feasible alternative appears to be an even-handed comparison of those costs and benefits that *can* be meaningfully quantified for both crime and punishment.

## Diminishing Returns: Have We Passed the Point Where Further Incarceration Is No Longer Cost-Effective?

From the beginning of research on prison population effects, it was widely recognized that the marginal returns of imprisonment decline as prison populations grow (Blumstein et al. 1978). The criminal justice system has always favored sending offenders to prison who showed signs of being more serious, repetitive offenders. Even prior to the popularity of the concept of selective incapacitation, this was true simply because defendants with more prior offenses on their record were more likely to be regarded as "incorrigibles" who had spurned previous acts of court leniency and therefore *deserved* harsher punishment. However limited and flawed the relevant information might be, prosecutors and judges tended to favor reserving prison sentences for offenders with more prior criminal convictions or other indications of serious criminal activity. The inevitable result was that, when the prison population increased and more of the serious offenders were imprisoned, the remaining criminals who might be sent to prison were, on average, less serious offenders, whose incarceration would have less crime-reducing impact. Thus, as the size of the prison population increases, the marginal effect on crime rates of adding still more inmates declines (Liedka et al. 2006; Spelman 1995; Zimring and Hawkins 1991).

The phenomenon of diminishing returns is a familiar one and applies to solutions to many social problems. It implies that as the scale of the solution that has already been implemented goes up, not only do the benefits accruing to one more unit of solution go down, but one may also eventually reach a point where further increases in benefit would be so small that they would be less than the costs of achieving them. Increasing the prison population by one more inmate could yield less benefit in crime prevention (because the new inmate would probably be a less serious criminal) than it cost to "produce" the bed. To incarcerate the average adult offender for a year costs about $30,000 and over $100,000 for the average juvenile (Nagin, Piquero, Scott, and Steinberg 2006). Thus, imprisonment is a very expensive form of punishment and must produce considerable crime-reduction benefit for it to be justifiable on narrow cost-benefit grounds. If prison populations reach a point where adding one more prison bed produces less crime control benefit than the bed cost, the level of incarceration would be counterproductive from a cost-benefit standpoint.

By the mid-1990s some scholars were concluding that America had already reached a point where the crime control benefits of locking up more criminals no longer justified its costs (Marvell

1994; Spelman 1994). Even advocates of increased imprisonment conceded that the diminishing returns phenomenon applied to expanding the use of incarceration for crime control purposes. For example, James Q. Wilson noted that "lengthening time served beyond some point will, like increasing the proportion of convicted criminal sent to prison, encounter diminishing returns (1994, 38–39; see also Wright 1994, 118). Wilson did not, however, explicitly concede that returns could diminish to the point where they no longer justified the costs of imprisonment or that the nation might already have passed this point. Other like-minded scholars, however, have conceded that for at least *some* of the offenders we were already incarcerating, imprisonment was not, in terms of crime prevention benefits, worth the cost of keeping them locked up. Piehl and DiIulio (1995) concluded, based on their survey of New Jersey prison inmates, that costs of incarceration exceed the benefits of crime prevention for over 25 percent of the inmates, the least criminally active ones. These authors did not, however, conclude that we were imprisoning too many criminals, but merely that we were imprisoning the wrong ones, such as drug dealers whose imprisonment brought few crime control benefits.

These discussions raised the possibility that America might have already grown its prison population past the point where the benefits of adding more prison spaces exceed their costs. A number of sophisticated empirical studies now support this conclusion. As early as 1994, Spelman had concluded, based on his simulation study results, that "unless it can be shown that the indirect benefits of a marginal reduction in crime (e.g., reduction in public fear) are substantial, and at least twice the direct benefits, it is hard to justify further jail and prison construction" (1994, 310). Thus, he was suggesting that America, by no later than the early 1990s, had already passed the point where adding more prison spaces was cost-effective.

Spelman (2005) later conducted an extensive panel analysis of Texas counties covering 1990–2000 that confirmed this tentative conclusion. He found that the marginal benefits of prison growth declined throughout the 1990s and that, at least in Texas, increases in prison and jail populations had already ceased to be cost-effective by 1990. Because his data did not cover periods prior to 1990, he could not draw conclusions about exactly when the point had been passed where further incarceration became less-than-cost-effective, except that it appeared to be prior to 1990 (160–161). He cautiously refrained from generalizing his conclusions to the U.S. as a whole, but it is worth noting that the incarceration rate in Texas in 1990 was virtually identical to the rate prevailing in the U.S. that year (297 prison inmates per 100,000) and that the costs of incarcerating prisoners are far higher in the rest of the U.S. than in Texas. Unless the marginal effect of increases in prison population on crime rates is weaker in Texas than in the rest of the nation, Spelman's Texas findings imply that America as a whole likewise passed the point of where further incarceration would no longer be cost-effective some time prior to 1990.

Liedka and his colleagues (2006) tackled this issue head-on when they analyzed state-level data covering the entire U.S. for 1972–2000, allowing them to draw conclusions applicable to the entire nation, over nearly three decades. Like Spelman, they found that the effect of further increases in prison populations on crime declined over time as the scale of imprisonment increased. These analysts, however, went beyond Spelman and their other predecessors in finding that there was a point beyond which further increases in imprisonment rates apparently *increased* crime, presumably because the crime-increasing effects of "collateral damage" to inmates, their families, and their communities (Chapter 11) exceeded the crime-decreasing effects of increased incapacitation and deterrence. That is, the effects of increasing the nation's incarceration rates on crime rates had not merely diminished and become less than cost effective but had reached a point, Liedka and his colleagues argue, where they were counterproductive and caused more crime.

These conclusions have not yet been confirmed in other studies and should be treated with caution. Nevertheless, it is worth considering what their policy implications would be if they prove to be true. Liedka et al. estimated this "inflection point"—the point where further increases in the incarceration rate produced crime increases—at around 3.40 state prisoners per 1000 population (266). Most states, over most years of the 1972–2000 study period, had not reached this counter-productive point, but enough had done so to allow the analysts to observe "crime augmentation" effects at the highest incarceration levels—states had gone beyond the inflection point in at least 10 percent of the state-years in their dataset (267–268). Like Spelman, Liedka et al. concluded that "prison expansion, beyond a certain point, will no longer serve any reasonable purpose. It seems that that point has been reached" (272). The authors expressed caution about their findings that prison increases beyond a particular point cause increases in crime because of worries about "simultaneity bias" (269–270). As we have noted, however, there is no sound basis for believing that simultaneity exists because of contemporaneous two-way causation between prison and crime rates and no strong evidence that crime rates increase prison rates.

The implications of these findings for current state imprisonment policies are profound. By 2005, 34 states had passed the counter-productive "inflection point" of 3.40 prisoners per 1,000 population (Harrison and Beck 2006, 4), while the national prison rate passed this point back in 1993 (Table 1.2). Taken at face value, the most sophisticated analyses available to us indicate that state legislatures have been financing an expansion of prisons that not only went beyond the point where expansion reduced crime enough to be cost-effective but may have even passed the point where further expansion, on net, began to *increase* crime.

## Conclusions

Does locking up criminals prevent crime via an incapacitation effect on those locked up? The answer is surely "Yes." Would locking up still more criminals than we lock up now prevent at least a little more crime? The answer is "maybe," but even this modest claim may turn out to be accurate only in states that have not reached the "inflection point" where further prison increases may begin to produce crime increases, if there is such a point. Neither of these observations, however, answers the policy question of greatest importance: Is locking up still more criminals a good idea from a crime control point of view? The answer is clearly "No," even for states that currently have relatively low imprisonment rates that are still below the inflection point. In those states, increases in the incarceration rate might not increase crime, but they do not decrease it enough to make the increase in incarceration cost effective.

Increasing the number of criminals incarcerated will, up to a point, reduce crime somewhat, as long as some of the additional group of offenders incarcerated would have committed at least a few crimes in the remainder of their criminal careers and as long as these benefits are not outweighed by the various crime-increasing effects that legal punishment has on those punished. The marginal crime-reduction effects of adding more inmates, however, diminish as the number of criminals already incarcerated grows. Once you have all the serious offenders (among those you can arrest and convict) locked up, the offenders remaining to be imprisoned will be less serious offenders and you will prevent less crime through their incarceration than you did back when you were imprisoning only the most serious offenders. Eventually, the value of the crime-control benefit deriving from the marginal effect of adding one more inmate falls below the cost of incarcerating him. Incapacitation scholars generally agree that this point has almost certainly been passed and may have been passed as far back as the 1980s.

Further, many states have now reached a point where further prison expansion *may* even have begun to increase crime, as a result of a combination of diminishing returns in terms of crime prevention, and the crime-increasing "collateral damage" inflicted by incarceration on inmates, their families, and their communities (a topic addressed in the next chapter). This point is not so well-supported or widely agreed upon, and depends on the tentative findings of the single study by Liedka and his colleagues (2006). It is too grave a matter, however, to be simply ignored or discounted as impossible on "common sense" grounds.

To clarify, one could arrive at any of the following three conclusions as to the likely effects of increasing the number of criminals incarcerated:

(1) It will reduce crime, and the crime control benefits will exceed its costs;
(2) It will reduce crime, but its benefits will be lower than its costs, due to diminishing returns from incarcerating decreasingly serious offenders; or
(3) It will increase crime, due to (a) the crime-increasing effects of incarceration on inmates following release and crime-increasing effects on the families and communities of the incarcerated exceeding and (b) the diminishing benefits of incarcerating decreasingly serious offenders.

We think the evidence clearly refutes the first position and strongly supports at least the second position. It remains to be seen whether the third position is valid. At this point it can only be said that it cannot be ruled out on logical ground and is consistent with what little empirical evidence we have on the question.

A case for still further prison expansion could always be based on claims of intangible benefits of mass incarceration other than crime reduction, for example, that locking up more criminals reduces fear of crime, increases community solidarity, or increases the share of the population who feel that enough retribution is being inflicted on the wicked to satisfy a subjective sense that justice is being done. The achievement of some minimal, baseline level of punishment may well produce benefits of this sort, but there is no empirical evidence whatsoever that increases in the level of punishment beyond a minimal level cause any increases in these intangible benefits. Indeed, the results from Chapter 9 suggest that criminals and noncriminals alike are, as far as their perceptions are concerned, almost totally insensitive to variations in punishment levels. Thus, regardless of whatever subjective benefits that could hypothetically be produced by a widespread perception that punishment levels had increased, routine increases in actual punishment levels are unlikely to yield such benefits because they are unlikely to alter popular perceptions of punishment levels.

In any case, a balanced assessment of intangible effects of imprisonment would require the evaluation of intangible costs as well as intangible benefits. As we shall see in the next chapter, incarceration has serious costs beyond those of building and operating prisons and jails. It inflicts costs on the punished, some of which are intended effects of imprisonment, such as the suffering of the inmate. It also, however, has harmful unintended costs that affect the general public, such as increased recidivism of offenders due to the stigmatization and blocked economic opportunities that are by-products of punishment. Still other costs are born by innocent parties such as the spouses and children of incarcerated criminals and members of the communities in which large numbers of such offenders reside. Even if we assumed that the undocumented intangible benefits of prison expansion equaled these somewhat more empirically supported costs, we would still have to conclude that the United States has passed the point where the crime-reduction benefits of incarcerating more criminals outweighed its costs. The case for continued prison expansion, therefore, relies on nonscientific grounds—the unsupported speculation that the intangible (and probably unmeasurable) benefits of further incarceration outweighs the better documented costs of "collateral damage" from mass incarceration.

Leaving aside one-sided speculations, the policy implications of the research are these. First, we could afford to cease building further prisons and close those that reach the end of their designed lifespan and continue doing so until we reached the prison rate levels that prevailed in the mid-1980s, without suffering any increase in crime, as long as the money saved was invested in even minimally effective (and almost invariably cheaper) alternative crime control strategies (addressed in Chapter 12). Second, a return to the very low incarceration rates that prevailed prior to the 1970s would, other things being equal, carry some serious costs in the form of substantially increased crime.

Thus, there is an intermediate imprisonment level between these extremes, a level that is optimal with regard to crime-reducing effects and cost-effectiveness. The tentative results of Liedka et al. (2006) suggest that this point of maximum crime-reduction impact (which is not necessarily the most cost-effective) in recent decades was somewhere around 3.4 state prisoners per 1,000 population, a level that most states have already passed, and the nation as a whole reached in 1993. And if we are past this incarceration rate, then pushing for ever-increasing imprisonment levels will cause more crime, in the sense that it leads to more crime than there would have been had the money that was invested in excessive expansion of prison populations instead been invested in more cost-effective alternatives. If the Liedka et al. results are valid, we would need to return to the average incarceration rates that prevailed in the nation around 1993 just to stop the crime-increasing effects of excess incarceration, regardless of whatever else we did about crime. And it bears emphasizing that incarcerating at those levels would not be cost-effective—they would merely reduce crime to the maximum extent that can be achieved with incarceration.

Incarceration rates would have to be still lower to reach a point where crime control benefits exceeded the marginal costs of imprisoning criminals. Analyses by William Spelman, the nation's leading expert on incapacitation effects, imply that we could afford to go back to the still lower incarceration rates prevailing back in the 1980s without increasing crime, as long as we invested the saved dollars in even minimally effective crime-control alternatives, such as drug treatment programs (Spelman 1994, 2000b).

Some people would abolish prisons for humanitarian reasons, while others would continue resorting, perhaps indefinitely, to increased imprisonment levels as their primary crime-control strategy. The most rational policy from a narrowly utilitarian standpoint, however, is to reduce prison populations to a more nearly optimal level and then search for more cost-effective alternative strategies to produce further reductions in crime (Spelman 1994). Thus, it is time to recognize that we probably have wrung all the benefit we can out of mass incarceration and need to devote more attention, political will, and resources to other approaches.

The size of the incarcerated population can be reduced by a moratorium on construction of new prisons and allowing older prisons to close once they become too costly to maintain. Courts could reduce the inflow of newly sentenced offenders by being more selective in imposing prison sentences, reserving them for violent recidivists, those who have committed extremely serious crimes like murder or sexual assault, and a very select minority of the remainder of offenders. Non-incarceration sentences should be the presumptive punishment for nonviolent crimes, including drug offenses, so that the burden of proof would be on judges to justify prison sentences. As things stand now, nearly half of inmates of state prisons, and far more than half of federal inmates, are in prison for nonviolent offenses (U.S. Bureau of Justice Statistics 2015). Legislatures could help reduce the flow of new offenders into prisons by returning sentencing discretion back to local courts, allowing flexibility in deciding appropriate sentences for those best able to make the decisions. More specifically, lawmakers could repeal ill-advised laws that rigidly mandate prison sentences or require minimum prison sentence lengths, that permit extraordinarily long maximum terms to be imposed, that require that a very high percentage of prison terms be served or that establish excessively onerous conditions for early release.

There is an unfortunate reflexive tendency among some scholars to label any proposal that contradicts current policy or trends as politically naïve or impractical. This attitude could be described as cynical naiveté—an unrealistically pessimistic assessment that is inconsistent with the evidence.

In reality, hard evidence on both public opinion and recent trends in actual sentencing practices indicates that it is eminently realistic to believe that (a) most Americans support less use of prisons, (b) most Americans support more effort to rehabilitate criminals and ameliorate the "root causes" of crime, and (c) sentencing authorities are both willing and able to make their sentencing practices less punitive, in accord with popular opinion. Evidence presented in Chapter 1 documented that criminal sentencing *did* in fact become less harsh after 1994 (Table 1.1) and that the prison population declined after 2009. Public opinion data likewise indicate that the public wants less reliance on prison and more reliance on treatment and prevention approaches. Some of these alternative approaches are outlined in the concluding chapter.

## References


Akers, Ronald. 1973. *Deviant Behavior: A Social Learning Approach.* Belmont, CA: Wadsworth.

Avi-Itzhak, B., and R. Shinnar. 1973. Quantitative models in crime control. *Journal of Criminal Justice* 1:185–217.

Batton, Candice, and Steve Wilson. 2006. United States 1947 to 1998 police murders: An examination of historical trends in the killing of law Enforcement officers in the United States 1947 to 1998. *Homicide Studies* 10:79–97.

Bernard, Thomas, and Richard Ritti. 1991. The Philadelphia birth cohort and selective incapacitation. *Journal of Research in Crime and Delinquency* 28:33–54.

Besci, Zsolt. 1999. Economics and crime in the states. *Federal Reserve Bank of Atlanta Economic Review* 84:38–56.

Biles, David. 1983. Crime and imprisonment. *British Journal of Criminology* 23:166–172.

Blumstein, Alfred, Jacqueline Cohen, and Daniel Nagin. 1978. *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates.* Washington, DC: National Academy Press.

Blumstein, Alfred, Jacqueline Cohen, Jeffrey Roth, and Christy Visher. 1986. *Criminal Careers and Career Criminals, Volume 1.* Washington, DC: National Academy of Sciences.

Bowker, Lee H. 1981. Crime and the use of prisons in the United States: A time series analysis [1958–78]. *Crime and Delinquency* 27:206–212.

Cappell, Charles, and Gresham Sykes. 1991. Prison commitments, crime, and unemployment: A theoretical and empirical specification for the US 1933–1985. *Journal of Quantitative Criminology* 7:155–199.

Chaiken, Jan, and Marcia Chaiken. 1982. *Varieties of Criminal Behavior.* Santa Monica: Rand Corporation.

Clarke, Stevens. 1974. Getting em out of circulation: Does incarceration of juvenile offenders reduce crime. *Journal of Criminal Law and Criminology* 65:528–535.

Cohen, Lawrence E., and Kenneth C. Land. 1987. Age structure and crime: Symmetry versus asymmetry and the projection of crime rates through the 1990s. *American Sociological Review* 52:170–183.

Cohen, Mark A. 1988. Pain, suffering, and jury awards: A study of the cost of crime to victims. *Law & Society Review* 22:537–555.

Cohen, Mark A., Ted R. Miller, and Shelli B. Rossman. 1994. The costs and consequences of violent behavior in the United States. In *Understanding and Preventing Violence, Volume 4: Consequences and Control*, eds. Albert J. Reiss, Jr. and Jeffrey A. Roth, 67–166. Washington, D. C.: National Academy Press.

Cook, Phillip, and Jens Ludwig. 2000. *Gun Violence: The Real Costs.* New York: Oxford University Press.

Corman, Hope, and Nanci Mocan. 2005. Carrots, sticks and broken windows. *Journal of Law and Economics* 48:235–266.

D'Alessio, Stewart, and Lisa Stolzenberg. 1998. Crime, arrests and retrial jail incarceration: An examination of the deterrence thesis. *Criminology* 36:735–762.

DeFina, Robert, and Thomas Arvanites. 2002. The weak effect of imprisonment on crime: 1971–1998. *Social Science Quarterly* 83:635–653.

Devine, Joel, Joseph Sheley, and M. Dwayne Smith. 1988. Macroeconometric and social-control policy influences on crime rate changes 1948–1985. *American Sociological Review* 51:407–421.

Donohue, John, and Steven Levitt. 2001. The impact of legalized abortion on crime. *Quarterly Journal of Economics* 116:379–420.

Ehrlich, Isaac. 1973. Participation in illegitimate activities: A theoretical and empirical investigation. *Journal of Political Economy* 81:521–565.

Galster, George C., and Laure A. Scaturo. 1985. The U.S. criminal justice system: Unemployment and the severity of punishment. *Journal of Research in Crime and Delinquency* 22:163–189.

Gottfredson, Michael, and Travis Hirschi. 1986. The true value of lambda would appear to be zero. *Criminology* 24:213–234.

Gottfredson, Michael, and Travis Hirschi. 1990. *A General Theory of Crime*. Stanford, CA: Stanford University Press.

Grasmick, Harold, and Robert Bursik. 1990. Conscience, significant others and rational choice: Extending the rational choice model. *Law and Society Review* 24:837–861.

Gray, Charles. 1979. *The Costs of Crime*. Beverly Hills, CA: Sage.

Greenberg, David. 1975. The incapacitative effect of imprisonment: Some estimates. *Law and Society Review* 9:541–580.

Greenwood, Peter. 1982. *Selective Incapacitation*. Santa Monica, CA: Rand Corporation.

Greenwood, Peter, and Susan Turner. 1987. *Selective Incapacitation Revisited*. Santa Monica, CA: Rand Corporation.

Harrison, Paige M., and Allen J. Beck. 2006. *Prison and Jail Inmates at Midyear 2005*. Bureau of Justice Statistics. Washington, DC: U.S. Government Printing Office.

Hausman, Jerry A. 1993. *Contingent Valuation: A Critical Assessment*. Amsterdam: North Holland.

Hennessy, James, Vincent Rao, Jennice Vilhauer, and Joyce Fensterstock. 1999. Crime and punishment: Infrequently imposed sanctions may reinforce criminal behavior. *Journal of Offender Rehabilitation* 29:65–75.

Hirschi, Travis, and Michael Gottfredson. 1983. Age and the explanation of crime. *American Journal of Sociology* 89:552–584.

Inverarity, James, and Daniel McCarthy. 1988. Punishment and social structure revisited. *Sociological Quarterly* 29:263–279.

Jacobsen, Michael. 2005. *Downsizing Prisons*. New York: New York University Press.

Klaus, Patsy. 1994. *The Cost of Crime to Victims*. Crime Data Brief. Washington, DC: Bureau of Justice Statistics.

Kleck, Gary. 1979. Capital punishment, gun ownership, and homicide. *American Journal of Sociology* 84:882–910.

Kleck, Gary. 1984. The relationship between gun ownership levels and rates of violence in the United States. In *Firearms and Violence*, ed. Don B. Kates, Jr., 99–135. Cambridge: Ballinger.

Kleck, Gary. 2001. Review of *Gun Violence: The Real Costs*, by Philip Cook and Jens Ludwig. *Criminal Law Bulletin* 37:544–547.

Kovandzic, Tomislav. 2001. The impact of Florida's habitual offender law on crime. *Criminology* 29:179–203.

Kovandzic, Tomislav, and John Sloan. 2002. Police levels and crime rates revisited. *Journal of Criminal Justice* 30:65–76.

Kovandzic, Tomislav, John Sloan, and Lynne Vieraitis. 2002. Unintended consequences of politically popular sentencing policy. *Criminology & Public Policy* 1:399–424.

Kovandzic, Tomislav, John Sloan, and Lynne Vieraitis. 2004. "Striking out" as a crime reduction policy: The impact of "three strikes" laws on crime rates in U.S. cities. *Justice Quarterly* 21:207–239.

Kovandzic, Tomislav, and Lynne Viereaitis. 2006. The effect of county-level prison population growth on crime rates. *Criminology & Public Policy* 5:213–243.

Kovandzic, Tomislav, Lynne Vieraitis, and Denise Boots. 2009. Does the death penalty save lives? New evidence from a state panel data 1977 to 2006. *Criminology & Public Policy* 8:803–843.

Langan, Patrick. 1991. America's soaring prison population. *Science* 251:1568–1573.

Levitt, Steven D. 1995. *Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?* Cambridge, MA: National Bureau of Economic Research.

Levitt, Steven D. 1996. The effect of prison population size on crime rates: Evidence from prison overcrowding litigation. *Quarterly Journal of Economics* 3:319–351.

Levitt, Steven. 1997. Using electoral cycles in police hiring to estimate the effect of police on crime. *American Economic Review* 87:270–290.

Levitt, Steven D. 1998. Juvenile crime and punishment. *Journal of Political Economy* 106:1156–1185.

Levitt, Steven D. 2001. Alternative strategies for identifying the link between unemployment and crime. *Journal of Quantitative Criminology* 17:377–390.

Levitt, Steven D. 2004. Understanding why crime fell in the 1990s: Four factors that explain the decline and six that do not. *Journal of Economic Perspectives* 18:163–190.

Liedka, Raymond, Anne Piehl, and Bert Useem. 2006. The crime-control effect of incarceration: Does scale matter? *Criminology & Public Policy* 4:245–276.

Lynch, Michael J. 1999. Beating a dead horse: Is there any basic empirical evidence for the deterrent effect of imprisonment? *Crime, Law, and Social Change* 31:347–362.

Males, Mike, and Dan Macallair. 1999. Striking out. *Stanford Law and Policy Review* 11:65–102.

Maltz, Michael, and Stephen Pollock. 1980. Artificial inflation of a delinquency rate by a selection artifact. *Operations Research* 28:547–559.

Marquis, Kent H., and Patricia A. Ebener. 1981. *Quality of Prisoner Self-Reports, Arrest and Conviction Response Errors.* Santa Monica: Rand Corporation.

Marvell, Thomas. 1994. Is further prison expansion worth costs? *Federal Probation* 58:59–62.

Marvell, Thomas, and Carlisle Moody. 1994. Prison population growth and crime reduction. *Journal of Quantitative Criminology* 10:109–140.

Marvell, Thomas, and Carlisle Moody. 1996a. Specification problems, police levels and crime rates. *Criminology* 34:609–646.

Marvell, Thomas, and Carlisle Moody. 1997. The impact of prison growth on homicide. *Homicide Studies* 1:205–233.

Marvell, Thomas, and Carlisle Moody. 1998. The impact of out-of-state-prison population on state homicide rates: Displacement and free-rider effects. *Criminology* 36:513–535.

Marvell, Thomas, and Carlisle Moody. 1999. Female and male homicide victimization rates: Comparing trends and regressors. *Criminology* 37:879–900.

Marvell, Thomas, and Carlisle Moody. 2001. The legal effects of three strikes laws. *The Journal of Legal Studies* 30:89–106.

Matsueda, Ross. 1988. The current state of differential association theory. *Crime and Delinquency* 34:277–306.

McGuire, William J., and Richard G. Sheehan. 1983. Relationships between crime rates and incarceration rates: Further analysis. *Journal of Research in Crime and Delinquency* 20:73–85.

Miller, Ted, Mark Cohen, and Brian Wiersema. 1996. *Victim Costs and Consequences.* Washington, DC: National Institute of Justice.

Nagin, Daniel. 1978. Crime rates, sanction levels, and constraints on prison populations. *Law and Society Review* 12:341–366.

Nagin, Daniel, and Raymond Paternoster. 1994. Personal capital and social control: The deterrence implications of individual differences in criminal offending. *Criminology* 32:581–606.

Nagin, Daniel, Alex Piquero, Elizabeth Scott, and Laurence Steinberg. 2006. Public preferences for rehabilitation versus incarceration of juvenile offenders. *Criminology & Public Policy* 5:627–652.

Piehl, Anne, and John DiIulio. 1995. Does prison pay? revisited: Returning to the crime scene. *The Brookings Review* 21–25.

Raphael, Steven, and Rudolf Winter-Ebmer. 2001. Identifying the effect of unemployment on crime. *Journal of Law and Economics* 44:259–283.

Reiss, Albert. 1988. Co-offending and criminal careers. *Crime and Justice* 10:117–170.

Reynolds, Morgan. 1990. Crime pays, but so does punishment. *Journal of Social, Political, and Economic Studies* 15:259–300.

Saridakis, Geroge. 2004. Violent crime in the United States of America. *European Journal of Law and Economics* 18:203–221.

Shepherd, Joanna. 2002. Fear of the first strike: The full deterrent effect of California's two- and three-strikes legislation. *Journal of Legal Studies* 31:159–201.

Shinnar, Reuel, and Shlomo Shinnar. 1975. The effect of the criminal justice system on the control of crime: A quantitative approach. *Law and Society Review* 9:581–611.

Smith, Kevin. 2004. The politics of imprisonment. *Journal of Politics* 66:925–938.

Spelman, William. 1994. *Criminal Incapacitation.* New York: Praeger.

Spelman, William. 1995. The severity of intermediate sanctions. *Journal of Crime and Delinquency* 32:107–135.

Spelman, William. 2000a. The limited importance of prison expansion. In *The Crime Drop in America*, eds. Alfred Blumstein and Joel Wallman. New York: Cambridge University Press.

Spelman, William. 2000b. What recent studies do (and don't) tell us about imprisonment and crime. *Crime and Justice* 27:419–494.

Spelman, William. 2005. Jobs or jail? The crime drop in Texas. *Journal of Policy Analysis and Management* 24:133–165.

Stolzenberg, Lisa, and Stewart J. D'Allessio. 1997. Three strikes and you're out: The impact of California's new mandatory sentencing law on serious crime rates. *Crime & Delinquency* 43:457–469.

Strom, Kevin. 2000. *Profile of State Prisoners Under Age 18 1985–1997, BJS Special Report.* Washington, DC: Bureau of Justice Statistics.

Sutherland, Edwin. 1947. *Criminology* (4th ed.). Philadelphia: Lippincott.

Sweeten, Gary. 2010. E-mail communication to Gary Kleck from Professor Gary Sweeten of the School of Criminology and Criminal Justice, Arizona State University, Phoenix, AZ.

Sweeten, Gary, and Robert Apel. 2007. Incapacitation: Revisiting an old question with a new method and new data. *Journal of Quantitative Criminology* 23:303–326.

U.S. Bureau of Justice Statistics. 2002. *Prisoners in 2000.* BJS Bulletin. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2006c. *Criminal Victimization in the United States—Statistical Tables.* Tables for 2005. Washington, DC: Bureau of Justice Statistics. Available online at www.ojp.usdoj.gov/bjs/abstract/cvusst.htm.

U.S. Bureau of Justice Statistics. 2006d. *Felony Defendants in Large Urban Courts 2002.* Washington, DC: Bureau of Justice Statistics.

U.S. Bureau of Justice Statistics. 2007. *Prison and Jail Inmates at Midyear 2006.* BJS Bulletin. Washington, DC: U.S. Government Printing Office.

U.S. Bureau of Justice Statistics. 2010. *Criminal Victimization in the United States—Statistical Tables.* Tables for 2007. Washington, DC: Bureau of Justice Statistics. Available online at http://bjs.ojp.usdoj.gov/content/pub/pdf/cvus/current/cv0737.pdf.

U.S. Bureau of Justice Statistics. 2015. *Prisoners in 2014.* BJS Bulletin. Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 1999. *Crime in the United States—1998.* Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 2005. *Crime in the United States—2004.* Washington, DC: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation. 2016. *Crime in the United States—2015.* Available online at https://ucr.fbi.gov/crime-in-the-u.s/2015/crime-in-the-u.s.-2015/tables/table-1.

Van Dine, Stephan, Simon Dinitz, and John Conrad. 1977. The incapacitation of the dangerous offender: A statistical experiment. *Journal of Research and Delinquency* 14:22–34.

Visher, Christy. 1987. Incapacitation and crime control. *Justice Quarterly* 4:514–543.

Von Hirsch, Andrew. 1985. *Past or Future Crimes: Deservedness and Dangerousness in the Sentencing of Criminals.* New Brunswick, NJ: Rutgers University Press.

Von Hoffer, Hanns, and Henrik Tham. 1989. General deterrence in a longitudinal perspective: A Swedish case: Theft, 1841–1985. *European Sociological Review* 5:25–45.

Walker, Samuel. 2005. *Sense and Nonsense about Crime and Drugs.* Belmont, CA: Wadsworth.

Williams, Kirk, and Richard Hawkins. 1992. Wife assault, costs of arrest and the deterrence process. *Journal of Research in Crime and Delinquency* 29:292–310.

Wilson, James. 1994. Prisons in a free society. *Public Interest* 117:37–40.

Withers, Glenn. 1984. Crime, punishment and deterrence in Australia: An empirical investigation. *Economic Record* 60:176–85.

Witt, Robert, and Ann Witte. 2000. Crime, prison, and female labor supply. *Journal of Quantitative Criminology* 16:69–85.

Wolpin, Kenneth. 1980. A time series-cross-sectional analysis of international variation in crime and punishment. *Review of Economics and Statistics* 62:417–423.

Worrall, John. 2004. The effect of three-strikes legislation on serious crime in California. *Journal of Criminal Justice* 32:283–296.

Wright, Richard. 1994. *In Defense of Prisons.* Westport, CT: Greenwood Press.

Zedlewski, Edwin. 1987. *Making Confinement Decisions.* NIJ Research in Brief. Washington, DC: U.S. Government Printing Office.

Zimmerman, Paul. 2004. State executions, deterrence, and the incidence of murder. *Journal of Applied Economics* 1:163–193.

Zimmerman, Paul. 2006. Estimates of the deterrent effect of alternative execution methods in the United States: 1978–2000. *American Journal of Economics and Sociology* 65:909–941.

Zimmerman, Paul, and Bruce Benson. 2007. Alcohol and rape: An economics-of-crime perspective. *International Review of Law and Economics* 27:442–473.

Zimring, Franklin, and Gordon Hawkins. 1988. The new mathematics of imprisonment. *Crime and Delinquency* 34:425–436.

Zimring, Franklin, and Gordon Hawkins. 1991. *The Scale of Imprisonment.* Chicago: University of Chicago Press.

Zimring, Franklin, Gordon Hawkins, and Sam Kamin. 2001. *Punishment and Democracy: Three Strikes and You're Out in California.* New York: Oxford University Press.

# 11

# CRIME-INCREASING EFFECTS OF PUNISHMENT

No account of the effects of legal punishment claiming to be balanced could ignore the harmful effects of legal punishment, in particular its crime-increasing effects. Some are better documented than others, but there clearly are such effects. Chapter 6 showed that findings regarding the effect of punishment on the person punished are more likely show a net crime-increasing impact than a net crime-reducing impact. We do not, however, know much about just why these detrimental effects occur, since most of the studies only addressed the overall impact of sanctioning experiences. Further, the Chapter 6 studies did not address the harmful effects that punishment could have on the families or communities of those punished, nor the opportunity costs of spending resources on punishment that might have been invested instead in other, more cost-effective crime control strategies.

Unfortunately, there have been far fewer studies directly designed to explore harmful effects of punishment than there have been studies investigating crime-reducing effects. The volume of empirical research on any one type of crime-increasing effect is generally too small to justify a systematic review of the kind reported in Chapters 5–10, so we have performed that sort of systematic review for only the diverse set of nonspecific crime-increasing impacts lumped under the general heading of "labeling effects." Before reviewing empirical evidence on crime-increasing effects of legal punishment, we begin by discussing some of the causal mechanisms by which legal punishment *might* have such effects.

## Some Possible Crime-Increasing Effects of Punishment on the Person Punished

### *Reducing Perceived Risk Rather Than Increasing It*

In Chapter 6 we noted that some studies find that punishment experiences can reduce the offender's perception of the future risks of legal punishment, possibly because the offender falls prey to the "gambler's fallacy". This might increase later lawbreaking if risk perceptions have a deterrent effect. Most of the evidence summarized in Chapter 5, however, indicates that perceptions of punishment risk generally do not show the significant negative association with offending that deterrence theory requires. Nevertheless, in light of the minority of findings that do support perceptual deterrence, it is possible that perceived risks affect lawbreaking for some offenders. For this subset of offenders, a downward updating of perceived risk could cause increased offending. Nevertheless, this particular response to the experience of punishment appears to be fairly rare. Thus it is unlikely that this mechanism is responsible for any very substantial crime-increasing effects.

## *Criminal Learning in Prison—Prisons as Schools for Crime*

### *The Hardening of Pro-Criminal Attitudes*

Imprisonment is, short of capital punishment, the most severe and life-altering form of legal punishment. The most extreme form of social isolation is produced by incarceration, which makes interaction with noncriminal others virtually impossible, aside from contacts with staff and brief visits by family members. Imprisonment insures that the offender's social interactions are almost entirely confined to criminals, who will generally express attitudes more favorable or tolerant towards criminal behavior than the average person in the outside world. Enforced cohabitation with large numbers of almost exclusively pro-criminal others will at best preserve pro-criminal attitudes in the inmate, at worst strengthen them and discourage conventional, conforming attitudes.

Any number of pro-criminal norms and values may be reinforced, such as the view that "honest work is for suckers," or the idea that all people are manipulative and exploitative, so "you better get the other guy before he gets you." Whatever lingering respect for law the inmate may have possessed before entering prison may be eliminated and replaced by the view that the legal system and the criminal justice system are morally hypocritical, and those who enforce the laws are unfair, corrupt, racist, and possibly even brutal. Other attitudes that could encourage crime would include the belief that "life on the streets" is short, so "you better live for the present," ignore the long-term future, and "get what you can now, the quick way, regardless of risks."

Prisons are social islands isolated from conventional law-abiding communities. Unless an inmate could somehow isolate himself entirely from the society of his fellow inmates, he is likely to be influenced by their attitudes to some degree. Thus, any antisocial attitudes he brought into prison with him are likely to be reinforced, and any prosocial attitudes are likely to be weakened for lack of support from his fellow inmates. The result could be, notwithstanding what an inmate might tell his parole board or prison personnel, attitudes even more favorable to lawbreaking at the end of a prison stay than they were at the start.

Inmates may also learn specific rationalizations for crime from their fellow inmates, sometimes called "techniques of neutralization." These are mental tricks that people can use with themselves to evade the dictates of their conscience, by persuading themselves that those dictates do not apply in connection with a specific contemplated crime. A person may, for example, tell themselves that a victim is evil and deserves punishment, that the crime will not significantly harm the victim, that the offender cannot control himself and is therefore not responsible for his behavior, or that the crime serves some greater good (Matza and Sykes 1957). Hearing these rationalizations expressed repeatedly and fervently by their fellow inmates can persuade an imprisoned criminal that there is truth to these views and that they are more than just phony excuses that let the criminal "off the hook."

The ideal way to test the proposition that incarceration builds antisocial attitudes would be to measure such attitudes at the beginning and end of prison stays among a representative sample of inmates and compare the changes with a matched sample of otherwise similar offenders who were given nonincarceral sentences. While there are many studies of the net *overall* impact of incarceration on inmate reoffending, we are not aware of any research systematically measuring such changes in antisocial attitudes. It certainly makes good sense that forced association with antisocial persons would encourage the acquisition of antisocial attitudes, but there is little in the way of empirical documentation of the phenomenon.

### *The Learning of Improved Criminal Skills*

Inmates can learn more than just pro-criminal attitudes and rationalizations for criminal behavior from their fellow inmates. Indeed, it is quite possible that by the time criminals have been sent to prison they have already had ample time to develop all the rationalizations they need to justify crimes. It has long been recognized, however, that prison life also teaches specific criminal techniques to its "students" (Bentham 1789; Clemmer 1940; Sykes 1958). Prison life provides a great deal of idle time, much of it filled with conversation with fellow inmates. Talk among criminals, as it does among noncriminals who share the same occupation, often turns to the details of how they do their jobs. Inmates talk endlessly about old scores or plan for new ones, discuss how they would do crimes smarter and better in the future, and how they would avoid the mistakes that got them caught and sent to prison. Some of the talk concerns how to find and recognize better criminal opportunities, which serves to increase the rewards of future crimes. Inmates may learn specific techniques that allow them to tackle more rewarding targets, such as learning how to use weapons and strategic threats to manage crowds of victims in a bank or how to make nitroglycerin to blow open safes (Letkemann 1973, 122–130). Inmates can exchange information on how to identify especially attractive targets, smuggle contraband, avoid alarms, evade police patrols, stand up under a police interrogation, locate a good defense attorney, and a host of other topics bearing on the successful commission of crimes and the avoidance of arrest, conviction, or incarceration. Criminals can, of course, learn from their own prior experiences with crime, but their learning can be considerably enriched by the experiences of numerous others as well. Inmates who take advantage of these learning opportunities, and retain the information thereby acquired, are presumably more likely to benefit from crime and less likely to suffer from legal punishment should they resume offending after release from prison.

To be sure, it is possible that imprisonment, by dramatizing the pains of punishment by incarceration, may cause the inmate to fear punishment more and have all the stronger a desire to avoid repeating the punishment experience (Andenaes 1966). Prison may, however, also increase his ability to avoid the feared punishment without curtailing his criminal activities. Thus, prison "education" can cause a net reduction in the inmate's perceived risk of arrest after release from prison and thereby reduce the deterrent effect of sanctions. In short, part of the reason the prison experience does not have a net crime-reducing effect may be because enforced association with experienced criminals reduces the perceived threat of legal sanctions by training inmates how to avoid those sanctions in future.

Although some scholars have noted the existence of criminal mentors who share their knowledge with less experienced offenders (e.g., Morselli, Tremblay, and McCarthy 2006), they have not focused on mentoring in prisons. There is certainly anecdotal evidence of some inmates learning criminal techniques while in prison (e.g., Letkemann 1973, 122–139), but we know of no direct, systematic evidence on how widespread such learning is, or whether criminal expertise or knowledge on net grows during incarceration for the mass of inmates. Many inmates may be oblivious to the expertise of those around them. Indeed, since inmates are by definition criminals who have failed to evade punishment, it is even possible that contact with inept mentors could degrade the quality of an inmate's criminal techniques. Likewise, inmates might forget some of the criminal knowledge they brought with them to prison or their knowledge could become outdated with the passage of time. In sum, although the concept of prisons as schools for crime is perfectly plausible, we have no sound empirical basis for judging whether incarceration, on net, increases the criminal expertise of the inmate population. Although there is considerable supportive anecdotal information on this point, we are aware of no systematic empirical evidence comparing levels of criminal knowledge and skill among large samples of offenders before and after prison experience.

### *Punishment and the Loss of Social Capital*

People who have a conventional reputation, as law-abiding and generally respectable persons, are more likely to be deterred by the prospect of legal punishment because they have more social capital, in the form of a conventionally respectable reputation, to lose (Chapter 2). The prospect of punishment has a stronger deterrent effect for persons with more social capital, but once people are legally punished, their conventional social respectability is reduced and the deterrent power of potential punishment is reduced. Thus, punishment reduces the very social respectability that would, if it could be preserved, magnify punishment's deterrent effects. In this sense, punishment in the past reduces the power of punishment to deter in the future. Punishment therefore works best when it is threatened but not actually inflicted, because the threat can have general deterrent effects before being actually inflicted and these deterrent effects have not yet been undercut by the offender's loss of respectability and other deviance-amplifying effects of actually experiencing punishment.

### *Crime-Increasing Effects of Contact With the Justice System and Official Labeling*

There is a very large body of research that has empirically assessed the impact of official sanctions on the offending of those sanctioned, much of it reviewed in Chapter 6. As noted there, some of these studies were intended to test for labeling effects, while others were designed as tests of specific deterrence. Regardless of the way the research question was framed by researchers, most of these studies were actually capable only of assessing the overall net effect of punishment, not the contribution of any one intervening mechanism. We therefore preface the following discussion with a caution that scholars who profess to have documented "labeling effects" do not usually distinguish from each other specific, distinct crime-increasing effects, such as the adoption of a criminal self-identity vs. blocking of legitimate opportunities.

When people come in contact with the juvenile or criminal justice system as a suspect, they are thereby "labeled" as delinquents or criminals. Some official labels are transitory and not widely known, while others are permanent and may become known to many. Records pertaining to juveniles are especially likely to be either transitory or held in confidence so that they are not widely disseminated or known to persons outside the juvenile justice system. In contrast, records of adult arrests, convictions, and incarcerations are generally permanent, widely accessible, and thus potentially known to a wide array of people, such as friends, relatives, and potential employers of the person labeled.

Labeling theory has been used to explain, among other things, why punishment might increase criminal behavior of those punished. There are many versions of "labeling theory," and they are used to explain many phenomena related to rule-breaking (e.g., how some rule-breakers are selected for sanctioning and not others), but we are concerned only with those variants that suggest that official labeling of people as criminals or deviants can increase subsequent offending (Tittle 1980). These variants assert that official sanctioning of people can increase subsequent offending in either of two ways. First, the imposition of an official label as a criminal can induce an "identity transformation," causing the sanctioned person to regard themselves as deviant or criminal, to "accept" the deviant label, and perhaps even adopt the offender identity as a "master status" that is more important than all other statuses. Second, labeling can lead to structural obstacles to conformity, such as reduced chances for legitimate employment or educational attainment (Sampson and Laub 1993).

Labeling theory states that the imposition of an official label can change the way the person perceives himself or herself or the way the person is perceived by others. The imposition of the criminal or delinquent label could make it more likely others will see the person as a criminal, possibly to the extent that "criminal" becomes their "master status," the one that trumps all others and comes to be the primary identity that others attribute to the person. Everything the person says or does is judged from the perspective that the person is, above all else, a criminal.

The official label contributes to the credibility of this criminal status above and beyond the person's criminal behavior itself because the official labeling action is often publicly known ("he's been to juvenile court," "he's been away for two years because he was in prison"), while unpunished criminal behavior itself is usually secretive and not directly known by many other people. Further, the official label may influence others' view of the rule breaker beyond the rule-breaking itself because many assume that the criminal justice system is an especially reliable identifier of criminals or that it only labels especially serious rule-breakers as criminals. In sum, the official label can stigmatize the labeled person as deviant, encourage others to regard the person as a deviant, and consequently encourage the person to think of themselves as a criminal.

If others regard the person as a criminal and behave towards him as if he were dishonest, immoral, or perhaps even dangerous, this might shift the person's own self-concept in a criminal direction. He may eventually "internalize" the criminal identity and adopt it as his own. Those who think of themselves as criminals feel less discomfort in considering illegal acts because such acts are in accord with their own self-identity and because they feel they have less to lose from committing the acts—one cannot lose respect among people who already regard you as disreputable (Becker 1963; Lemert 1951; Tannenbaum 1938).

This changed self-concept can then lead to "secondary deviance," deviance that is due to official labeling itself (see Tittle 1980 for a critical review). Skeptics might reasonably argue, however, that official labels are imposed too late in a criminal career to have much impact on offenders' self-concepts. Given the low absolute certainty of arrest for crimes (Chapter 1), a person would typically have committed many crimes before their first arrest and thus had ample opportunity to develop a self-concept of themselves as criminals by virtue of their own behavior, without the aid of an official label. Thus, even if a criminal self-concept did affect offending behavior, official labeling may not affect self-concept in the first place, and thus not affect offending via influences on self-concept.

On the other hand, some versions of labeling theory do not rely on the concept of identity transformation, instead stressing the detrimental effects of official sanctioning on the person's prospects for legitimate employment and educational attainment (e. g., Bernburg and Krohn 2003; Chiricos, Barrick, Bales, and Bontrager 2007). Thus, there are sound reasons for expecting crime-increasing effects of official labeling even if the labels do not affect how sanctioned persons view themselves.

### *Alteration of Others' Expectations*

Regardless of whether officially labeling alters the offender's self-concept, it is likely to affect how other people view him. Others may be more likely to expect behavior consistent with their view of the person as a criminal and with their notions of what being criminal implies. Those who did not already know the labeled person well would be especially likely to be influenced by the label because they possessed so little information about an individual that might contradict stereotypical expectations of how a criminal behaves. The official labeling that accompanies punishment can have such effects above and beyond the effects of the criminal behavior itself because far more people are likely to be aware of a person's official criminal record than they are of the criminal acts themselves.

A shift in others' expectations could in turn undermine the labeled person's incentives to obey the law. If others already perceive the person to be criminal in character, and the person has therefore already lost a respectable reputation among prosocial others, there is that much less to lose from continuing or increasing criminal conduct. In a sense, one major cost of further criminal behavior—loss of respectability—is eliminated once it has already been experienced as a result of past criminal activity.

### *Social Isolation From the Law-Abiding*

Being officially and publicly defined as a criminal, then, communicates something about the offender's moral character to all who know of these defining events, whether through interpersonal communication or through the written records that these events generate. Stigmatization produced by a publicly known criminal label can in turn lead to exclusion from conventional activities, reduced opportunities for lawful activities, and loss of membership in prosocial groups. Conventional others may cut their ties with the labeled person, thereby reducing the restraining influence on deviant behavior that these associations previously exerted on the labeled person. Others may shy away from initiating an association with a disreputable, devalued person. This may occur either because the punishment makes people aware of the offender's criminal behavior and causes them to avoid the offender, or because the offender avoids those who are likely to react negatively to the label.

For his part, the offender may feel more comfortable associating with persons less likely to be shocked by a criminal record, less likely to condemn criminal behavior, and more likely to engage in such behavior themselves. As a result, noncriminal associations shrink and criminal associations grow. The effect is to narrow the criminal's circle of associates to those who are more tolerant of criminal behavior and to increase the relative share of social influences that are antisocial (Becker 1963).

## *Defiance-Triggering Effects of Punishment*

Going beyond labeling theory, Sherman (1993) offered another explanation why the experience of punishment might increase criminal behavior, arguing that under certain conditions offenders react to punishment with "defiance." His explanation was an amalgam of elements drawn from three existing perspectives.

First, Braithwaite (1989) proposed that punishment inflicted in a way that shamed the offender rather than just portraying the criminal act as shameful tended to stigmatize the offender. When rule enforcers treat the violators with disrespect, they reject the actor as well as the act, placing him outside the law-abiding community and encouraging continued offending. In contrast, "reintegrative shaming" focuses shame on the act while treating the offender with respect, working to welcome the offender back into the community.

Second, Tyler (1990) posited that punishment was more likely to produce obedience to the law if the punished person believed the punishment to have been inflicted in a procedurally just manner, and the authorities treated him with respect. Conversely, punishment was less likely to produce deterrence if the offender did not perceive the punishment to have been inflicted with procedural fairness, because the punished person would accord less legitimacy to those enforcing the law.

Third, Scheff and Retzinger (1991) argued that people can react in either of two ways in response to efforts to punish and shame them. Those who have strong social bonds to the punishing authorities and society in general tend to acknowledge and accept the shame, thereby acknowledging the moral rightness of the rule they had violated. In contrast, those with weak social bonds are likely to feel ashamed of feeling ashamed and to therefore deny their feelings of shame. They react to punishment with anger and a denial of the rightness of the rule they violated as a way of establishing that there was no reason for them to feel ashamed. They subsequently may violate the rule again to demonstrate their rejection of its moral legitimacy.

Sherman (1993) fused these ideas into an explanation of when punishment was likely to produce defiance rather than deterrence. He hypothesized that defiance occurs when four conditions are met: (1) the punishment was perceived by the offender as unfair, (2) the offender was poorly bonded to the conventional order, (3) the punishment was perceived as stigmatizing, and (4) the offender denied the shame produced by the sanction instead taking pride in his acts and his isolation from the community. When none these elements are present, deterrence rather than defiance occurs, and when only some of them are present, a mixture of deterrence and defiance occurs.

Thus, the personal experience of punishment has different effects on different people, and these effects may be partially contingent upon whether the punished individual acknowledges shame over committing the criminal act. Whether this occurs is in turn dependent on whether the person perceived their punishment as just and on how strongly bonded the person was to the conventional social order. Punishment that is regarded as excessive or unfairly inflicted may lead to "unacknowledged shame and defiant pride," which increases subsequent criminal behavior. An offender who was already weakly bonded to the community that punished him finds it easier to reject the community's right to condemn him.

Such an offender does not acknowledge feeling any shame for his act because he denies or ignores its wrongful character and focuses instead on the perceived wrongful conduct of rule enforcers who (he believes) acted in a disrespectful or unfair way towards him. The punished person becomes angry at the punishers instead of focusing on the reasons for the punishment and the wrongful quality of his criminal act. He denies his shame over the act and embraces his separation from the community that unfairly punished him. Denying the moral legitimacy of the rules and rule enforcers encourages future defiance.

## Empirical Evidence on Crime-Increasing Effects of Legal Punishment on Those Punished

Many potentially crime-increasing effects of punishment have been subjected to few or no empirical tests, but there is a considerable body of work on what are broadly designated as "labeling effects." We review here essentially the same body of research as was reviewed in Chapter 6 but with a view to learning what it has to say about labeling effects. The studies we reviewed did not have to possess the stated purpose of examining labeling theory to qualify for inclusion in this review, but rather only had to report an association between a measure of official sanctioning and recidivism. We have built our review on an excellent review of the pre-2008 labeling research produced by Kelle Barrick (2014) and have expanded it to cover research published in 2008–2015.

There are many methodological problems afflicting research in this area. For example, it is essential that researchers control for prior offending if they are to distinguish labeling effects from "selection effects." Offenders who are regarded as more likely to repeat their offending are more likely to be selected for sanctioning or for more serious sanctioning. Since those who committed more crimes in the past are also more likely to break the law in future, the purported labeling effects of sanctions might instead reflect the greater preexisting propensity to offend that prevails among those selected for sanctioning (Chiricos et al. 2007).

In our Table 11.1 tabulations, we note that 50 of 159 total findings were produced by analyses in which the authors did not in any way control for prior offending. Since findings of this type do nothing to distinguish labeling effects from selection effects, the rest of the Table 11.1 tabulations (aside from those relating to Type of Control) pertain only to the 109 findings produced by analyses in which prior offending was controlled.

Without distinguishing stronger research from weaker research, most findings do not support labeling theory in that the evidence did not support a significant net positive effect of punishment

**TABLE 11.1** Labeling findings by research methods (percent of findings)

| | *Total # Findings* | *− sig* | *− ns* | *+ ns* | *+ sig* | *− p = ?* | *+ p = ?* |
|---|---|---|---|---|---|---|---|
| Total | 159 | 12.0 | 18.2 | 25.2 | 34.6 | 2.5 | 6.9 |
| Controlled for Criminal History | 109 | 13.8 | 17.4 | 25.7 | 37.6 | 0.9 | 4.6 |
| *Type of Control for Criminal History* | | | | | | | |
| None | 50 | 8.0 | 20.0 | 24.0 | 28.0 | 6.0 | 12.0 |
| Random Assignment | 2 | 50.0 | 0.0 | 0.0 | 50.0 | 0.0 | 0.0 |
| Binary | 26 | 19.2 | 15.4 | 23.1 | 42.3 | 0.0 | 0.0 |
| Count | 75 | 12.0 | 17.3 | 29.4 | 37.4 | 1.3 | 2.7 |
| Weighted Count | 6 | 0.0 | 33.3 | 0.0 | 16.7 | 0.0 | 50.0 |
| *Sample Size** | | | | | | | |
| 1,000 or Less | 68 | 20.6 | 22.0 | 25.0 | 25.0 | 1.5 | 5.9 |
| More Than 1,000 | 41 | 2.5 | 9.8 | 26.8 | 68.3 | 0.0 | 2.5 |
| *Follow-up Period** | | | | | | | |
| 1 Year or Less | 14 | 14.3 | 28.6 | 21.4 | 14.3 | 0.0 | 21.4 |
| More Than 1 year | 94 | 12.7 | 15.9 | 26.6 | 41.5 | 1.1 | 2.1 |
| *Recidivism Measure** | | | | | | | |
| Self-Report | 19 | 0.0 | 0.0 | 15.8 | 68.4 | 0.0 | 15.8 |
| Arrest/Contact/Report | 63 | 12.7 | 25.4 | 26.9 | 31.8 | 1.6 | 1.6 |
| Conviction | 15 | 20.0 | 13.3 | 33.3 | 26.6 | 0.0 | 6.6 |
| Incarceration | 2 | 50.0 | 0.0 | 50.0 | 0.0 | 0.0 | 0.0 |
| Charges Filed | 8 | 37.5 | 12.5 | 25.0 | 25.0 | 0.0 | 0.0 |
| Unfavorable Parole | 2 | 0.0 | 0.0 | 0.0 | 100.0 | 0.0 | 0.0 |

*Note*: * Includes only findings produced by analyses that controlled for criminal history

experience on subsequent offending. A large minority of the findings, however, is supportive. Further, findings indicating a significant net crime-increasing effect are far more common (38 percent) than those showing a significant net crime-reducing effect (14 percent). The results are consistent with the hypothesis that punishment has a mixture of crime-reducing and crime-increasing effects on those punished but that the latter outweigh the former more often than the reverse.

We also considered a number of other potentially consequential variations in research methods. First, studies with larger sample sizes have greater statistical power to reliably establish associations between sanctions and later offending. Thus, findings based on larger samples should be more likely to indicate a significant labeling effect, and Table 11.1 confirms this expectation. When researchers were able to study 1,000 or more people, 68 percent of the findings support a net crime-increasing effect due to labeling or other deviance enhancing effects. Thus, many researchers may have failed to obtain findings significantly supporting labeling merely because analysts studied unduly small samples of people.

Labeling effects may take a considerable amount of time to manifest themselves, in contrast to specific deterrent effects, which are likely to be strongest shortly after the sanctioning event when the memory of the punishment is still vivid. Thus, studies with longer follow-up periods should be more likely to obtain findings indicating net labeling effects. Table 11.1 confirms this expectation. Findings based on research using follow-up period exceeding one year were nearly three times as likely to support labeling as those based on follow-up periods of one year or less. Studies with long-term follow-up periods provide better estimates of the long-term effects of punishment experience, which is especially important for detecting the crime-increasing effects of punishment. While any special deterrent effects are likely to operate shortly after the punishment experience, detrimental effects on employment and education may only be fully evident after many years. Unfortunately, very few studies have used long follow-up periods. For example, only 11 percent of the findings generated by studies with known follow-up periods were based on follow-ups over five years.

Finally, some measures of recidivism are better than others for testing labeling theory. The theory predicts that sanctioning will increase subsequent offending, both that which is detected by the authorities and that which goes undetected. Most offending does not result in the offender's arrest, conviction, incarceration, or other officially recorded event. Self-report methods, though not perfect, can provide a more complete picture of reoffending than official recidivism measures that count only arrests, convictions, or reincarcerations. Nearly all labeling studies use either of just two measures of recidivism, self-reports or arrests/police contacts, so we cannot say anything meaningful about the handful of studies using other measures. Table 11.1 shows that findings based on the more complete self-report measures of reoffending are substantially more likely to support labeling effects than those based on arrests or police contacts. One reasonable way to tie these contrasting findings together is to hypothesize that official labeling does indeed increase reoffending, but this increased offending gives the criminal experience and an improved ability to avoid later arrest. If this is true, an exclusive reliance on arrest or other official records will miss some of the increased offending produced by labeling.

Since each of these methodological variations, considered individually, appear to affect the character of labeling findings, we explored whether findings generated by studies with multiple methodological strengths were more likely to support labeling. The tabulations in Table 11.2 show how the distribution of findings changes as the set of studies considered is limited to those with an increasingly large numbers of advantageous methodological features. The results indicate that the more one limits attention to methodologically stronger studies, the more the findings support labeling or other crime-increasing effects of legal punishment. Among findings based on analyses that (a) controlled for prior criminal history, (b) controlled for multiple potential confounders by using multivariate analysis methods, (c) used large samples, *and* (d) had longer follow-up periods, 70.6 percent supported labeling or some other crime-increasing effects.

In addition to methodological differences across studies, findings may also differ because labeling effects may be more pronounced with some kinds of official sanctions, for some kinds of sanctioned offenders, or with regard to some kinds of offending. Consider first the possibility that some sanctioning experiences may be more consequential than others. Some official labels may be more likely to have a lasting effect because they are recorded, such as an adult conviction or arrest recorded in written or computer form, in contrast to a mere police contact. Other punishment

**TABLE 11.2** Labeling findings by methodological strength (percent of findings)

| | *Total # Findings* | *– sig* | *– ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
|---|---|---|---|---|---|---|---|
| Controlled Criminal History | 109 | 13.7 | 17.4 | 25.7 | 37.6 | 0.9 | 4.6 |
| Controlled Criminal History, and Multivariate Analysis | 99 | 15.2 | 19.2 | 24.2 | 35.4 | 1.0 | 5.1 |
| Controlled Criminal History, Multivariate Analysis, and Large Sample | 41 | 2.5 | 9.8 | 22.0 | 63.4 | 0.0 | 2.5 |
| Controlled Criminal History, Multivariate Analysis, Large Sample, and Follow-up > 1 Year | 34 | 3.0 | 5.9 | 20.6 | 70.6 | 0.0 | 0.0 |

experiences may be more emotionally consequential because they are more extended or traumatic, such as incarceration.

Labeling theory predicts that official sanctioning will most strongly affect first offenders or otherwise less experienced offenders than more experienced offenders and that first or early sanctioning experiences will have stronger effects than later ones. Paternoster and Iovanni (1989) attribute this pattern to the ways that sanctioning leads the sanctioned person away from conventional associations and activities: "Once an actor is excluded from major conventional life situations it is not unreasonable to assume that further exclusion would have little additional meaning." First sanctioning experiences typically occur when the labeled person is young, so labeling effects should be more evident among juveniles than among adults. The patterns of findings shown in Table 11.3 confirm this expectation—labeling effects are more often found in studies of samples of juveniles than in studies of adults and more likely to be found among naïve first offenders than among experienced offenders.

Correspondingly, the first arrest or conviction experienced by a person has more detrimental effects on that person than later arrests, convictions, or incarcerations. In other words, labeling/punishment experiences are more likely to have deviance-amplification effects for naïve or first offenders than for experienced offenders. For example, Babst and Mannering (1965) found that

**TABLE 11.3** Labeling findings under various substantive conditions (percent of findings)

| | *Total # Findings* | *– sig* | *– ns* | *+ ns* | *+ sig* | *– p = ?* | *+ p = ?* |
|---|---|---|---|---|---|---|---|
| Total | 159 | 12.0 | 18.2 | 22.7 | 34.6 | 2.5 | 6.9 |
| Official Sanction* | | | | | | | |
| Arrest/Contact | 26 | 11.5 | 15.4 | 11.6 | 50.0 | 0.0 | 11.5 |
| Conviction | 6 | 16.7 | 0.0 | 66.7 | 16.7 | 0.0 | 0.0 |
| Juvenile Justice | 15 | 0.0 | 6.7 | 13.3 | 80.0 | 0.0 | 0.0 |
| Incarceration | 31 | 6.5 | 16.1 | 45.2 | 22.6 | 3.2 | 6.5 |
| Incarceration Length | 30 | 30.0 | 30.0 | 16.7 | 23.3 | 0.0 | 0.0 |
| Adjudication | 1 | 0.0 | 0.0 | 0.0 | 100.0 | 0.0 | 0.0 |
| Population* | | | | | | | |
| Juvenile | 39 | 2.6 | 7.7 | 15.4 | 64.1 | 0.0 | 10.3 |
| Adult | 70 | 20.0 | 22.9 | 31.4 | 22.8 | 1.4 | 1.4 |
| Offender Type* | | | | | | | |
| Naïve | 5 | 0.0 | 20.0 | 25.6 | 28.2 | 1.3 | 3.8 |
| Experienced | 5 | 0.0 | 20.0 | 60.0 | 0.0 | 0.0 | 20.0 |
| Offense Type* | 49 | 12.3 | 14.2 | 20.4 | 44.9 | 0.0 | 8.2 |
| Not Specified | | | | | | | |
| Drugs | 6 | 0.0 | 16.7 | 66.7 | 16.7 | 0.0 | 0.0 |
| Domestic Violence | 18 | 38.9 | 22.2 | 33.3 | 5.6 | 0.0 | 0.0 |
| Drunk Driving | 4 | 0.0 | 25.0 | 50.0 | 25.0 | 0.0 | 0.0 |
| Violent | 12 | 16.7 | 8.3 | 33.3 | 41.7 | 0.0 | 0.0 |
| Property | 11 | 0.0 | 45.5 | 18.2 | 27.3 | 9.1 | 9.1 |
| Status Offense | 2 | 0.0 | 0.0 | 0.0 | 100.0 | 0.0 | 0.0 |
| Misdemeanor | 2 | 0.0 | 0.0 | 0.0 | 100.0 | 0.0 | 0.0 |
| Non-Violent Felony | 2 | 0.0 | 0.0 | 50.0 | 50.0 | 0.0 | 0.0 |
| Variety Score | 3 | 0.0 | 0.0 | 33.3 | 66.7 | 0.0 | 0.0 |

*Note*: * Includes only estimates that control for criminal history incarceration (vs. probation) was more likely to have a significant and positive association with reoffending (consistent with a net crime-increasing effect) for those with no prior felonies than for those with one or more priors. Similarly, Dejong (1997) found that among male arrestees, being sentenced to jail (vs. no incarceration), had stronger criminogenic effects among those with no prior arrests than among those with priors. For the body of research as a whole that broke down findings by whether offenders had prior records, we found that 28.2 percent of findings pertaining to naïve offenders were positive and significant, while none of the findings for experienced offenders were positive and significant. These findings support the view that initial labeling experiences increase offending but that later labeling experiences (such as arrests) have no further effect. While there may be some special deterrent effects of punishment on naïve offenders, they are evidently outweighed by deviance amplification effects.

Finally, some kinds of criminal behavior might be more influenced by official labeling, though labeling theorists have not agreed as to which crime types those might be. There have been too few studies on specific crime types, so no very pronounced pattern is evident in Table 11.3, though there is some suggestion that violent crimes are the type of offending most likely to increase in response to sanctioning.

Interpretations of this body of research must be tempered by the fact that few of these studies established what mechanisms linked sanctioning experiences with recidivism. Although there are many studies that indicate that official sanctions somehow increase subsequent offending, few studies provide direct evidence that labeling effects, as distinct from other crime-increasing mechanisms, were responsible. For example, notwithstanding the importance of changes in self-concept to some versions of labeling theory, there are few studies that directly tested the effect of formal sanctions on deviant self-concept or "criminal identity." Thomas and Bishop (1984) found a statistically significant positive association of contact with the police or with juvenile court authorities on a three-item measure of deviant self-concept but also noted that these formal sanctions increased the explained variation in self-concept by only a trivial 1 percent. Thus, there is very little evidence supporting the core principle of some varieties of labeling theory that official labeling causes an "identity transformation" or makes it more likely that labeled individuals will view themselves as criminals.

On the other hand, there is modest support for other potential links between official labeling and increased criminal offending. De Li (1999) analyzed data from a panel study of a sample of London youth and found that conviction for a crime made it more likely that the youth would be unemployed later, which in turn increased subsequent offending. Bernburg and Krohn (2003) examined panel data on male youth in Rochester, New York and found that official intervention in early life increased later criminal behavior and that this effect was partly mediated by lower employment and educational achievement. That is, youth processed by the juvenile justice system were less likely to get jobs or continue their schooling and both of these consequences increased their criminal behavior.

In a later study of the same data, Bernburg, Krohn, and Rivera (2006) also found that formal processing by the juvenile justice system made it more likely that youth would join a gang and, more generally, increased their associations with delinquent youth, both of which encouraged subsequent offending. Thus, official labeling appears to alter social associations, increasing the exposure of those labeled to people likely to encourage or tolerate offending rather than discourage it.

### *Detrimental Effects of Punishment on Educational Attainment of Those Punished*

Some studies focus not on the punishment/crime association per se, but rather on the impact of punishment on some of the variables believed by labeling theorists to mediate the effect of punishment on crime. Being punished and officially labeled as criminal, as distinct from engaging in crime per se, can make it more difficult to continue formal education and gain educational credentials. Some colleges are not willing to admit convicted felons. This makes the prospects for success in any lawful career significantly dimmer and, by comparison, makes criminal employment relatively more attractive (Sampson and Laub 1993).

Tanner, Davies, and O'Grady (1999) found that criminal justice system contact reduced educational attainment, independent of criminal behavior, for males but not for females. Hannon (2003) used a panel design with a national sample of youth and found that the more times a youth was arrested or charged the fewer years of schooling they completed and the more likely they were to drop out of school. Unfortunately, Hannon did not control for prior offending, leaving open the possibility that supposed effects of sanctioning on education were actually the product of criminal behavior or its correlates. Analyzing the same data, however, Sweeten (2006) controlled for prior offending and still found that youth with arrests or court appearances were less likely to graduate from high school.

### *Detrimental Effects of Punishment on Employment Prospects of Those Punished*

Many studies have found that people with criminal records are less likely to be hired by employers (Boshier and Johnson 1974; Buikhisen and Dijksterhuis 1971; Davies and Tanner 2003; Freeman 1991; Kurlychek, Brame, and Bushway 2007; Pager, Bonikowski, and Western 2009; Pager and Quillian 2005; Schwartz and Skolnick 1962; Western and Beckett 1999), even by those employers who claim they are willing to hire ex-offenders (Uggen, Vuolo, Lageson, Ruhland, and Whitham 2014).

Schwartz and Skolnick (1962) hired an employment agent to present employers with folders describing prospective employees for unskilled jobs and varied whether the folders indicated whether the potential applicant had a criminal record. The folders were otherwise identical, thereby controlling for other traits that might bear on the applicant's qualifications. They found that employers were far less likely to consider hiring an applicant with a criminal record. More recently, Pager (2003) found that job applicants with criminal records were only half as likely to be called back for a job interview as applicants without a record. Stoll and Bushway (2008) surveyed 609 Los Angeles businesses and found that employers legally required to check for criminal records generally did so and were significantly less likely to hire ex-offenders, while there was no effect of a criminal record on hiring among employers not legally required to make such checks. Using survey panel data on youth, Western and Becket (1999) found that incarceration during adolescence decreased a youth's chances of employment in young adulthood and that these effects were strongest for young, unskilled, minority men. Freeman (1991) likewise found that juvenile justice intervention in adolescence reduced employment in the young adult years.

Even low-level criminal records, such as arrests for misdemeanors, can reduce the chances of being hired to a modest degree (Uggen et al. 2014). There is also some evidence that the detrimental effects of a criminal record on employment can persist for a long time. One study showed that youths who had been arrested or experienced an official police contact in their mid-teens were more likely to be unemployed and more likely to be on welfare 15 years later, compared to otherwise similar youth without an arrest record (Lopes, Krohn, Lizotte, Schmidt, Vasquez, and Bernburg 2012).

Given the difficulties that an official criminal label creates for obtaining lawful employment, it is not surprising that persons with criminal records tend to have lower income. Western (2002) showed that young men who had served prison sentences experienced slower wage growth as they got older than otherwise similar young men because they were less likely to have obtained the steady career jobs that usually produce earnings growth.

Unfortunately, many studies of the effects of an official criminal record on employment fail to distinguish the effects of the official label on employment from the effects of criminal behavior itself. Using illegal drugs, spending time on criminal work, and staying out late committing crimes can make it harder to hold a job, whether or not one is caught and punished. Further, the rewards of crime may discourage an offender from trying to get lawful employment in the first place. Thus, criminal behavior can reduce the prospects for employment, regardless of whether the criminal is caught and punished. Bushway (1998), however, separately assessed the effects of criminal activity and the effects of an official arrest record and found that the official record had its own detrimental effect on job stability, above and beyond that of criminal activity. Likewise, Bernburg and Krohn (2003) controlled for the self-reported delinquency of their subjects and still found detrimental effects of contact with police or juvenile authorities on employment in early adulthood and on whether the youth graduated from high school.

Do these detrimental effects of an official label on employment and education increase deviance, and thus mediate the effect of being labeled on subsequent offending? Bernberg and Krohn (2003) directly addressed this question, finding that contact with the police and the juvenile justice system were positively associated with criminal behavior in early adulthood, and that this relationship was partly or entirely mediated by employment and educational attainment. That is, official labeling, independent of the criminal behavior that gave rise to the label, appears to increase later offending at least partly because it impairs the labeled person's life chances, by reducing opportunities for lawful employment and educational attainment.

Notwithstanding the reluctance of many employers to hire ex-offenders, it is not clear that imprisonment always reduces an offender's chances for later employment. Although some scholars have found that imprisonment reduces the inmate's chances for employment, others have found no effect and some even find that ex-inmates experience employment gains relative to their (usually abysmal) pre-incarceration levels (Loeffler 2013, 138). The diversity of these findings begs for an explanation. It is possible that, for many offenders, there is a "basement effect" operating. Once one's employment prospects reach extremely low levels, especially in a highly competitive labor market, there may be little room for them to be significantly worsened by the widespread bias against ex-prisoners among employers. Further, many inmates participate in job training or work release programs that can, if well-designed, make participants better qualified to get related jobs after release (Gaes, Flanagan, Motiuk, and Stewart 1999, 404–408).

It is also possible that the main detrimental effect of incarceration is to discourage initial entry into the labor force in the first place, rather than to cause post-release joblessness among those who had previously experienced significant lawful work. Recent research suggests that it is being out of the labor force that is related to serious offending, not being officially unemployed or underemployed (Kleck and Jackson 2016). Young offenders who enter prison with no significant record of prior lawful employment may have their future prospects for initial entry into the labor force reduced by imprisonment, whereas older felons may be immune to any further degradation of their already minimal employment prospects. Our Table 11.3 review indicated that crime-increasing effects of sanctioning are more likely among juveniles and first offenders than among adults and more experienced offenders. This pattern may be partially explained by the fact that juvenile offenders are far more likely than adult offenders to have never entered the labor force, so the handicap of an official record of incarceration or other punishment experience may discourage younger offenders from making their initial entry into the work force. Their prison record would make it harder to secure a legal job, while having little effect on older offenders who have already experienced prison and thus already suffered all the harms to their employment prospects that imprisonment can produce.

### The Effect of Imprisonment on Marriage

Many have argued that being married reduces criminal behavior (Sampson and Laub 1993), though others are skeptical (Skardhamar, Savolainen, Aase, and Lyngstad 2015). Incarceration may affect marriage for a number of reasons. It physically separates those who are already married, weakens emotional bonds, brings shame and stigma to both spouses, and thereby makes divorce more likely. Among those not married, it obviously separates them from the community, limiting opportunities to form relationships. Further, it reduces employment prospects, income, and social respectability, making them less attractive marriage prospects. Huebner (2007) found that imprisonment substantially reduces the probability that the imprisoned person will subsequently be married.

Further, divorce is far more common among incarcerated persons. Lynch and Sabol (2004) found that 66 percent of ever-married prisoners were currently divorced, compared to 17 percent among nonimprisoned adults. In sum, incarceration may increase the criminal behavior of those incarcerated partly by reducing their likelihood of getting married and by increasing the chances of their marriage ending in divorce.

### Defiance Effects

There have been few direct empirical tests of defiance theory, and findings of partial or indirect tests have been mixed. A study by Bouffard and Piquero (2010) was probably the strongest direct test. The researchers found that youths with strong social bonds reacted to police contacts with less subsequent offending, supporting defiance theory. On the other hand, they did not find any effect of perceived fairness of the sanction or denial of shame on reoffending, contradicting defiance theory. Another strong but more narrowly focused study of bullying (Ttofi and Farrington 2008) obtained more consistently supportive findings—(a) youth who perceived sanctions as unfair were more defiant and more likely to repeat their bullying, (b) unacknowledged shame increased defiance and subsequent bullying of siblings, though not peers in school, and (c) those who were more strongly bonded to their mothers were more likely to view sanctions as fair and less likely to experience unacknowledged shame, which tended to reduce later bullying.

On the other hand, a study of youth who participated in restorative justice conferences in Australia and New Zealand found no significant association between a 3-item index of defiance indicators and reoffending, once controls for the restorative character of the conferences and other variables were introduced (Hipple, Gruenewald, and McGarrell 2014). The index of procedural fairness was also unrelated to recidivism, though youths whose conferences had a more restorative character were less likely to recidivate. Likewise, Slocum, Wiley, and Esbensen (2016) found that the perception of "procedural injustice does not have a direct effect on delinquency, nor does it mediate the effect of [police] contact on delinquency" (19).

Considerable evidence nevertheless supports the proposition that punishments perceived as unjust by the punished person increase the likelihood of subsequent offending (see Tyler 1990) and are less likely to deter subsequent offending (e.g., Paternoster, Bachman, Brame, and Sherman 1997; Augustyn and Ward 2015; see also studies reviewed by Bouffard and Piquero 2010, 230–231).

Bouffard and Piquero (2010) found support for the fairness and bonding elements of defiance theory but not for acceptance of shame. Those who perceived their punishment as fair were less likely to reoffend, and those who perceived it as unfair but were more strongly bonded to their communities were less likely to reoffend than those who perceived their sanctions as unfair and were poorly bonded. Acknowledgment of shame was only indirectly measured, and bonding was somewhat dubiously measured as whether the person had graduated from high school, so it is unclear whether this study really directly tested defiance theory. An earlier study by Piquero and

Bouffard (2003) reported an even more indirect test of the theory, finding that "confrontational and physical actions on the part of police were more likely to produce specific defiance (defined as "refusing to cooperate, cursing at the officer, or physically aggressing against the officer") on the part of suspects. The relevance of this finding to defiance theory depends on the assumption that suspects perceived the confrontational actions of police officers as unfair. Since a perception of unfairness was not directly measured, however, it is unclear whether perceived unfairness of enforcement agents caused defiance.

In sum, there are few strong direct tests of defiance theory, and those few have yielded very mixed results. Thus, the very limited extant research does not allow us to say anything with confidence about the theory's merits. Defiance theory nevertheless has interesting implications for criminal justice policy and the importance of procedural justice, should it prove to be valid. While many believe that respecting the procedural rights of suspects and defendants "handcuffs" police and prosecutors in battling crime, defiance theory suggests that making sure that police and prosecutors follow the rules of procedural justice could have crime control benefits, because it could make it less likely that punishment would trigger defiance among those punished.

### *The Net Effect of Imprisonment on Criminal Behavior of Those Imprisoned*

It is clear that there are a wealth of plausible mechanisms by which personally experiencing prison could increase the punished person's criminal behavior and considerable empirical evidence supporting some of those mechanisms. Conversely, experiencing this severe penalty can deter subsequent offending by some of those punished. What, then, is the overall *net* effect of imprisonment on those who experience it? This was the subject of a thorough and extensive review by Nagin, Cullen, and Jonson (2009); see also Gendreau, Goggin, and Cullen (1999) for an earlier review arriving at similar conclusions). They reviewed 50 empirical studies that compared the post-punishment criminal behavior of offenders given custodial sentences with that of offenders given noncustodial sentences such as probation and another 20 studies of the effect of sentence length on reoffending. They concluded that, on net, being imprisoned (vs. receiving some nonincarceration punishment) had a crime-increasing effect on those punished. This did not mean there were no special deterrent or other crime-reducing effects of imprisonment, but only that any such effects were outweighed by crime-increasing effects such as labeling or stigmatization effects, loss of educational and employment opportunities, and so forth. Nor does it mean that there are no criminals whose criminal behavior is, on net, reduced. Effects vary across offenders, and some reduce their offending in response to the prison experience, while others increase it. On the whole, however, across all imprisoned criminals, the net effect appears to be predominantly criminogenic. On the other hand, the evidence concerning the effects of sentence length is distinctly mixed and generated by considerably weaker research, so Nagin and his colleagues declined to draw any conclusions on the effects of longer prison sentences.

## Crime-Increasing Effects of Punishment on the Families of Punished Persons

### *Effects of Incarceration on Children of Those Incarcerated*

So far we have addressed only the crime-increasing effects of legal punishment on those directly punished. It is, however, also possible that punishment can increase criminal behavior among those closely connected with the persons punished, especially their children.

It is not self-evident that parental incarceration would harm their children. Criminals are not, on average, the best parents in the world, so one could plausibly argue that children might be better off without the imprisoned parent. Children might benefit from separation from parents who were neglectful at best and abusive at worst. Nevertheless, the empirical record suggests that incarceration of parents increases antisocial behavior among their children.

The most thorough review of this work was done by Murray, Farrington, and Sekol (2012), who evaluated 40 studies of antisocial behavior of inmates' children conducted in seven different countries. They reviewed only studies that used control groups and numerical outcome measures,. Some compared prisoners' children with samples of the general youth population, while others—regarded as less useful by the authors—used control groups composed of youth who were at risk for reasons other than incarceration of their parents. The review yielded 45 distinctive findings pertaining to the association between parental imprisonment and child antisocial behavior. On average, the odds of antisocial behavior among the children of prisoners were roughly 60 percent higher than among other children.

These differences are not necessarily due to parental incarceration per se, since the parental criminality that resulted in imprisonment has its own effects on child misbehavior. For example, the bad example of parental crime could increase child misbehavior through imitation or modeling mechanisms. Likewise, genetic factors that increased a parent's criminal behavior could be passed on to children and have similar effects on their behavior. Some of the studies attempted to control for these confounding factors by either matching children of inmates with otherwise similar children or via statistical controls for the confounders. For example, Murray and Farrington (2005) compared children with a parent who had been imprisoned before the child was born with those whose parent had been imprisoned during the child's lifetime. Both groups should be subject to the same genetic transmission of criminogenic factors, but only the latter group should be subject to the effects of parental incarceration per se. The authors found that there was still a far higher risk of antisocial behavior among children whose parent had been incarcerated during the child's lifetime. The odds of such behavior were more than three times higher for such children, supporting the view that incarceration had detrimental effects independent of any genetic or social deficits that already characterized imprisoned parents before they were incarcerated.

Many different mechanisms could account for the effect of parental incarceration on the antisocial behavior of their children. The imprisonment of parents may be traumatic for children and cause emotional distress. It may make children more aware of their parent's criminality and lead to the child modeling the parent's antisocial behaviors. It deprives the family of a parent's income and produces economic strain that could encourage economic crimes by the children. It can reduce the quality of parental care and supervision of children. Imprisonment of a parent may also be stigmatizing for their children, provoking ridicule from other children. There is little systematic empirical evidence on these mediating mechanisms, though some evidence suggests that parental imprisonment increases antisocial behavior among children by harming the children's educational performance (Murray et al. 2012, 186). On the other hand, the evidence does not support the hypothesis that parental imprisonment increases mental health problems or drug use of their children.

The limited evidence developed to date suggests that incarceration of mothers has stronger detrimental effects than incarceration of fathers, that parental imprisonment for longer periods of time has stronger effects than shorter times of incarceration, and may have more detrimental effects in more punitive social contexts. The greater effect of maternal imprisonment is especially worrisome because rates of imprisonment of women have increased even faster than those of men in recent decades (Murray et al. 2012).

The authors summarized their meta-analysis thusly: "The most rigorous studies showed that parental incarceration is associated with higher risk for children's antisocial behavior" (Murray et al. 2012, 175), while also cautioning that studies conducted so far do not allow strong causal inferences.

### *Effects of Incarceration on Spouses of Those Incarcerated*

Incarceration also appears to contribute to the breakup of marriages, thereby directly affecting the spouses of those imprisoned. Research has consistently found that "incarceration dramatically increases the odds of divorce" (Siennick, Stewart, and Staff 2014, 372). After reviewing the prior research, Siennick and her colleagues concluded that (1) only incarcerations occurring during (versus before) marriage lead to divorce, (2) inmates' marriages continue to be at risk of dissolving even after the inmate is released, (3) the effect is large, with studies reporting a doubling or more of the odds of, divorce among the formerly incarcerated, and (4) the effect increases with incarceration length, with each additional year behind bars increasing the odds of divorce by 32 percent.

The mechanisms that link a spouse's incarceration to divorce have rarely been studied, but the work by Siennick and her colleagues indicates that incarceration reduces marital love, increases relationship violence following the period of incarceration, and increases the odds of extramarital sex, all of which in turn increase the odds of the marriage ending in divorce. This is a harmful effect of incarceration on spouses in and of itself, but it also indirectly affects children of the imprisoned. Since parental divorce increases children's delinquent behavior, parental incarceration therefore increases children's criminal behavior by increasing the odds that their parents' marriage will end following a parent's imprisonment.

## Effects of Mass Incarceration on the Communities of the Punished

Punishment can have ripple effects that extend beyond the families of the imprisoned. High aggregate rates of imprisonment can have indirect crime-increasing effects on the communities from which large numbers of inmates are drawn (Clear 2008). Imprisonment is not randomly or evenly imposed on all segments of the population. It is far more likely for males, nonwhites, young adults, and poor people. For example, it has been estimated that nearly six of ten black males who do not finish high school will go to prison some time during their lives (Harrison and Beck 2006). In some urban neighborhoods, nearly one in five young males are locked up on an average day, and one third of persons age 16–24 will be sent to jail or prison within a given year (Clear 2008, 103). Because these subpopulations are spatially concentrated, incarceration rates are far higher in some places than others. It is these areas of concentrated incarceration where harmful effects on communities are believed to be most pronounced.

Many of the effects of mass incarceration on communities are simply scaled-up consequences of the effects of imprisonment on the individuals imprisoned and their families, aggregated up to the community level. Imprisonment destabilizes the families of inmates, increases the likelihood that marital and stable partner relationships are broken, weakens parent-child bonds, reduces employment prospects, and increases the likelihood of reoffending.

Further, imprisoning many residents of a neighborhood necessarily implies the later release of many ex-inmates into the community. As we showed in Chapter 6, the prison experience, on net, increases an inmate's post-release offending. Thus, the most direct community-level effect of mass incarceration is the release of large numbers of offenders who are even more criminally inclined than they would have been without the prison experience. Therefore, higher rates of incarceration among the residents of a neighborhood directly produce higher aggregate rates of those problems in the community.

Mass incarceration can, however, also produce community-level problems, especially reduced informal control. Greater formal control in the form of imprisonment causes weakened informal social control in the form of damaged familial and community controls (Lynch and Sabol 2004). For example, parents exert control over their own children, but in the aggregate, mass incarceration of a community's population reduces the supply of parent-age adults and thus the number of people who can act as monitors of other people's children (Clear 2008).

High incarceration rates can also impact the economic well-being of a community as a whole, not just the families of the incarcerated. The loss of the imprisoned parent's job income (most inmates were employed before imprisonment—U.S. Bureau of Justice Statistics 1993) produces financial hardship for his or her family and may necessitate the remaining parent securing a job, reducing supervision of the children. But it also reduces the amount of money circulating in the neighborhood as a whole, which hurts local businesses, causing some to fail and others to move out, further reducing employment opportunities (Fagan and Freeman 1999).

Since imprisonment reduces the income of inmates' families, high rates of imprisonment can collectively increase the poverty rate of entire populations. Recent evidence confirms this expectation. Using instrumental variables statistical methods that took account of the possible two-way causal relationship between poverty rates and imprisonment rates, DeFina and Hannon (2013) found that mass incarceration substantially increased the poverty rates of states in 1980–2004. Although there was substantial growth in the U.S. economy as a whole during this period, poverty rates were essentially stagnant, declining by only 0.3 percentage points. The authors estimated that, had the imprisonment rate not increased beyond its 1980 level, the poverty rate would have fallen by 2.8 percentage points. These findings imply that there were millions of additional people living in poverty as a result of mass incarceration.

As previously noted, going to prison reduces an inmate's likelihood of getting married and increases the likelihood of divorce once married. In the aggregate, high rates of incarceration in a given community reduce the number of marriageable men, since women do not find ex-inmates attractive candidates for marriage. Ex-prisoners are more likely to cohabit with women rather than marrying them, leading to less stable relationships between partners and less stable environments for their children. For their part, single mothers are more reluctant to marry the fathers of their children if the men are ex-prisoners who are both unlikely to find a good job and likely to return to crime (Uggen, Wakefield, and Western 2005). This increases the sexual competition among women for intimate partners who can serve as good parents and breadwinners for their children. This in turn can make women more willing to initiate relationships with men who are less suitable prospects to take on the parent role and reluctant to end relationships with men who prove neglectful or abusive as parents. High rates of incarceration could lead to higher rates of sexually transmitted diseases as a result of this intensified sexual competition. Pregnancy at early ages is another result of the competition for limited numbers of attractive male partners, so increased incarceration rates are followed by increased rates of childbirth by teenage mothers (Thomas and Torrone 2006). The children of teenage mothers in turn are more likely to have lower wages, more unemployment, greater dependence on welfare, and ultimately more involvement in crime.

Mass incarceration can also impair the effectiveness of the criminal justice system in controlling crime. Excessive levels of imprisonment in a community can encourage the belief that the criminal justice system is unfair, cruel, and (especially in minority neighborhoods) discriminatory. Large numbers of released inmates contribute to the spread of this perception. As a result, many residents—not just released inmates—attribute less legitimacy to police and the courts, which weakens the ability of those institutions to operate effectively (Tyler and Fagan 2008).

The research reviewed in Chapter 10 indicated that increases in prison populations contribute to crime reductions via incapacitative effects, but that the crime-reducing impact of a given increase declines as the size of the prison population grows due to a familiar diminishing returns effect—once the most active criminals, among those that can be arrested and convicted, are incarcerated, further increases in prison numbers will have to be drawn from the pool of less active offenders, whose incarceration will yield fewer incapacitative benefits. In light of the detrimental community-level effects of mass incarceration, this raises the possibility that the rate of incarceration in many high-crime communities has reached such a high level that the crime-increasing effects of mass incarceration equal or exceed the (declining) incapacitative benefits of imprisoning some of the community's resident criminals. Supporting this possibility, a study of Florida counties in the 1980–2000 period found that increases in prison population had failed to yield net crime-reducing effects (Kovandzic and Vieraitis 2006).

This is also another explanation of the declining impact of increases in the number of imprisoned criminals, especially in high-incarceration neighborhoods. Part of the deterrent effect of the threat of legal punishment is not due to the painful character of the punishment itself but rather to the way people around the sanctioned individual react to the punishment. If people can realistically anticipate that associates who matter to them will react with disgust to news of their punishment and may break off their relationships with them, this will increase the deterrent power of the threat of punishment (Chapter 2). If, however, a neighborhood experiences so high an aggregate level of imprisonment and other legal punishments that these experiences are commonplace, this can reduce the shamefulness of imprisonment and its stigmatizing power, thereby reducing the deterrent effect of a given "unit" of punishment (Hirschfield 2008). There may now be so many neighborhoods in urban America where this phenomenon has occurred that it is having a significant impact on the aggregate effect of larger prison populations.

Thus, Hirschfield (2008) argued that the wholesale criminalization of urban black youth has diluted the stigmatizing power of criminal labels applied to individuals. His conversations with black youth lead him to conclude that where arrests are common, an arrested youth's peers whose opinions matter to the youth are likely to have themselves been arrested, and thus sympathetic to the arrestee's experience. This phenomenon can reduce the deterrent effect of the threat of punishment because the interpersonal costs of punishment are reduced.

## Diversion of Resources From Other Crime-Reducing Efforts

Resources are finite, and more devoted to any one effort to reduce crime (or any other social problem) implies a smaller pool of resources available for other efforts. Punishment activities therefore entail opportunity costs in the form of lost opportunities to reduce crime using nonpunitive strategies such as offender treatment or poverty reduction. Likewise, punitive activities of one type limit investment in punitive actions of other, possibly more effective, types. For example, Benson, Iljoongm, Rasmussen, and Zuehlke (1992) showed that drug enforcement efforts reduce resources for enforcing other laws, causing increases in other types of crime, especially property crimes. This is an instance of one kind of punitive activity taking away resources from other punitive activities. More broadly, punitive efforts of all types considered collectively can take resources away from nonpunitive alternatives, such as offender rehabilitation, drug treatment, jobs programs, welfare spending, education and job training, parental training, child care, after-school recreational programs, and almost anything else that could conceivably reduce crime. This does not mean there will be dollar-for-dollar shifts away from one activity to another. Political realities insure that just because policymakers are willing to reduce spending on punitive crime control does not mean that they would be willing to increase spending, in amounts equal to the reduction, on other initiatives. It just means that when more resources are spent on punitive efforts there are fewer available for nonpunitive efforts, and thus less *potential* for investment of resources in those alternatives.

The diversion is harmful, of course, only to the extent that the alternative efforts from which resources were diverted actually would have been effective in reducing crime. The effectiveness of many nonpunitive alternative crime control strategies is documented in Chapter 12. It suffices here to state that many nonpunitive efforts have proven effective.

## Conclusions

There are many plausible theoretical reasons why punishment could increase the criminal behavior of those punished, members of their families, and the communities in which legal punishment is widespread but only limited empirical evidence bearing on any one of the possible mechanisms by which these effects operate.

A good deal of evidence indicates that incarceration, on average, increases offending of those incarcerated. While it may reduce the subsequent offending of some inmates, it apparently increases the offending of more inmates. Incarceration reduces the inmate's chances for marriage, increases divorce among those already married, impairs subsequent employment prospects, and reduces income. A prison term may also harden the inmate's pro-criminal attitudes or sharpen his criminal skills, but there is little systematic evidence bearing on these issues.

There is far less evidence on the effects of lesser punishments such as probation, fines, or community service. Arrest or contact with the police is associated with increased later criminal behavior, even when researchers control for prior criminal behavior. There is little evidence that this is due to the offender's experiencing an identity transformation. It is more likely that it is due to labeling reducing educational attainment, impairing prospects for employment, and increasing association with pro-criminal others. Punishment is most likely to increase the offending of younger, less experienced offenders and is more likely to have this effect the first time it is imposed on a given offender.

Incarceration also increases criminal behavior among the children of inmates, though there is no consensus as to how this effect operates. Limited evidence suggests that incarceration of mothers has stronger detrimental effects on children than incarceration of fathers and that parental imprisonment for longer periods of time has stronger effects than shorter times of incarceration.

Research has just begun on the impact of mass incarceration on the communities contributing large numbers of inmates, but scattered evidence accumulated so far suggests that it contributes to widespread poverty and weakens informal social controls and the social relationships needed to control the behavior of residents. It reduces the supply of marriageable males and may increase sexual competition among women for that constricted supply, which can contribute to the spread of sexually transmitted diseases.

As always, more research would be helpful, but it is safe to say that legal punishment of crime has a variety of serious harmful effects. Punishment cannot be viewed as nothing more than a source of crime control or an expression of the community's moral values. The harmful effects must be acknowledged and taken into account when considering the relative merits of punitive crime control compared with alternative strategies.

## References

Andenaes, Johannes. 1966. The general preventive effects of punishment. *University of Pennsylvania Law Review* 114:949–983.

Augustyn, Mear Bears, and Jeffrey T. Ward. 2015. Exploring the sanction-crime relationship through a lens of procedural justice. *Journal of Criminal Justice* 43:470–479.

Babst, Dean, and John Mannering. 1965. Probation versus imprisonment for similar types of offenders—a comparison by subsequent violations. *Journal of Research in Crime and Delinquency* 2:60–71.

Barrick, Kelle. 2014. A review of prior tests of labeling theory. *Advances in Criminological Theory* 18:89–112.

Becker, Howard. 1963. *Outsiders: Studies in the Sociology of Deviance.* New York: The Free Press.

Benson, Bruce, Kim Iljoongm, David Rasmussen, and Thomas Zuehlke. 1992. Is property crime caused by drug use or by drug enforcement policy? *Applied Economics* 24:679–692.

Bentham, Jeremy. 1988[1789]. *The Principles of Morals and Legislation.* Buffalo, NY: Prometheus Books.

Bernburg, John, and Marvin Krohn. 2003. Labeling, life chances, and adult crime: The direct and indirect effects of official intervention in adolescence on crime in early adulthood. *Criminology* 41:1287–1317.

Bernburg, Jon, Marvin Krohn, and Craig Rivera. 2006. Official labeling, criminal embeddedness, and subsequent delinquency: A longitudinal test of labeling theory. *Journal of Research in Crime and Delinquency* 43:67–88.

Boshier, Roger, and Derek Johnson. 1974. Does conviction affect employment opportunities? *British Journal of Criminology* 14:264–268.

Bouffard, Leana Allen, and Nicole Leeper Piquero. 2010. Defiance theory and life course explanations of persistent offending. *Crime & Delinquency* 56:227–252.

Braithwaite, John. 1989. *Crime, Shame, and Reintegration.* Cambridge: Cambridge University Press.

Buikhisen, Wouter, and Fokke Dijksterhuis. 1971. Delinquency and stigmatization. *British Journal of Criminology* 11:185–187.

Bushway, Shawn. 1998. The impact of an arrest on the job stability of young white American men. *Journal of Research in Crime and Delinquency* 35:454–479.

Chiricos, Theodore, Kelle Barrick, William Bales, and Stephanie Bontrager. 2007. The labeling of convicted felons and its consequences for recidivism. *Criminology* 45:547–581.

Clear, Todd R. 2008. The effects of high imprisonment rates on communities. *Crime and Justice* 37:97–132.

Clemmer, Donald. 1940. *The Prison Community.* New York: Holt, Rinehart & Winston.

Davies, Scott, and Julian Tanner. 2003. The long arm of the law: Effects of labeling on employment. *Sociological Quarterly* 44:385–404.

Defina, Robert, and Lance Hannon. 2013. The impact of mass incarceration on poverty. *Crime & Delinquency* 59:562–586.

Dejong, Christina. 1997. Survival analysis and specific deterrence: Integrating theoretical and empirical models of recidivism. *Criminology* 35:561–575.

De Li, Spencer. 1999. Legal sanctions and youths' status achievement: A longitudinal study. *Justice Quarterly* 16:377–401.

Fagan, Jeffrey, and Richard B. Freeman. 1999. Crime and work. *Crime and Justice* 25:225–290.

Freeman, Richard. 1991. *Crime and Employment of Disadvantaged Youth.* Cambridge, MA: Harvard University.

Gaes, Gerald G., Timothy J. Flanagan, Lawrence L. Motiuk, and Lynn Stewart. 1999. Adult correctional treatment. *Crime and Justice* 26:361–426.

Gendreau, Paul, Claire Goggin, and Francis Cullen. 1999. *The Effects of Prison Sentences on Recidivism.* Ottawa: Solicitor General of Canada.

Hannon, Lance. 2003. Poverty, delinquency, and educational attainment: Cumulative disadvantage or disadvantage saturation? *Sociological Inquiry* 73:575–594.

Harrison, Paige M., and Allen J. Beck. 2006. *Prison and Jail Inmates at Midyear 2005.* Bureau of Justice Statistics. Washington, DC: U.S. Government Printing Office.

Hipple, Natalie Kroovand, Jeff Gruenewald, and Edmund F. McGarrell. 2014. Restorativeness, procedural justice, and defiance as predictors of reoffending of participants in family group conferences. *Crime & Delinquency* 60:1131–1157.

Hirschfield, Paul. 2008. The declining significance of delinquent labels in disadvantaged urban communities. *Sociological Forum* 23:575–601.

Huebner, Beth M. 2007. Racial and ethnic differences in the likelihood of marriage: The effect of incarceration. *Justice Quarterly* 24:156–183.

Kleck, Gary, and Dylan Jackson. 2016. Adult unemployment and serious property crime: A national case-control study. *Journal of Quantitative Criminology* 32:489–513.

Kovandzic, Tomislav V., and Lynne M. Vieraitis. 2006. The effect of county-level prison population growth on crime rates. *Criminology & Public Policy* 5:213–244.

Kurlychek, Megan, Robert Brame, and Shawn Bushway. 2007. Enduring risk? Old criminal records and predictions of future criminal involvement. *Crime & Delinquency* 53:64–83.

Lemert, Edwin. 1951. *Social Pathology.* New York: McGraw-Hill.

Letkemann, Peter. 1973. *Crime as Work.* Englewood Cliffs, NJ: Prentice-Hall.

Loeffler, Charles. 2013. Does imprisonment alter the life course? Evidence on crime and employment from a natural experiment. *Criminology* 51:137–166.

Lopes, Giza, Marvin D. Krohn, Alan J. Lizotte, Nicole M. Schmidt, Bob Edward Vasquez, and Jon Gunnar Bernburg. 2012. Labeling and cumulative disadvantage: The impact of formal police intervention on life chances and crime during emerging adulthood. *Crime & Delinquency* 58:456–488.

Lynch, James P., and William J. Sabol. 2004. Assessing the effects of mass incarceration on informal social control in communities. *Criminology & Public Policy* 3:267–293.

Matza, David, and Gresham Sykes. 1957. Techniques of neutralization: A theory of delinquency. *American Sociological Review* 22:664–670.

Morselli, Carlo, Pierre Tremblay, and Bill McCarthy. 2006. Mentors and criminal achievement. *Criminology* 44:17–43.

Murray, Joseph, and David Farrington. 2005. Parental imprisonment: Effects on boys' antisocial behaviour and delinquency through the life-course. *Journal of Child Psychology and Psychiatry* 46:1269–1278.

Murray, Joseph, David Farrington, and Ivana Sekol. 2012. Children's antisocial behavior, mental health, drug use, and educational performance after parental incarceration: A systematic review and meta-analysis. *Psychological Bulletin* 138:175–210.

Nagin, Daniel, Francis Cullen, and Cheryl Jonson. 2009. Imprisonment and reoffending. *Crime and Justice* 38:115–200.

Pager, Devah. 2003. The mark of a criminal record. *American Journal of Sociology* 108:937–975.

Pager, Devah, Bart Bonikowski, and Bruce Western. 2009. Discrimination in a low-wage labor market: A field experiment. *American Sociological Review* 74:777–799.

Pager, Devah, and Lincoln Quillian. 2005. Walking the talk? What employers say vs. what they do. *American Sociological Review* 70:355–380.

Paternoster, Raymond, Ronet Bachman, Robert Brame, and Lawrence Sherman. 1997. Do fair procedures matter? The effect of procedural justice on spouse assault. *Law and Society Review* 31:163–204.

Paternoster, Raymond, and Leeann Iovanni. 1986. The deterrent effect of perceived severity: A reexamination. *Social Forces* 64:751–777.

Paternoster, Raymond, and Leeann Iovanni. 1989. Labeling perspective and delinquency: An elaboration of the theory and an assessment of the evidence. *Justice Quarterly* 6:359–394.

Piquero, Nicole Leeper, and Leana Allen Bouffard. 2003. A preliminary and partial test of specific defiance. *Journal of Crime and Justice* 26:1–21.

Sampson, Robert, and John Laub. 1993. *Crime in the Making.* Cambridge, MA: Harvard University Press.

Scheff, Thomas J., and Suzanne M. Retzinger. 1991. *Emotions and Violence: Shame and Rage in Destructive Conflicts.* Lexington, MA: Lexington Books.

Schwartz, Richard, and Jerome Skolnick. 1962. Two studies of legal stigma. In *The Other Side*, ed. Howard Becker, 103–117. New York: The Free Press.

Sherman, Lawrence W. 1993. Defiance, deterrence, and irrelevance: A theory of the criminal sanction. *Journal of Research in Crime and Delinquency* 30:445–473.

Siennick, Sonja, Eric Stewart, and Jeremy Staff. 2014. Explaining the association between incarceration and divorce. *Criminology* 52:371–398.

Skardhamar, Torbjorn, Jukka Savolainen, Kjersti Aase, and Torkild Lyngstad. 2015. Does marriage reduce crime? *Crime and Justice* 44:385–446.

Slocum, Lee Ann, Stephanie Ann Wiley, and Finn-Aage Esbensen. 2016. The importance of being satisfied: A longitudinal exploration of police contact, procedural injustice, and subsequent delinquency. *Criminal Justice and Behavior* 43:7–26.

Stoll, Michael, and Shawn Bushway. 2008. The effect of criminal background checks on hiring ex-offenders. *Criminology & Public Policy* 7:367–370.

Sweeten, Gary. 2006. Who will graduate? Disruption of high school education by arrest and court involvement. *Justice Quarterly* 23:462–480.

Sykes, Gresham. 1958. *The Society of Captives.* Princeton: Princeton University Press.

Tannenbaum, Frank. 1938[1994]. The dramatization of evil. In *Classics of Criminology*, ed. Joseph E. Jacoby, 259–261. Prospect Heights, IL: Waveland Press, Inc.

Tanner, Julian, Scott Davies, and Bill O'Grady. 1999. Whatever happened to yesterday's rebels? Longitudinal effects of youth delinquency on education and employment. *Social Problems* 46:250–274.

Thomas, Charles, and Donna Bishop. 1984. The effect of formal and informal sanctions on delinquency: A longitudinal comparison of labeling and deterrence theories. *The Journal of Criminal Law and Criminology* 75:1222–1245.

Thomas, James, and Elizabeth Torrone. 2006. Incarceration as forced migration: Effects on selected community health outcomes. *American Journal of Public Health* 96:1–5.

Tittle, Charles. 1980. Labelling and crime: An empirical evaluation. In *The Labelling of Deviance* (2nd ed.), ed. Walter Gove, 241–263. Beverly Hills: Sage.

Ttofi, Maria M., and David Farrington. 2008. Bullying: Short-term and long-term effects, and the importance of defiance theory in explanation and prevention. *Victims & Offenders* 3:289–312.

Tyler, Tom R. 1990. *Why People Obey the Law.* New Haven, CT: Yale University Press.

Tyler, Tom R., and Jeffrey Fagan. 2008. Legitimacy and cooperation: Why do people help the police fight crime in their community? *Ohio State Journal of Criminal Law* 6:231–251.

Uggen, Christopher, Mike Vuolo, Sarah Lageson, Ebony Ruhland, and Hilary Whitham. 2014. The edge of stigma: An experimental audit of the effects of low-level criminal records on employment. *Criminology* 52:627–654.

Uggen, Christopher, Sarah Wakefield, and Bruce Western. 2005. Work and family perspectives on reentry. In *Prisoner Reentry and Crime in America*, eds. Jeremy Travis and Christy Visher. New York: Cambridge University Press.

U.S. Bureau of Justice Statistics. 1993. *Survey of State Prison Inmates 1991.* Washington, DC: U.S. Government Printing Office.

Western, Bruce. 2002. The impact of incarceration on wage mobility and inequality. *American Sociological Review* 67:526–546.

Western, Bruce, and Katherine Beckett. 1999. How unregulated is the U.S. labor market? The penal system as a labor market institution. *American Journal of Sociology* 104:1030–1060.

# 12

# CONCLUSIONS

Punitive crime control in America has reached a dead end. The policy was grounded in an unrealistic theory of human decision-making and largely supported by fatally flawed empirical evidence. What little potential there was for controlling crime through the approach, primarily via the incapacitating effects of imprisonment, has largely been exhausted. The additional returns that can be reasonably expected from further expansion of the policy of mass incarceration have diminished to the point where they can no longer be justified on the basis of cost-effectiveness. The evidence for deterrent effects of legal punishment has been found to be unreliable, and the best available evidence indicates that changes in punishment levels do not affect the deterrent impact of punishment on crime rates.

## Premature Good News

In light of the mass of contrary evidence, why do so many serious scholars persist in believing that deterrence through legal punishment remains a viable approach for controlling crime? Daniel Nagin is one of the most distinguished and prolific contributors to the study of the deterrent effects of criminal punishment. In one of the most frequently cited reviews of the deterrence literature (Nagin 1998), he provided a prime example of an unduly favorable conclusion about the deterrent effects of legal punishment: "I am confident in asserting that our legal enforcement apparatus exerts a substantial deterrent effect" (36). The conclusion was presumably based on Nagin's review of the research literature, but it was nevertheless a *non sequitur*, one that he could reach only if he ignored his own caveats that (a) this research had not yet established that activities of the CJS actually influence perceptions of punishment risk and that (b), without such a connection, there could be no deterrence. Subsequent research indicated that his concern about this problem was well-justified—punishment generated by CJS activity does not routinely increase perceptions of punishment risk either within the general population or among potential offenders (Chapter 9; Kleck, Sever, Li, and Gertz 2005) or among those punished (Chapter 6).

Nagin's conclusion likewise ignored or discounted another grave problem that he had forthrightly acknowledged—that virtually all the macro-level studies that supposedly found evidence of deterrent effects failed to distinguish the incapacitative effects of incarcerating criminals from the deterrent effects of punishment. A large number of studies that arrive at the same conclusion, but that also all share the exact same potentially fatal flaw, do not constitute a sound basis for such a sweeping and definitive conclusion, one that was especially unfortunate because of the prestigious and widely read place in which it was published, *Crime and Justice.* Nagin had in effect conceded that (a) the individual-level research might be largely irrelevant to the issue of deterrence-based crime control, since it could turn out (as indeed it did) that actual punishment levels do not routinely impact individual perceptions of legal risk and that (b) the macro-level research was almost entirely inconclusive regarding deterrent effects because it did not distinguish deterrence from incapacitative effects.

In the context of his accurate assessments of the critical weaknesses of the pro-deterrence literature, it is hard to see how Nagin arrived at his remarkably favorable conclusion or what justified his assertion that he was "confident" about it. Later research did little to provide any *ex post facto* justification for the conclusion. Nagin, however, was not alone in drawing unduly optimistic conclusions about the empirical support for deterrence-based crime control. Over the years, the authors of many other influential and widely cited reviews have been similarly willing to draw similarly positive conclusions from the research literature (Cook 1980; Levitt 2002; Shepherd 2004; Van den Haag 1975; Wilson and Herrnstein 1985, Ch. 15; Wright 1994).

## Why Did Scholars Reach This Conclusion?

Why have some scholars come to the conclusion that more punishment produces less crime, and why were they so confident of the conclusion? To be sure, there is an enormous volume of evidence bearing on the question, as the length of our reference lists attests. Further, many of the relevant studies did draw conclusions favoring this assessment. The strongly pro-punishment conclusion was certainly not drawn without empirical support, yet it is still very likely to be generally wrong. How, then, did this huge body of evidence lead to this erroneous conclusion?

One answer is fairly straightforward. Each major category of research was afflicted by crucial methodological errors that systematically biased findings in favor of pro-punishment conclusions. We have covered them in detail previously, especially in Chapter 4, so they need only be summarized here.

### *Macro-Level Research*

This category of research was the one that was most favorable to a pro-deterrence conclusion, but even this body of work supported only the deterrent impact of punishment certainty. That is, it often seemed to indicate that making legal punishment more certain reduced crime rates. Research on severity of punishment mostly failed to find any impact on crime rates, while evidence bearing on swiftness of punishment is virtually nonexistent and certainly cannot sustain any firm inferences. The seemingly supportive results for certainty, however, were largely or entirely artifacts of various combinations of methodological flaws—sometimes one flaw, sometimes another, sometimes different combinations of the flaws. First, many researchers failed to properly model two-way causal effects. In the less sophisticated studies the negative associations found at least partly reflected the effect of crime rates on certainty of punishment, rather than the reverse—higher crime rates overwhelm the ability of the CJS to maintain levels of punishment certainty. Secondly, the use of ratio variables to measure both certainty of punishment (e.g., arrests/crimes) and crime rates (crimes/population), in combination with considerable error in measuring crimes, contributed to artifactual negative associations between macro-level measures of certainty of punishment and crime rates. Third, studies apparently supporting the deterrent effect of certainty failed to measure and control for factors that influence crime rates but that are also correlated with levels of legal punishment, i.e. confounding variables. The most important of these may be the level of public intolerance for crime, which reduces crime through informal social mechanisms but also increases public support for increased punishment of criminals. To our knowledge, this variable has never been controlled in even a single published macro-level study of deterrence. Fourth, most seemingly supportive studies failed to distinguish supposed deterrent effects of more certain punishment from the undoubted incapacitative effects of locking up criminals. Even if higher levels of punishment certainty have no deterrent effect on crime, they do contribute to larger numbers of criminals being incarcerated, and it is more likely the latter that actually reduces crime through purely incapacitative mechanisms, i.e. the simple fact that incarcerated criminals cannot commit crimes against the general public (Chapter 10). Finally, these studies assumed that actual levels of punishment could serve as adequate proxies for average perceived risks of punishment among population—an assumption that is clearly untenable (Chapter 9).

Since every single macro-level study that seemingly supports pro-punishment conclusions is afflicted by at least one, and usually most, of these flaws, it means that the entire body of macro-level research, enormous though it may be, amounts to a house of straw, incapable of sustaining the conclusion that more punishment produces less crime. The underlying research was worse than merely weak—it tended to systematically distort findings in favor of negative associations between certainty of punishment and crime, thereby creating a misleading appearance of support for the deterrence thesis.

## *Individual-Level Research*

Due to the unreliable nature of the macro-level evidence, more sophisticated supporters of the deterrence doctrine now lean more heavily on individual-level research. This body of work, largely based on survey research on self-reported criminal and delinquent acts, has the powerful merit of directly measuring perceptions of legal risk. Most individual-level research, however, fails to support deterrent effects (Chapters 5 and 6).

It certainly finds little support for a deterrent effect of perceived severity of punishment on criminal behavior. As with macro-level research, support for the pro-punishment position in the individual-level deterrence literature is largely confined to findings pertaining to perceived certainty of punishment. Even this body of findings, however, is mostly unsupportive of deterrence. Nevertheless, a substantial minority of these studies, often conducted by highly skilled scholars, appears to support it. Why? The answer is basically the same as it was with regard to macro-level research—methodological flaws in the research systematically distorted findings in favor of the conclusion that perceiving higher legal risks of criminal behavior causes a lower likelihood of committing crime. These are the more critical flaws:

1. *The failure to establish the correct causal order.* Doing more crime causes people to shift their perceived risks of punishment downward. That is, the more that people commit crime and escape without punishment, the lower they perceive the certainty of punishment to be. Most perceptual studies ignored the causal order issue or addressed it with inappropriate or ineffective strategies (Chapter 5). Even the strongest of the approaches, using panel designs, were not as capable of disentangling the causal order problem as their users sometimes implied, so the negative associations sometimes observed may still have reflected the impact of criminal behavior on perceived certainty of punishment, rather than the reverse (Chapter 5).
2. *Invalid measures of criminal or delinquent behavior.* Self-report methods require some minimal level of honesty among survey respondents, in order for the methods to be able to distinguish more criminal persons from less criminal persons. Notwithstanding some optimistic assessments of the validity of the method, the methodologically strongest assessments indicate that false denials of criminal conduct are both common and nonrandom. If people who are most fearful of legal punishment are, for that very reason, likely to conceal more of their criminal acts from survey researchers, this can create a negative association between perceived legal risk

and *self-reported* crimes that is misleading and entirely an artifact of error in measuring criminal behavior with the self-report methodology.

3. *Artificial Harmonizing of Risk Perceptions and Forecasts of Future Offending.* In the large body of one-shot survey studies using vignette methods or asking respondents to forecast their future offending, it is possible for participants to artificially "harmonize" their offending and risk perception responses so that they are consistent with a self-image as a rational person. Those presented with vignettes with a high risk of punishment may claim they would be unlikely to offend in those circumstances, even if they in fact would commit, or already had committed, crimes in high-risk circumstances, for the sake of maintaining a rational self-image. Those who reported a high perceived certainty of punishment might falsely deny that they would commit crimes in the future because to do so would make them seem like foolish, irrational people. The desire to maintain cognitive consistency and a favorable self-image biases the results of such studies in favor of the deterrence doctrine.
4. *Samples Biased in Favor of Deterrence.* Self-report deterrence studies largely examine middle-class student samples, largely because they are convenient and cheap for college professors to survey. Because middle-class people and college students have more to lose from criminal punishment than lower-class people, the threat of legal punishment is likely to have stronger effects on the former than on the latter because it brings greater costs beyond the pains of the legal punishment itself. Thus, the nature of the samples that are typically studied in individual-level research has led to findings that are more supportive of deterrence than they would have been if samples had been studied that were more diverse and representative of the population as a whole.

Even if we set aside these serious flaws, the entire body of perceptual research may turn out to be largely irrelevant to the policy question of whether more punishment will produce less crime, since increases in actual punishment do not, under ordinary circumstances, produce any sustained and measurable increases in individual perceptions of the risk of punishment (Chapter 9). Thus, even if the minority of individual-level studies that found a negative effect of perceived certainty of punishment on criminal behavior turns out to be valid, it implies nothing about whether raising actual certainty of punishment will reduce crime via increased deterrence.

Once these problems are fully appreciated, it becomes evident that there is no sound basis for even tentatively concluding that higher levels of punishment reduce crime through deterrence. Incapacitative effects are another matter—they clearly exist and are substantial. The policy issue, however, is not whether we will continue to incarcerate criminals. We clearly will continue to do so for purely moral reasons under any foreseeable circumstances, since most Americans think it is right to punish the wicked and to punish more serious crimes with a prison sentence. The real policy-relevant issue is whether *increasing* incarceration rates above current levels will reduce crime, to a degree that would justify the considerable costs of doing so, or whether *decreasing* those rates would produce intolerable increases in crime. The best available evidence suggests that the nation has passed the level of incarceration that was cost-effective at the margin, i.e. the point where the value of crimes prevented by locking up an additional batch of criminals exceeds the costs of incarcerating them. As long as the money saved was invested in even moderately effective alternative crime control efforts, we could afford to reduce the size of the inmate population far below its present level (Chapter 10).

## Salvaging the Deterrence Doctrine as a Guide to Crime Control Policy

Durlauf and Nagin (2011) attempted to salvage the deterrence doctrine as a guide to crime reduction policy by weakly conceding the unsupportive findings regarding punishment severity and that maintenance of current imprisonment rates or further expansion of those rates is not cost effective, but stressed a punitive policy that focuses on certainty of punishment and, especially, certainty of police arrest. This salvage strategy will not do. The evidence on the effects of punishment certainty is *also* largely unsupportive of the deterrence doctrine—just not as badly contrary as the evidence concerning punishment severity. Further, the evidence on the effectiveness of police-based strategies is especially weak because of its near-total absence of any serious effort to distinguish crime reduction from mere crime displacement (Chapter 3).

## Should Scholars Even Draw Policy Conclusions From Research?

One common scholarly response to the enormous body of research on this topic has been to draw a "no conclusion" conclusion and to assert that the evidence is not strong and consistent enough to justify drawing any conclusions regarding public policy. Many scholars who have reviewed various subsets of the research have declined to draw conclusions as to what policies or changes in policy might be justified in light of the findings of research. For example, after reviewing 70 individual-level studies of the impact of incarceration on recidivism, most of which indicated a net crime-*increasing* impact of incarceration on those imprisoned, Nagin, Cullen, and Jonson (2009) concluded that incarceration "appears to have a null or mildly criminogenic effect on future criminal behavior" but nevertheless asserted that "this conclusion is not sufficiently firm to guide policy" (115).

Although Nagin and his colleagues quite accurately described numerous defects in the studies they reviewed, their assertion that the body of research was "not sufficiently firm to guide policy" was nevertheless a *non sequitur*, because the fact that research is flawed does not logically imply that it cannot be used to guide policy. There are three major flaws in the implicit reasoning underlying this and similar assertions found in dozens of other sources. First, the reasoning falsely assumes that there is some specific, identifiable minimum level of research quality, quantity, and consistency that *would* justify drawing policy conclusions but that has not yet been achieved, that the authors can specify that level, and would be able to recognize when and if it was reached at some point in the future. Nagin and his colleagues did not specify such a level, nor is it likely they or anyone else making similar claims could do so. We know of no scholars who have ever been able to articulate a specific level of research quality that is "good enough" to justify policy recommendations, as distinct from merely identifying research that is better than what has been done so far. Research generally gets gradually better over time, but no particular point on the improvement curve can be objectively identified as "the" point where it gets "good enough." Specifying any such point would inevitably be arbitrary and subjective.

Second, the argument ignores the seemingly self-evident point that all human decisions are, and always will be, made on the basis of incomplete and flawed information, for the obvious reason that there is no other kind of information. Thus, the fact that information derived from scholarly research is flawed and incomplete does not give it any especially inferior status in the realm of human knowledge. Rather, it is, in this respect, just like all other information used to guide human decisions. Yet, policy decisions will continue to be made regardless of whether policy-related research is conducted and regardless of whether those who conduct the research derive explicit policy implications from their research findings. No decision-maker ever waits, or could wait, until literally "all the information is in," or until the information has been confirmed as free of all flaws, since such a wait would be eternal. Instead, all policy decisions are made, and will continue to be made, entirely on the basis of flawed and incomplete information.

Third, this line of reasoning fails to compare basing policy on extant research with any alternative ways of deciding on public policy. It is self-evident that policy makers are more likely to understand the policy implications of research if those who carry out and review the research—that is, those who understand the research best—make those policy implications explicit. Thus, scholars who decline to state clear policy recommendations based on research presumably do so because they think the research cannot be relied upon, not because they object to explicit statements of policy implications per se. A refusal to derive policy conclusions from research, based on the mere fact that research is flawed, fails to consider the relative merits of the alternative—devising policy *without* any explicit guidance from scholars that is based on the best available research evidence.

The relevant consideration is not whether research-based recommendations might be wrong. Of course they might, and have been, wrong. Rather, the issue is whether policies guided by the best available scientific evidence are likely to be wrong more often than those not so guided. We are not aware of any evidence or logic that policy choices made without reference to research evidence consistently yield better results than choices influenced by the best available scholarly evidence.

The status quo of policy-making appears to be to make decisions heavily based on the short-term self-interest of the policy makers, influenced by considerations such as which policies are easiest and cheapest to implement (or to continue with, as a matter of inertia), which ones will play well with the mainstream media and thereby put the policy-maker in a more favorable light with their constituents, which are most likely to preserve or increase an administrator's agency's budget, which ones will encounter the least resistance from those who implement the policies, which ones will alienate the fewest special interests or potential campaign contributors, or which will yield the greatest increase in support among such persons. Above all, policy choices are influenced by which options will yield the greatest increase in an elected policy maker's chances for reelection. Worse still, the making of crime control policy may be driven by covert racial animus. We are not aware of any evidence that deciding crime-related public policy based on considerations such as these is consistently more likely to reduce crime or otherwise serve the public interest than decisions informed by the best available scholarly research.

Thus, the mere fact that research is flawed is not an acceptable rationale for concluding that a body of research is too weak to guide policy. Instead, we believe that the proper response to a large body of policy-relevant research evidence is to identify the methodologically strongest studies and, based on this "best available evidence," draw explicit conclusions as to what policy implications follow from the evidence. These conclusions should be expressed in terms understandable to the non-specialist policymaker but accompanied by plainly worded caveats concerning the most important research flaws that might threaten the validity of the conclusions.

Simply refraining from drawing policy implications from research is more of an evasion of responsibility than the exercise of appropriate scientific caution. The danger of refusing to draw policy conclusions from research, based on the justification that the body of research is "not strong enough," is that it can easily be used as a pretext for avoiding conclusions with which the assessor of evidence is personally uncomfortable. A scholar may dislike a conclusion implied by the best available evidence because it supports policies he dislikes, or the rejection of policies he favors, on personal, ideological, or cultural grounds. Or a scholar may want to avoid drawing a conclusion that contradicts conclusions he has drawn in previous publications or that casts doubt on findings from his own prior research. Or the evidence may cast doubt on some fundamental professional, scholarly, or disciplinary principle that the scholar is not willing to abandon.

For example, many social scientists are reluctant to accept the findings of research pointing to powerful effects of genetic traits on criminal behavior because it implies less relative importance of the social environmental factors on which social scientists are trained to focus. Likewise, legal scholars who have devoted their lives to the importance of the law may dislike accepting research-based conclusions indicating that changes in law have little impact on the frequency of criminal behavior. Economists are trained to believe in the precepts of some version of rational choice theory and, in particular, price theory, which asserts that when the cost (or at least the perceived cost) of an activity increases, the frequency of that activity will decline, other things being equal. Economists may therefore be loath to accept the notion that increased punishment of criminal behavior does not reduce crime and may reject evidence in support of such a conclusion because the evidence supposedly "is not strong enough."

An unwarranted reluctance to draw policy conclusions from research casting doubt on punitive crime control strategies effectively tends to favor the punitive status quo. Elliot Currie (2011), responding to an article by Nagin and a colleague, criticized what he called "spurious prudence"—the tendency of some scholars to downplay the sheer mass of the accumulated evidence bearing on a policy-related topic and to refrain from drawing any clear, definite conclusions. Currie concluded that "the logic of spurious prudence works in support of the status quo, and if the status quo is wasteful and destructive, then spurious prudence is implicated" (113). Thus, spurious prudence is not mere scientific conservatism or caution, and it is not politically neutral, regardless of its practitioners' professed intentions. Nagin's unwillingness to explicitly recommend against expansion or maintenance of existing prison populations, or even advise against more severe prison sentences, in the face of sound evidence that supports such recommendations effectively works as a passive endorsement of a highly punitive, destructive, and wasteful status quo. To demand flawless or overwhelmingly strong evidence favoring a change in the status quo before endorsing change is tantamount to taking sides in favor of maintaining things as they are.

It bears repeating that the issue is *not* whether the evidence bearing on the effect of punishment on crime is flawed. It is flawed and always will be. Critics have accurately identified many flaws in the relevant research, and in the future will always be able to do so. Our Chapter 4 was entirely devoted to detailed discussion of the more important methodological flaws afflicting punishment research. Nor is the issue whether flaws should be identified and corresponding cautions be attached to any conclusions that are based on the research—these are obviously responsible practices. Rather, we merely want to emphasize that it is wrong to draw the *non sequitur* conclusion that no policy conclusions or recommendations can be drawn from a body of research merely because it is flawed.

To be sure, there are bodies of research about which most scholars would agree that no policy conclusions are warranted. There is sometimes strong, well-founded agreement among scholars that research is not strong enough to support even the most tentative policy conclusions, e.g., when the body of research is extremely small, is of indisputably and uniformly poor quality, or when the technically better studies yield mutually contradictory findings. Likewise, scholars might legitimately refrain from drawing policy-related conclusions when available research is simply irrelevant to policy because it has not squarely addressed any empirically testable propositions on which policies rely. Under such circumstances, refraining from drawing policy conclusions is relatively defensible. It becomes increasingly indefensible, however, the larger, stronger, more consistent, and more relevant the research gets.

We believe that the wiser practice to follow is that policy conclusions *should* be drawn from research and explicitly stated, as long as scholars drawing the conclusions have clearly described the flaws and limitations of the evidence on which the conclusions were based, and it is recognized that the conclusions could be subject to change as methodologically superior research is developed and yields findings undercutting those conclusions. The body of research on the effects of punishment on crime is huge by any reasonable standard, and most of it squarely addresses policy-relevant issues. A large minority of it is even of good technical quality. Further, once one recognizes key methodological flaws, it becomes evident that there is considerable consistency in findings among the technically stronger studies. In this light, we derive the following tentative conclusions about the effects of punishment on crime and then outline some plausible policy responses to those conclusions.

## Summary of the Book's Findings

Our wide-ranging reviews indicate that the strength of research supporting the effectiveness of punitive crime control policies has been grossly overstated. The soundness of macro-level research has been compromised by its failure to establish that increases in the certainty or severity of punishment actually affect the perceptions of legal risk among prospective offenders. The best available evidence directly bearing on this question indicates there is no such effect (Kleck et al. 2005; Chapter 9). Further, our review of this body of research indicates that researchers, without exception, have failed to establish that it is punishment levels that reduce crime rather than the higher levels of social condemnation of crime and the intensified informal social controls that accompany higher public support for punitive strategies for reducing crime.

The huge body of individual-level deterrence research has directly tackled the link between perceptions of legal risk among prospective offenders and criminal behavior, thereby providing more direct tests of deterrent effects than macro-level research could provide. It brought new ambiguities, however, because most of this research does not establish that perceptions of legal risk affect criminal behavior, rather than the reverse. Most of the research findings in this area can be interpreted as merely indicating that the more people do crime, and escape punishment (as perpetrators usually do), the less they believe that criminal behavior is risky. Further, the few studies that use methods, such as panel designs, that address this causal order issue somewhat more convincingly, indicate that perceived severity of punishment has no measurable impact on criminal behavior, while perceived certainty of punishment has only questionable effects or an impact that is highly contingent on other factors, such as the prospective offender's stake in conformity (Chapter 5).

Even if one sets aside these serious reservations about the perceptual deterrence research, its relevance to policy is doubtful because there is generally no link between changes in punitive policies and perceptions of risk among potential offenders. Raising the actual legal risks of crime does not, on average, increase perceived risks, so there is usually no sound reason for policy-makers to expect any increase in the deterrent effect of punishment to result from increases in actual punishment levels (Chapter 9).

Regarding capital punishment, the best available evidence (all of it macro-level, with no direct measures of perceptions of the risk of execution) on use of the death penalty likewise indicates that it has at most a miniscule short-term effect on homicide, notwithstanding the contrary conclusions of numerous researchers, most of them economists. The bulk of this research probably has little to say about deterrent effects of executions because it studies time units (usually years; in a few studies, months) too large to detect any likely short-term deterrent effects, and, like other macro-level research, makes no effort to separate purported deterrent effects from either the incapacitative effects of imprisoning large numbers of violent people (something that almost invariably characterizes the same places and times that have higher execution rates) or the effects of intensified public condemnation of violent behavior (Chapter 8).

The research that comes closest to measuring perception of execution risk, albeit indirectly, are studies of execution publicity. This body of research as a whole indicates that there is no unique deterrent impact of execution publicity. This is, however, a very small body of research, allowing only weak inferences, and it remains possible that individual executions have some temporary deterrent effect on homicide frequency. It nevertheless is unlikely that use of the death penalty has much aggregate impact on homicide rates, since executions are carried out so rarely—only a few dozen times per year.

Research on the effects of punishment on the individuals punished, and specific deterrence, has been even less supportive of punitive policies than research on general deterrence. The research indicates that any special deterrent effects that the experience of punishment may have are cancelled out by crime-enhancing effects of legal punishment, such as the detrimental effects it has on prospects for lawful employment or the stigmatizing effects of a criminal record. Worse still, the most serious punishment short of death—imprisonment—appears to actually *increase* criminal behavior among inmates once they are released. That is, on net, offenders who serve prison sentences are more likely to reoffend once they are released than otherwise similar offenders sentenced to punishments not involving incarceration (Nagin et al. 2009; Smith, Goggin, and Gendreau 2002; Chapter 6). In sum, incarcerating more people fails to increase general deterrence because the greater risk of incarceration is not communicated to prospective offenders in the general population (Chapter 9), and it *increases* post-prison criminal behavior among those incarcerated more often than it decreases their offending (Chapter 6).

On the other hand, locking up more criminals does increase the collective incapacitative effect of incarceration. This beneficial effect is, however, characterized by diminishing returns—each additional amount of increase in the number of criminals incarcerated produces less increase in aggregate incapacitation effects. This is because the latest additional batch of criminals added to the prison population tends to be, on average, less serious, active offenders than those already in prison, so that locking them up prevents fewer crimes. Further, the U.S. has probably passed the point where the crime preventive benefits of further increases in the size of the prison population are large enough to justify the costs of imprisoning more criminals. Worse still, it is even possible that we have reached the point where further increases in the prison population *increase* crime, due to the criminogenic effects of imprisonment on incarcerated criminals outweighing the diminishing incapacitative effects of imprisoning less serious offenders (Chapter 10).

## A Compact Summary of Some Lessons That Crime and Punishment Research Has to Teach Us

1. People are rational decision makers only to an extremely limited degree (Chapter 3). In any case, this does not imply that higher punishment levels produce more deterrent effect, because this punishment reality has little or no effect on the perceptions of risk among prospective offenders (Chapter 9).
2. People who perceive a higher severity of legal punishment are just as likely to commit crime as are people who believe punishment is less severe. Increasing the perceived severity of punishment has no effect on criminal behavior (Chapter 5).
3. People who perceive a higher certainty of legal punishment may be less likely to commit crime, but even this weak conclusion must be tempered due to the inability to convincingly distinguish the effects of perceived certainty on criminal behavior from the effects of criminal behavior on perceptions of certainty (Chapter 5).
4. The effects of punishment are contingent on the attributes of the people who might be affected. Punishment is better at keeping noncriminals law-abiding than it is at making criminals stop committing crimes. Punishment works best with the people who need its effects least. It deters crime more among those who are only marginally criminal than among those who are seriously criminal (Chapter 2, 6).
5. Even personally experiencing punishment does not consistently increase the punished individual's perceived risk of subsequent punishment. Many criminals, upon being arrested, draw the conclusion that they have exhausted their bad luck and are therefore *less* likely to be caught in future. Most punished criminals do not shift their perceptions of legal risk significantly up or down. This is one reason why punishment is not likely to reduce the punished person's criminal behavior (Chapter 6).
6. Actual levels of legal punishment such as arrest, conviction, or imprisonment rates have, on average, no effect on average perceived risks of punishment among prospective offenders. That is, arresting, convicting, or imprisoning more criminals does not generally increase perceived

certainty, severity, or swiftness of punishment, and thus does not increase the general deterrent effect of legal punishment on crime (Chapter 9).

7. Macro-level research indicates that higher average levels of punishment severity do not reduce crime (Chapter 7).
8. Macro-level research often finds a negative association between average levels of punishment certainty and crime, but the association is partly, and possibly entirely, spurious, due to higher levels of public intolerance for crime. That is, higher public intolerance for crime increases support for increased punishment but has an independent crime-reducing effect of its own. To the extent that the association reflects any kind of causal effect on crime, it is more likely that it reflects the effect of incapacitating large numbers of criminals than a general deterrent effect (Chapter 7).
9. On average, being imprisoned, compared to being sentenced to a nonincarceration sentence, *increases* the likelihood that the incarcerated offender will continue committing crimes after release (Chapter 6).
10. Locking up more people reduces crime via incapacitative mechanisms but is cost-effective only up to a point. The effect of larger prison populations on crime rates is nonlinear, showing diminishing returns. That is, a given increase has less impact on crime the larger the prison population has already become. In the U.S., the prison population has passed the point where further increases in the number of criminals incarcerated can produce enough crime reduction to justify the costs of imprisoning more people (Chapter 10).
11. Capital punishment, in the aggregate, has little impact on murder rates, though it remains possible that individual executions deter some homicides via short-term deterrence. The huge volume of research on death penalty deterrence has little to say on this question because the time units studied are usually too long for small short-term effects to be detectable. There is no evidence that higher actual execution rates cause higher levels of perceived risk of execution. The research that comes closest to measuring perceptions of risk, studies of execution publicity, generally finds no deterrent effect of executions on homicide (Chapter 8).
12. More generally, being officially labeled a criminal increases subsequent criminal behavior, at least partly because it interferes with obtaining a good education and lawful employment. This labeling, or other deviance-amplifying effects, on average, outweighs whatever special deterrent effect the experience of punishment may have on the person punished (Chapter 11).
13. Punishment in the form of imprisonment increases criminal behavior of the inmate's children (Chapter 11).
14. Very high rates of incarceration among the residents of a community *may* increase crime rates in that community (Chapter 11).
15. Punishment as a crime reduction strategy has run out its string—we have squeezed what benefits we can out of it and have come to rely far too much on punishment for crime control. We need to look more seriously at the alternatives and to consider reducing punishment levels back down to more reasonable levels.

## What Can Be Done? How Might Excessive Reliance on Legal Punishment Be Reduced?

### *Amend Sentencing Provisions of Criminal Statutes*

We can start by repealing provisions in criminal statutes authorizing mandatory minimum sentences or their close cousins such as harsh fixed sentences (a single severe penalty is mandated), three-strikes laws, and "Truth in Sentencing" laws requiring that a minimum percentage of imposed sentences be served. All of these laws are putatively mandatory and aimed at making penalties more severe. Mandatory sentencing provisions in criminal law theoretically require judges to impose specific sentences or sentences of a specified minimum severity. In practice, these provisions are often evaded but nevertheless have the effect of increasing the average severity of sentencing. As we have seen (Chapter 7; Doob and Webster 2003), higher average severity levels do not reduce crime. Therefore, it is not surprising that the vast majority of evaluations of the impact of mandatory sentences finds that they do not reduce crime rates (Tonry 2009).

Instead, their effects are almost entirely harmful. They reduce the certainty of punishment (the property of punishment that may actually exert some deterrent effect on some people) by increasing no-prosecution decisions by prosecutors and dismissals by judges due to the desire to avoid imposing excessively severe penalties. They also cause injustice by forcing judges to impose identical sentences for crimes of very different seriousness and making it impossible for them to take proper account of mitigating circumstances. Our concern here is solely with their ineffectiveness in reducing crime, but the full array of harms produced by mandatory penalties is thoroughly covered by Tonry (2009), who concludes that "they do little good and much harm" (106).

Worse still, the best available evidence from three technically sophisticated studies indicates that three-strikes laws substantially *increase* the murder rate, by about 16 to 29 percent in the long term (Kovandzic, Sloan, and Vieraitis 2002, 2004; Marvell and Moody 2001). One possible explanation for this is that the laws make it sensible for offenders with two convictions to kill their victims so as to avoid the extremely severe punishment that would result from conviction for a third-strike offense. It is always advantageous for offenders to kill their victims to prevent them from identifying the offender to the police, but this advantage is normally outweighed by the far greater penalty that would be imposed for the killing if the offender were caught anyway. Three-strikes laws, however, largely eliminate, for offenders with two prior convictions, the difference in perceived penalty severity between crimes in which they killed the victim and those in which they did not. By making it likely the offender with two prior convictions will get an extremely severe penalty like life imprisonment regardless of whether they kill their victim, legislators inadvertently remove the main disincentive for criminals like robbers to kill their victims.

We could amend sentencing provisions to reduce the upper limits of imprisonment ranges to more closely accord with those used in other industrialized Western nations. We could amend federal and state sentencing guidelines to specify less severe penalties, especially for simple drug possession and most property offenses committed without violence. Incarceration could be eliminated as a penalty for these offenses or nonincarceration sentences could be made the presumptive sentence, such that judges would be required to provide written reports establishing special circumstances that warranted deviations from the presumptive sentence.

If judges were not forced to impose long prison sentences, they could make more use of probation supervision as a substitute for, rather than a supplement to, incarceration for crimes like drug possession and most nonviolent property offenses. Community corrections costs a fraction of imprisonment and produces crime reductions that, while short of those delivered by incarceration, are substantial enough to easily justify its modest per-offender cost (Tonry 2009).

Judges could also make greater use of intermediate sanctions—punishments less severe than imprisonment but more severe than ordinary probation. For example, they could use day fines to punish drug possession and minor property crimes. A day fine is a fine defined in terms of the convicted offender's income, such that an offender punished with a 30-day fine would have to pay the equivalent of 30 days of his wages. Thus, persons with higher pay must pay higher fines, insuring that wealthier offenders are subjectively hurt to roughly the same degree as poorer offenders. House arrest, intensive probation/parole supervision, electronic monitoring, and community service could be used as substitutes for imprisonment, rather than as supplements used in addition to imprisonment. Likewise, judges could more often impose sentences of intensive probation accompanied by demonstrably effective treatment, as a substitute for incarceration, not as a form of net-widening. These alternatives would be especially helpful in reducing the detrimental effects of imprisonment on the children of those imprisoned because they would largely eliminate the separation of children from their parents (Tonry 1998, 2003).

### *Slow the Building of New Prisons and Jails, Close Others as They Wear Out*

Once statutory sentencing provisions are amended and the inflow of persons sentenced to prison is slowed, it would become easier to address the historically unprecedented scale of incarceration in contemporary America. State legislatures and governors, along with Congress and the President at the national level should limit the building of new prisons and jails to those needed to replace existing facilities that are not cost-effective to rehabilitate. New prison construction has been justified as being needed to reduce overcrowding, but has it never accomplished this goal because courts just sentence more offenders to prison terms, filling up all spaces regardless of how many there are (Table 10.1). Instead, overcrowding should be reduced by sharply reducing the number of criminals sentenced to terms of incarceration.

There is no doubt that it is politically possible to reduce the prison population, since the process to produce this result has already begun. As far back as 2007, prison admissions, figured either as a rate per 100 violent offenses or as a rate per population, began to decline, and in 2013 the total prison population declined for the first time in 30 years (Table 1.3). Prison overcrowding, measured as prison population relative to design capacity, has steadily declined since 2011 (Table 10.1).

To produce further reductions in the prison population, we could allow existing prisons to close when they reach the end of their life spans and become unduly expensive, unhealthy, or insecure to continue to operate. If prison capacity is not thereby reduced, many judges and prosecutors will be tempted to fill up all the available spaces, regardless of the minor character of the offenders whom they will send to fill those prison cells, just as has been the case in the preceding 40 years (Krebs, Sever, and Clear 1999). Revising sentencing provisions and practices will reduce the demand for prison spaces, while closing prisons will reduce the supply, each policy facilitating the other.

The fiscal crisis facing America after the 2008 recession had at least one silver lining—an opportunity to reform sentencing and corrections driven by economic necessity. We cannot afford to continue incarcerating over two million people and keep them locked up for extremely long periods of time. Governments need to cut costs somewhere, and cutting the prison population would not only meet some of that need, but would also make sense from the standpoint of making our crime control efforts more cost effective.

## Effective Crime Reduction Alternatives to Punishment

If more punishment is no longer an effective way to further reduce crime, what is? Crime will continue to be a serious problem in America regardless of what we do regarding punitive strategies, so we need to consider feasible alternatives. The following discussion is not intended as an exhaustive coverage of such alternatives or as a systematic review of evaluations of the impact of alternative strategies, since such an effort would require a separate book of its own. Rather, it is intended only to establish that there *are* numerous crime-control alternatives to ever-increasing levels of legal punishment—alternatives whose effectiveness has been demonstrated in empirical research—and to highlight some of the options that are most likely to be significantly effective in reducing crime. For more thorough assessments of crime reduction strategies, the interested reader may consult Kleiman (2009); Weisburd, Farrington, and Gill (2016); or Walker (2005). Support for the following strategies may be found in those sources.

### *Poverty Reduction*

Many crime-control efforts do not involve the operation of the criminal or juvenile justice systems in any capacity, punitive or nonpunitive. Crime reduction can be pursued via poverty reduction. Although political support for this approach declined with the rise of conservative political power in the last decades of the twentieth century, there is just as strong a theoretical and empirical foundation for believing that poverty reduction would bring crime reduction today as there was in the 1960s when the Johnson administration's War on Poverty was also regarded as a war on crime. The link between poverty and ordinary street crime like murder, assault, robbery, and burglary is as strong now as it was then. Such crime remains far more common in areas of concentrated poverty, especially racially-defined urban ghettos, and continues to be committed by and against poor people to a far greater extent than among middle class people.

In the 1960s and early 1970s, the war on poverty faded as the war in Vietnam heated up. In the competition between guns and butter, between the war overseas and the war on poverty, defense spending won out. As America's postwar economic expansion came to a halt in the late 1960s and early 1970s, policymakers decided that the nation could not afford both (a) spending on the Cold War and intermittent "hot" wars like the Vietnam conflict and (b) domestic spending on programs that benefitted the poor, so support for the latter declined.

The end of the Cold War opened up the possibility for a major reallocation of spending priorities. The potential has not been realized to the extent that many had hoped, but the fact remains that the need for massive defense expenditures has declined, and the billions thereby freed up could be devoted to crime reduction via poverty reduction. This will not happen, however, as long as expanding the nation's capacity for punishment continues to be viewed as the most effective and politically feasible way to reduce crime.

We can reduce poverty by using government spending to create more good-paying jobs in high-crime areas and by simultaneously providing the vocational training that would prepare residents of these areas to perform the jobs. Further, we could restore the minimum wage back to the levels, in terms of buying power, that it had in the late 1960s—at least up to a level that would allow the holder of a full-time job to stay above the poverty level. One major advantage of this approach to crime reduction compared to punishment-based strategies is that even if reductions in poverty failed to reduce crime, the effort could still be a success because any reductions in poverty that were achieved would be worthwhile in and of themselves.

### *Improve Parenting Skills*

Effective parenting is crucial in preventing delinquency but does not come naturally to everyone, especially those who first became parents at a young age or who did not enjoy the benefit of good parents and of learning from them by modeling. It is, however, a skill that can be taught, and programs have been developed to impart this skill. Multiple systematic reviews of the literature have found that parent training programs are effective in reducing antisocial behavior, especially among younger children (e.g., Brestan and Eyberg 1998; Kazdin 1997). Research has also found these programs to be cost-effective. A Surgeon General's report on youth violence concluded that parent training is an effective and cost effective preventive intervention, costing only $392 per crime averted (U.S. Department of Health and Human Services 2001), while Aos, Lieb, Mayfield, Miller, and Pennucci (2004) determined that family-based programs for juvenile offenders had a return of $8.68 for each dollar spent. A Rand Corporation study likewise found parent training to be one of the most cost-effective early interventions for preventing recidivism (Greenwood, Model, Rydell, and Chiesa 1998).

## *Offender Treatment to Reduce Recidivism*

We can redirect money that would have gone into the construction of new prisons and jails and maintenance of aging prisons into demonstrably successful treatment programs, i.e. those that have proven track records of reducing recidivism. Contrary to claims made in the 1980s that "nothing works" in correctional treatment, methodologically strong evaluations of treatment programs have found that many of them do work. A long series of systematic reviews of hundreds of evaluation studies supports the effectiveness of correctional treatment in reducing reoffending among treated offenders (Lipsey 1992; 1995; Lipton, Pearson, Cleland, and Yee 2002; Pearson and Lipton 1999; Pearson, Lipton, Cleland, and Yee 2002; Walker 2005). For example, Pearson and his colleagues (2002) reviewed 69 evaluations (treated/untreated comparisons) of the effect of behavioral/cognitive programs (e.g., social skills development programs) on recidivism among adults, many of high methodological quality, and concluded that these programs are effective in reducing recidivism.

To be sure, other treatment programs are not effective in reducing recidivism, and those that are generally effective do not reduce crime among all those treated. For example, treatment of juvenile offenders is more likely to be cost-effective than intervention with hardened adult offenders. And among juveniles, early intervention is better than late intervention. There is, however, nothing to preclude policymakers from funding the more effective treatment programs and defunding the ineffective ones like boot camps and "Scared Straight" programs for juveniles. Among those that are effective, many have substantial effects on recidivism rates. In his massive review of 443 studies of the treatment of juvenile offenders, Lipsey (1992) concluded that the best of the treatments reduced criminal behavior by 20 to 40 percent (123).

Some treatment programs target drug abuse. Because so large a share of crime is committed by drug-addicted offenders, there is huge potential for crime reduction by expanding the availability of treatment. Drug treatment has been found to be effective in substantially reducing substance abuse, despite the fact that most treated users repeatedly relapse. While some treated users do eventually quit altogether, there are crime-prevention benefits even from temporary cessation of illicit drug use or reduced levels of use. Pearson and Lipton's (1999) massive meta-analytic review of over 1,500 evaluations of drug treatment programs found that a variety of corrections-based treatments were effective in reducing drug abuse.

Many drug-addicted offenders recognize their drug problems and willingly seek out treatment. For addicted offenders unwilling to voluntarily submit to treatment, drug courts are an alternative. Under these programs, drug-addicted offenders, including those addicted to substances other than opiates, who show high rates of property offending but are unwilling to voluntarily enter treatment are diverted from the prison sentences they would otherwise receive. Suitable candidates are instead sentenced to community supervision (probation) that entails random drug testing and mandatory participation in drug treatment programs. Those who voluntarily enter and continue to participate in treatment programs will continue to do so without the intervention of the courts, but drug courts impose treatment on those who would not seek it out voluntarily (Kleiman 2009, 159–163). Drug courts have been found to reduce drug use and criminal behavior and to save more money than they cost (Rossman, Roman, Zweig, Rempel, and Lindquist 2011).

Some of the most clearly effective drug-related interventions do not aim at reducing drug use per se, but rather aim at minimizing some of the harms associated with illicit drug use, such as the property crime that is committed to pay for drugs that were made very expensive by their criminalization. This is the goal of methadone maintenance and its "opiate-substitution" cousins. Under these programs, persons who can demonstrate via positive drug test results that they already have access to opiates are given maintenance-level doses of legal substitutes for heroin and other illegal opiates. This eliminates their need to steal to obtain the money needed to pay for those drugs, without ending their addiction. It is now beyond reasonable dispute that these programs are extremely effective. Mark Kleiman, one of the nation's leading drug policy analysts, concluded that "opiate-substitution therapy dramatically reduces crime among those who receive it [and] unlike most treatment modalities, substitution has little trouble attracting and retaining clients." These programs should be greatly expanded, as "only about one eighth of U.S. heroin addicts are currently enrolled" in these programs (Kleiman 2009, 161).

### *Community Crime Prevention*

A variety of efforts to reduce crime could be lumped under the heading "community crime prevention" because they aim to prevent crime rather than punish or treat offenders once they have turned to crime and because the efforts are pursued in the community, outside the confines of the criminal and juvenile justice systems. For example, delinquent behavior is more likely for youth subject to less adult supervision, so programs that increase supervision can thereby reduce crime. One simple way to accomplish this is to provide subsidized day care that enables lower income single parents to hold jobs without leaving their children unsupervised. Parents pay on a sliding scale reflecting their ability to pay.

A variety of other broad categories of programs have also been found to reduce juvenile recidivism in a cost-effective way, including multidimensional foster care, diversion of low-risk offenders from the justice system, functional family therapy for juveniles on probation, multi-systemic therapy, and aggression replacement training (Washington State Institute for Public Policy 2006).

## Do Americans Support Nonpunitive Alternatives?

Politicians are unlikely to support changes in crime-control policies if their constituents do not support those changes. Therefore, it is worth summarizing some key findings from public opinion polls regarding what broad crime control approaches Americans support. Do they favor only punitive approaches entailing the expansion of the criminal justice system, or are they also open to less punitive alternatives to crime control?

Whether justified as rational crime control policy or not, there clearly is widespread support among Americans for punitive policies—primarily based on notions of retributive justice rather than their effectiveness in reducing crime. Certainty these notions of just deserts provide the primary reasons for support of the death penalty (U.S. Bureau of Justice Statistics 2005, Table 2.55). And notwithstanding the evidence that longer prison sentences do not increase the deterrent impact of punishment (Chapter 7), most Americans think court sentencing should be harsher (Table 1.5).

It would, however, be a mistake to infer from support for punitive measures that Americans do not support nonpunitive alternatives. Most Americans do not regard it as an either/or choice. In fact, a solid majority of Americans favor both approaches and support a wide array of nonpunitive approaches such as efforts at rehabilitation of offenders and poverty reduction via job training. Further, if forced to choose between the two, most Americans prefer putting more money and effort into nonpunitive approaches to reducing crime than increasing efforts to catch and punish more criminals. In repeated Gallup polls, U.S. adults have been directly presented with the alternative of (a) "attacking the social and economic problems that lead to crime through better education and job training" versus (b) "deterring crime by improving law enforcement with more prisons, police, and judges." In every one of nine national polls posing this question to Americans between 1989 and 2006, a solid majority (from 51 to 69 percent) preferred the option of reducing social and economic problems, with no more than 39 percent favoring the deterrence-focused alternative in any of the polls (Gallup Organization 2010). In sum, Americans favor both broad approaches, but, if forced to choose between them, they prefer the nonpunitive "root causes" alternative.

Punishment will always be one of society's responses to crime, to satisfy a sense of right and wrong if for no other purpose, so legal punishment will continue regardless of its effectiveness for reducing crime. And none of the foregoing should be interpreted as saying that "punishment does not deter crime." Modest levels of punishment produce more deterrence than no punishment at all, but massive levels of punishment do not necessarily produce any more deterrence than moderate levels. The phenomenon of diminishing returns characterizes the effect of punishment on crime just as it governs the impact of most other solutions to social problems, and America appears to have gone past the point where further punishment will yield enough further crime reduction to justify the costs of producing additional punishment.

In future, when politicians advocate "tougher laws," "more cops" to get criminals "off the street," and further expansion of our already massive prison system, these calls should be seen for what they are—appeals for more retribution, symbolic expressions of ideology-based notions of right and wrong, and, often, little more than pandering for votes among a frightened and desperate electorate, not evidence-based steps to making Americans safer. It is time to kick our single-minded addiction to excessively punitive strategies for the prevention and control of crime and start giving serious attention and resources to the alternatives.

## References

Aos, Steve, Roxanne Lieb, Jim Mayfield, Marna Miller, and Annie Pennucci. 2004. *Benefits and Costs of Prevention and Early Intervention Programs for Youth*. Olympia: Washington State Institute for Public Policy.

Brestan, Elizabeth, and Sheila Eyberg. 1998. Effective psychosocial treatments of conduct-disordered children and adolescents: 29 years, 82 studies, and 5,272 kids. *Journal of Clinical Child Psychology* 27:180–189.

Cook, Phillip. 1980. Research in criminal deterrence: Laying the groundwork for the second decade. *Crime and Justice* 2:211–268.

Currie, Elliott. 2011. On the pitfalls of spurious prudence. *Criminology and Public Policy* 10:109–114.

Doob, Anthony, and Cheryl Webster. 2003. Sentence severity and crime: Accepting the null hypothesis. *Crime and Justice* 30:143–196.

Durlauf, Steven, and Daniel S. Nagin. 2011. Imprisonment and crime: Can both be reduced? *Criminology and Public Policy* 10:13–54.

Gallup Organization, Inc. 2010. *The Gallup Poll*. Available online at www.galluppoll.com.

Greenwood, Peter, Karyn Model, C. Peter Rydell, and James Chiesa. 1998. *Diverting Children from a Life of Crime: Measuring Costs and Benefits*. Santa Monica: Rand Corporation.

Kazdin, Alan. 1997. Practitioner review: Psychosocial treatment for conduct disorder in children. *Journal of Child Psychology & Psychiatry* 38:161–178.

Kleck, Gary, Brion Sever, Spencer Li, and Marc Gertz. 2005. The missing link in general deterrence research. *Criminology* 43:623–660.

Kleiman, Mark. 2009. *When Brute Force Fails*. Princeton, NJ: Princeton University Press.

Kovandzic, Tomislav, John Sloan, and Lynne Vieraitis. 2002. Unintended consequences of politically popular sentencing policy. *Criminology & Public Policy* 1:399–424.

Kovandzic, Tomislav, John Sloan, and Lynne Vieraitis. 2004. "Striking out" as a crime reduction policy: The impact of "three strikes" laws on crime rates in U.S. cities. *Justice Quarterly* 21:207–239.

Krebs, Christopher, Brion Sever, and Todd Clear. 1999. Disparate sentencing: A tragedy of the commons. *Corrections Management Quarterly* 3:60–76.

Levitt, Steven. 2002. Using electoral cycles in police hiring to estimate the effect of police on crime: A reply. *American Economic Review* 92:1244–1250.

Lipsey, Mark. 1992. Juvenile delinquency treatment: A meta-analytic inquiry into the variability of effects. In *Meta-Analysis for Explanation*, eds. Thomas Cook, Harris Cooper, David Cordray, Heidi Hartmann, Larry Hedges, Richard Light, Thomas Louis, and Frederick Mosteller, 83–128. New York: Russell Sage Foundation.

Lipsey, Mark. 1995. What do we learn from 400 research studies on the effectiveness of treatment with juvenile delinquents? In *What Works: Reducing Reoffending*, ed. James McGuire, 63–78. New York: John Wiley.

Lipton, Douglas, Frank Pearson, Charles Cleland, and Dorline Yee. 2002. The effects of therapeutic communities and milieu therapy on recidivism. In *Offender Rehabilitation and Treatment: Effective Programmes and Policies to Reduce Re-Offending*, ed. James McGuire, 39–78. Chichester: John Wiley & Sons.

Marvell, Thomas, and Carlisle Moody. 2001. The legal effects of three strikes laws. *The Journal of Legal Studies* 30:89–106.

Nagin, Daniel. 1998. Criminal deterrence research at the outset of the twenty-first century. *Crime and Justice* 23:1–42.

Nagin, Daniel, Francis Cullen, and Cheryl Jonson. 2009. Imprisonment and reoffending. *Crime and Justice* 38:115–200.

Pearson, Frank, and Douglas Lipton. 1999. A meta-analytic review of the effectiveness of corrections-based treatments for drug abuse. *The Prison Journal* 79:384–410.

Pearson, Frank, Douglas Lipton, Charles Cleland, and Dorline Yee. 2002. The effects of behavioral/cognitive-behavioral programs on recidivism. *Crime & Delinquency* 48:476–96.

Rossman, Shelli, John Roman, Janine Zweig, Michael Rempel, and Christine Lindquist. 2011. *The Multi-Site Adult Drug Court Evaluation: Executive Summary*. New York: Urban Institute.

Shepherd, Joanna. 2004. Murders of passion, execution delays, and the deterrence of capital punishment. *Journal of Legal Studies* 33:283–322.

Smith, Paula, Claire Goggin, and Paul Gendreau. 2002. *The Effects of Prison Sentences and Intermediate Sanctions on Recidivism: General Effects and Individual Differences*. Ottawa: Solicitor General of Canada.

Tonry, Michael. 1998. Intermediate sanctions. In *The Handbook of Crime & Punishment*, ed. Michael Tonry, 683–711. New York: Oxford University Press.

Tonry, Michael. 2003. Reducing the prison population. In *Confronting Crime: Crime Control Policy Under New Labor*, ed. Michael Tonry, 211–233. Devon: Willan Publishing.

Tonry, Michael. 2009. The mostly unintended effects of mandatory penalties. *Crime and Justice* 38:65–114.

U.S. Bureau of Justice Statistics. 2005. *Sourcebook of Criminal Justice Statistics 2003*. Washington, DC: U.S. Government Printing Office.

U.S. Department of Health and Human Services. 2001. *Youth Violence: A Report of the Surgeon General*. Rockville, MD: U.S. Department of Health and Human Services.

van den Haag, Ernest. 1975. *Punishing Criminals: Concerning a Very Old and Painful Question*. New York: Basic Books.

Walker, Samuel. 2005. *Sense and Nonsense about Crime and Drugs*. Belmont, CA: Wadsworth.

Washington State Institute for Public Policy. 2006. *Evidence-Based Juvenile Offender Programs: Program Description, Quality Assurance, and Cost*. Olympia, WA: Washington State.

Weisburd, David, David Farrington, and Charlotte Gill. 2016. *What Works in Crime Prevention and Rehabilitation: Lessons from Systematic Reviews*. New York: Springer.

Wilson, James, and Richard Herrnstein. 1985. *Crime and Human Nature*. New York: Simon and Schuster.

Wright, Richard. 1994. *In Defense of Prisons*. Westport, CT: Greenwood Press.

# INDEX