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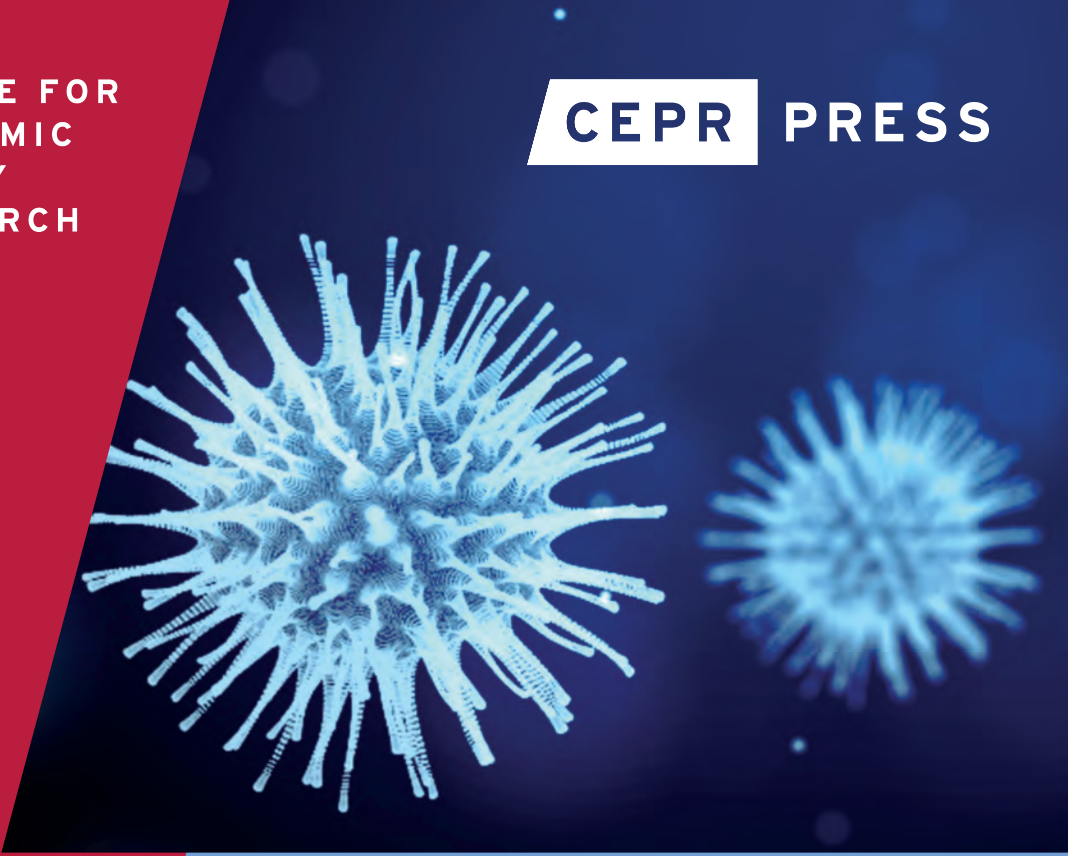

COVID ECONOMICS

VETTED AND REAL-TIME PAPERS

ISSUE 62
18 DECEMBER 2020

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Covid Economics
Vetted and Real-Time Papers

Covid Economics, Vetted and Real-Time Papers, from CEPR, brings together formal investigations on the economic issues emanating from the Covid outbreak, based on explicit theory and/or empirical evidence, to improve the knowledge base.

Founder: Beatrice Weder di Mauro, President of CEPR
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Covid Economics will feature high quality analyses of economic aspects of the health crisis. However, the pandemic also raises a number of complex ethical issues. Economists tend to think about trade-offs, in this case lives vs. costs, patient selection at a time of scarcity, and more. In the spirit of academic freedom, neither the Editors of *Covid Economics* nor CEPR take a stand on these issues and therefore do not bear any responsibility for views expressed in the articles.

Submission to professional journals

The following journals have indicated that they will accept submissions of papers featured in *Covid Economics* because they are working papers. Most expect revised versions. This list will be updated regularly.

American Economic Review
American Economic Review, Applied Economics
American Economic Review, Insights
American Economic Review, Economic Policy
American Economic Review, Macroeconomics
American Economic Review, Microeconomics
American Journal of Health Economics
Canadian Journal of Economics
*Econometrica**
Economic Journal
Economics of Disasters and Climate Change
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*Journal of Econometrics**
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Journal of Financial Economics
Journal of International Economics
*Journal of Labor Economics**
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Journal of Public Economics
Journal of Public Finance and Public Choice
Journal of Political Economy
Journal of Population Economics
Quarterly Journal of Economics
*Review of Corporate Finance Studies**
Review of Economics and Statistics
*Review of Economic Studies**
Review of Financial Studies

(*) Must be a significantly revised and extended version of the paper featured in *Covid Economics*.

Covid Economics
Vetted and Real-Time Papers

Issue 62, 18 December 2020

Contents

Real-time real economic activity entering the pandemic recession[1]

Francis X. Diebold[2]

Date submitted: 15 December 2020; Date accepted: 16 December 2020

Entering the Pandemic Recession, we study the high-frequency real-activity signals provided by a leading nowcast, the ADS Index of Business Conditions produced and released in real time by the Federal Reserve Bank of Philadelphia. We track the evolution of real-time vintage beliefs and compare them to a later-vintage chronology. Real-time ADS plunges and then swings as its underlying economic indicators swing, but the ADS paths quickly converge to indicate a return to brisk positive growth by mid-May. We show, moreover, that the daily real activity path was extremely highly correlated with the daily COVID-19 path. Finally, we provide a comparative assessment of the real-time ADS signals provided when exiting the Great Recession.

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1 For helpful discussion I thank Boragan Aruoba, Scott Brave, Andrew Patton, Glenn Rudebusch, Frank Schorfh ide, Chiara Scotti, Minchul Shin, Keith Sill, Mark Watson, Tom Stark, Jim Stock, Herman van Dijk, and Simon van Norden. I am also grateful to conference/seminar participants at the Federal Reserve Bank of Philadelphia, the Society for Financial Econometrics, Amazon, the Department of Statistics at the Wharton School, and the International Association for Applied Econometrics. For outstanding research assistance and related discussion I thank Philippe Goulet Coulombe, Tony Liu, and Boyan Zhang. The usual disclaimer applies.

2 Paul F. Miller, Jr. and E. Warren Shafer Miller Professor of Social Sciences, and Professor of Economics, Finance, and Statistics, University of Pennsylvania.

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1 Introduction

Accurate assessment of of current real economic activity ("business conditions") is key for successful decision making in business, finance, and policy. It is difficult, however, to track business conditions in real time, both because no single observed economic indicator *is* "business conditions", and because different indicators are available at different observational frequencies, and with different release delays. Nevertheless there exists the tantalizing possibility of accurate real-time business conditions assessment ("nowcasting"), and recent decades have witnessed great interest in nowcasting methods and applications (e.g., Banbura et al. (2011)).

The workhorse nowcasting approaches involve dynamic factor models, which relate a set of observed real activity indicators to a single underlying latent real activity factor. Both "small data" approaches (e.g., based on 5 indicators) and "Big Data" approaches (e.g., based on 500 indicators) are available. Small data approaches were developed first, and they typically involve maximum likelihood estimation (e.g., Stock and Watson (1989)). Subsequent Big Data approaches, in contrast, typically involve two-step estimation based on a first-step extraction of principal components (e.g., Stock and Watson (2002), McCracken and Ng (2016)).

Both introspection and experience reveal that Big Data nowcasting approaches are not necessarily better. First, they are more tedious to manage, and less transparent. Second, they may not deliver much improvement in factor extraction accuracy, which increases and stabilizes quickly as the number of indicators increases (Doz et al., 2012). Third, casual inclusion of many indicators can be problematic because a poorly-balanced set of indicators can create distortions in the extracted factor (Boivin and Ng, 2006), whereas small data approaches promote and facilitate hard thinking about a well-balanced set of indicators (Bai and Ng (2008)).

Against this background, in this paper we assess the performance of a leading small-data nowcast, the Aruoba-Diebold-Scotti (ADS) Index of Business Conditions (Aruoba et al., 2009). ADS is designed to track real business conditions at high frequency, and it has been maintained and released in real time by the Federal Reserve Bank of Philadelphia continuously since 2008.[1] Its modeling style and underlying economic indicators build on classic

[1]The production version used by FRB Philadelphia differs in some ways (e.g., included indicators and treatment of trend) from the prototypes provided by Aruoba et al. (2009) and Aruoba and Diebold (2010), which themselves differ slightly. All discussion in this paper refers to the FRB Philadelphia version. All materials, including the full set of vintage nowcasts, are available at `https://www.philadelphiafed.org/research-and-data/real-time-center/business-conditions-index`.

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early work in the tradition of Burns and Mitchell (1946), Sargent and Sims (1977), and Stock and Watson (1989). The underlying indicators span high- and low-frequency information on real economic flows: weekly initial jobless claims; monthly payroll employment growth, industrial production growth, personal income less transfer payments growth, manufacturing and trade sales growth; and quarterly real GDP growth.

Crucially, we assess ADS using only information actually available in real time. This is required for truly credible real-time evaluation, and it can only be achieved by using nowcasts produced and permanently recorded in real time, which is very different from simply removing final-revised data and inserting vintage data into an otherwise ex post analysis. Unfortunately, such evaluations are rare, because there simply are not many instances of long series of nowcasts produced and recorded in real time. ADS, however, has been produced and recorded in real time roughly twice weekly since late 2008, so we can provide real-time performance assessments both exiting the Great Recession and entering the Pandemic Recession.

We proceed as follows. In section 2 we provide background on aspects of ADS construction, updating, ex post characteristics, and performance evaluation. In section 3 we examine ADS entering the Pandemic Recession, and we relate the real-time ADS path to the real-time COVID-19 path. In section 4 we provide a comparative examination of ADS exiting the Great Recession. We conclude in section 5.

2 Nowcast Construction, Characteristics, and Assessment

Here we provide background on the ADS index construction (section 2.1), ex post historical characteristics (section 2.2), and general issues of relevance to assessing ex ante nowcasting performance (section 2.3).

2.1 Construction and Updating

ADS is a dynamic factor model with multiple mixed-frequency real activity indicators driven by a single latent real activity factor. The ADS index is an estimate of that latent real activity factor. Importantly, the model is specified such that *the real activity factor tracks the demeaned growth rate of real activity.* Progressively more negative or positive values indicate progressively worse- or better-than-average real growth, respectively. Because ADS tracks real activity growth, not level, a positive value does not necessarily mean "good times"; rather, it means "good growth", which may be from a level well below trend, as for example in the early stages of a recovery.

ADS is specified at daily frequency, allowing as necessary for missing data for the less-frequently observed variables.[2] Importantly, despite complications from missing data, time-varying system matrices, aggregation across frequencies, etc., the Kalman filter and associated Gaussian pseudo likelihood evaluation via prediction-error decomposition remain valid, subject to some well-known modifications.[3] Model estimation is therefore straightforward, after which the Kalman smoother produces an optimal extraction of the underlying real activity factor. That is, the Kalman smoother produces the ADS index: The extracted sequence at any time t^* *is* the vintage-t^* ADS sequence, $\{ADS_1, ADS_2, ...ADS_{t^*}\}$.

The first ADS vintage was released 12/5/2008, covering 3/1/1960 through 11/30/2008. Since then, ADS has been continuously updated whenever new data are released. The Kalman smoother is re-run, generally within two hours of the release, and the newly-extracted index from 3/1/1960 to "the present" is re-written to the web. ADS has been updated approximately eight times per month on average since inception.

2.2 Ex Post Characteristics

In Figure 1 we show the ADS index from 03/01/1960 through 12/31/2013, as assessed in the 6/26/2020 vintage. The sample range is well before the vintage pull date, so the chronology displayed is (intentionally) ex post. We do this because it is instructive to examine the ex post chronology before passing to real time assessment, which can only be done after ADS went live in late 2008.[4]

Several features are noteworthy. For example, the ADS chronology coheres strongly with the NBER chronology, plunging during NBER recessions. In addition, several often-discussed features of the business-cycle are evident in ADS, such as the pronounced moderation in volatility during the Greenspan era.

The ADS value added relative to the NBER chronology stems from the facts that (1) it is a cardinal measure, allowing one to assess not only recession durations, but also depths and

[2]The model must be specified at daily frequency, despite the fact that the highest-frequency indicator is is weekly initial jobless claims, to account for the varying number of days/weeks per month, which also produces time-varying system parameter matrices.

[3]See, for example, Durbin and Koopman (2001) on missing data, and Harvey (1991) on aggregation of flow variables.

[4]The sample period intentionally excludes the Pandemic Recession, which we will subsequently examine in detail.

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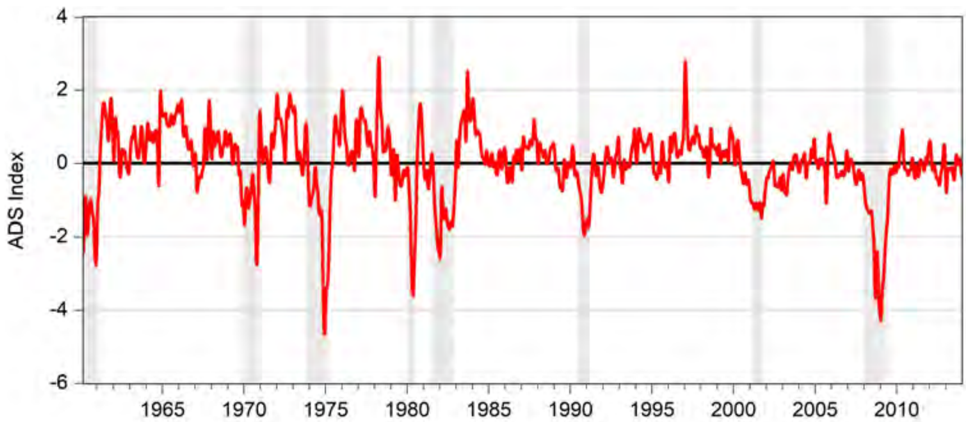
Figure 1: ADS Index: Ex Post Path 03/01/1960 - 12/31/2013 (Vintage 6/26/2020)



Notes: The shaded regions are NBER-designated recessions.

patterns (see Table 1), and (2) its updates arrive in timely fashion, whereas the starting and ending dates NBER recessions are typically not announced until well after the fact (again see Table 1). Of course, if ADS is to be a useful guide for business and policy decisions, its frequently-arriving updates must provide reliable signals in real time, not just ex post as in Figure 1. We now turn to that issue.

2.3 Performance Assessment

Truly credible nowcasting performance assessment requires using *vintage information*, which emerges as the limit of a sequence of progressively more realistic and credible nowcast/forecast evaluation approaches:[5]

(1) Use full-sample estimation, and use final revised data

(2) Use expanding-sample estimation, and use final revised data

(3) Use expanding-sample estimation, and use vintage data ("Pseudo Real Time")

(4) Use expanding-sample estimation, and use vintage information ("Real Time").

[5]Note that nowcasts are effectively just h-step-ahead forecasts with horizon h=0.

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Table 1: NBER Recessions

Recession Dates		Recession Characteristics		
Peak Month	Trough Month	Duration	Depth	Severity
April 1960	February 1961	10	2.7	27.0
December 1969	November 1970	11	2.8	30.8
November 1973	March 1975	16	4.7	75.2
January 1980 (6/3/1980)	July 1980 (7/8/1981)	6	3.6	21.6
July 1981 (1/6/1982)	November 1982 (7/8/1983)	16	2.9	46.4
July 1990 (4/25/1991)	March 1991 (12/22/1992)	8	1.7	13.6
March 2001 (11/26/2001)	November 2001 (7/17/2003)	8	1.5	12.0
December 2007 (12/1/2008)	June 2009 (9/20/2010)	18	4.3	77.4
February 2020 (6/6/2020)	?	?	?	?

Notes: Recession dates and durations in months are from the NBER chronology; see `https://www.nber.org/cycles.html`. When available, the announcement dates appear in parentheses. The NBER trough month for the Pandemic Recession has not yet been announced. Recession depth is the minimum absolute daily ADS value during the recession; more precisely, the depth D of recession R is $D = |min_i(ADS_i)|$, $i \in R$, where i denotes days. Recession severity S is the product of depth and duration. Both D and S use a late-vintage ADS chronology and the NBER recession chronology.

Approaches (1) and (2) are clearly unsatisfactory: Approach (1) uses time periods and data values not available in real time, and approach (2) is an improvement but still uses data values not available in real time. Approach (3), involving vintage *data*, is typically viewed as the gold standard. It is implemented comparatively infrequently, however, due to the tedium involved and the fact that vintage data are often unavailable.[6] Approach (4), involving vintage *information*, limits the information set to that available and actually used in real time, which is more restrictive than merely limiting the *data* to that available in real time. It is, however, almost never implemented.

To appreciate why fully-credible assessment requires vintage information rather than just vintage data, consider the following:

(1) Econometric/statistical theory and experience evolve, prompting changes to the estimation procedure; the frequency and timing of re-estimation and its interaction with benchmark revisions; the estimation sample period; allowance for parameter variation

[6]The two key sources of U.S. vintage data are the Real-Time Dataset for Macroeconomists at the Federal Reserve Bank of Philadelphia (`https://www.philadelphiafed.org/research-and-data/real-time-center/real-time-data/`), and ALFRED at the Federal Reserve Bank of St. Louis (`https://alfred.stlouisfed.org/`).

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and breaks; the treatment of outliers; the strength of regularization employed; the predictive loss function employed; etc.

(2) Economic theory and empirical economic experience evolve. Over time this may prompt, for example, the removal or re-weighting of some component nowcast indicators and/or addition of others (e.g., Diebold and Rudebusch (1991)), as well as deeper changes in the nowcasting model.

(3) Exact times and reliability of nowcast/forecast calculation and release may differ due to technological problems; outright mistakes in nowcast/forecast construction; evolving or changing software algorithms and associated bugs; parallel problems at the agencies responsible for the underlying data and decisions regarding how to deal with them in forecast/nowcast construction; etc.

For these and other reasons, just as truly credible evaluation requires refraining from endowing agents with better data than were actually available in real time, so too does it require refraining from endowing them with better economic or statistical models and related tools than were actually available in real time, better judgment and decision-making abilities/choices than were actually manifest in real time, etc.

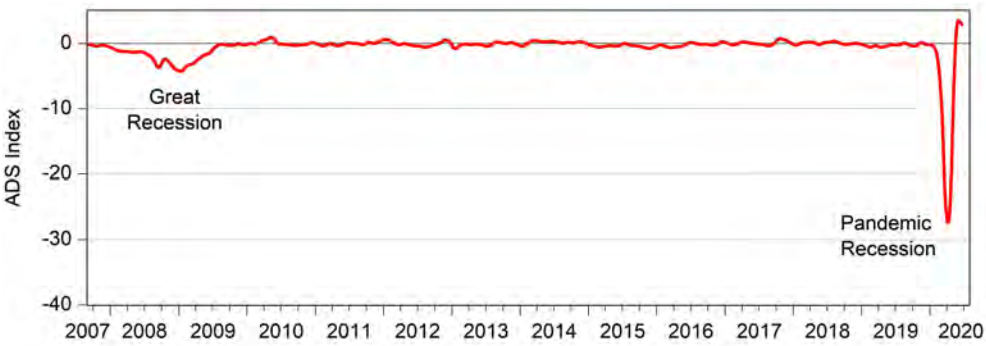
The upshot is clear: Truly credible real-time evaluation – that is, evaluation using vintage information rather than just vintage data – can only be obtained by using nowcasts produced and permanently recorded in real time. ADS has been produced and recorded in real time since late 2008, so we can credibly study the key episode of current interest, entry into the Pandemic Recession. We now proceed to do so.

3 The Pandemic Recession Entry

We focus in this section on the Pandemic Recession that started in March 2020. It is instructive to begin by comparing it to the Great Recession of 2007-2009. To that end we show the ADS path in Figure 2, from late 2007 through June 2020.[7] The so-called "Great Recession" appears minor by comparison.

[7] We refer to an ADS extraction as a path.

Figure 2: ADS Index: Ex Post Path 12/1/2007 - 6/26/2020 (Vintage 6/26/2020)



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3.1 A Detailed Look at the Later-Vintage Path

Figure 2 reveals the jaw-dropping ADS drop in the Pandemic Recession, more than five times that of any other recession since 1960. The ADS drop is entirely appropriate, due to similarly jaw-dropping and historically unprecedented movements in its underlying indicators.

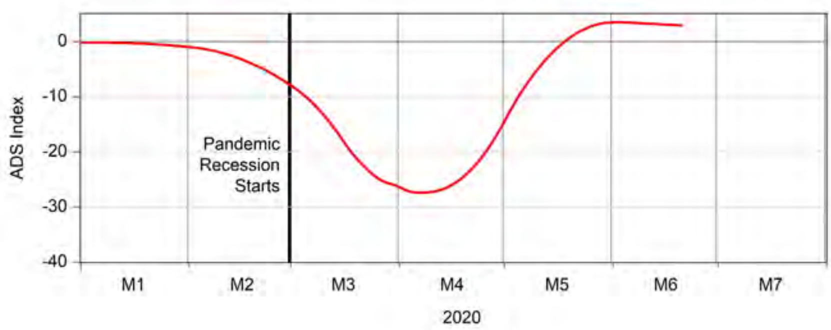
As of this writing, the official trough month for the Pandemic Recession has not been announced. It could be as early as May 2020, in which case the Pandemic Recession would be the shortest in history. Indeed a May trough turns out to be likely. In Figure 3 we show the later-vintage Pandemic Recession path. The overall extracted path is smooth and convex, with a minimum in early April, and a return to positive growth by mid-May. We emphasize again, however, that ADS measures real activity growth, not level. Hence positive ADS does not necessarily mean "good times"; rather, it means "good growth", which may be from a very bad initial condition. That was the situation in late May, as the battered U.S. economy evidently resumed growth.

3.2 Real-Time Vintages

3.2.1 Five Snapshots

In Figure 4 we show several end-of-month paths in black, starting with February 2020. For comparison, in each panel we also show the later-vintage path in red. Moving through the five panels of Figure 4:

Figure 3: ADS Index: Ex Post Path 1/1/2020 - 6/26/2020 (Vintage 6/26/2020)



(1) In the top panel we show the 2/28/2020 path. ADS has not moved.

(2) In the second panel we show the 3/27/2020 path, which looks very different. ADS has become acutely aware of the disastrous situation; indeed most of the 3/27 path is well below the previous all-time (post-1960) ADS low during the 1970s oil-shock recession.[8]

(3) In the third panel we show the 4/30/2020 path. The April initial claims news is bad, but less bad than March, which is good, and ADS shows a minimum in late March followed by a rise toward normalcy by the end of April.

(4) In the fourth panel we show the 5/29/2020 path. The May news is very bad, dominated by the shockingly bad May 8 payroll employment number (for April), and the late-May path is massively down-shifted relative to the late-April path. The new minimum is in mid-April rather than late March, and the 5/29 ADS value is thoroughly dismal, nowhere near normalcy.

(5) In the fifth panel we show the 6/26/2020 path. Thanks to the strong May payroll employment number (released June 5), ADS moved into normal territory, and stayed there. There is clear (albeit highly-tentative evidence for a Pandemic Recession trough in mid-May, when ADS hits 0.)

[8]It is also apparent that the Kalman smoother may be smoothing "too much", producing low ADS values well before mid-March, going back into February and even January. Its smoothing is optimal relative to the patterns in historical data, but the March initial jobless claims movements were unprecedentedly sharp.

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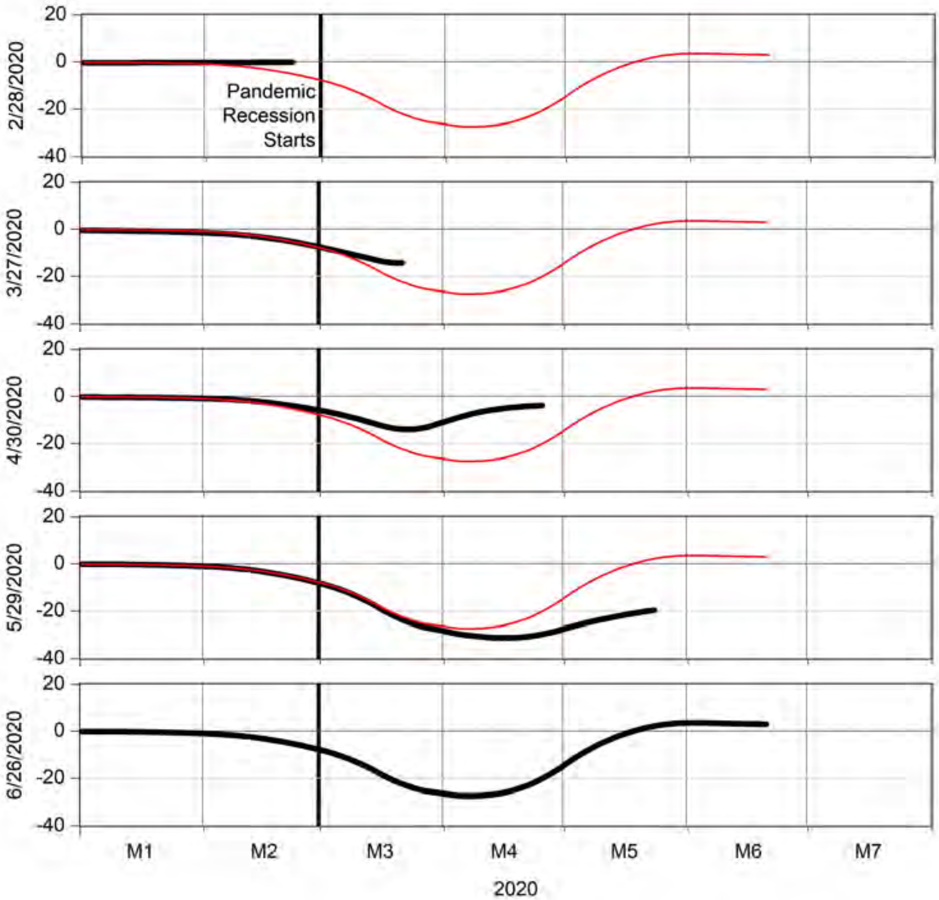
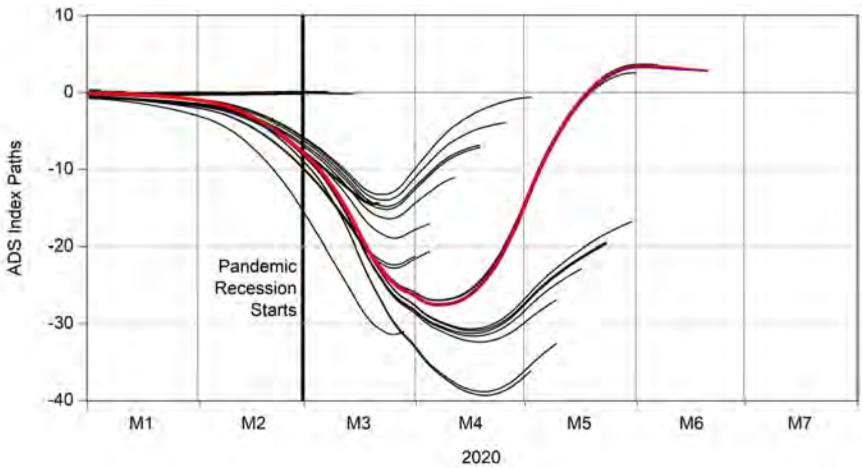
Figure 4: Entering the Pandemic Recession: Monthly Real-Time ADS Paths



Notes: We show five monthly real-time ADS paths in black. From top to bottom they are 2/28/2020, 3/27/2020, 4/30/2020, 5/29/2020, and 6/26/2020. For comparison we show the 6/26/2020 path in red in all panels. (In the bottom panel, we show only black, since black and red are identical.)

Figure 5: Entering the Pandemic Recession: Real Time ADS Path Plot



Notes: We show all real-time ADS paths in black, through 6/26/2020. For comparison we show the complete later-vintage path (6/26/2020) in red.

3.2.2 The Full Path Plot and Dot Plot

In Figure 5 we show the complete path plot during the Pandemic Recession through 6/26/2020, with the later-vintage path in red for comparison. The path plot is the set of all real-time paths; by following rightward through the sequence of paths, moving through time, we track the evolution of ADS beliefs about the chronology of business conditions.

There are wide real-time divergences between individual early paths and the later vintage red path. There are interesting patterns, however, with several real-time "meta paths" evident:

(1) The first extends through the 3/19/2020 ADS announcement. ADS does not move. Initial claims rise from 0.2m to 0.3m, a large move by historical standards, confirming what everyone already knew: the pandemic would have important real economic consequences, but the Kalman smoother optimally but erroneously ascribes it to measurement error.

(2) The second meta-path begins with the 3/26/2020 and 4/2/2020 initial claims explosions. ADS plunges, but then recovers steadily despite a steady stream of bad news

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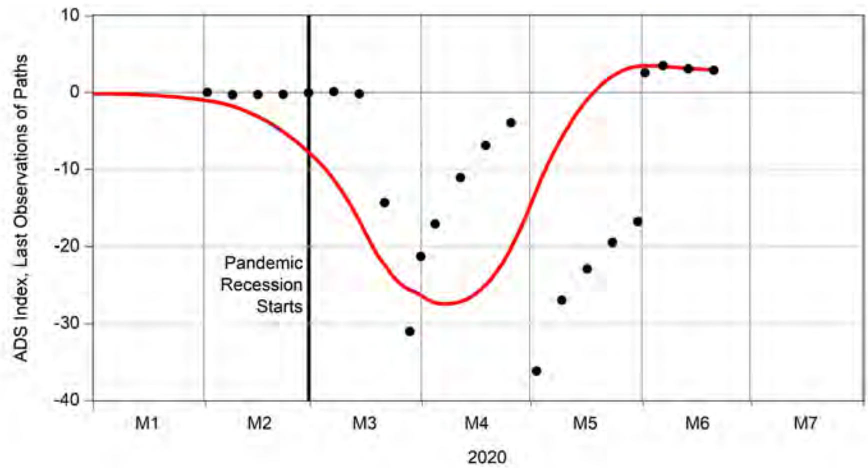
Figure 6: Entering the Pandemic Recession: Real-Time ADS Dot Plot



Notes: We show the last values of all real-time ADS paths in black. For comparison we show the complete later-vintage path (6/26/2020) in red.

(it is bad, but getting less bad), almost back to 0 by the 5/7/2020 initial claims announcement.

(3) The third meta-path begins with the horrific 5/8/2020 April payroll employment release, with ADS again plunging. It then again begins mean reverting, and does so completely when the strong May payroll employment number is released on 6/5/2020.

In Figure 6 we show the corresponding "dot plot", with the 6/26/2020 path again superimposed. Each dot is the last observation of its corresponding path in Figure 5. The dots are real-time filtered values, because smoothed and filtered values coincide for the last observation in a sample. The dot plot is highly volatile and emphasizes the various meta-paths.

3.3 Real Economic Activity and COVID-19

Because the March-April 2020 collapse in economic activity was obviously caused by COVID-19, it is of interest to directly examine the correlation between the two. We can do so at high frequency (daily), because we have both daily ADS and COVID new cases / deaths data. We want to correlate COVID new cases with ADS, but the direct new cases data are less reliable

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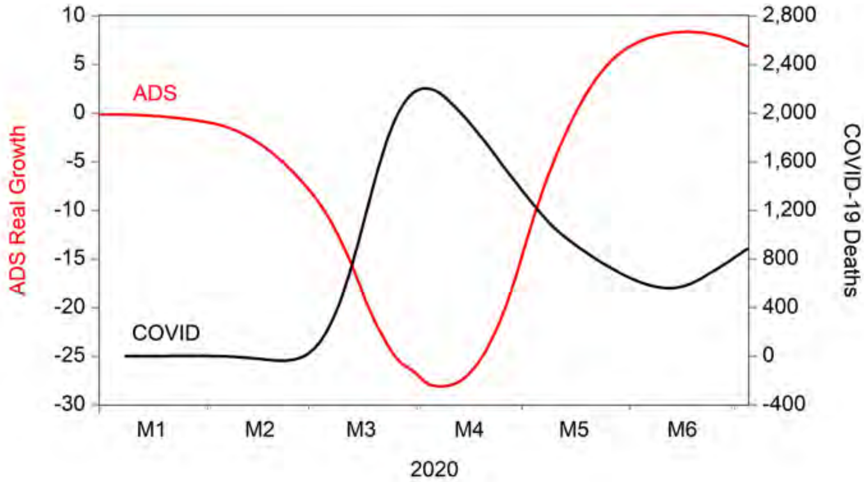
Figure 7: Daily ADS and Smoothed Daily COVID-19 Deaths



Notes: We show ADS (6/26/2020 vintage) vs HP-filtered daily COVID-19 deaths led by 20 days. See text for details.

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than deaths during the period of interest, because new cases were likely heavily influenced by changes in the amount of testing undertaken. Instead, a more reliable indicator of new cases is deaths, adjusted for the approximate 20-day period between infection and death. Hence we use deaths led by 20 days.[9] In Figure 7 we show ADS vs COVID deaths+20.[10] The strength of the negative correlation is striking. Of course economic activity plunged in March when COVID exploded, but there's much more than that – ADS and COVID continue to move in lockstep (inversely) through the April COVID peak, its April-May decline, and its June rebound.

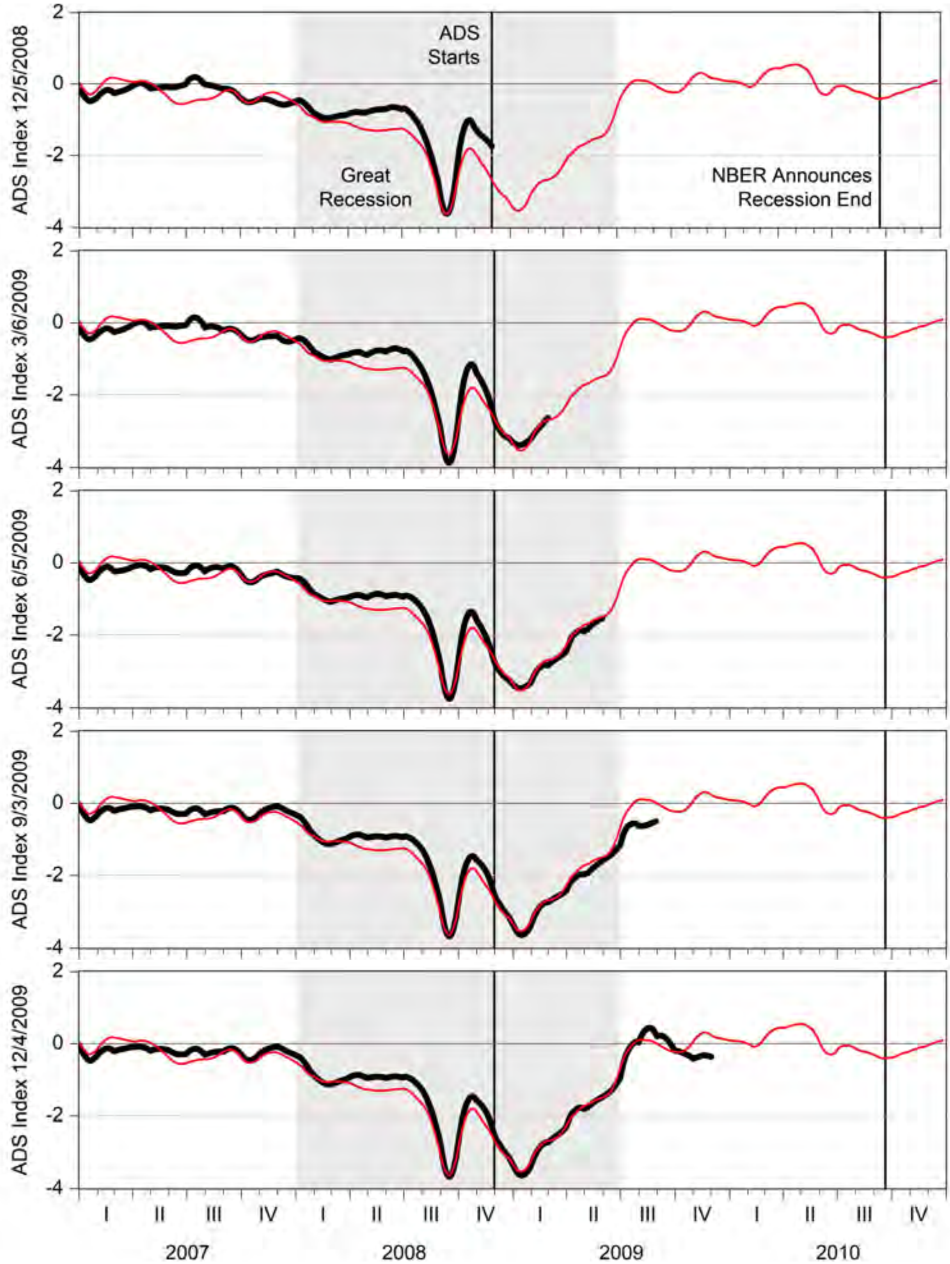
4 Comparison to the Great Recession Exit

It is informative to compare the evolution and congealing of views during the Pandemic Recession entry to those during an earlier, more "standard", recession, like the Great Re-

[9]We use the Johns Hopkins University CSSE COVID-19 daily deaths data; see Dong et al. (2020) and `https://github.com/CSSEGISandData/COVID-19`.

[10]We also smooth COVID deaths+20 using a Hodrick-Prescott filter to remove the strong calendar effects in recorded deaths.

Figure 8: Exiting the Great Recession: Five Quarterly Real-Time ADS Paths



Notes: We show five quarterly real-time ADS paths in black. From top to bottom they are 12/5/2008, 3/6/2009, 6/5/2009, 9/3/2009, 12/4/2009. For comparison we show a later-vintage ADS path (December 2010) in red.

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cession of 2007-2009. We can't examine real-time ADS when entering the Great Recession, because ADS did not start until December 2008, well after the great recession began. But we can examine it when *exiting* the great recession. In Figure 8 we show five paths in black, from ADS inception through the end of the Great Recession, at quarterly intervals. For comparison we also show a later-vintage path in red.

In the top panel of Figure 8 we show the first ADS path, 12/5/2008. ADS shows a very deep recession, almost the deepest on record since 1960, bottoming out in 2008Q3, with movement toward recovery in late Q3 and early Q4, even if it had stalled a bit by early December. As it turned out, however, the Great Recession subsequently featured a growth rate "double dip". The 12/5/2008 ADS path ends just after the first dip, which involved a sharp drop in September 2008 and an equally sharp rebound.[11] At the time it was easy to read the cards as saying that the recession was ending, and ADS was a bit too optimistic, moving upward toward recovery.

Now consider the remaining panels of Figure 8. In the second panel we show the next, and contrasting, 3/6/2009 ADS path. In the interim ADS has quickly learned the situation, the double dip in particular, and is very much on track, capturing the second dip in January 2009. ADS continues to climb steadily through the third and fourth panels (6/5/2009 and 9/3/3009, respectively), and by the time of the bottom panel (12/4/2009) it is clear that the Great Recession ended in June or July, with ADS basically fluctuating around 0 after that. (Recall that ADS=0 means average growth, not zero growth.)

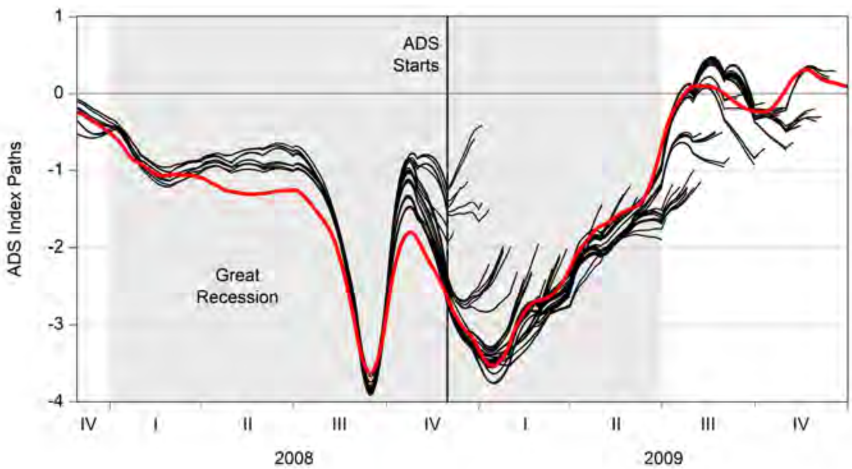
All told, the five quarterly real-time ADS paths generally match the ex post path closely, and they correctly identify the recession's end, well before the end of 2009 and indeed roughly 1.5 *years* before the official NBER announcement in September 2010.

To emphasize ADS timeliness, we plot the later-vintage ADS in Figure 8 all the way through 2010, which allows inclusion of the NBER's end-of-recession announcement on 9/20/2010, long after the fact and not helpful for real-time decision making.[12] ADS fills the gap left by the late-arriving NBER chronology, and it also provides a numerical measure that allows one to track the recession's pattern, depth, overall severity, etc., in addition to

[11]In particular, according to the Federal Reserve's G.17 Industrial Production (IP) release of October 16, 2008, September IP was severely affected by a highly-unusual and largely exogenous "triple shock" (Hurricanes Gustav and Ike, and a strike at a major aircraft manufacturer), which caused an annualized September IP drop of nearly fifty percent. A similar pattern exists for Manufacturing and Trade Sales (MTS). IP and MTS also rebounded unusually sharply in October – indeed IP appears to "overshoot" – presumably in an attempt by manufacturers to make up for September's loss.

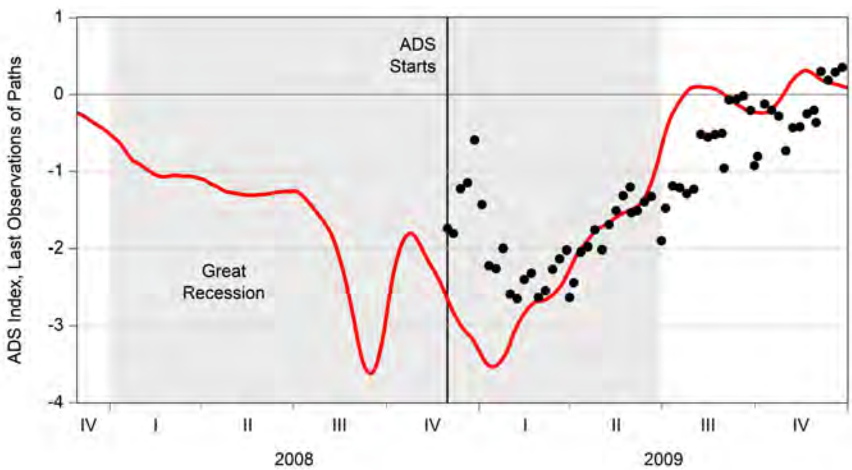
[12]Of course the NBER is not *seeking* to be helpful for real-time decision making; rather, they seek to meticulously construct the U.S. business cycle chronology of record, quite reasonably using all relevant information – even very late-arriving information.

Figure 9: Exiting the Great Recession: Real-Time ADS Path Plot



Notes: We show all 2008-2009 ADS paths since the first on 12/5/2008. We show real-time ADS paths in black, and a comparison late-vintage ADS path (December 2010) in red.

Figure 10: Exiting the Great Recession: Real-Time ADS Dot Plot



Notes: We show the last values of each 2008-2009 ADS path in black, with a comparison later-vintage ADS path (December 2010) superimposed in red.

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duration. For example and as recorded in Table 1, ADS identifies the Great Recession as the worst since 1960 and through 2010, with longest duration and second-greatest depth, resulting in the greatest overall severity (duration times depth).

In Figure 9 we show the complete path plot. Of course there are errors positive and negative as the recession evolves, but overall ADS performs well, sending a reliable and valuable signal for navigating the path out of recession. We show the corresponding dot plot in Figure 10.

5 Conclusion

We explored how views formed using a leading nowcast (ADS) evolved when entering the U.S. Pandemic Recession, which arrived abruptly and was caused by non-economic factors, tracking the evolution of real-time vintage beliefs and comparing them to a later-vintage chronology. ADS real activity growth plunged wildly in March 2020 and swung in real time as its underlying components swung, but it clearly returned to brisk growth by mid May, making the Pandemic Recession surely the deepest and likely the shortest on record.[13] We also documented a strong negative relationship between the real-time ADS Pandemic Recession entry path and the concurrent real-time COVID-19 entry path, and we compared the ADS Pandemic Recession entry path to the earlier Great Recession exit path.

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[13] The NBER has not yet announced the ending date of the Pandemic Recession.

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Buying the vote? The economics of electoral politics and small business loans[1]

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We study the role of electoral politics in government small business lending, employment, and business formation. We construct novel measures of electoral importance capturing swing and base voters using data from Facebook ad spending, independent political expenditures, the Cook Political Report, and campaign contributions. We find that businesses in electorally important states, districts, and sectors receive more loans following the onset of the Covid-19 crisis, controlling for funding demand and both health and economic conditions. Estimates from survey and observational data show that government funding weakens the adverse effects of the crisis on employment, small business activity, and business applications.

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1. Introduction

In this paper, we seek to provide novel empirical evidence on the role of election-year political incentives in the government's allocation of emergency funds and their real economic effects. We focus our attention on the Covid-19 outbreak, which was an unexpected economy-wide shock that triggered a large-scale government aid response. This response disbursed trillions of dollars across states, businesses, and individuals during a period of economic stress, when the benefit of government aid is potentially greatest. The outbreak also coincides with the 2020 presidential election year in the U.S, which is characterized by strident political polarization. According to Gallup, 82 percentage points separate Republicans' (89%) and Democrats' (7%) average job approval ratings of President Trump during his third year in office -- the largest degree of political polarization in any presidential year measured by Gallup.[1]

We argue that the confluence of a massive emergency government aid package and a polarized presidential race generates a unique setting to identify the role of electoral politics in the allocation of government funds and its economic consequences. In particular, a large body of evidence in political economy suggests that voters reward incumbents based on economic conditions in the year before Election Day rather than throughout their tenure (e.g., Kramer 1971; Fair 1978; Kiewiet 1983; Alesina, Londregan, and Rosenthal 1993; Achen and Bartels 2004). Furthermore, Achen and Bartels (2004) conclude that long-term economic growth contributes little or nothing to the incumbent party's electoral prospects. Such voter behavior introduces incentives to implement election-year policies that improve the reelection prospects of incumbents, possibly

[1] See: "Trump Third Year Sets New Standard for Party Polarization," by Jeffrey M. Jones, https://news.gallup.com/poll/283910/trump-third-year-sets-new-standard-party-polarization.aspx

with considerable economic effects and at the cost of long-term economic growth (e.g., Tufte 1978).

The empirical analyses focus on the Paycheck Protection Program (PPP), which is a central piece of the 2020 Coronavirus Aid, Relief, and Economic Security (CARES) Act. The PPP was administered by the Small Business Administration (SBA) and extended forgivable loans to businesses to cover payroll, utilities, mortgage, and rent costs. The combination of the attractive terms of the PPP and the sharp decline in economic activity resulting from the shelter-in-place policies implemented in response to Covid-19 led to oversubscription to the PPP and consequently to credit rationing. As such, the PPP could have been a powerful instrument to implement election-year allocative policies. Using detailed data on the allocation of forgivable PPP loans, we investigate how the politics of an election year affects the allocation of government funds in response to the Covid-19 crisis across states, congressional districts, and industries in the U.S., and the corresponding consequences for employment and business activity.

Our paper lies in the intersection of two voluminous literatures. The first studies the effect of government spending on economic outcomes during periods of economic stress (e.g., Clemens and Miran 2010; Chodorow-Reich et al. 2012; Wilson 2012; and Fishback and Kachanovskaya 2015). The second studies the link between politics and government spending (e.g., Ritt 1976; Ray 1980, 1981; Kiel and McKenzie 1983; Atlas et al. 1995; Levitt and Poterba 1999; Sapienza 2004; Dinc 2005; Hoover and Pecorino 2005; Faccio, Masulis, and McConnell 2006; Aghion et al. 2009; Cohen, Coval, and Malloy 2011; Duchin and Sosyura 2012, Goldman, Rocholl, and So 2013; Adelino and Dinc 2014; Tahoun 2014; Tahoun and van Lent 2019; Schoenherr 2019; Brogaard et al. 2020). We add to these literatures by emphasizing the role of election-year politics and political

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polarization in the government's response to the historic Covid-19 crisis and its economic consequences.

To investigate the role of electoral politics in the allocation of government funds, we introduce novel measures of states', districts' and industries' political importance in an election year. Our main hypothesis is that electoral political considerations tilted the allocation of PPP funds by the Trump administration towards firms in areas or industries that can have a significant impact on the results of the 2020 elections. The first set of measures aims to identify battleground states, congressional districts, and sectors. Prior research shows that presidential campaigns strategically concentrate their resource allocation in battleground areas (e.g., Bartels 1985; Shaw 1999; James and Lawson 1999; Shachar and Nale-buff 1999; Panagopoulos 2006; Shaw 2008; Akey, Dobridge, Heimer, and Lewellen 2018). We extend this research by studying battleground allocation of emergency government funds in an election year.

To identify battleground states, we collect detailed data on political ad expenditures by the Trump campaign and by third parties, which are collectively higher in states with more competitive elections. In particular, we collect data on political ad spending on Facebook, and measure the proportion of the Trump campaign's Facebook ad spending across states. We also collect data published by the Federal Election Commission (FEC) on state spending by third parties, which are not affiliated with any candidate, and measure the percent of third-party funding in opposition to Donald Trump's presidential campaign in each state. A combined higher proportion corresponds to more competitive ad spending, indicating that the state is perceived as more important by the Trump reelection campaign and by third-party political operatives.

To identify battleground congressional districts, we use the most recent Partisan Voting Index (PVI) produced by the Cook Political Report. The PVI uses data from the last two presidential

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elections to determine the voting performance of a congressional district relative to the national average. Battleground districts are those with a PVI between D+10 and R+10. Lastly, we identify battleground sectors based on the partisan industry classification of Gimpel, Lee and Parrott (2014, henceforth GLP), which uses a decade of campaign contributions to congressional candidates by corporations and trade associations.

The second set of measures aims to identify strategic political favoritism. According to this view, the combination of identity politics and strident political polarization gives rise to a strategic motive to allocate resources disproportionately to subgroups associated with the party's base since the outcome of elections is largely determined by the ability of politicians to mobilize base voters rather than swing or opposition voters.[2] These analyses extend existing research on political favoritism in the allocation of non-emergency government funds in the U.S. outside election years (e.g., Grossman 1994; Larcinese, Rizzo, and Testa 2006; Berry, Burden, and Howell 2010).

To measure strategic political favoritism at the state level, we use the most recent version of the Cook Political National Report preceding the passage of the CARES Act (March 9, 2020). This report categorizes states according to their likely voting outcome in the 2020 presidential election. We classify a state as Republican if it is identified as "Likely Republican" or "Solidly Republican." At the congressional district level, we classify a district as Republican if the PVI is greater than R+10. At the industry-level, we identify Republican sectors as those in the top tercile of Republican leaning according to GLP.

In the first set of analyses, we investigate the determinants of the allocation of PPP loans across states, districts, and sectors in the U.S. Since the allocation of PPP loans is an equilibrium outcome of both supply and demand, the analyses consider the demand for PPP loans by controlling for

[2] See, for example, Bernstein (2005) and Brown-Dean (2019) for an overview of identity politics and its rise in the United States.

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state- or sector-level applications for PPP loans. The analyses are also adjusted for population size or aggregate eligible payroll, depending on data availability, because the PPP's primary focus is on supporting employment through businesses' payroll expenses.

At the state-level, we find that battleground states and Republican states receive more PPP capital. Adjusted for a state's aggregate eligible payroll expenses, an increase of one standard deviation in battleground political ad spending corresponds to an increase of 2.9 percentage points in the allocation of PPP loans, or an increase of 4.7% relative to sample mean. Furthermore, Republican states receive 9.6 percentage points more PPP capital compared to other states, or 15.4% more relative to the sample mean. We find similar results at the congressional district and sector levels. On a per capita basis, electorally important districts – battleground and Republican districts – receive 20% and 12.7% more PPP loans, respectively, compared to other districts. Similarly, scaled by total eligible payroll, electorally important sectors receive roughly 30% more PPP loans relative to the sample mean.

These effects hold jointly, are highly statistically significant, and persist after controlling for population size, the number of confirmed Covid-19 cases, unemployment claims at the onset of the Covid-19 crisis, state-level gross domestic product (GDP) growth rates just before the onset of the crisis, and the presence of banks with historical ties to flagship SBA loan programs. The findings also hold for an aggregate index of electoral importance that combines the individual measures. We also investigate the demand for PPP loans and show that it does not vary with electoral importance, suggesting that credit demand is not driving the effects. Collectively, these estimates suggest that electoral politics plays an important role in the provision of emergency government funding during an election year, highlighting the strategic importance of both swing and base voters.

We also consider the hypothesis that the effects are exacerbated by the loose monitoring and continuously changing terms of the PPP.[3] To test this hypothesis, we exploit the staggered implementation of the PPP. We argue that the public outcry that followed the initial stages of the PPP led to an increase in scrutiny and public attention to the PPP between the first and second rounds of the program.[4] Consequently, we expect the effect of electoral politics on credit provision to weaken between the rounds. Consistent with this hypothesis, the estimates show that electoral politics plays a weaker role in the second round of the PPP. The effect of electoral politics on the allocation of second round PPP loans is economically small and mostly statistically insignificant.

Overall, this evidence is less consistent with the view that the measures of electoral importance capture credit demand or underlying economic conditions correlated with the allocation of the PPP, such as the economic exposure of states to the Covid-19 crisis, which a-priori should not change between the two rounds of the PPP.

In the second set of analyses, we provide evidence on the real economic effects of the allocation of PPP loans. First, we provide two-stage-least-squares (2SLS) estimates using data from the Small Business Pulse Survey (SBPS).[5] The survey is conducted by the U.S. Census Bureau and provides high-frequency information on the impact of Covid-19 on small businesses and on the participation of small businesses in government programs such as the PPP. In the first-stage regression, we predict the allocation of PPP loans using the measures of electoral importance. In the second-stage regressions, we investigate the effects of the predicted PPP allocation on the reported economic impact of Covid-19 on small businesses.

[3] See, for example: "House Passes Bill Loosening Rules on PPP Small-Business Loans" by Natalie Andrews and Amara Omeokwe, https://www.wsj.com/articles/community-lenders-to-get-10-billion-of-ppp-small-business-loans-11590678108.
[4] See, for example, "Ruth's Chris to Repay Loan Amid Outcry Over Rescue Program" by Peter Rudegeair, Heather Haddon, and Ruth Simon, https://www.wsj.com/articles/public-companies-have-to-repay-small-business-rescue-loans-11587670442
[5] See: https://portal.census.gov/pulse/data/#about for a detailed description of this survey.

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The 2SLS estimates suggest that the election-year allocation of PPP loans mitigates the negative effects of Covid-19 on small business activity and employment. The estimated effects are statistically significant and economically meaningful. An increase of 10% in predicted PPP allocation corresponds to a decrease of 8.5% in the percentage of survey respondents who report a negative effect of Covid-19 on their business and a decrease of 10% in the percentage of survey respondents who temporarily close. Similarly, it corresponds to a decline 11.2% in reported employment reductions. Overall, small businesses that received election-year PPP allocations are considerably more likely to expect a quick return to normal operations.

Second, we provide estimates from difference-in-differences tests of business applications and employment, where the first difference is between electorally important and all other states or districts and the second difference is before versus after the onset of the first round of the PPP. The estimates suggest that following the onset of the PPP, the decline in business applications was attenuated by 2.79-9.51% in electorally important regions. Further, the increase in unemployment was attenuated by 16.86% and the declines in aggregate employment and employment per capita were attenuated by 5.3% and 1.26%, respectively. These effects hold after controlling for state, week, or month fixed effects, as well as the interactions of the PPP time indicator with loan demand, population size, GDP growth rate, and the presence of SBA banks. In contrast, we do not find significant effects in placebo tests around the announcement of a national public health emergency before the onset of the PPP.

Collectively, our findings suggest that election-year political considerations tilted the allocation of emergency government funds in response to the Covid-19 crisis towards businesses in electorally important states, districts, and industries. Our results add to the literature pioneered by Stigler (1971) and Peltzman (1976) that studies how politics influences economic policy. These allocational tilts have important real effects on business activity and employment, which could impact the results of the 2020 elections.

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2. The Paycheck Protection Program (PPP)

The Coronavirus Aid, Relief, and Economic Security (CARES) Act was passed by Congress with overwhelming, bipartisan support and signed into law by President Trump on March 27th, 2020. In total, the CARES Act designated over $2 trillion dollars to combat the adverse economic impact of the Covid-19 pandemic, amounting to 10% of total U.S. gross domestic product (GDP), making it the largest economic relief package in the history of the United States.

The Paycheck Protection Program (PPP) is a centerpiece $659 billion business loan program established by section 1102 of the CARES Act, which authorized the Small Business Administration (SBA) to distribute loans to support payroll and overhead expenses to eligible small businesses through its nationwide network of lenders. Lenders who already participated in the SBA's flagship 7(a) program were automatically eligible to disburse PPP loans, while other lenders had to obtain authorization from the SBA.

Each PPP loan is guaranteed by the SBA and loan applicants did not need to provide any collateral or personal guarantees to apply or be approved for a PPP loan. Participating lenders earned an upfront origination fee proportional to the amount of the loan: 5% for loans under $350k, 3% for loans between $350k and $2 million, and 1% for loans above $2 million.

The PPP focuses on small businesses, and, as such, eligibility for the PPP is based on the existing statutory and regulatory definition of a "small business concern" under section 3 of the Small Business Act, 15 U.S.C. 632. A business can qualify if it meets the SBA employee-based or revenue-based small business size standard corresponding to its primary industry. Alternatively, a business can qualify for the PPP if it meets the SBA's "alternative size standard," which requires a maximum tangible net worth of $15 million and maximum average net income for the two full fiscal years before the date of the application of $5 million.

The terms of PPP loans are highly attractive for the borrower. First, the principal of a PPP loan can be either partially or fully forgiven based on the usage of the loan proceeds. Second, even if not forgiven, PPP loans carry a low interest rate of one percent. Third, both the principal and interest payments are deferred until the loan is forgiven or, if the borrower does not apply for loan forgiveness, ten months after the end of the 24-week cover period.[6] Consequently, millions of businesses in the U.S immediately applied for PPP loans, which were accepted, approved, and disbursed on a first-come first-served basis, leading to credit rationing and generating a setting susceptible to political favoritism.[7]

The first round of the PPP commenced on April 3, 2020 amidst government-mandated lockdowns in many states. Within 2 weeks, on April 16, 2020, the entire first round of $349 billion was depleted and the SBA stopped accepting new applications from lenders.[8] A bill to add $310 billion of funding was passed by Congress and signed into law by President Trump on April 24, and the SBA began accepting new applications from lenders on April 27. The PPP was due to expire at midnight on June 30 with funds remaining, but just hours before the expiration of the program Congress authorized an extension through August 8. This date passed without a second extension to the program, with the result that applying to the program is no longer possible. By the end of the program, the SBA disbursed $525 billion of the $659 billion appropriated by Congress to this program. These numbers indicate stark differences in the demand for loans between the two rounds of the PPP: First-round PPP capital was quickly depleted, whereas second-round PPP

[6] The SBA initially required that at least 75% of the loan be used for payroll, rent, mortgage interest, and utilities to be forgiven at the end of 8 weeks. On June 5, President Trump signed the PPP Flexibility Act, which reduced the proportion needed to be spent on payroll to 60% and extended the time period to use the funds from 8 to 24 weeks.
[7] While the SBA did not release information about the number of PPP applications or application approval rates, it reported a total of 4.67 million loans disbursed by June 20, 2020.
[8] See, for example, the article "Small business rescue loan program hits $349 billion limit and is now out of money," by Thomas Franck and Kate Rogers, published on CNBC on April 16: https://www.cnbc.com/2020/04/16/small-business-rescue-loan-program-hits-349-billion-limit-and-is-now-out-of-money.html

capital exceeded aggregate demand. Hence, the differences between the two rounds provide a natural setting to study the relation between political favoritism and credit rationing. [9]

Moreover, we conjecture that in addition to lower demand, the second round of the PPP was also accompanied by more stringent oversight, potentially reducing the scope for political favoritism in loan allocation. In particular, the first round was followed by public outcry surrounding the participation of large or public firms in the first round of the PPP.[10] Moreover, several lawsuits brought against J.P. Morgan Chase, Wells Fargo, Bank of America, and U.S. Bank by a range of California small businesses further alleged that the banks unfairly prioritized their large customers.[11] In a press briefing on April 22, 2020, Treasury Secretary Mnuchin warned of "severe consequences" for large businesses that received PPP funds.[12] Following Mnuchin's press briefing, the SBA instituted a "safe harbor" for the return of PPP funds by large businesses, and on April 28, the Treasury and SBA issued a joint statement that they would retroactively examine all loans over $2 million to certify that program qualifications were met.[13] We therefore hypothesize that the apparent differences in both demand and oversight between the two rounds of the PPP provide a natural backdrop against which to examine the impact of electoral politics on government funding amid changing oversight credit rationing conditions.

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[9] See "Tracker: Paycheck Protection Program Loans," by Thomas Wade: https://www.americanactionforum.org/research/tracker-paycheck-protection-program-loans/

[10] See, for example, the article "At Least 30 Public Companies Say They Will Keep PPP Loans," by Inti Pacheco, published by the Wall Street Journal on May 19, 2020: https://www.wsj.com/articles/at-least-30-public-companies-say-they-will-keep-ppp-loans-11589891223

[11] See, for example, the article "Chase and other banks shuffled Paycheck Protection Program small business applications, lawsuit says," by Dalvin Brown, published in USA Today on April 20: https://www.usatoday.com/story/money/2020/04/20/small-businesses-sue-chase-bank-over-handling-stimulus/5163654002/. For further details on the lawsuits, pleas see: https://www.classaction.org/news/class-actions-say-wells-fargo-jpmorgan-chase-held-back-small-businesses-paycheck-protection-program-funds

[12] See https://www.businessinsider.com/treasury-mnuchin-consequences-big-companies-taking-ppp-small-business-loans-2020-4

[13] See https://factba.se/sba-loans for the list of public PPP borrowers, including those that subsequently returned the funds. The full PPP loan-level data can be found here: https://home.treasury.gov/policy-issues/cares-act/assistance-for-small-businesses/sba-paycheck-protection-program-loan-level-data.

3. Data and Variables

In this section, we describe our data sources and the construction of the variables used in the analyses. We begin by describing the measures of PPP loan allocation at the congressional district, state, and sector levels. We then describe our measures of electoral importance across districts, states, and sectors. We conclude by describing measures of credit demand, credit supply, and local economic conditions.

3.1. The Allocation of PPP Loans

We measure the allocation of PPP loans across congressional districts, states, and sectors, and scale it by aggregate measures of eligible payroll when available because the primary goal of the PPP was to support payroll expenses. These data come from the SBA, which provides detailed data on PPP loans, and from the Statistics of U.S. Businesses (SUSB), which provides detailed payroll data.

To study the allocation of PPP loans across states and sectors, we use aggregate state-level and sector-level loan data released by the SBA for each week of the PPP. We use the SUSB payroll data to estimate the total amount of payroll that is eligible for PPP funds within a particular state or sector. In particular, we use the latest edition of the SUSB (2017) and calculate the total annual payroll for all firms in NAICS sector 72 (Accommodation and Food Services) and for all firms with 500 employees or fewer in all other sectors.[14] Next, we aggregate these totals at the state or sector level (adjusted to 2019 dollars) and divide them by 12 to calculate the aggregate monthly payroll. Lastly, we multiply the monthly payrolls by 2.5 to approximate the procedure used by the SBA to determine maximum PPP loan amounts, which aim to cover 2.5 months of payroll expenses.

[14] Firms in the Accommodation and Food Services were exempt from the 500-employee PPP eligibility cap.

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Table 1: Summary Statistics

PPP Funding	Obs	Mean	p25	p50	p75	SD
State						
First-round PPP ($millions)	50	$6,803.89	$2,006.86	$4,465.70	$8,721.17	$6,852.59
Second-round PPP ($millions)	50	$3,357.13	$522.36	$1,672.79	$3,790.20	$5,508.32
Total PPP ($millions)	50	$10,161.02	$2,519.39	$6,359.24	$12,249.02	$12,013.29
First-round PPP/Elig. Payroll	50	0.625	0.54	0.623	0.711	0.121
Second-round PPP/Elig. payroll	50	0.222	0.154	0.202	0.275	0.081
Total PPP/Elig. Payroll	50	0.848	0.814	0.85	0.882	0.066
District						
First-round PPP per capita	428	4.908	2.816	4.64	6.68	2.667
Second-round PPP per capita	428	9.256	5.835	8.117	11.378	5.196
Sector						
First-round PPP/Elig. Payroll	18	0.547	0.438	0.529	0.665	0.184
Second-round PPP/Elig. payroll	18	0.251	0.179	0.266	0.319	0.097
Political Measures	Obs	Mean	p25	p50	p75	SD
State						
Trump Facebook ad share	50	0.02	0.007	0.017	0.025	0.019
Third-party spend share	50	0.008	0	0	0.001	0.032
Battleground state	50	0.018	0.006	0.013	0.02	0.021
Republican state	50	0.42	0	0	1	0.499
Electorally important state	50	0.433	0.333	0.333	0.667	0.263
District						
Republican district	428	0.287	0	0	1	0.453
Battleground district	428	0.444	0	0	1	0.497
Electorally important district	428	0.731	0	1	1	0.444
Sector						
Battleground sector	18	0.333	0	0	1	0.485
Republican sector	18	0.278	0	0	1	0.461
Electorally important sector	18	0.611	0	1	1	0.502
Local Economic Conditions	Obs	Mean	p25	p50	p75	SD
State						
Eligible payroll ($millions)	50	$12,937.76	$3,210.72	$7,982.23	$16,135.03	$15,923.70
Ln(population)	50	15.206	14.399	15.332	15.846	1.025
Unem. per capita (04/04/2020)	50	0.044	0.032	0.04	0.055	0.017
GDP growth	50	0.02	0.016	0.021	0.024	0.008
% Small SBA lenders	50	0.106	0.041	0.075	0.177	0.081
Ln(Covid-19 cases) (04/03/2020)	50	7.4	6.292	7.341	8.5	1.467
District						
Ln(population)	428	13.515	13.492	13.513	13.539	0.049
Unem. Rate	428	0.038	0.031	0.036	0.042	0.009
GDP growth	428	0.028	0.019	0.027	0.034	0.015
% Small SBA lenders	428	0.07	0.019	0.042	0.101	0.074
Ln(Covid-19 cases) (04/03/2020)	428	5.236	3.923	5.312	6.627	1.999
Survey Responses	Obs	Mean	p25	p50	p75	SD
State						
% Applied to PPP (04/30/2020)	50	0.736	0.713	0.743	0.772	0.044
Neg. effect on business	50	0.475	0.428	0.47	0.515	0.076
Temp. closed business	50	0.381	0.327	0.37	0.433	0.085
Return to normal <= 1 month	50	0.031	0	0.035	0.051	0.027
Return to normal > 6 month	50	0.296	0.262	0.296	0.332	0.051
Sector						
% Applied to PPP (04/30/2020)	18	0.72	0.625	0.744	0.827	0.169
Real Effects Variables (per capita	Obs	Mean	p25	p50	p75	SD
Total business applications	850	0.21	0.15	0.186	0.232	0.103
Corporation applications	850	0.024	0.012	0.016	0.024	0.022
High propensity applications	850	0.071	0.051	0.063	0.078	0.034
Continued unem. Claims	800	0.029	0.006	0.015	0.047	0.028
Employment	2,764	0.026	0.008	0.019	0.042	0.021

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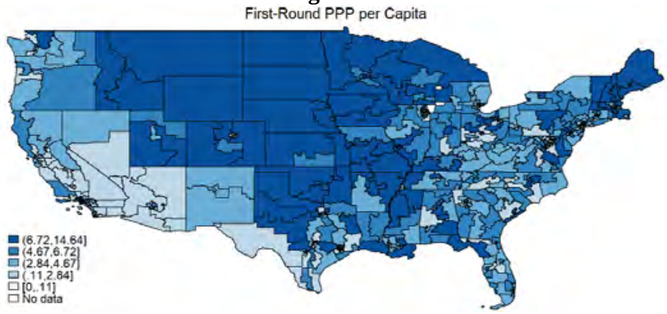
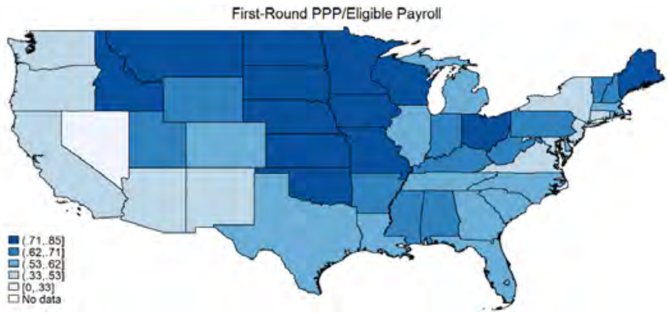
Figure 1: The Allocation of PPP Loans across Congressional Districts, States, and Sectors

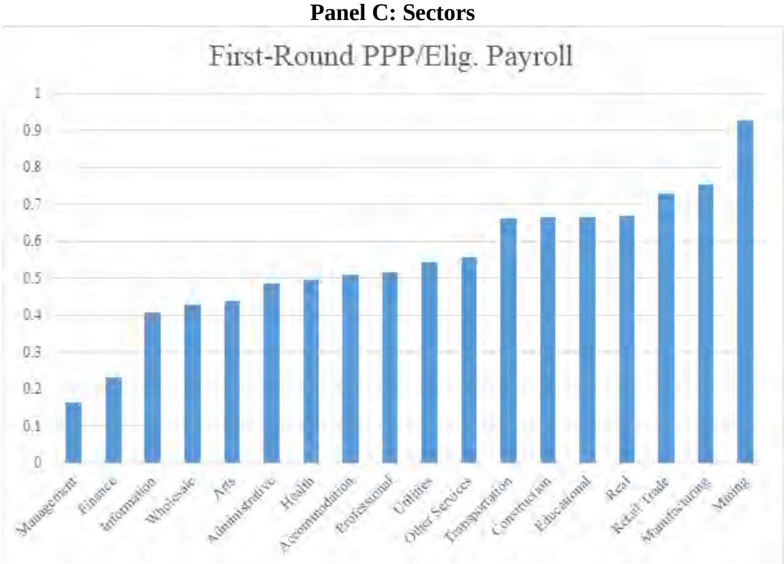
Panel A: Congressional Districts



Panel B: States



Panel C: Sectors



This figure shows the allocation of first-round PPP loans across congressional districts, states, and sectors in the United States. Panel A reports the number of first-round loans per capita across congressional districts. Panel B reports aggregate loan amounts scaled by eligible payroll across states. Panel C reports aggregate loan amounts scaled by eligible payroll across sectors, defined based on 2-digit NAICS.

To study the allocation of PPP loans across congressional districts, we cannot use aggregate loan data because they are only available at the state and sector levels. Instead, we use loan-level data released by the SBA on 7/6/2020. The availability of these data, however, varies by the amount of the loan. For loans over $150k, the data include the name, address, 6-digit NAICS industry, business type (sole proprietorship, corporation, etc.), number of jobs retained, and a range for the loan amount. The ranges for the loan amounts are as follows: $150-$350k, $350k-$1mil, $1mil-$2mil, $2mil-$5mil, and $5mil-$10mil. For loans under $150k, the name of the business is suppressed, and the loan amount is exact. Since loan amounts are imprecise and district-level payroll data are largely unavailable, we measure the allocation of PPP loans across congressional districts based on the number of loans scaled by the size of the population.

Table 1 shows that, on average, nearly 63% of state-level eligible payroll, and 55% of sector-level eligible payroll, were covered by the first round of PPP loans. At the district level, the average number of first-round PPP loans per 1,000 residents was 4.908, with a median of 4.64. The allocation of PPP loans is also depicted in Figure 1. The heat maps in Figure 1 show substantial variation in the allocation of PPP loans across districts (Panel A) and states (Panel B). In particular, Panel B shows that states in the Midwest and South received more PPP funding relative to their eligible payroll, while states on the coasts received relatively less. Panel C of Figure 1 shows the nontrivial variation in the allocation of PPP across sectors.

3.2. Electoral Importance

In this sub-section we describe our measures of electoral importance. We measure electoral importance via base (*Republican*) and swing (*Battleground*) voters across congressional districts,

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states, and sectors. Each unit of analysis utilizes unique data sources to quantify the extent to which districts, states, and sectors are important for the outcome of the 2020 presidential election.

Republican Districts, States, and Sectors

To measure the support of congressional districts for the Republican party, we utilize the Partisan Voting Index (PVI) provided by the Cook Political Report. The PVI measures how each congressional district's presidential voting results compare to the national average based on the previous two presidential elections. For example, a PVI value of R+10 indicates that the district voted 10 points more Republican in the 2012 and 2016 elections, on average, than the national average. We use the latest edition of the PVI as of 2017.[15] We define an indicator variable, *Republican district,* which equals one if the PVI is greater than R+10. Table 1 shows that roughly 29% of the congressional districts are *Republican districts*.

To measure the support for the Republican party across states, we use the most recent version of the Cook Political Report preceding the passage of the CARES Act (March 9, 2020). This report categorizes states according to their likely voting outcome in the 2020 presidential election. We define an indicator variable, *Republican state,* which equals one if a state is identified as "Likely Republican" or "Solidly Republican," and zero otherwise. As shown in Table 1, 42% of the states are *Republican states* based on the above definition.

We identify Republican 2-digit NAICS sectors using the partisan classification of Gimpel, Lee and Parrott (henceforth GLP) (2014). GLP examine a decade of campaign contributions made by corporations' and trade associations' political action committees to congressional candidates, and pinpoint which industries have a measurable preference for a particular political party. Importantly, their method identifies industries' political preferences after controlling for other

[15] See https://cookpolitical.com/introducing-2017-cook-political-report-partisan-voter-index for a detailed description of the PVI.

factors that likely drive campaign contributions, including parties' majority control of Congress, committee memberships, and the competitiveness of congressional seats. GLP then aggregate industries to the 2-digit NAICS level, and compute the percent of the industries within each 2-digit NAICS sector that favor a particular party (See GLP, Table 2).

We define an indicator variable, *Republican sector*, which equals one for sectors in the top tercile on Republican leanings. We provide a list of *Republican sectors* in Appendix Table B. Importantly, GLP do not report data on the Construction sector, which is a major participant in the PPP. Therefore, we augment the GLP data with contributions data by sector from the Center for Responsive Politics, and note that the Construction sector gave roughly 70% of its contributions to Republican candidates in the 2018 election cycle. We therefore classify the Construction sector as Republican, and note that our results are not sensitive to this inclusion. The estimates in Table 1 suggest that roughly 28% of the 2-digiti NAICS sectors are *Republican sectors*.

<u>Battleground Districts, States, and Sectors</u>

We identify battleground congressional districts using the PVI. In 2016, 23 districts with Republican representatives voted for Hillary Clinton with margins ranging from 0.6 to 19.7. On the other hand, 12 districts with Democrat representatives voted for Donald Trump with margins ranging from 0.7 to 30.8. To accommodate the wide range of battleground districts, we define an indicator variable, *Battleground district*, which equals one if the PVI is between D+10 and R+10. This definition provides a sufficient range to capture the congressional districts that are most "up for grabs" in the 2020 presidential elections. Table 1 shows that roughly 44% of congressional districts are *Battleground districts.* The variation in electoral importance across congressional districts is depicted in Panel A of Figure 2, which presents a heat map of the PVI across districts.

To identify battleground states, we calculate the proportion of both Facebook ad spending by the Trump campaign and expenditures by third-party political operatives in support of and in opposition to Donald Trump. This measure captures the level of competition in political ad spending. Moreover, the perceived electoral importance of states, as captured by the revealed preferences of political campaigns and operatives, is likely a potent instrument for electoral importance since it drives allocative decisions. We define the variable *Battleground state* as the share of Trump Facebook ad spending and third-party ad spending in each state.

We note that advertising spending in the 2020 presidential race has already reached unprecedented levels. According to the Media Project at Wesleyan University, total spending for the 2020 presidential elections had already eclipsed 2016 spending by Feb. 23, 2020. Further, digital political advertising is becoming increasingly important. Industry experts project an increase of 203% relative to 2016 for political digital marketing expenditures, versus an 82% increase for traditional TV advertising. Facebook has become the preferred digital platform for all politicians since it allows targeted advertising of individuals based on geographic location.[16]

Digital advertising is particularly important for the Trump campaign, which has devoted 70% of its advertising resources to digital advertising, of which the majority went to Facebook.[17] We estimate the variable *Trump Facebook ad share* using data provided by the Facebook Transparency Project as compiled by the Campaign 2020 Tracker.[18] It is defined as the share of the Trump campaign's total Facebook ad spending that goes to a particular state from March 30, 2019 (when the data is first available) to March 31, 2020. The estimates in Table 1 show that the average and median *Trump Facebook ad share* are 2% and 1.7%, respectively. There is considerable variation

[16] See: "Facebook accounts for 60% of all digital political advertising," https://www.emarketer.com/newsroom/index.php/us-political-ad-spending-to-hit-record-high/
[17] See the Wesleyan Media Project: http://mediaproject.wesleyan.edu/releases-022620/
[18] CampaignTracker 2020 data: https://2020campaigntracker.com/

across states, however. The minimum share is 0.2%, the maximum share is 9.1%, and the standard deviation across states equals 1.9%.

We collect data on third-party (independent) political expenditures from the Federal Elections Commission (FEC). The Code of Federal Regulations (CFR) defines an independent expenditure as "an expenditure by a person for a communication expressly advocating the election or defeat of a clearly identified candidate that is not made in cooperation, consultation, or concert with, or at the request or suggestion of, a candidate, a candidate's authorized committee, or their agents, or a political party committee or its agents." The FEC requires independent expenditures to be reported within 24-48 hours, and records the name of the spender, the location (state) of the spending, and whether it is in support of, or in opposition to, a particular candidate.

We calculate third-party political expenditures, *Third-party spend share*, as the state share of total independent expenditures supporting or opposing Donald Trump from January 2019 to March 2020. In the 2020 election cycle, the majority of third-party ad expenditures were in opposition to Donald Trump (roughly $23 million compared to $16 million). This measure captures the relative focus of third-party political operatives on a particular state. Table 1 shows that the median *Third-party spend share* is 0, indicating that most states did not have third-party political expenditures expressly supporting or opposing Donald Trump. The variation across states is nontrivial, however, with a maximum share of 19.2% and a standard deviation of 3.2%. Overall, third-party political operatives spent money in 19 states in the leadup to the PPP.

Lastly, we identify battleground 2-digit NAICS sectors using data from GLP (2014). We define an indicator variable, *Battleground sector*, which equals one for sectors whose Republican leanings are in the middle tercile and zero otherwise. A list of battleground sectors can be found in Appendix Table B.

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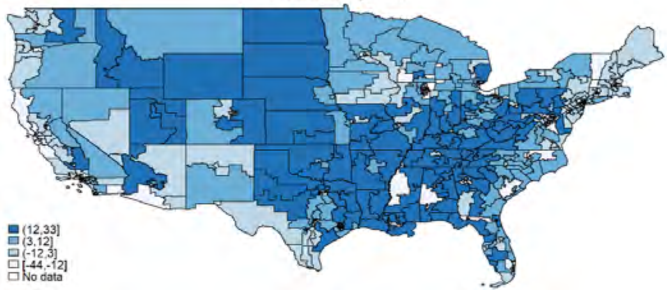
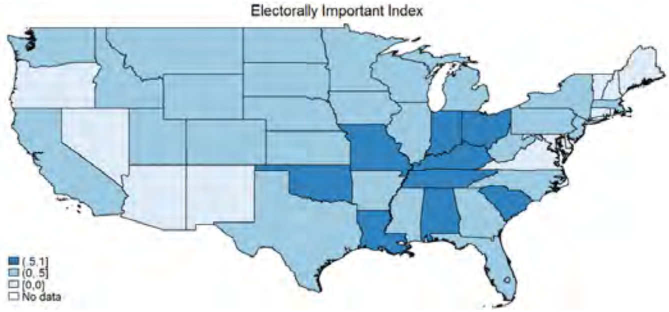
Figure 2: The Electoral Importance of Congressional Districts and States Sectors

Panel A: Congressional Districts



Panel B: States



This figure shows the electoral importance of congressional districts, states, and sectors for the 2020 presidential elections in the United States. Panel A reports the Partisan Voting Index across congressional districts. Panel B reports the *Electorally important* index, defined based on relative political ad spending and the *Republican* dummy, across states.

Electorally Important Index

We also construct a composite index of electoral importance at the district, state, and sector levels by combining the above elements. We define districts and sectors as *Electorally important* if they are either Republican or battleground districts/sectors. Similarly, we define states as *Electorally important* if they are either battleground or Republican after transforming the continuous variable *Battleground state* into an indicator variable that equals one if it is above the sample median, and taking the average of the two variables. Panel B of Figure 2 presents a heat map of the variation in the *Electorally important* index across states.

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3.3. Local Supply and Demand of PPP Loans

To measure the local supply of PPP loans, we collect data on SBA 7(a) lenders, which, as noted above, were immediately eligible to disburse PPP loans. In particular, we hand-match comprehensive SBA 7(a) loan data as of Dec. 31, 2019 to bank branch locations from the FDIC Summary of Deposits database, and compute the proportion of local (state or district) branches operated by SBA banks. We conjecture that access to PPP loans was easier in areas with greater presence of SBA lenders, especially in the first round. Furthermore, both anecdotal and academic evidence suggest that bank size played a role in access to PPP loans (e.g., Granja et al., 2020; Liu and Volker, 2020). Specifically, small, community banks were better able to navigate the labor-intensive PPP application system and obtain funds for their clients. Hence, we proxy for the local supply of PPP loans using the presence of small SBA banks (<$1 billion in assets). Table 1 shows that the average share of small SBA banks is 10.6% across states and 7.0% across districts, with large variation across both states and sectors (standard deviations = 8.1% and 7.4%, respectively).

To measure the demand for PPP loans, we utilize survey data provided by the Census Bureau. These data come from the Small Business Pulse Survey (SBPS), which was initiated to track the effects of the coronavirus and subsequent government interventions on small businesses. The target population for the survey is all nonfarm, single-location businesses with less than 499 employees. Although the sample for the SBPS is not a random sample, weights are applied to ensure that each weekly sample represents the full population of businesses. The SBPS conducts weekly email surveys that began on 4/26. We focus on the first survey, which catalogued responses as of 4/30. Our proxy for loan demand is the percent of state or sector respondents that reported applying to the PPP since 3/13/2020.[19]

Table 1 shows that an average of 73.6% of state survey respondents applied to the PPP, suggesting that a majority of small businesses in the United States applied for government aid. The interquartile range for PPP demand across the states is relatively small. The 25th percentile equals 71.3%, and the 75th percentile equals 77.2%. This lack of variation in the demand across states provides suggestive evidence that the variation in our political measures across states does not simply proxy for state-level demand for PPP loans. Similarly, an average of 72% of industry respondents applied to the PPP.

3.4. Economic Conditions

To control for the economic conditions within a particular congressional district or state, we supplement our analyses with various local economic indicators. At the district level, we include the weighted county-level unemployment rate as of 2019, GDP growth rates as of 2018, and the natural log of the population size, where weights are determined by the proportion of a district's population that resides in a particular county.

At the state level, we include GDP growth rates as of the 4th quarter of 2019, unemployment claims per capita as of the beginning of the first round of the PPP, and the natural log of the population size. Data on GDP come from the Bureau of Economic Analysis (BEA). Data on unemployment claims and rates come from the Bureau of Labor Statistics (BLS). Population data come from the Census Bureau. All variables represent the latest available data before the beginning of the first round of the PPP. Table 1 provides summary statistics for the above measures of local economic conditions across states and congressional districts.

[19] When analyzing the second round of the PPP, we use the most recent survey week (as of 6/25) to proxy for demand.

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We also control for the local exposure to Covid-19 by including the natural log of the number of Covid-19 cases as of the beginning of the PPP. These data are provided by USA Facts.

Lastly, we analyze the real effects of the allocation of PPP loans on business applications and unemployment claims. Data on weekly business applications come from the Census Bureau. These data report applications by businesses for an Employee Identification Number (EIN), and are divided into three buckets: total applications, corporate applications, and high-propensity business applications. Data on monthly sector employment by state come from the BLS. All variable definitions can be found in Appendix A.

4. Results

We begin the empirical analyses by investigating the role of electoral importance in the allocation of PPP loans across congressional districts, states, and sectors. We then examine the variation in the demand for PPP loans and the differences between the first and second rounds of the PPP. We conclude this section with an investigation of the real effects of the allocation of PPP loans on business activity and employment using both survey evidence and observational data on local economic conditions.

4.1. Electoral Importance and the Allocation of PPP Loans

To investigate the role of electoral importance in the allocation of PPP loans, Table 2 presents estimates from cross-sectional regressions explaining the allocation of PPP loans across congressional districts (columns 1-2), states (columns 3-4), and sectors (columns 5-6). These regressions focus on the allocation of PPP loans in the first round of the program, when credit was rationed and before the public outcry that led to more scrutiny and monitoring. Section 4.3 below provides evidence on the second round of the PPP.

Since aggregate PPP loan volume data and accurate payroll information are unavailable or noisy for congressional districts, we measure the allocation of PPP loans across districts by the number of loans scaled by the size of the population (columns 1-2). These data are available for states and sectors; hence, the dependent variables in columns 3-6 are the dollar volume of PPP loans scaled by aggregate eligible payroll.

The main variables of interest in the regressions are *Republican*, *Battleground*, and *Electorally important*, which measure the electoral importance of congressional districts, states, and sectors based on their relative support for the incumbent administration (base voters), their electoral competitiveness (swing voters), and the combination of the two, respectively. Depending on data availability at the district, state, and sector levels, the regressions control for PPP loan demand based on the SBPS (*% Applied to PPP*), local economic conditions (*Unemployment, GDP growth*), exposure to the Covid-19 crisis (*Ln(Covid 19 cases)*), the availability of PPP lenders (*% Small SBA lenders*), and population size (*Ln(Population)*).

We begin with an analysis of the allocation of PPP loans across congressional districts in columns 1 and 2 of Table 2. Column 1 provides estimates for the individual measures *Republican* and *Battleground*, whereas column 2 focuses on the composite index *Electorally important*. The estimates in columns 1 and 2 suggest that electoral importance played an important role in the allocation of PPP loans across districts. Republican districts and battleground districts received a higher number of PPP loans, scaled by the size of the population. These effects are statistically significant at the 1% and 5% levels, respectively, and hold after controlling for loan demand, local economic conditions, and the availability of PPP lenders. The economic magnitude of the effects is nontrivial. Relative to Democratic districts, Republican districts received 12.78% more loans per capita in the first round of the PPP, and battleground districts received 20.01% more loans.

Overall, the estimates in column 2 show that electorally important districts received 14.51% more loans per capita in the first round of the PPP.

Table 2: The Allocation of PPP Loans

Obs. Level	Cong. District	Cong. District	State	State	Sector	Sector
Column	(1)	(2)	(3)	(4)	(5)	(6)
Republican	0.982***		0.096***		0.16	
	-2.725		-3.953		-1.496	
Battleground	0.627**		1.394***		0.169**	
	-2.068		-4.634		-2.53	
Electorally important		0.712**		0.150***		0.165**
		-2.401		-4.736		-2.52
% Applied to PPP	3.346	3.734	0.993***	1.021***	0.568**	0.0565**
	-0.658	-0.735	-3.344	-3.569	-2.37	-2.44
Unemployment	-81.321***	-81.480***	-0.016	-0.022**		
	(-6.809)	(-6.875)	(-1.464)	(-2.258)		
Ln(Covid 19 cases)	0.001	-0.024	-0.040**	-0.049***		
	-0.022	(-0.402)	(-2.427)	(-3.167)		
Ln(population)	-2.98	-2.743	-0.661	-0.89		
	(-1.121)	(-1.028)	(-0.878)	(-1.505)		
GDP growth	-18.921**	-18.721**	2.176**	2.317**		
	(-2.507)	(-2.443)	-2.215	-2.226		
% Small SBA lenders	13.339***	13.504***	0.710***	0.693***		
	-7.866	-7.85	-5.897	-5.529		
Constant	44.747	41.416	0.460*	0.631***	0.038	0.04
	-1.263	-1.163	-1.881	-2.768	-0.204	-0.223
Observations	428	428	50	50	18	18
R-squared	0.305	0.302	0.805	0.793	0.431	0.431

This table examines the effect of electoral importance on the allocation of first-round PPP loans across congressional districts (columns 1-2), states (columns 3-4), and sectors (columns 5-6). The dependent variable in columns 1-2 is the number of PPP loans in a district scaled by its population size. The dependent variable in columns 3-4 and 5-6 is the aggregate amount of PPP loans in a state or sector, respectively, scaled by eligible payroll. All variable definitions are given in Appendix A. Heteroskedasticity-robust t-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

We obtain similar results in columns 3 and 4, which study the allocation of PPP loans across states. The estimates suggest that electorally important states received a higher dollar volume of PPP loans relative to their aggregate levels of eligible payroll. In particular, the results indicate

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that in the first round of the program, Republican states received 15.36% more funding per eligible payroll than Democratic states, and battleground states received 4.68% more funding. Overall, based on column 4, electorally important states received 7.82% more funding than electorally unimportant states in the first round of the PPP. These findings are highly statistically significant at the 1% level.

Finally, columns 5 and 6 of Table 2 provide the results for the allocation of PPP loans across sectors. Despite the small number of observations (18 sectors), we find that electoral importance played a statistically significant role in the allocation of PPP funds in the first round of the program. The coefficient estimates show that battleground sectors received 30.9% more proportional funding than Democratic sectors. The coefficient on *Republican sectors* is positive and of similar magnitude to *Battleground sectors*, but is insignificant at conventional levels. The composite index of political importance (column 6) remains highly economically and statistically significant.

Taken together, the results in this section suggest that political favoritism in an election year operates through two distinct channels: base voters (*Republican*) and swing voters (*Battleground*). Given that voters focus on recent economic outcomes (e.g., Achen and Bartels (2004)), the results are consistent with the incumbent administration strategically tilting government funds towards areas and industries that could play an important role in the 2020 presidential election.

These effects can operate via several distinct mechanisms. First, the incumbent administration can pressure the SBA, formally or informally, to prioritize loan applications from electorally important districts, states, and sectors. Second, the lending financial institutions can themselves prioritize applications from electorally important borrowers to cater to the current administration. Lastly, the program design itself may inherently favor electorally important borrowers.

4.2. The Demand for PPP Loans

A possible concern with the analyses is that the political importance of districts, states, and sectors is correlated with the demand for PPP loans. Under this view, the role of electoral importance in the allocation of PPP loans is driven by the demand for loans rather than by political favoritism. We address this concern in several ways. First, the summary statistics in Table 1 show that there is little variation in the demand for loans across states and sectors. Second, the regressions in Table 2 explicitly control for the demand for loans. In this section, however, we also seek to provide direct evidence on the variation in the demand for loans across states and sectors (data on loan applications are unavailable for congressional districts.)

In Table 3, we estimate predictive regressions explaining the demand for PPP loans in the first round of the program across states (columns 1-2) and sectors (columns 3-4). The main takeaway from Table 3 is that electoral importance in unrelated to the demand for PPP loans. Across all four columns of Table 3, the estimates suggest that electoral importance is unrelated to the demand for loans in the first round of the PPP. The coefficient estimates on *Republican, Battleground, and Electorally important* are economically small, flip signs, and statistically insignificant at conventional levels. Moreover, columns 1 and 2 show that the demand for PPP loans across states is unrelated to any of the control variables, including local economic conditions and the exposure to the Covid-19 crisis. As expected, the only exception is the size of population, which is positively related to the aggregate demand for PPP loans.

Collectively, these results suggest that the effect of electoral importance on the allocation of PPP loans in the first round of the program is not driven by variation in the demand for PPP across electorally important states or sectors.

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Table 3: The Demand for PPP Loans

Dependent Variable	State Demand		Sector Demand	
Column	(1)	(2)	(3)	(4)
Republican	0.008		0.004	
	-0.62		-0.038	
Battleground	0.158		-0.053	
	-0.867		(-0.520)	
Electorally important		0.012		-0.027
		-0.76		(-0.317)
Ln(Covid-19 cases)	0.001	0.001		
	-0.18	-0.108		
Ln(population)	0.020*	0.020*		
	-1.732	-1.904		
Unemployment	0.42	0.41		
	-0.958	-1.063		
GDP growth	-0.483	-0.485		
	(-0.463)	(-0.487)		
% Small SBA lenders	-0.049	-0.049		
	(-0.656)	(-0.681)		
Constant	0.413**	0.421***	0.736***	0.736***
	-2.568	-3.06	-10.022	-10.35
Observations	50	50	18	18
R-squared	0.303	0.3	0.025	0.007

This table examines the effect of electoral importance on the demand for PPP loans across states (columns 1-2) and sectors (columns 3-4). The dependent variable is the percentage of Small Business Pulse Survey (SBPS) respondents in a state or sector that reported applying to the PPP by 4/30. All variable definitions are given in Appendix A. Heteroskedasticity-robust t-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

4.3. The Second Round of the PPP

The analyses thus far focused on the first round of the PPP. In this section, we investigate the role of electoral importance in the allocation of loans in the second round of the PPP. We conjecture that the effects of electoral importance on loan allocation are driven by loose monitoring and credit rationing. To test this conjecture, we exploit the differences between the two rounds of the PPP. We argue that the public outcry that followed the initial stages of the PPP led to an increase in

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scrutiny and public attention to the PPP in its second round. This claim is supported by numerous articles and actions taken by policy makers (see section 2 above). Furthermore, the supply of PPP loans exceeded the demand for loans in the second round, suggesting that credit was not rationed. Consequently, we expect the effect of electoral politics on credit provision to weaken between the two rounds.

To test this conjecture, Table 4 repeats the analyses of the allocation of PPP loans (Table 2) replacing the dependent variables with the allocation of loans in the second round of the PPP. We also replace the measures of PPP loan demand with analogous measures using SBPS data as of 6/25/2020.

Consistent with our hypothesis, the results in Table 4 suggest that electoral importance did not play a significant role in the allocation of loans in the second round of the PPPP across congressional districts, states, and sectors. In particular, the coefficient estimates on the different measures of electoral importance flip signs across specifications and are statistically insignificant in the majority of cases (8 out of the 10 cases). When significant (2 out of 10 cases), they have a negative sign.

Combined with the results on the allocation of loans in the first round of the PPP, these results have two important implications. First, they suggest that omitted variables correlated with the design of the PPP, which likely remained constant through both rounds of the PPP, cannot explain the effects of electoral importance on the allocation of loans. Second, they indicate that lax monitoring and credit rationing serve as key mechanisms and motivations in political favoritism. The loosening of credit conditions and the increase in monitoring and public scrutiny reduced the motivation and scope, respectively, for political favoritism in the allocation of loans in the second round of the PPP.

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Table 4: Second-Round Allocation of PPP Loans

Obs. Level	Cong. District	Cong. District	State	State	Sector	Sector
Column	(1)	(2)	(3)	(4)	(5)	(6)
Republican	0.399		-0.032		-0.062	
	-0.534		(-1.473)		(-1.481)	
Battleground	0.712		-1.078***		0.059	
	-1.053		(-5.441)		-1.382	
Electorally important		0.637		-0.064**		0.003
		-0.965		(-2.094)		-0.076
% Applied to PPP	-6.149	-6.491	-0.003*	-0.003*	0.004***	0.003**
	(-0.764)	(-0.811)	(-1.700)	(-1.786)	-3.875	-2.354
Unemployment	-0.963***	-0.961***	0.003	0.003		
	(-4.254)	(-4.237)	-0.221	-0.248		
Ln(Covid 19 cases)	0.904***	0.927***	0.038**	0.039**		
	-5.445	-6.073	-2.45	-2.259		
Ln(population)	1.83	1.622	0.311	0.329		
	-0.461	-0.41	-0.805	-0.971		
GDP growth	0.355**	0.353**	0.261	0.082		
	-2.166	-2.159	-0.271	-0.088		
% Small SBA lenders	-2.325	-2.471	-0.433***	-0.429***		
	(-0.840)	(-0.882)	(-4.930)	(-4.643)		
Constant	-13.223	-10.288	-0.124	-0.123	-0.005	0.024
	(-0.248)	(-0.194)	(-0.602)	(-0.571)	(-0.057)	-0.217
Observations	428	428	50	50	18	18
R-squared	0.189	0.188	0.666	0.643	0.535	0.293

This table examines the effect of electoral importance on the allocation of second-round PPP loans across congressional districts (columns 1-2), states (columns 3-4), and sectors (columns 5-6). The dependent variable in columns 1-2 is the number of PPP loans in a district scaled by its population size. The dependent variable in columns 3-4 and 5-6 is the aggregate amount of PPP loans in a state or sector, respectively, scaled by eligible payroll. All variable definitions are given in Appendix A. Heteroskedasticity-robust t-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

4.4 Real Economic Effects

The evidence thus far suggests that the electoral importance of congressional districts, states, and sectors played a role in the allocation of PPP loans. A natural question that arises is whether these allocations had real economic consequences. In this section, we seek to provide this evidence by utilizing survey evidence on small business activity as well as observational economic data on business applications and employment.

4.4.1 Survey Evidence on Small Business Activity

We begin the investigation of real economic effects with evidence from responses to the Small Business Pulse Survey. To capture the impact of electoral importance on the allocation of PPP loans and consequently on real economic outcomes, we employ a two-stage-least-squares approach. The first-stage regression estimates the effect of electoral importance on the allocation of PPP loans. The second-stage regressions use the predicted allocation of PPP loans from the first-stage to explain the variation in survey responses. The analyses focus on the first round of the PPP, where the evidence shows that electoral importance played a role in the allocation of loans. Furthermore, they focus on the variation in the allocation of PPP loans and survey responses across states because the survey does not provide responses across congressional districts.

The analyses focus on survey responses to the following questions:

1) Overall, how has this business been affected by the COVID-19 pandemic?
2) In the last week, did this business temporarily close any of its locations for at least one day?
3) In the last week, did this business have a change in the number of paid employees?
4) In your opinion, how much time do you think will pass before this business returns to its usual level of operations?

We construct the outcome variables as the percent of survey responses to each of the above questions in each state. For example, *Neg. effect on business* is the percent of survey respondents in a state that answered “Large negative effect” in response to question 1. *Temp. business closure*

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is the percent of respondents answering "Yes" to question 2. Appendix A provides the detailed definition of each variable.

Table 5 reports these results. Column 1 provides estimates from the first-stage regression, which show that electoral importance played a role in the allocation of PPP loans across states in the first round of the program. This result is evident from the positive and statistically significant coefficient on the composite index of electoral importance (first-stage F-statistic of 22.43). Columns 2-6 report the second stage estimates of the regressions of surveyed small business activity on predicted PPP funding in the first round. The evidence is consistent across all the survey-based variables. Small businesses in states that received higher allocation of PPP loans, as predicted by their electoral importance, were less likely to report a negative effect on their business (column 2), less likely to temporarily close their business (column 3), less likely to reduce employment (column 4), more likely to expect a return to normal in less than a month (column 5), and less likely to expect a return to normal in more than 6 months (column 6). Qualitatively, these results suggest that the allocation of PPP funds to electorally important states attenuated the negative effects of the Covid-19 crisis on small business activity.

The economic magnitudes of these effects are meaningful. A 10-percentage point increase in the predicted allocation of PPP loans decreases the percentage of survey respondents who report a negative effect of Covid-19 on their business by 8.5%, the percentage of respondents who temporarily closed their business by 10%, and the percentage of respondents reporting a decrease in employment by 11.2%. Further, a 10-percentage point increase in predicted PPP allocation increases the percent of respondents who expect a return to normal business operations in less than 1 month by 78.7% and decreases the percent of respondents who expect a return to normal business operations in more than 6 months by 12.96%. Taken together, these results provide suggestive

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evidence that the politically motivated allocation of PPP funds in the first round of the program to electorally important states had important real economic effects for small businesses.

Table 5: Second-Round Allocation of PPP Loans

	1st Stage	2nd Stage				
Dependent variable	First-round PPP loans/Eligible payroll	Neg. effect on business	Temp. business closure	Reduce employment	Return to normal <= 1 month	Return to normal > 6 months
Column	(1)	(2)	(3)	(4)	(5)	(6)
Electorally important	0.150***					
	-4.736					
$\widehat{\text{First-round PPP loans/Elig. payroll}}$		-0.405***	-0.380**	-0.283**	0.244**	-0.374***
		(-2.987)	(-2.337)	(-2.214)	-2.314	(-2.807)
% Applied to PPP	1.021***	0.045	-0.019	-0.041	-0.366**	0.232
	-3.569	-0.164	(-0.069)	(-0.202)	(-2.171)	-1.076
Ln(Covid-19 cases)	-0.022**	0.020*	0.018	0.002	0.012**	-0.011
	(-2.258)	-1.9	-1.363	-0.324	-2.154	(-1.088)
Ln(population)	-0.049***	-0.012	-0.021	0.002	0.014*	0.003
	(-3.167)	(-0.966)	(-1.257)	-0.144	-1.648	-0.238
Unemployment	-0.89	1.402**	2.197***	1.790***	0.245	1.027*
	(-1.505)	-2.509	-3.688	-3.534	-0.771	-1.951
GDP growth	2.317**	-0.965	-1.018	-0.65	0.568	0.974
	-2.226	(-1.227)	(-1.196)	(-1.122)	-1.191	-0.965
% Small SBA lenders	0.693***	0.038	-0.053	-0.065	-0.089	0.260*
	-5.529	-0.249	(-0.306)	(-0.442)	(-0.850)	-1.707
Constant	0.631***	0.684***	0.746***	0.359***	-0.166	0.297
	-2.768	-3.65	-3.638	-2.69	(-1.577)	-1.619
Observations	50	50	50	50	50	50
R-squared	0.793	0.612	0.582	0.612	0.098	0.241

This table provides estimates from two-stage-least-squares regressions of the effect of electoral importance on the allocation of PPP loans and subsequently small business activity. The first-stage regression (column 1) predicts the allocation of first-round PPP loans across states using electoral importance. The second-stage regressions (columns 2-6) explain small business activity using the predicted values from the first-stage regressions. The dependent variables in the second-stage regressions are based on responses to the Small Business Pulse Survey. All variable definitions are given in Appendix A. Heteroskedasticity-robust t-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

4.4.2 Difference-in-Difference Evidence on Business Applications and Employment

The survey-based analysis in Table 5 provides evidence from a single cross-section of states. In the next set of analyses, we provide difference-in-differences estimates from panel regressions that

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include state, sector, week, and month fixed effects, which alleviate concerns about unobservable economic indicators and time trends that might confound the analyses.

We begin by analyzing the effect of PPP funding on the number of weekly business applications per capita. If the allocation of PPP loans in the first round of the program matters for economic recovery, we would expect that electorally important states experience higher business applications following the onset of the PPP compared to less electorally important states. To test this prediction, we construct a state-week panel from 1/4/2020 to 4/25/2020, and estimate the following regression:

$$Y_{s,t} = \beta_1 Electorally\ important * Round\ 1 + \beta_2 X_{s,t} * Round\ 1 + \gamma_s + \alpha_t + e_{s,t}$$

Where $Y_{s,t}$ is one of three measures of business applications per capita for state *s* in week *t*, *Round 1* is a dummy variable equal to 1 after the beginning of the first round of the PPP (4/4/2020), $Electorally\ important_{s,t}$ is the composite index of electoral importance, and $X_{s,t}$ contains all the control variables used in our cross-sectional analyses. This specification allows us to control for permanent differences between treatment (electorally important) states and control states, along with aggregate time trends at the granular weekly level. Importantly, we allow all the explanatory variables, including states' electoral importance, loan demand, economic conditions, and exposure to the Covid-19 crisis, to have a differential impact before and after the onset of the first round of the PPP. This approach ensures that the observed impact of electoral importance during the first round of the PPP is not due to a differential response of electorally important states along other observable dimensions.

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Table 6: Difference-in-Difference Evidence on Business Applications and Employment

Dependent variable	Total business applications	Corporation business applications	High propensity business applications	Cont. unem claims	Ln(emp.)	Emp. per capita
Column	(1)	(2)	(3)	(4)	(5)	(6)
Electorally important * Round 1	0.018**	0.007**	0.007*	-0.015**	0.052**	0.001**
	-2.331	-2.423	-2.003	(-2.295)	-0.022	-0.001
% Applied to PPP * Round 1	0.039	0.021	0.001	0.02	-0.203	-0.001
	-0.289	-0.924	-0.012	-0.343	-0.199	-0.004
Ln(Covid-19 cases)	-0.003**	0.000	-0.001	0.000	-0.005**	-0.000***
	(-2.566)	(-0.717)	(-0.786)	-0.863	-0.002	0.000
Ln(population) * Round 1	-0.002	-0.004**	-0.002	0.001	0.002	0.000
	(-0.290)	(-2.079)	(-0.813)	-0.519	-0.011	0.000
GDP growth * Round 1	-0.336	0.109	-0.074	-0.234	0.234	0.004
	(-0.694)	-0.934	(-0.411)	(-0.890)	-1.038	-0.022
% Small SBA lenders * Round 1	0.062	0.021*	0.025	0.014	0.120	0.002
	-1.435	-1.781	-1.507	-0.503	-0.098	-0.003
Observations	600	600	600	600	2,744	2,764
R-squared	0.969	0.939	0.943	0.875	0.931	0.856
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector FE					Yes	Yes
Week FE	Yes	Yes	Yes	Yes		
Month FE					Yes	Yes

This table examines the effect of electoral importance on weekly new business applications and employment across states. *Total business applications* are the number of businesses applying for an employer identification number (EIN) scaled by population. *Corporation business applications* are the number of corporations applying for an EIN scaled by population. *High propensity business applications* are applications for an EIN that have a high likelihood of turning into businesses with a payroll. *Cont. unem claims are* weekly continued unemployment claims per capita. *Ln(emp.)* is log monthly employment by state and sector. *Emp. per* capita is monthly employment per capita by state and sector. All the regressions include state and week or month fixed effects. Columns 5-6 include sector fixed effects. *Round 1* is a dummy variable equal to 1 from 4/4/2020 to 4/25/2020. All variable definitions are given in Appendix A. Standard errors are clustered at the state level. T-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

We present these results in Table 6. The key explanatory variable is the interaction term *Electorally important* * *Round* 1, which captures the differences across electorally important and unimportant states following the onset of the crisis. The estimates show that following the onset of the first round of the PPP, electorally important states experienced increased business applications compared to unimportant states. These results are evident from the positive coefficient on the interaction term *Electorally important* * *Round* 1. The results hold across the different definitions of business applications and are economically nontrivial. Following the onset of the first round of the PPP, A one standard deviation increase in the *Electorally important* index increases total business applications per capita by 2.79%, corporate applications by 9.51%, and high-propensity business applications by 3.21%.

Columns 4-6 of Table 6 provides estimates from a similar difference-in-differences analysis of weekly continued unemployment claims and monthly employment rates by state and sector. Column 4 shows that continued unemployment claims per capita rose less in electorally important states following the onset of the first round of the PPP. This result is captured by the negative coefficient on the interaction term *Electorally important* * *Round* 1 in column 4. Columns 5 and 6 show that state-by-sector declines in employment were attenuated by electoral importance following the onset of the first round of the PPP. These results are captured by the positive coefficients on the interaction terms *Electorally important* * *Round* 1 in columns 5 and 6.

Furthermore, the magnitudes of the effects are meaningful. A one standard deviation increase in the *Electorally important* index attenuates the rise in continued unemployment claims per capita by 16.86%, and the fall in log employment and employment per capita by 5.3% and 1.26%, respectively, following the onset of the first round of the PPP. The estimates are also statically significant at the 10% level or higher.

Collectively, the results suggest that the strategic allocation of emergency government funds in an election year helped mitigate the deleterious effects of the Covid-19 crisis on employment, and helped to spur economic recovery by promoting new business applications. Given voters' tendency to focus on recent economic performance, these positive economic effects could impact the results of the 2020 elections.

In Table 7, we address the remaining concern that the positive effects of states' electoral importance on business applications and employment are driven by contemporaneous factors that

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are unrelated to the allocation of PPP loans. For example, electorally important states may have responded better to the Covid-19 emergency. To address this concern, we exploit the granular nature of the weekly business applications and continued unemployment claims to conduct placebo tests around dates that coincide with the Covid-19 crisis and are unrelated to the onset of the PPP.

Table 7: Placebo Tests

Dependent variable	Total business applications	Corporation business applications	High propensity business applications	Cont. unem claims
Column	(1)	(2)	(3)	(4)
Electorally important * Public health	0.00	-0.001	-0.001	0
	(-0.068)	(-0.844)	(-0.213)	(-0.130)
% Applied to PPP * Public health	-0.121*	-0.018	-0.03	-0.001
	(-1.846)	(-1.209)	(-1.068)	(-0.841)
Ln(weekly Covid-19 cases)	-0.001	0.00	0.00	0.00
	(-0.730)	-0.544	-0.004	-0.608
Ln(population) * Public health	0.007	0.003**	0.004	0.00
	-1.323	-2.052	-1.675	(-0.175)
GDP growth * Public health	-0.132	-0.027	-0.027	-0.005
	(-0.525)	(-0.451)	(-0.226)	(-0.635)
% Small SBA lenders * Public health	-0.066*	-0.009	-0.027	0.00
	(-1.990)	(-1.445)	(-1.611)	-0.692
Observations	600	600	600	600
R-squared	0.969	0.939	0.943	0.875
State FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes

This table provides estimates from placebo difference-in-differences regressions that replace the allocation of first-round PPP loans (*Round 1* in Table 6) with the declaration of a national public health state of emergency on 1/31/2020. *Public health* is an indicator variable that equals 1 in the 4 weeks following the declaration and 0 in the 4 weeks prior to the declaration. All the regressions include state and week fixed effects. All variable definitions are given in Appendix A. Standard errors are clustered at the state level. T-statistics are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

In particular, we examine the relative response of electorally important states versus electorally unimportant states around the declaration of a national public health emergency on January 31, 2020. If electorally important and unimportant states vary in their exposure or response to the Covid-19 crisis, we should also observe differences in business applications and unemployment

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claims across electorally important and unimportant states around this date. We focus our analysis on the 9 weeks surrounding the declaration of the public health emergency, to avoid an overlap with the initiation of the PPP.

The results are reported in Table 7. The estimates show that around the declaration of a national public health emergency, business applications and unemployment claims in electorally important states were indistinguishable from those in electorally unimportant states. Together, these findings mitigate concerns that unobservable state characteristics correlated with the allocation of PPP loans to electorally important states are driving the difference-in-differences effect of the PPP on business applications and unemployment claims.

5. Concluding Remarks

This paper investigates the impact of election-year politics on the allocation of emergency government funds through the flagship Paycheck Protection Program (PPP), which disbursed forgivable loans to small businesses in the United States in response to the Covid-19 crisis, and its real economic consequences. We construct novel measures of electoral importance that aim to capture strategic capital allocation to swing and base voters. These measures use data from Facebook ad spending, independent political expenditures, the Cook Political Report, and campaign contributions across states, congressional districts, and sectors.

We provide two main results. First, businesses in electorally important states, districts, and sectors receive more government funds following the onset of the Covid-19 crisis, controlling for funding demand and both health and economic conditions. Second, the tilt in government funding weakens the adverse effects of Covid-19 on employment, business applications, and small business

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activity. These estimates are corroborated by both small business survey data and aggregate economic data released by the Bureau of Labor Statistics.

Collectively, these estimates provide novel evidence on the allocative distortions and real effects of electoral politics. These findings have important implications for the design and governance of government investment programs, suggesting that regulators and policy makers should pay particular attention to the implementation of such programs during election years.

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Appendix A
Variable Definitions

Variable	Definition	Source
First-round PPP loans/Elig. payroll	Total $ PPP funds allocated to a given state or sector from 4/3-4/14, scaled by payroll of firms with less than 500 employees. Payroll for all firms in NAICS sector 72 (Accommodation and Food Services) are also included.	SBA, SUSB
Second-round PPP loans/Elig. payroll	Total $ PPP funds allocated to a given state or sector from 4/3-6/30 minus total $ PPP funds allocated from 4/3-4/14, scaled by payroll of firms with less than 500 employees. Payroll for all firms in NAICS sector 72 (Accommodation and Food Services) are also included.	SBA, SUSB
First-round PPP per capita	Total # PPP funds allocated to a given congressional district from 4/3-4/14, scaled by district population	SBA, Census
Second-round PPP per capita	Total # PPP funds allocated to a given congressional district from 4/3-6/30 minus total # PPP funds allocated from 4/3-4/14, scaled by district population	SBA, Census
Republican state	Likely or Solidly Republican state	Cook Political Report (3/9/2020)
Third-party spend share	State share of third-party political spending in opposition to and in support of Donald Trump (01/01/2019-03/31/2020)	FEC
Trump Facebook ad spending	State share of Trump political ad spending on Facebook from 03/30/2019-04/04/2020	Facebook, Campaign Tracker 2020
Battleground state	State share of total *Trump Facebook ad spending* and *Third-party spend share*	Facebook, FEC, Campaign Tracker 2020
Electorally important state	Average of *Republican state* and a dummy variable for above-median *Battleground state*	Cook Political Report, Facebook/Campaign Tracker 2020, FEC
Republican district	Congressional districts that have a Partisan Voting Index of greater than R+10	Cook Political Report

Battleground district	Congressional districts that have a Partisan Voting Index of between D+10 and R+10	Cook Political Report
Electorally important district	Union of *Republican district* and *Battleground district*	Cook Political Report
Republican sector	2-Digit NAICS sectors in the top tercile of Republican leaning	GLP 2014
Battleground sector	2-Digit NAICS sectors in the middle tercile of Republican leaning	GLP 2014
Electorally important sector	Union of *Republican sector* and *Battleground sector*	GLP 2014
% Applied to PPP	% of Survey respondents that applied for PPP loan (as of 4/30 for first-round PPP or as of 6/27 for second-round PPP)	Small Business Pulse Survey
Ln(population)	Natural log of population	Census
Ln(Covid-19 cases)	The natural log of Covid-19 cases as of 4/03 or 4/25. Measured at the state and congressional district level.	USA Facts
Unemployment	The sum of state continued unemployment claims and initial unemployment claims as of 4/04 or 4/25 for states, and population-weighted county unemployment rate from 2019 for districts	BLS
GDP growth	State GDP 2019 Q4 growth. Defined as the population-weighted county GDP growth from 2018 for congressional districts	BEA
% Small SBA lenders	Proportion of branches of banks in a state or district under $1 billion in assets that participated in the SBA 7(a) program from 2015-2019	SBA, Summary of Deposits
Total business applications	Weekly Applications for an EIN	Census
Corporate business applications	High-propensity business applications from a corporation or personal service corporation, based on the legal form of organization stated in the IRS Form SS-4	Census

High-propensity business applications	Business applications that have a high propensity of turning into businesses with a payroll. High-propensity applications include applications: (a) from a corporate entity, (b) that indicate they are hiring employees, purchasing a business or changing organizational type, (c) that provide a first wages-paid date (planned wages); or (d) that have a NAICS industry code in manufacturing (31-33), retail stores (44), health care (62), or restaurants/food service (72)	Census
Continued unemployment claims	Number of weekly state continued unemployment claims	DOL
Employment	Total employment by state-sector-month. Measured in logs or per capita	BLS
Neg. effect on business	The percent of survey respondents who reported a "Large negative effect" or "Moderate negative effect" of Covid-19 on their business based on the 4/26 survey	SBPS
Temporary business closure	The percent of survey respondents who reported temporary closing at least one business location in the last week based on the 4/26 survey	SBPS
Reduced employment	The percent of survey respondents who reported reducing employment in the last week based on the 4/26 survey	SBPS
Return to normal <= 1 month	The percent of survey respondents who predict a return to normal levels of operation in less than 1 month based on the 4/26 survey	SBPS
Return to normal > 6 Months	The percent of survey respondents who predict a return to normal levels of operation in more than 6 months based on the 4/26 survey	SBPS

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Appendix B

A.2. List of Republican and Battleground Sectors

This list shows the NAICS sectors designated as having Republican preference in the top tercile (*Republican sectors*) and middle tercile (*Battleground sectors*) according to historical congressional campaign contributions. Partisan preference by sector comes from Gimpel, Lee, and Parrott (2014) and the Center for Responsive Politics.

NAICS Sector	NAICS Description
Republican Sectors	
11	Agriculture, Forestry, Fishing and Hunting
21	Mining, Quarrying, and Oil and Gas Extraction
23	Construction
31-33	Manufacturing
72	Accommodations and Food Service
52	Finance and Insurance
Battleground Sectors	
44-45	Retail Trade
48-49	Transportation and Warehousing
81	Other Services
22	Utilities
51	Information
53	Real Estate and Rental and Leasing

Growth forecasts and the Covid-19 recession they convey: End-2020 update[1]

Javier G. Gómez-Pineda[2]

Date submitted: 14 December 2020; Date accepted: 15 December 2020

The paper updates the results in Gómez-Pineda (2020) about the depth, length and shape of the covid-19 recession using information up to the December-2020 forecast vintage. The method is a decomposition of output between potential output and the output gap, the former explained by supply shocks and the later, by demand shocks. We find that, compared to the July-2020 forecast vintage, in the December-2020 forecast vintage the median depth of the recession improved 1.1 percentage points in advanced economies while deteriorated 2.3 percentage points in emerging and developing economies. This change in the outlook may be explained by the increase in the prevalence of the disease in the second half of 2020 in emerging and developing economies as well as by the more limited reach of monetary and fiscal policies in emerging and developing economies. The recession is still V-shaped with partial recovery in advanced economies and L-shaped in emerging and developing economies. The results point to the relevance and urgency of policies to support emerging and developing economies.

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1 This paper is an update of "Growth forecast and the Covid-19 recession they convey" published in issue 40 of *Covid Economics* in July 2020. The author thanks David Felipe López for excellent research assistance. The findings, recommendations and interpretations expressed in this paper are those of the author and do not necessarily reflect he view of the Banco de la República or its Board of Directors.

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1. Introduction

In the December forecast vintage, the projected depth of the recession improved in advanced economies and deteriorated in emerging and developing economies. In advanced economies the median depth of the recession improved about 1.1 percentage points to –7.9 percent, from –9.1 percent in July.[1] In emerging and developing economies the median depth of the recession deteriorated about 2.3 percentage points to –8.8 percent, from nearly –6.4 percent in July.[2] In the near future the risks in advanced economies are to the upside, as massive vaccinations can turn mandatory and voluntary distance measures less necessary. In contrast, in emerging market and developing economies the risks are to the downside, as the number of cases and deaths raise and delays in vaccinations prolong the need for mandatory and voluntary distance measures.

At the time the July forecast vintage was made, the multiyear projections could be considered relatively reliable for the year 2020 but not as reliable for the medium term. As suggested by the forecast record during the Global Financial Recession, to give an example, medium term projections, particularly those for emerging and developing economies, had major revisions (see Gómez-Pineda, 2020b).

The covid-19 recession is explained by a combination of demand and supply shocks. In advanced economies supply shocks appear to be better explained as output level shocks while in emerging and developing economies they appear to be better explained as output growth shocks (Gómez-Pineda, 2020a).

In this paper we update the results in Gómez-Pineda (2020a) using the information available at end-2020. Section 2 presents a brief verbal description of the model, section 3 mentions the main features of the latest available data, section 4 presents the updated results and section 5 concludes.

[1] Figures may not add up because of rounding.

[2] These numbers can change with the measure of central tendency; that is, whether the median or the weighted average is used; with the definition of distance, whether logarithmic or percent distance is used; and also with the sample of economies in the study. The reported figures correspond to the median, the logarithmic deviation and are based on a sample of economies described in the data section.

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2. The model

The model consists of a breakdown of output between potential output and the output gap. Potential output follows supply shocks while the output gap follows demand shocks. Supply shocks can be output level, output growth shocks or a combination of both. Output level shocks affect the output level permanently while output growth shocks affect the potential-output growth rate about the long-term, potential-output growth rate temporarily. The long-term, potential-output growth rate is obtained as the forecasted output growth at the end of the forecasting horizon. This forward-looking measure of the long-term, potential-output growth rate might be more relevant for the recession that is being projected compared with a rate obtained from past data.

Detrended output is obtained subtracting trend potential output; the later is obtained as the potential output that obtains in the absence of supply shocks. Trend potential output is normalized at 0 in the base year 2019.

3. The data

The sample of economies is the one available to us in our Focus Economics service; that is, 65 economies, 29 advanced and 36 emerging and developing.[3] In 2019, the economies in the sample accounted for 83.5 percent of world output evaluated at PPP exchange rates, 38.7 for advanced economies and 44.8 for emerging and developing economies. Excluding China, the emerging and developing economies account for 25.6 of world output. A more detailed description of the database is available in Gómez-Pineda (2020a).

Most of this paper compares the December 2020 with the July 2020 forecast vintages. In the Focus Economics source, the December forecast vintage includes data from November 8 through November 22. In turn, the July forecast vintage includes data from June 14 through June 28.[4]

[3] The countries in the sample are, in the group of advanced economies, Australia, Austria, Belgium, Canada, Cyprus, Estonia, Finland, France, Germany, Greece, Hong Kong SAR, Ireland, Italy, Japan, Korea, Latvia, Lithuania, Luxembourg, Netherlands, New Zealand, Portugal, Singapore, Slovak Republic, Slovenia, Spain, Switzerland, Taiwan Province of China, United Kingdom and United States; and in the group of emerging and developing economies, Argentina, Bangladesh, Belize, Bolivia, Brazil, Brunei Darussalam, Cambodia, Chile, China, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala , Haiti, Honduras, India, Indonesia, Jamaica, Lao P.D.R., Malaysia, Mexico, Mongolia, Nicaragua, Pakistan, Panama, Paraguay, Peru, Philippines, Russia, Sri Lanka, Thailand, Trinidad and Tobago, Uruguay and Vietnam.

[4] Because the information for Latin America and the Caribbean is the latest available, for the economies in this region we use the forecast vintage of the previous month.

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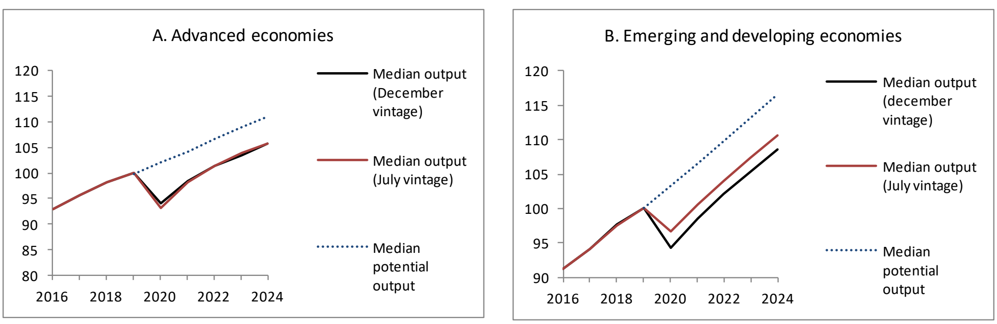
Importantly, most of the December forecast vintage did not include any of the news about the effectiveness of vaccines that came out on November 17. Future forecast vintages might show how the news about the effectiveness, approval and distribution of vaccines affect growth forecasts.

4. The information in the end-2020 forecast vintage

In the December forecast vintage, the depth of the recession in 2020 improved in advanced economies and deteriorated in emerging market and developing economies (Figure 1). The new projections can be considered relatively reliable for 2020, as they are now based on observed data for up to the second and third quarters of the year. In contrast, the projection of the depth of the recession in the medium term, as said above, may not be as reliable. Medium term projections can change importantly, for example, as mentioned above, current medium term projections are subject to massive upside and downside risks.

Figure 1. A deeper recession is projected for emerging and developing economies

Median output and median potential output in the Focus Economics December and July 2020 forecast vintages



Source: authors calculations based on data from Focus Economics.

In order to gauge the shape of the recession, a detrended method may be used. Some analysts prefer to compare current projections with pre covid-19 projections. I prefer to use the rate of growth currently projected for the end of the forecasting horizon because that is the new steady state that will have to be reached during the recovery from the ongoing recession. Using this method, the projected recession, with information up to end-2020, remains V-shaped with partial recovery in advanced economies and L-shaped in emerging market and developing economies (Figure 2). The shape of the recession is broadly maintained across quartiles (Figure 2). In addition,

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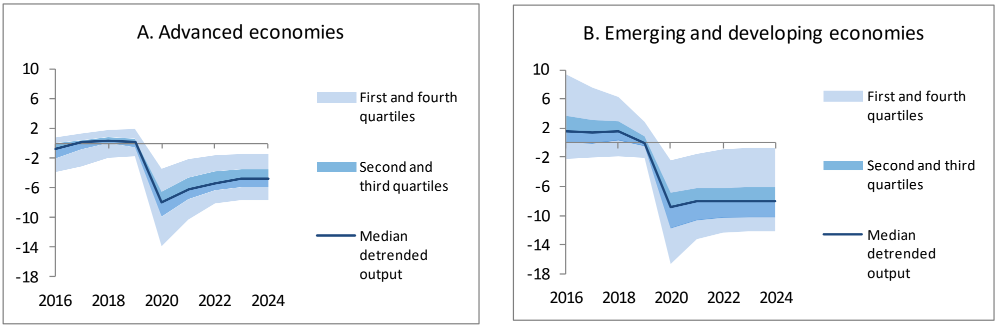
the extent of the interquartile range, with the more standard economies, is larger in emerging market and developing economies (Figure 2).

Figure 2. The projected depth and shape of the covid-19 recession

Quartile distribution of detrended output in the Focus Economics December 2020 forecast vintage

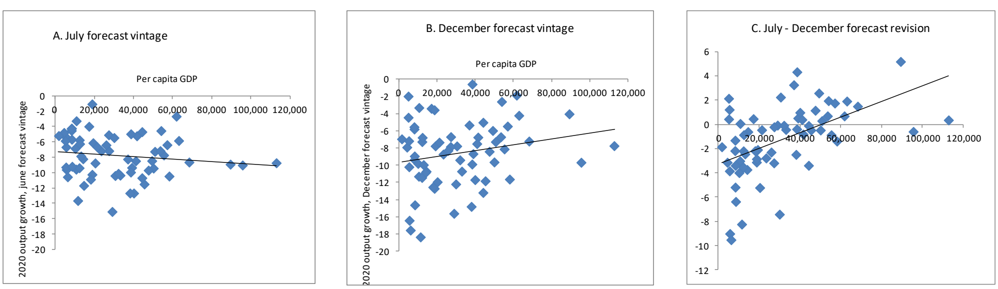


Source: authors estimations based on data from Focus Economics.

The new information in the December-2020 forecast vintage is about the depth of the recession. Indeed, back in July 2020 growth forecasts tended to be negatively correlated with per capita income (Figure 3, Panel A). In contrast, in December 2020, growth forecasts tend to be positively correlated with per capital income (Figure 3, Panel B). As a result, the prospects for advanced economies have improved while those for emerging and developing economies have deteriorated (Panel C).

Figure 3. Growth forecast deteriorated in emerging market and developing economies

PPP-weighted, per capita GDP *vs*. growth forecasts in the July and December 2020 forecast vintages

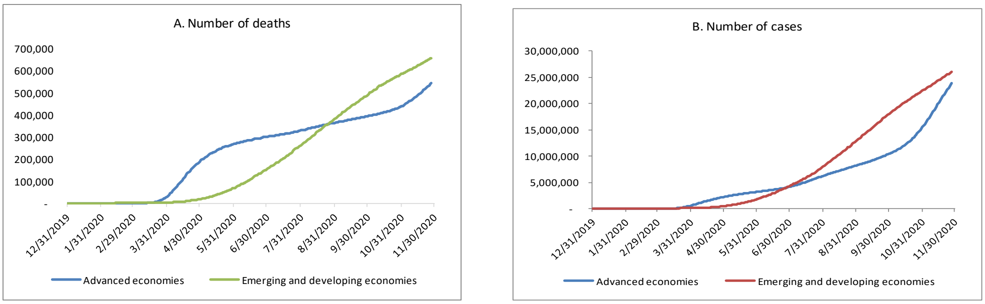


Source: authors calculations based on data from Focus Economics and John Hopkins University.

The deeper projected recession in emerging market and developing economies may be explained by the increase in the prevalence of the disease in the second half of 2020, as gauged by the number of deaths and cases (Figure 4). It can also be explained by the much more limited reach of fiscal and monetary policies, see for instance Alberola et al. (2020), Cavallino and De Fiore (2020) and Deb et al. (2020).

Figure 4. Number of cases and deaths

Number of deaths and cases in the countries in the sample



Source: authors calculations based on data from John Hopkins University.

In our methodology, we work backwards from the growth forecasts to the shocks that may be explaining the recession. Here we maintain the assumption we used by mid-2020 about the distribution of the drop in output between supply and demand shocks. We maintain that the distribution of the shocks between demand and supply is half and half only because of the large uncertainty underlying this assumption. Some studies suggest demand shocks are larger while others suggest the opposite, see Baqaee and Farhi (2020), Balleer et al. (2020) and see also the overview in Macaulay and Surico (2020). Nonetheless, regardless the assumed distribution of shocks, the result in Gómez-Pineda (2020a) about the likely type of supply shock in advanced and emerging and developing economies is maintained in the December forecast vintage. The result is that in advanced economies supply shocks are better characterized as output level shocks while in emerging and developing economies they are better characterized as output growth shocks.

Using the updated information, the size of the output level shock in advanced economies in 2020 is smaller while the size of the output growth shock in 2020 in emerging and developing economies is larger (Figure 5). The same applies demand shocks.

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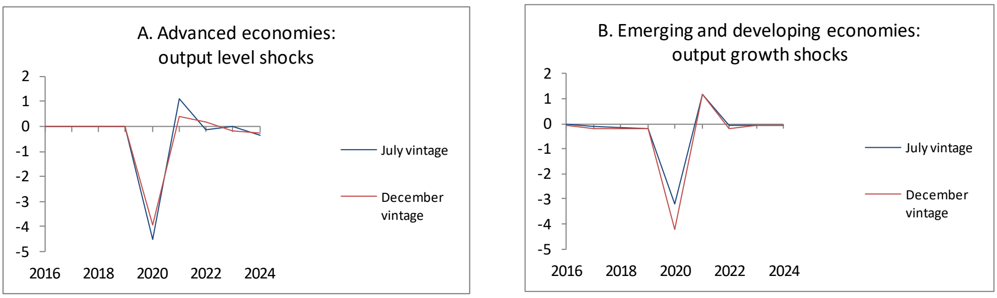
Figure 5. The projected output level and output growth shocks in the covid-19 recession

Median output-level and growth shocks in the Focus Economics July 2020 and December 2020 forecast vintages



Source: authors estimations based on data from Focus Economics.

5. Conclusion

The new information contained in the end-2020 growth forecasts is that in advanced economies the projected recession is less deep while in emerging and developing economies the recession is deeper. The deeper recession in emerging and developing economies may be explained by a larger prevalence of the disease during the second half of 2020 as well as by the much more limited reach of fiscal and monetary policies.

The risks to the outlook are to the upside in advanced economies, as massive vaccinations are expected to contain the prevalence of the disease. In turn, in emerging market and developing economies the risks to the outlook are to the downside, as delays in vaccinations may not help contain the raise in the prevalence of the disease.

The updated information points to the relevance and urgency of policies to support emerging and developing economies.

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The economics of stop-and-go epidemic control[1]

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Date submitted: 11 December 2020; Date accepted: 13 December 2020

We analyse 'stop-and-go' containment policies which produce infection cycles as periods of tight lock-downs are followed by periods of falling infection rates, which then lead to a relaxation of containment measures, allowing cases to increase again until another lock-down is imposed. The policies followed by several European countries seem to fit this pattern. We show that 'stop-and-go' should lead to lower medical costs than keeping infections at the midpoint between the highs and lows produced by 'stop-and-go'. Increasing the upper and reducing the lower limits of a stop-and-go policy by the same amount would lower the average medical load. But, increasing the upper and lowering the lower limit while keeping the geometric average constant would have the opposite impact. We also show that with economic costs proportional to containment, any path that brings infections back to the original level (technically a closed cycle) has the same overall economic cost.

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1 Introduction

As governments grapple with the second wave in Europe, they are usually taking a much more gradual and graduated approach than during the initial phase of the pandemic. At that time the number of seriously ill increased so rapidly that it overwhelmed health systems and in particular hospital capacities in several countries. The second wave, which started with the onset of the flu season in the autumn of 2020, has so far led to a somewhat moderated challenge for health systems.

After the first peak, the urgency to 'flatten the curve' [1] has subsided to a certain extent. However, governments still feel the need to take measures to slow down the spread of the virus when the medical load is high. One key strategic issue facing the authorities is whether they should try to preserve a status quo, or whether to alternate lock-downs with periods of easing.

In Italy and England, the central governments have instituted a tiered system with different levels of social distancing restrictions. In regions with a higher incidence of COVID-19, the restrictions are tighter. Within such a 'traffic light' system a region (city or other subdivision) can graduate to a lower level of restrictions if its epidemiological parameters improve, and, vice versa, restrictions will be tightened if cases increase again. These countries have thus adopted de facto a 'stop and go' policy at the regional level.

A change into a higher or lower category will of course become more frequent the closer the parameters defining the various tiers are. One key issue for this 'regional traffic light' approach is thus how wide apart these parameters should be set. We investigate this issue keeping in mind that social distancing measures have an economic cost, which increases with their severity. The choice of parameters should be informed by their economic cost, relative to the health benefits in terms of lower infections, hospitalizations and deaths [2].

We do not consider a general optimal control problem. Our aim is limited to comparing policies that make intuitive sense and that describe the choices of different European countries. At the national level one can observe that Germany's curve is relatively flat, compared to that of France, Belgium or Spain. See Figure 1.

There is one simple economic argument that would favour the 'stop and go' strategy of alternating harsh restrictions with wide easing. The economic cost of closing restaurants, closing schools or imposing restrictions on movements is the same whether the current rate of infections is high or low. This implies that one should use harsh restrictions when the case count is high because one would then achieve the largest fall in cases (in absolute numbers).

The argument against the 'stop and go' strategy is that the cost of the harsh restrictions to achieve a quick fall in infection is likely to be convex. A small proportional reduction in infections (or rather the reproduction number) can be achieved by measures which have little impact on the economy (e.g. mask wearing, etc.). Achieving a more rapid deceleration in the diffusion of the virus requires substantially stronger restrictions of the type mentioned above.

One could of course argue that stop and go policies are inferior to the 'East

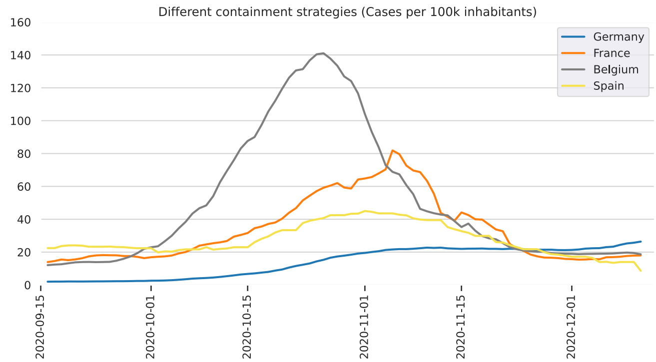

Figure 1: **Examples of second-wave control strategies.** For the control of the second Covid-19 wave in Europe, most countries imposed comparatively strict lock-downs. Illustrative examples are, as shown, France, Belgium and Spain. As an exception, Germany, starting around the beginning of November 2020 opted for a 'semi-lockdown', which resulted in near constant infections. Graphic generated using the Goethe Interactive Covid-19 Analyser [6].

Asian' option of eradicating the virus [3], which then allows a total reopening of the economy. But this option has been abandoned in Europe as the draconian measures, including border closures that would be required, have apparently been widely judged as unacceptable. Note, however, that it is currently yet to be settled whether additional factors, like evolutionary adaptions, contribute to differences in the path the spread of the SARS-CoV-2 pathogen took in European and East-Asian populations [4].

The remainder of our contribution is organised as follows: We start by briefly reviewing the standard SIR model to which we add a relationship which describes the economic cost of reducing the spread of the virus. This framework is then used to examine the economic control costs of two alternative policies: keeping the medical load constant [5] versus a stop and go policy. We then compute the medical load implied by these two policies over a given time path and compare the resulting relative economic costs against the benefits in terms of a lower overall number of infected. Finally, we consider the implications of a time varying native reproduction factor, for example an increase due to colder weather, leading to more indoor interactions.

Throughout, our purpose is not to describe and solve a general optimal control problem, but to compare the economic cost of different concrete policy options. Figure 2 illustrates schematically the 'stop-and-go' epidemic control which we model below.

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2 Modelling framework

We start with a short presentation of the standard SIR model where we denote with $S = S(t)$ the fraction of susceptible (non-affected) people, with $I = I(t)$ the fraction of the population that is currently ill (active cases, which are also infectious), and with $R = R(t)$ the fraction of recovered. Normalization demands $S + I + R = 1$ at all times. We write the continuous-time SIR model as

$$\tau\dot{S} = -gSI, \qquad \tau\dot{I} = \big(gS - 1\big)I, \qquad \tau\dot{R} = I\,, \tag{1}$$

which makes clear that τ is a characteristic time scale. Normalization is conserved, as $\dot{S} + \dot{I} + \dot{R} = 0$. Infection and recovery rates are g/τ and $1/\tau$. The number of infected grows as long as $\dot{I} > 0$, namely when $gS > 1$. Herd immunity is consequently attained when the fraction of yet unaffected people dropped to $S = 1/g$. The total number of past and present infected is $X = 1 - S = I + R$.

From (1) one sees that g/τ governs the transition between two compartments, from susceptible to infected. This transition rate is constant within the basic version of the SIR model. There are two venues to relax this condition:

- **Non-linear reproduction rates.** The basic reproduction factor g may depend functionally on the actual number of infected I [7, 8], or on the total number X, [2]. This happens when societies react on an epidemic outbreak.

- **Time-dependent reproduction rates.** From the viewpoint of the pathogen, certain changes in the transmission rate $g = g(t)$ are external, e.g. because hosts decide more often to quarantine.

Here we focus on time-dependent $g = g(t)$, mostly as induced by stop and go politics, as illustrated in Figure 2. We assume a stop and go cycle which repeats after $T = T_{\rm up} + T_{\rm down}$:

$$g(t) = \left\{ \begin{array}{cc} g_{\rm up} > 1 & t \in [0, T_{\rm up}] \\ g_{\rm down} < 1 & t \in [T_{\rm up}, T] \end{array} \right. . \tag{2}$$

The time spans during which epidemics expands/contracts are respectively $T_{\rm up}$ and $T_{\rm down}$. The policy is cyclic if $I(0) = I(T)$, viz when the starting case number is reached again.

2.1 Low incidence approximation

We assume that infection counts are substantially lower than the population, viz that $I \ll 1$. For example, even in a highly affected country like Italy, the total number of daily infections has rarely exceeded thirty thousand [9], which corresponds to less than 0.0005 of total population. The total number of cases has reached 1.5 million, which is equivalent to $S \approx 0.975$. The number

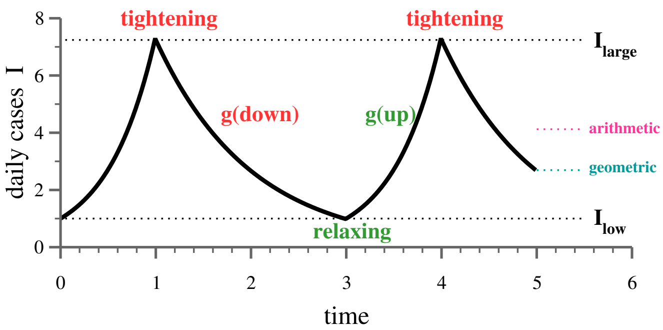

Figure 2: **Stop-and-go epidemic control.** Containment policies that alternate periodically between I_{low} and I_{large}. When relaxing, the number of daily cases $I = I(t)$ expands at a rate $g(\text{up}) \equiv g_{\text{up}} > 1$. Policies are tightened again when case number are too high. Daily case numbers then contract with a rate $g(\text{down}) \equiv g_{\text{down}} < 1$. In the example shown, up- and down times are $T_{\text{up}} = 1$ and $T_{\text{down}} = 2$. For a presumed time scale of one month the period $T_{\text{up}} + T_{\text{down}}$ of the control cycle would be here three months. Also indicated are the arithmetic and the geometric means of I_{low} and I_{large}, as used in Sect. 4.1. Note that contant control is recovered in the limit $I_{\text{low}} \to I_{\text{large}}$.

of susceptibles remains then close to one, essentially constant and we can set $S \to 1$. Hence we need to deal only with

$$\tau \dot{I} = \big(g-1\big)I, \qquad\qquad I(t) = I(t_0)\,\mathrm{e}^{(t-t_0)(g-1)/\tau}\,. \tag{3}$$

In the quasi-stationary state we have simple exponential growth/decay. For stop-and-go control the reproduction factor is piece-wise constant, which allows us to evaluate explicitly the time evolution of case numbers, and with this the associated economic costs.

3 Economic costs of disease control

The economic costs of imposing social distancing on a wider population, closing restaurants or retail trade, are at the core of policy discussions. The key issue here is how these costs vary with the social distancing measures (so-called Non-Pharmacological Interventions, NPI) imposed. They increase in severity from mask requirements, abstaining from travel or restaurant meals to more invasive interventions like closure of schools, lock-downs or curfews. Limiting social interaction necessarily reduces economic activity. This suggests that the economic cost of the social distancing, measured as the proportional loss of GDP, should

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increase with the reduction in the transmission rate described by g,

$$E = c_e \frac{g_0 - g}{g_0}\,, \tag{4}$$

where g_0 is the native reproduction factor. Here we assumed that social distancing costs are proportional to the percentage-wise reduction of the reproduction factor, viz to $(g_0 - g)/g_0$.

Our basic assumption, that social distancing costs are proportional to the reduction in the reproduction parameter, differs from the assumptions underlying matching models, as used in [10] or [11], which typically arrive at a quadratic relationship between the economic cost and the reduction in contagion, whereas [12] postulates simply a convex cost curve.

A concrete example can illustrate the key mechanism behind the matching framework, which usually assumes that the 'lock-down' takes the form of the confinement of a proportion of the population, and that contagion is possible only outside. If one half of the population has to stay at home, only the other half can go out and get potentially infected. But the half which is not confined will find only one half of their potential partners outside, resulting in one fourth of the number of matches. Contagion should thus be reduced by a factor of four when confining one half of the population.

In the matching framework, the economic cost is assumed to be proportional to the percentage of the population which is confined - not the number of matches. This framework thus separates social activity (matches, meetings of people) from economic activity, assuming that contagion is fostered only through social activity. Another implicit assumption behind this view is that those who are not confined will not have longer meetings with the ones they still find outside; or that those who are free to move accept to have only half of matches, and do not decide to meet somebody else if their preferred match is not available. These implicit assumptions are crucial. For example, contagion would only be halved if those not confined would meet twice as long with the remaining matches they find. Some authors, e.g. [11] acknowledge these considerations by allowing for different economies of scale in matching.

Our view of lock-down or rather social distancing is that governments mandate the closure of some part of the economy, in reality mainly the services sector (restaurants, bars, shops, etc.). This is different from a strict confinement of a part of the population. A restaurant which is closed (or limited in its opening hours) results in less value added created and diminishes at the same time the potential for contagion. But the restaurant owners and their workers are not confined, they can meet others. There is thus no quadratic effect in terms of contacts. We thus start from the assumption that economic activity involves occasions for contagion, implying that the loss of economic activity should be directly proportional to the reduction in occasions for contagion and thus the effective reproductions rate. For related approaches see [2], [13], [14] [15].

This view of 'lock-down' corresponds closer to the measures adopted by many governments during this second wave. The matching model might have been more appropriate during the first wave when indeed in some countries large

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parts of the entire population were forbidden to leave their home, except for essential business.

Finally we note that social distancing measures (NPIs) cannot affect the number of infected, only the rate at which their number grows over time. Eq. 4 implies that the only way to avoid all contagion is to completely shut down the economy. The parameter c_e represents a scaling factor, which would depend on the structure of the economy (importance of services requiring close contact, like tourism) and the degree to which the population effectively adheres to official restrictions. It hast been estimated that c_e is of the order of 0.25 [2].

3.1 Uniform control

We first briefly examine the implications of a policy which keeps the number of infected [5] and thus the medical load constant. Such a policy is of course not optimal, but it serves as a useful benchmark for our more general results. It can be considered as the limiting case of the 'traffic light' system in which the difference in parameters between tiers or levels becomes very small.

Formally, uniform control implies a constant fraction $I \to I_{\text{const}}$ of infected. This is achieved for $g = 1$, independent of the value of I_{const}. The economic cost E_{const} per time unit is therefore

$$E_{\text{const}} = c_e \left. \frac{g_0 - g}{g_0} \right|_{g=1} = c_e \frac{g_0 - 1}{g_0} , \tag{5}$$

Note that E_{const} is independent of the value of the medical load one wants to retain. As already mentioned above, the economic costs of keeping the reproduction factor at one is independent of how many infected there are.

3.2 Stop and go control

Stop and go, or 'bang bang', control corresponds to an on-off policy as illustrated in Figure 2 above, which can be described within the framework developed here by the following rule: Control is increased when $I = I(t)$ reaches an upper threshold I_{large}, and decreased when $I = I(t)$ falls below a lower threshold I_{low}.

We denote with $g_{\text{down}} < 1$ the small reproduction rate corresponding to strong control, and with $g_{\text{up}} > 1$ the large reproduction factor corresponding to weak control. Using the low-incidence approximation (3) we find

$$T_{\text{up}} = \frac{\tau}{g_{\text{up}} - 1} \ln \left(\frac{I_{\text{large}}}{I_{\text{low}}} \right), \qquad T_{\text{down}} = \frac{\tau}{g_{\text{down}} - 1} \ln \left(\frac{I_{\text{low}}}{I_{\text{large}}} \right) \tag{6}$$

for the time T_{up} needed for $I(t)$ to grow from I_{low} to I_{large}, with a respective expression for the down-time T_{down}. On a per time basis the economic costs are then

$$E_{\text{bang}} = \frac{c_e}{g_0} \Big[(g_0 - g_{\text{up}}) T_{\text{up}} + (g_0 - g_{\text{down}}) T_{\text{down}} \Big] \frac{1}{T_{\text{up}} + T_{\text{down}}} \tag{7}$$

for band-bang, viz stop-and-go control.

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3.3 Vanishing cost differential

The cost difference between bang-bang and constant control is

$$E_{\text{bang}} - E_{\text{const}} = \frac{c_e}{g_0}\left[1 - \frac{g_{\text{up}}T_{\text{up}} + g_{\text{down}}T_{\text{down}}}{T_{\text{up}} + T_{\text{down}}}\right]. \tag{8}$$

Note that $\ln(I_{\text{large}}/I_{\text{low}}) = -\ln(I_{\text{low}}/I_{\text{large}})$, which implies that both τ and $\ln(I_{\text{large}}/I_{\text{low}})$ drop out of (8), which vanishes as

$$E_{\text{bang}} - E_{\text{const}} = \frac{c_e}{g_0}\left[1 - \frac{g_{\text{up}}(g_{\text{down}}-1) - g_{\text{down}}(g_{\text{up}}-1)}{g_{\text{down}} - g_{\text{up}}}\right] \equiv 0\,. \tag{9}$$

This implies that both control types, constant and stop-and-go control, come with the same economic costs.

3.4 Neutrality theorem

The result, that the cost differential between constant and stop-and-go control vanishes, can be generalised if we rewrite (3) as

$$\tau \dot{I}_{\text{log}} = g - 1, \qquad I_{\text{log}} = \ln(I)\,, \tag{10}$$

where $g = g(t)$ is now an arbitrary function of time. We then have

$$I_{\text{log}}(t_{\text{end}}) - I_{\text{log}}(t_{\text{start}}) = \int_{t_{\text{start}}}^{t_{\text{end}}} \dot{I}_{\text{log}} dt = \int_{t_{\text{start}}}^{t_{\text{end}}} \frac{g(t)-1}{\tau} dt\,, \tag{11}$$

which proves that

$$\frac{1}{t_{\text{start}} - t_{\text{end}}} \int_{t_{\text{start}}}^{t_{\text{end}}} g(t)dt = 1 \tag{12}$$

for closed trajectories, viz when $I_{\text{log}}(t_{\text{end}}) = I_{\text{log}}(t_{\text{start}})$. Comparing with (5) shows that average economic costs are independent of which timeline $g(t)$ is used for controlling the epidemic. Note that the case of constant control, $g(t) \equiv g$, is included as a special case. If the economic costs of control are proportional to the reduction in the reproductions rate, one finds thus a 'neutrality theorem' for epidemic control. All trajectories which return to the point of departure (in terms of the infection rate) will lead to the same economic cost.

4 Mean number of infected under different control policies

The number of infected becomes the key criterion if the economic cost of different control policies (over complete cycles) is the same.

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For constant infection rates g, the cumulative number of infected between two times $t = t_0$ and $t = t_1$ is

$$\begin{aligned} X_{0,1} &= \int_{t_0}^{t_1} I(t)dt = I(t_0)\int_{t_0}^{t_1} \mathrm{e}^{(t-t_0)(g-1)/\tau}\, dt \\ &= \frac{I_0\tau}{g-1}\left(\mathrm{e}^{(t_1-t_0)(g-1)/\tau} - 1\right) = \frac{(I_1-I_0)\tau}{g-1} \end{aligned} \tag{13}$$

when using (3) and that $I_1 = I_0 \exp((t_1-t_0)(g-1)/\tau)$. Noting that (6) holds generally for constant g, we have

$$\Delta T_{0,1} = t_1 - t_0 = \frac{\tau}{g-1}\ln\left(\frac{I_1}{I_0}\right), \tag{14}$$

which leads to

$$\frac{X_{0,1}}{\Delta T_{0,1}} = \frac{I_1 - I_0}{\ln(I_1/I_0)} \tag{15}$$

for the overall number of infected, on the average per time unit. Note that

$$\ln\left(\frac{I_1}{I_0}\right) = \ln\left(\frac{I_1 - I_0 + I_0}{I_0}\right) \approx \frac{I_1 - I_0}{I_0} \tag{16}$$

for $I_1 \approx I_0$, from which the limit

$$\lim_{I_0\to I_1} \frac{X_{0,1}}{\Delta T_{0,1}} = I_0 \tag{17}$$

is recovered. Empirically it been observed that the cumulative medical load during the 'down phase' is about 30% higher than the cumulative load which follows the peak [16].

4.1 Bang-bang infection numbers

For stop-and-go control we have two periods with constant g, when I goes up and respectively down. With (15) we find that the total cumulative fraction of infected is determined by;

$$X_{\text{bang}} = \frac{I_{\text{large}} - I_{\text{low}}}{\ln(I_{\text{large}}/I_{\text{low}})} T_{\text{up}} + \frac{I_{\text{low}} - I_{\text{large}}}{\ln(I_{\text{low}}/I_{\text{large}})} T_{\text{down}} \tag{18}$$

for bang-bang control, and hence

$$\bar{I}_{\text{bang}} = \frac{X_{\text{bang}}}{T_{\text{up}} + T_{\text{down}}} = \frac{I_{\text{large}} - I_{\text{low}}}{\ln(I_{\text{large}}/I_{\text{low}})} \tag{19}$$

for the time-averaged number $\bar{I}_{\text{bang}}$ of infections.

Here we are interested in on-the-average stationary control strategies, for which the level I of infections is kept at the average. In Sect. 3.4 we did show

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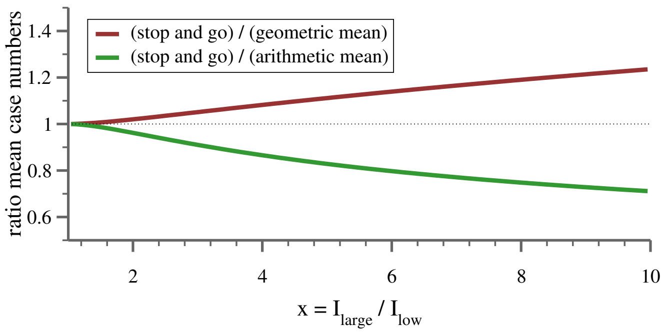

Figure 3: **Relative case numbers for distinct control policies.** For stop-and-go control case numbers oscillate between I_{low} and I_{large}. Alternatively considered are constant incidences, either at the arithmetic mean, $(I_{\text{low}} + I_{\text{large}})/2$, or at the geometric mean, $\sqrt{I_{\text{low}} I_{\text{large}}}$. Shown are the ratios of the respective per time case numbers, as given by Eqs. (20) and (21).

that stationary control results always in identical economic costs. This holds however not for the medical load, which can be considered to be the proportional to the average infection number $\bar{I}$. It is in particular of interest to compare the medical $\bar{I}_{\text{bang}}$, of stop-and-go control, with the policies keeping I at a constant, intermediate level or benchmark. A natural choice for this benchmark could the arithmetic mean, or midpoint, $I_{\text{mid}} = (I_{\text{large}} + I_{\text{low}})/2$. However, we will consider as well the geometric mean $I_{\text{geo}} = \sqrt{I_{\text{large}} I_{\text{low}}}$.

For the arithmetic mean, the ratio $\bar{I}_{\text{bang}}/\bar{I}_{\text{mid}}$ in mean infection numbers is

$$\begin{aligned} \delta\bar{I} \;=\; \bar{I}_{\text{bang}}/\bar{I}_{\text{mid}} &= \frac{X_{\text{bang}}}{T_{\text{up}} + T_{\text{down}}}\,\frac{2}{I_{\text{large}} + I_{\text{low}}} \\ &= \frac{x-1}{\ln(x)}\,\frac{2}{x+1} \;\le\; 1\,, \end{aligned} \tag{20}$$

when denoting $x = I_{\text{large}}/I_{\text{low}}$. In the limes $I_{\text{large}} \to I_{\text{low}}$, viz $x \to 1$, one has $\lim_{x\to 1} \delta\bar{I} = 1$, as expected. The limes $x \to 1$ is performed using the small $x - 1$ expansion $\ln(x) = \ln(1 + (x-1)) \approx x - 1$.

That $\bar{I}_{\text{bang}}/\bar{I}_{\text{mid}}$ is strictly smaller than unity for $x > 1$ can be seen, e.g., by plotting (20) as a function of x, as done in Figure 3. Numerically one finds a reduction of 29% at $x = 10$, which is somewhat higher than ratio of 7 to 1 observed in the actual values displayed in Fig. 1. Note that one has $2/(x+1) \le 1$ when $x \ge 1$, for the second term in (20), with $(x-1)/\ln(x) \le 1$ holding for the first term. The last relation holds because the log-function is concave, with a slope $d\ln(x)/dx = 1$ at $x = 1$.

The result that $\bar{I}_{\text{bang}} \le \bar{I}_{\text{mid}}$ implies that a policy of stop and go is superior to (less bad than) a policy of keeping infections constant half-way between the

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peak and trough. The two policies would have the same economic cost, but stop and go would lead to a lower overall medical load. Neither policy would of course be optimal in an unconstrained policy space. But our aim is merely to consider policies that have been adopted. The intuition behind this result can be seen from Figure 2 above. The infection curve lies more time below than above the arithmetic average, a well-known property of exponential growth.

The constant control policy serves only as benchmark. The results are much more general: As can be seen from (20) the average medical load falls as the ratio of the upper limit to the lower limit increases, while holding the (arithmetic) average constant. This implies that the medical load resulting from a 'stop and go' policy improves as one increases the upper and reduces the lower limit by the same amount.

Regarding the comparison to the geometric mean I_{geo}, we consider

$$\begin{aligned}\bar{I}_{\text{bang}}/\bar{I}_{\text{geo}} &= \frac{I_{\text{large}}-I_{\text{low}}}{\ln(I_{\text{large}}/I_{\text{low}})}\,\frac{1}{\sqrt{I_{\text{large}}I_{\text{low}}}}\\ &= \frac{x-1}{\ln(x)}\,\frac{1}{\sqrt{x}} \geq 1\,, \qquad (21)\end{aligned}$$

where we used $x = I_{\text{large}}/I_{\text{low}}$, as for (20). The functional dependence is included in Figure 3. It can be seen that it is favorable to keep the incidence at the geometric mean, instead of letting it oscillate between I_{low} and I_{large}. Moreover, the ratio of the respective average medical loads increases only modestly as the ratio between upper and lower limit increases. Letting case number vary by a factor of ten leads to an increase of 24%. It should not be surprising that keeping infections at the geometric mean is worse, given that the distance between the two means increases as the ratio of the upper to the lower limit becomes larger. For $9 = I_{\text{large}}/I_{\text{low}}$, the arithmetic mean is equal to 5 whereas the geometric mean is 3. Choosing the geometric as the intermediate point thus amounts to choosing a higher benchmark.

From a general viewpoint, if follows from (21) that the average medical load increases as the ratio of the upper limit to the lower limit increases, when holding the geometric average constant. This implies that the medical load resulting from a 'stop and go' policy increases as one increases the upper and reduces the lower limit by the identical factor. These considerations suggest that the decision regarding how wide apart to set the limits of a 'stop and go' or a regional 'traffic light' policy depends on whether one wants to keep the arithmetic or the geometric average constant.

4.2 Significance

The results discussed above apply only within the limits of hospital capacity. Once that limit has been reached any further increase in the medical load would imply rapidly rising medical and ethical costs. A lower medical load constitutes a sufficient criterion for preferring the stop and go policy, given that the respective economic costs vanish for different control paths as long as the initial infection

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incidence is reached again. Note that the medical load can be translated into economic costs [15], [17], [18], [19].

Most of the literature focuses on the economic value of the lives lost. However, that might be a mistake [2], as infections with less severe symptoms can also lead to considerable economic costs. One example of the economic cost of infections would be the loss of working time of those infected and with mild symptoms, when these individual have to self-isolate and cannot work for a certain time. This loss could be calculated as the number of weeks of working time due to symptoms (and self-isolation needs) and would be equal to a proportion of GDP [2]. To this one would have to add the hospitalisation costs for those with stronger symptoms and finally the economic value of lives lost. The lower medical load implied by a stop and go policy would thus also lead to lower overall economic costs.

A key difference between the present study and most existing literature concerns the kind of policies examined. Here we focus on a comparison between two representative, real-world policies, stop and go vs. constant control. Optimal control, which usually does not result in reversals of restrictions, is in contrast the goal for the majority of studies published so far. Contributions like [10] and [11] employ the matching framework, which is based on confinement as the main policy instrument and allows for economies of scale in lowering contagion as explained above. In such a framework it is clear that the optimal policy would be to impose tight restrictions from the start until the desired incidence is attained. For example, [10] finds that "the optimal confinement policy is to impose a constant rate of lock-down until the suppression of the virus in the population.".

Other contributions, which do not incorporate economies of scale in containment policies (either because they use constant returns in matching or because of a different view of social distancing restrictions) arrive at somewhat differentiated conclusions. For example, [15] where containment works like a consumption tax, find that containment should build up gradually and peak early when there exists the perspective of a vaccine being discovered.

5 Changing epidemic parameters

The native reproduction factor g_0 is, as a matter of principle, an intrinsic property of the virus. As such it can be measured only at the very start of a pandemic, namely when nobody is yet aware of what is happening. However, this reproduction factor can change over time, for example with the season. It is well known that the danger of contagion is much higher indoors than outdoors. It is in fact nearly impossible to catch the Coronavirus outside [20]. In the case of influenza virus this change in the reproduction rate leads to the typical 'flu season', which starts with the onset of colder weather (late in the year in the Northern Hemisphere). For the case of the Covid-19 virus other seasonal factors have also been mentioned, for example fluctuations sin UV light[21].

One thus needs to consider time-dependent g_0, which would correspond

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to the underlying spreading rate in a given societal state, 'without explicit, government-imposed restrictions'.

We assume that g_0 changes right at the top for the case of bang-bang control. Going up/down we then have $g_0 = g_0^{(\mathrm{up})}$ and $g_0 = g_0^{(\mathrm{down})}$. As a further simplification we postulate

$$T_{\mathrm{up}} = T_{\mathrm{down}}, \qquad g_{\mathrm{up}} - 1 = 1 - g_{\mathrm{down}}\,, \tag{22}$$

which is equivalent to $g_{\mathrm{up}} + g_{\mathrm{down}} = 2$. Compare (6).

5.1 Costs for the economy

Given that we assume that $T_{\mathrm{up}} = T_{\mathrm{down}}$, the per time economic costs of keeping constant infection numbers is

$$E_{\mathrm{const}} = \frac{c_e}{2}\left[\frac{g_0^{(\mathrm{up})}-1}{g_0^{(\mathrm{up})}} + \frac{g_0^{(\mathrm{down})}-1}{g_0^{(\mathrm{down})}}\right]. \tag{23}$$

For bang-bang control we have instead

$$E_{\mathrm{bang}} = \frac{c_e}{2}\left[\frac{g_0^{(\mathrm{up})}-g_{\mathrm{up}}}{g_0^{(\mathrm{up})}} + \frac{g_0^{(\mathrm{down})}-g_{\mathrm{down}}}{g_0^{(\mathrm{down})}}\right]. \tag{24}$$

The difference is

$$E_{\mathrm{bang}} - E_{\mathrm{const}} = \frac{c_e}{2}\left[\frac{1-g_{\mathrm{up}}}{g_0^{(\mathrm{up})}} + \frac{1-g_{\mathrm{down}}}{g_0^{(\mathrm{down})}}\right], \tag{25}$$

which simplifies to

$$E_{\mathrm{bang}} - E_{\mathrm{const}} = \frac{c_e}{2}\left[\frac{1}{g_0^{(\mathrm{down})}} - \frac{1}{g_0^{(\mathrm{up})}}\right]\underbrace{\big(1-g_{\mathrm{down}}\big)}_{>0} \tag{26}$$

when using the $T_{\mathrm{up}} = T_{\mathrm{down}}$ condition that $g_{\mathrm{up}} - 1 = 1 - g_{\mathrm{down}}$, see (22). Going doing down we restrict, viz $g_{\mathrm{down}} < 1$ holds.

Bang-bang control is therefore favorable when $g_0^{(\mathrm{down})} > g_0^{(\mathrm{up})}$. This result would support the pattern observed in Europe where during the summer (when g_0 was lower than before) governments eased restrictions, imposing them again during the fall when g_0 increased again. The economic cost of the restrictions could thus be concentrated during the period when they had a higher yield in terms of infections avoided.

6 Conclusions

During the first wave of the Covid-19 pandemic the key concern was to 'flatten the curve'. Harsh lock-down measures were needed when health systems were

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overwhelmed by the sudden increase in hospitalizations, many of which required intensive care units. The second wave has so far resulted in a somewhat lower medical load, but it proved nevertheless indispensable to re-introduce some social distancing measures as otherwise the case load would have continued to increase at a near exponential rate. Countries have taken in this regard different approaches. In some, the measures have been just enough to stabilize infections. In others, the measures have led to a strong fall in new cases and governments plan to lift restrictions soon - which is likely to lead to a renewed increase.

Several countries introduced, interestingly, regionally graduated systems, which allow regions and cities to oscillate between periods of harsh restrictions that are triggered when infection numbers surpass certain thresholds, and periods of lower restrictions which start when numbers have fallen again, now below a given threshold. These kind of quasi-automatized threshold containment policies are graded realization of the stop and go policy examined in the present study.

We have shown in the context of our model, that any time path of restrictions which returns to the point of departure in terms of the infection rate, a scenario like to occur within rule-based 'traffic light' systems, imply the same overall economic costs. Our analysis suggests furthermore that stop and go policies might not be as costly as it might appear at first sight, at least if compared to the alternative of keeping the incidence rate constantly the midpoint. The economic cost would be the same, but the overall medical load of stop and go should be lower. The result is reversed for the alternative of keeping infections at the (lower) geometric mean. Compare Figure 2.

Applied to regional 'traffic light' systems, our results imply that increasing the upper, and reducing at the same time the lower threshold by the same absolute amount ΔI, via $I_{\text{upper}} \to I_{\text{upper}} + \Delta I$ and $I_{\text{lower}} \to I_{\text{lower}} - \Delta I$ leads to a lower medical load. The opposite would be true if one were to increase the upper, and at the same time reduce the lower threshold by the same proportion, $f_I > 1$, this time using the rescaling $I_{\text{upper}} \to I_{\text{upper}} f_I$ and $I_{\text{upper}} \to I_{\text{upper}}/f_I$. The latter procedure would increase the arithmetic average since the absolute increase in the upper limit would be now higher than the fall in the lower limit. This implies that any choice regarding the range between the upper and lower thresholds in a regional 'traffic light' must take this difference between arithmetic and geometric mean into account.

We also show that it makes sense for policies to react to seasonal variations in the native reproduction factor. The economic cost of tight social distancing should be incurred in winter, when infections would otherwise be high and rising. Bang hard on the virus when Santa Claus knocks at the door.

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Consumption responses to COVID-19 payments: Evidence from a natural experiment and bank account data[1]

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We document households' spending responses to a stimulus payment in Japan during the COVID-19 pandemic. The Japanese Government launched a universal cash entitlement program offering a sizable lump sum of money to all residents to alleviate the financial burden of the pandemic on households. The timings of cash deposits varied substantially across households due to unexpected delays in administrative procedures. Using a unique panel of 2.8 million bank accounts, we find an immediate jump in spending during the week of payments, followed by moderately elevated levels of spending that persist for more than a month. The implied marginal propensity to consume is 0.49 within 6 weeks. We also document sizable heterogeneity in consumption responses by recipients' financial status and demographic characteristics. In particular, liquid asset holdings play a more crucial role than total asset holdings, suggesting the importance of the wealthy hand-to-mouth.

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1 Introduction

The COVID-19 pandemic and the subsequent lockdowns had severe impacts on household budgets. A large number of studies have documented drastic declines in income, spending, and debt payments in various countries, including the United States (Baker et al., 2020a; Chetty et al., 2020; Coibion et al., 2020a; Cox et al., 2020), the United Kingdom (Hacioglu et al., 2020; Carvalho et al., 2020), Spain (García-Montalvo and Reynal-Querol, 2020), Sweden and Denmark (Sheridan et al., 2020), and Japan (Watanabe, 2020). Moreover, the pandemic shock disproportionately affected groups with certain socioeconomic backgrounds and from different sectors; for instance, those in the service industry, those in occupations that cannot be performed at home, low socioeconomic status households, minorities, women, youth, and parents were particularly susceptible (See, e.g., Forsythe et al. (2020); Alon et al. (2020); Montenovo et al. (2020) for the US, Blundell et al. (2020) for the UK, Kikuchi et al. (2020) for Japan, and Adams-Prassl et al. (2020); Belot et al. (2020) for cross-country studies).

To mitigate economic shocks, governments have enacted urgent relief measures, primarily in the form of cash payments. Most OECD countries have introduced new systems for household cash transfers (OECD, 2020). These programs' extraordinary budgets and legislators' inclinations toward further stimulus call for a serious quantitative evaluation of COVID-19 cash transfer policies. These quantitative evaluations also provide implications for key macroeconomic variables, such as households' marginal propensities to consume (MPCs) and fiscal multipliers.

In this paper, we examine the *Special Fixed Benefits* program (hereafter, SFB), a large-scale cash-transfer program launched by the Japanese government in response to the COVID-19 pandemic. The program entails a fixed and sizable cash transfer amounting to 100,000 JPY (approximately 950 USD) to every individual in Japan who applies regardless of age, income, family size, and employment. The payment policy provides a "natural experiment" because the timing of pay-

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ments was rendered nearly random due to the huge overcapacity in the administrative procedures at local offices. Our data reveal a continuous and bell-shaped distribution of payment days between the second week of May and the end of August. Variations in timing of payments allows us to estimate precisely the immediate effect of SFB payments on household spending within a brief temporal window. Our event study approach lets us separate the effects of SFB payments from the effects of pandemic shocks, stay-at-home measures, and other policies.

We examine high-frequency transaction-level data for 2.8 million personal accounts at Mizuho Bank, one of Japan's three largest commercial banks, that recorded SFB payments during 2020. The dataset is a panel of bank accounts, containing information about the account balances, the inflows to and outflows from the accounts, and basic demographic information about the account holders. We explore the heterogeneous effects on consumption of such demographic characteristics as account holders' income, income loss attributable to the pandemic, total assets, demand deposit balances, and liquidity constraints. Our main indicator of spending is total outflows, including ATM withdrawals, transfers to other bank accounts, and credit card charges. As a result, our estimates of MPC constitute an upper bound on household spending.[1]

Our main findings imply that the MPC is 0.49 within six weeks of receipt among all samples. Our estimates are comparable to those obtained from studies of the U.S. Coronavirus Aid, Relief, and Economic Security (CARES) Act.[2] Among dynamic response, we find an immediate jump in spending during the week of payments and thereafter moderate increase that persists more than a month.[3] ATM withdrawals constitute 63% of the total outflows, a reasonable percentage given

[1]Evidence from the US COVID-19 stimulus package suggests this upper bound reasonably captures actual spending. Small differences appear between MPCs estimated by a consumption survey (Coibion et al., 2020b) and by total outflows from bank accounts (Karger and Rajan, 2020).

[2]Karger and Rajan (2020) and Misra et al. (2020) find MPCs of 0.50 and 0.43, respectively, using the financial transaction data from Facteus.

[3]The sustained increase in expenditure may be attributable to our large sample size. Previous evidence from Japan is unstable to specifications and data. Shimizutani (2006) analyzes the 1998 tax cut and reports an MPC of 0.6 during its first month of implementation, but its effects nearly dissipate as shown by negative coefficients during the second month. Koga and Matsumura (2020) leverage a subjective question in the Survey of Household Finance and elicit a self-reported MPC of 0.73. However, they note a significant bias in self-reported data and find a more

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that cash is the major payment method in Japan (Fujiki and Tanaka, 2018).

We observe heterogeneity in MPC across recipients' financial status and demographic characteristics. One substantial variation is caused by credit constraints. Recipients with binding credit constraints spend more (59%) of their SFB transfers. MPC hinges on the liquidity of assets. We find significant variation in MPC by quartile of demand deposits, one of the most liquid assets. However, total asset holdings make almost no difference in MPC. We also find a large MPC among wealthy hand-to-mouth, who hold a small demand deposit balances but extensive total assets. This result implies the crucial roles of the distributions over both liquid and illiquid asset holdings in fiscal and monetary policy analyses (Kaplan and Violante, 2014; Kaplan et al., 2018). Analysis of household income reveals a modestly large MPC (0.54) among households that suffered COVID-related losses exceeding 50% of their 2019 income. On the other hand, there is little variation in MPC across income quartiles. We also find that SFB exhibits a uniform effect across family size. Taken together, our results suggest both the potential improvements and limitations of targeting transfers when designing programs to stimulate consumption.[4]

Related Literature Our study extends a growing literature on the estimates of the MPCs from COVID-19 stimulus payments. One strand of the literature examines the US CARES Act. Baker et al. (2020b) use financial transaction data from fintech app users to derive an MPC spanning 0.25 to 0.35. Respondents to a survey by Coibion et al. (2020b) self-report an average MPC of approximately 0.4, although MPC varies considerably across respondents' attributes such as homeownership and liquidity constraints. Karger and Rajan (2020) use transaction-level data for

conservative MPC of 0.16 by tracking transitory income shocks in the Japan Household Panel Survey. Examining another type of cash transfer, Hsieh et al. (2010) found an MPC of 0.1–0.2 for a shopping coupon policy implemented in 1999. Macroeconomists have supposed the recent Japanese MPC values to be low because of the small fiscal multipliers estimated from time-series analyses; for example, see Auerbach and Gorodnichenko (2017). Employing a quantitative macroeconomic model, Braun and Ikeda (2020) emphasize the role of Japanese SFB policy during the current pandemic in mitigating consumption inequality.

[4]Indeed, stimulating aggregate consumption does not directly lead to welfare improvement. Nygaard et al. (2020) theoretically studies optimal policy design and find that the CARES Act would have improved social welfare by redistributing payments toward low-income and young recipients.

debit cards from Facteus and find an average MPC of 0.5. They also find considerable heterogeneity in consumption responses that can be partially attributed to observable characteristics such as age, income, and location. Using regional variations in the timing of stimulus payments and Zip code data from Facteus, Misra et al. (2020) find an average MPC of 0.43 during the initial four days upon receiving a payment and decompose their results by product categories. Chetty et al. (2020) build a real-time zip code-level database of socio-economic variables obtained from private companies. They find significant jumps in consumer spending and business revenue around mid-April, which overlaps the period of payments from CARES Act. Previous studies also estimate MPCs from the 2001 Economic Growth and Tax Relief Reconciliation Act and the 2008 Economic Stimulus Act in the US (Shapiro and Slemrod, 2003; Johnson et al., 2006; Agarwal et al., 2007; Shapiro and Slemrod, 2009; Parker et al., 2013; Broda and Parker, 2014; Misra and Surico, 2014).

A few studies estimate MPCs outside the United States. Liu et al. (2020) study a temporary small-scale digital discount coupon intended to aid Chinese consumers during the COVID-19 crisis. Using transaction-level data from Alipay e-wallet, they find strikingly high MPC of 3.4 to 5.8, which they attribute to consumers' behavioral reactions. Kim and Lee (2020) examine a household consumption voucher initiative in South Korea, and find that most survey respondents planned to spend more on necessities.

Our study offers multiple contributions to the literature concerning stimulus payments and MPC, especially as pertains to the COVID-19 pandemic. First, we exploit the random variation in timings of payments in an event study design. Second, our results are easily interpreted due to the Japanese government's fixed-payment scheme. Third, our use of high-frequency bank account data featuring millions of observations lets us derive precise estimates. Fourth, our results accurately reflect personal MPCs because we observe the date of SFB payments.

This study also ties to empirical studies that use bank account data to investigate the economic

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effects of the COVID-19 pandemic. Farrell et al. (2020) examine the effects of US Unemployment insurance benefits using Chase bank account data that shares characteristics with our data. The estimated MPC of unemployment insurance benefits (0.73) is reasonably comparable with our results for financially constrained households. Sheridan et al. (2020) use data from Danske Bank in Scandinavia to compare individual spending responses in Denmark, which imposed strict shutdown policies, and Sweden, which imposed moderate stay-at-home orders. Their results imply that social distancing causes only minor decreases in spending. Carvalho et al. (2020) use transaction-level data from the BBVA in Spain to document changes in consumption during the pandemic and lockdown. Bounie et al. (2020) document a sharp rise in wealth inequality during the pandemic using randomly selected accounts at a French bank, Credit Mutuel Alliance Federale.

2 The COVID-19 Pandemic and the Special Fixed Benefits Policy in Japan

2.1 COVID-19 Crisis in Japan

The first COVID-19 case was reported on January 6, 2020, which is relatively early compared with the timeline of the first cases in other countries. The number of reported cases began to increase after January 28 as Japan faced shortages of masks and hand sanitizers. Several weeks into the pandemic, an outbreak occurred on the Diamond Princess cruise ship that resulted in 712 infections and 13 deaths. The spread of COVID-19 was substantially slower in Japan than in many other countries, but the number of cases in Japan still exhibited a pattern of exponential growth. To combat the rise in infections, the Japanese Government mandated a temporary closure of all elementary, middle, and high schools on February 27. On March 24, Japan postponed the Tokyo 2020 Summer Olympics and Paralympics.

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The number of cases rose rapidly after late March, given Japan's weak surveillance and limited capacity for PCR testing. The Japanese Government announced its first state of emergency on April 7 for urban areas, and extended it nationwide on April 16. Unlike the stringent lockdown policies of other countries, Japan's announcement lacked legal binding force. Nevertheless, the de-facto stay-at-home order reduced outings 20% (Watanabe and Yabu, 2020). The first-wave of the pandemic peaked around mid-April and was nearly contained by mid-May. The state of emergency was lifted selectively on May 14 and everywhere on May 25. Aggregate damage to public health in Japan was relatively minor: by May 25, Japan had reported 16,706 cases and 846 deaths among its 126.5 million people.

Economic damage was substantial, however. Compared to consumption in April and May of 2019, consumption in 2020 was 11.1% lower in April and 16.2% lower in May 2020, according to the Family Income and Expenditure Survey. The unemployment rate changed little because of Japan's entrenched employment protection, according to the labor force survey, but hours worked fell 3.9% in April and 9.3% in May. Wages of full-time workers dropped 0.7% in April and 2.8% in May.

2.2 The Special Fixed Benefit Payment

We evaluate responses of household expenditures to the SFB policy, the largest COVID-19 relief program in Japan's first supplemental-spending bill.[5] The program entitled all Japanese residents to a one-time payment without restrictions on age, income, family size, or nationality. The amount allocated per person was 100,000 Japanese JPY (approximately 950 USD), about 42% of the median monthly earned income of a full-time worker.

All Japanese households were notified by mail, and asked to apply online or by mail. A head of households applied for benefits for all family members in the same residence. Application requires

[5] Ando et al. (2020) summarize Japan's COVID-19 relief programs.

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individual identification numbers of all family members and bank account information.[6] In households with more than one resident, all benefits were deposited into the head of household's bank account.

The SFB payment is unique and it is ideal for studying consumer responses to a one-time fiscal stimulus for two reasons. The SFB was the only universal, fixed-sum COVID-19 relief payment among advanced economies. According to OECD (2020), all other OECD countries imposed conditions on transfers (e.g., income eligibility thresholds or enrollment in social security).[7] Second, and more importantly, the payment date was nearly random within a range of several weeks because of administrative constraints. Local governments had to check each household's application and send remittances to bank accounts manually.[8] Applications were primarily by mail because online application failed following technical difficulties. This led to an overflow of mailed applications and subsequent administrative errors. When such errors occurred, local offices had to correspond with applicants by phone or email to correct them. Some municipalities denied applications with incomplete entries or errors. For instance, 20% of applications in Saga city, a middle-sized municipality, had errors.[9]

News reports suggest that payment dates varied, depending on cities' administrative capacities and the experience of office staff. Among large cities, Sapporo city had nearly completed remittances to all applicants by mid-June, Nagoya city had not even finished sending applications to

[6] Although applying was not mandatory, nearly all Japanese households applied. For instance, 98% of residents in Yokohama had applied for SFB by August 31, 2020.
City of Yokohama webpage: https://www.city.yokohama.lg.jp/lang/covid-19-en/fixed-sum.html.

[7] South Korea's universal COVID-19 financial relief program issued vouchers (Kim and Lee (2020)), but, the amount was small ($83 to $332 USD per person) and depended on household size and municipality of residence. Vouchers could be used only within a household's region of residence, except for bars, clubs, online stores, and large retailers. The CARES Act in the US was the only relief program comparable to Japan's SFB payments (Baker et al., 2020b; Chetty et al., 2020; Coibion et al., 2020b; Li et al., 2020; Misra et al., 2020). Amounts provided by CARES were large enough to detect consumer responses to the stimulus, but they were means-tested: a single person earning below 75,000 USD was entitled to the full amount (1,200 USD), but higher-income households received substantially less. A similar income-payment scheme was applied to married couples. Each child was eligible for 500 USD.

[8] Individual identification numbers are not linked to bank accounts in Japan, unlike elsewhere (e.g., the US).

[9] Saga TV, *20% of Application Documents for 100,000 JPY Payment are Incomplete in Saga City, Saga Prefecture* (in Japanese), May 21, 2020.
https://www.sagatv.co.jp/news/archives/2020052102735

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residents.[10] In Tokyo Prefecture, 85% of applicants living in Nerima Ward received payments by June 30, whereas only 34.5% of residents in Edogawa Ward had received payments by then.[11] Even applicants from the same municipality received payments at different time. Applications submitted on the same day could result in differences in the timing of payments by several days.[12] One error on an application could delay a payment by more than two weeks.[13] Such lack of uniformity in time-to-payment was unique among COVID-19 relief programs worldwide. For example, time-to-payment in the US differed only by whether payment was by direct deposit or paper check (Baker et al. (2020b)).

3 Data and Descriptive Analysis

3.1 Data

Our data are account-level daily transactions from more than 24 million accounts at Mizuho Bank spanning January 2019 to August 2020. Data include transaction dates, payments and withdrawals, remarks about each transaction, and end-of-month balances. Data also identify some specific transactions (e.g., salary payments and ATM withdrawals). We also have demographic information such as age, gender, and municipality level address. Data were anonymized for all account holders.

To examine how the SFB payment affected household consumption behavior during the COVID-19 crisis, we restrict our sample to accounts that had received the payment by August 31. We identify those accounts through transaction remarks and deposit amount.[14] The resulting sample of 2.8 million head-of-household accounts constitutes 4.8% of all Japanese households. Although

[10] Nikkei news, "*Significant delay in 100,000 JPY benefit in Nagoya city: long time for system maintenance*" (in Japanese), June 26, 2020.

[11] J-CAST news, *What we ask the Governor of Tokyo is "Quick transfer of 100,000 yen."* (in Japanese), July 6, 2020.

[12] Some municipalities, including Edogawa Ward, announced such case in their web page.

[13] Saga TV, May 21, 2020.

[14] We identify SFB payments in two way: by remarks on accounts indicating an SFB payment (*Teigaku* or *Kyufu* in Japanese) and by deposits that were exactly multiples of 100,000 JPY.

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Table 1: Summary Statistics

Panel A: Account level variables	N	Mean	St. Dev.	25%	Median	75%
SFB Payment (JPY)	2,832,537	204,125	118,615	100,000	200,000	300,000
Week of Deposit	2,832,537	26.913	2.778	25	27	29
Age	2,804,678	53.065	17.707	39.000	52.000	66.000
Female dummy	2,809,140	0.258	0.438	0.000	0.000	1.000
Wealth (JPY)	2,809,140	4,200,102	17,019,527	127,000	650,000	3,440,000
Demand-deposit balance (JPY)	2,809,140	2,721,917	11,549,024	94,000	444,000	2,092,000
Monthly Salary in 2019 (JPY)	1,419,299	276,653	392,953	169,328	250,446	342,939
Monthly Salary in 2020 (JPY)	1,419,299	272,650	374,138	166,342	245,522	338,404
COVID-19 Shock Dummy 1	1,419,299	0.129	0.335	0.000	0.000	0.000
COVID-19 Shock Dummy 2	1,419,299	0.097	0.296	0.000	0.000	0.000
Liquidity Constraint Dummy	1,201,443	0.283	0.450	0.000	0.000	1.000
Panel B: Account-week level variables	N	Mean	St. Dev.	25 %	Median	75%
Total Withdrawal in 2020 (JPY)	99,138,795	132,855	2,476,128	0	19,569	95,860
Cash Withdrawal in 2020 (JPY)	99,138,795	31,885	134,501	0	0	21,000

Notes: COVID-19 shock dummy 1 and 2 represent 15-50% and more than 50% decline in monthly salary in April and May 2020, respectively. Liquidity constraint dummy takes 1 if an end-of-month account balance was below account holders' monthly income.

our sample size is abundant and rich in data, it represents only accounts at Mizuho Bank. Mizuho has branches throughout Japan, but accounts concentrate in larger cities, especially around Tokyo. Therefore, our analysis may reflect the behavior of urban recipients of the SFB payment.

3.2 Descriptive Statistics

Table 1 presents summary statistics of our dataset. We drop the minimums and the maximums of all variables to maintain anonymity. Panel A summarises account-level variables. The average SFB payment is approximately 200,000 JPY. This is because payments are fixed at 100,000 JPY per person, and there are, on average, two family members residing in each household of our sample. Their average wealth in April 2020 was approximately 4.2 million JPY; demand-deposit balance (2.7 million JPY) comprised 65% of wealth. Table 1 reports that head-of-household income averaged 270,000 JPY in April and May 2019 and 2020. It had slightly declined between 2019 and 2020.

Note that monthly salary is customarily deposited in employees' bank accounts in Japan.[15]

We construct two variables to measure financial distress from the COVID-19 crisis. The first variable (COVID-19 Shock Dummy 1) is a dummy that indicates whether account holders experienced a 15-50% declines in monthly salary in April and May 2020 relative to those months of 2019. This group includes employees placed on temporary leave.[16] Leave allowances usually equal 60% to 80% of salaries. The second variable (COVID-19 Shock Dummy 2) indicates declines in monthly earned income exceeding 50%. As Japan's unemployment rate remained nearly unchanged, this variable captures self-employed workers whom the COVID-19 crisis damaged considerably. The Japanese government assists small business whose revenues fell more than 50% from pre-crisis levels. Table 1 indicates that 13% of households experienced 15–50% declines in income and that 10% experienced declines exceeding 50% from the same months of the previous year.

Our dummy to measure liquidity constraints takes a value of 1 if an end-of-month account balance was below account holders' monthly income. We define "the end of the month" as the month preceding to the account's SFB payment. Results show that about 28% of accounts are liquidity constrained.

Panel B of Table 1 presents summary statistics of weekly account total and cash withdrawals as time-variant dependent variables of interest. Withdrawals include remittances to other bank accounts, for utilities and rent, and credit card payments. The mean (median) of weekly cash withdrawals is 132,855 JPY (19,569 JPY). The mean (median) of weekly cash withdrawals is 31,885 JPY (0 JPY), indicating no cash was withdrawn from at least half of sampled accounts in any given week.

[15] This salary likely reflects the head of household's monthly disposable income because tax and social insurance usually are deducted monthly in Japan.

[16] Granting temporary leave has been Japanese companies' dominant response to COVID-19 because layoffs are difficult under Japan's employment protection. In response to the economic recession related to COVID-19, Japanese Government has proposed Employment Adjustment Subsidies for paying leave allowance.

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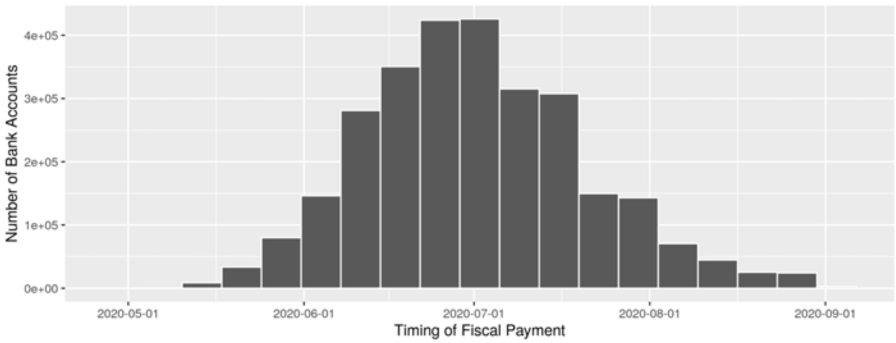
Figure 1: Timing of Deposits of SFB Payments



3.3 Timing of the SFB Payment

Figure 1 is a histogram of numbers of SFB payments to households over the sampled period. The majority of transfers appear between late June and early July, the earliest appears in May, and the last appears during the final week of August. As Section 2.2 discussed, the administrative delays made the timing of payments unpredictable. We provide further support for this claim by regressing the week of households' SFB recipience on various demographic variables and geographic indicators in Appendix A.

4 Empirical Strategy

To estimate the effect of SFB payments on household spending, we leverage weekly variations in timing of SFB payments across households in the following event study specification:

$$y_{itw} = \alpha_i + \alpha_{iw} + \alpha_{tw} + \sum_{k=a}^{b} \gamma^k D_{itw}^k + u_{itw}, \tag{1}$$

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where y_{itw} is an outcome measure for bank account i in week $w(= 1, \ldots, 35)$ in year $t(= 2019, 2020)$. We use both cash and total withdrawal per capita as a measure of household spending.[17] α_i denote account-level fixed effects that capture time-invariant heterogeneity across households. α_{iw} is account-by-week fixed effects that control for seasonal patterns of consumption that are specific to each household. For instance, families with small children increase consumption significantly more than single-member households around Children's day in early May. α_{tw} are the year-by-week fixed effects that capture aggregate shocks and national policies. Later, we allow these time fixed effects to vary across regions (i.e., prefectures) to account for the potentially heterogeneous economic effects of COVID-19 across prefectures. u_{itw} is idiosyncratic error.

The independent variable of interest is D^k_{itw}, where $D^k_{itw} = \mathbf{1}\{w - T_i = k\}$, and T_i denotes the week in which account i receives an SFB payment. Let $k \in [a, b]$ be the event-time relative to the week households receive SFB payments. The week prior to the deposit corresponds to $k = -1$, and the week of payment is given by $k = 0$. We set $a = -5$ and $b = 5$ in our analysis. Coefficient γ^k (for $k >= 0$) captures household spending responses k-weeks after deposit of SFB payments. We also include the lead terms (for $k < 0$) to test for the presence of the pre-trends in the k weeks preceding the payment. We normalize the coefficient γ^{-1} to 0.

Estimating Eq. (1) directly using a within transformation is computationally intractable because of the enormous sample size, which amounts to approximately 200 million weekly-account cells spanning two years of transactions. Therefore, we begin by computing differences across observational years to eliminate fixed effects α_i and α_{iw} from Eq. (1). The resulting specification is

$$\Delta y_{iw} = \Delta\alpha_w + \sum_{k=a}^{b} \gamma^k D^k_{iw} + \Delta u_{iw}, \tag{2}$$

[17] We are able to infer household size from amounts of SFB payments because each person receives 100,000 JPY.

where $\Delta x_{iw} = x_{i,2020,w} - x_{i,2019,w}$ denotes changes in variable x from 2019 to 2020 within each unique bank account and week. We then employ ordinary least squares to estimate Eq. (2) and cluster the standard error at prefectural level to account for the serial correlation across households and over time.

5 Estimation Results

5.1 Results on Full Sample

Table 2 reports estimation results of our event-study analysis. Results of full-sample regressions appear in Columns (1)-(4). The dependent variable in Columns (1) and (2) is total outflows from accounts in a given week. Columns (3) and (4) examine weekly cash withdrawals as the dependent variable. Columns (2) and (4) allows the time fixed effects to interact with prefecture fixed effects.

Households immediately increased total withdrawals by approximately 19,000 JPY during the week they received SFB payments. Although the response is moderate in the weeks after the deposit, it remains sizable and statistically significant. Columns (3) and (4) indicate that households withdraw 15,000 JPY in cash during the week of the deposit, suggesting that spending responses are driven primarily by cash withdrawals. This finding accords with Japanese households' preference for cash over credit or debit cards for purchases.

The right and left panels of Figure 2 plot the event study coefficients $\hat{\gamma}^k$ from Columns (2) and (4), respectively, in Table 2. The figure confirms a spike in withdrawals upon receiving SFB payments. Moderate but statistically significant positive coefficients of withdrawals persisted for five weeks.

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Table 2: Results from Event Study Analysis

	Dependent Variables																
	Total Outflows		ATM Withdrawal		Total Outflows												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
5 weeks prior to payment	−3,452	−3,522	−152	−146	−6,405	−1,319	−2,123	341	−76	−3,741	4,953	−220	−5,038	−309	2,299	−1,351	−7,388
	(1,977)	(1,933)	(79)	(73)	(3,768)	(1,310)	(954)	(1,052)	(401)	(790)	(3,336)	(616)	(2,097)	(265)	(3,137)	(1,664)	(4,642)
4 weeks prior to payment	955	879	−106	−135	2,527	99	−2,776	−2,103	−262	−3,198	2,786	−332	982	−1,088	3,612	−128	2,812
	(1,486)	(1,502)	(67)	(67)	(2,587)	(1,431)	(978)	(938)	(704)	(1,455)	(2,158)	(688)	(1,262)	(323)	(2,890)	(651)	(3,285)
3 weeks prior to payment	−587	−627	150	106	−1,105	1,961	−2,710	−4,511	54	−1,568	2,025	−579	293	−960	5,275	−3,230	−660
	(1,179)	(1,102)	(109)	(119)	(2,559)	(2,631)	(1,363)	(708)	(693)	(1,171)	(889)	(599)	(1,100)	(288)	(2,879)	(1,318)	(2,475)
2 weeks prior to payment	2,648	2,619	49	8	4,174	1,998	−338	−513	1,041	2,082	3,594	−1,379	1,324	−184	6,459	121	5,307
	(1,054)	(1,128)	(92)	(99)	(2,217)	(1,881)	(1,328)	(492)	(789)	(1,042)	(3,063)	(527)	(1,766)	(176)	(3,550)	(1,328)	(2,341)
Week of payment	19,190	19,050	15,100	15,096	16,418	18,030	21,281	21,393	16,946	18,906	26,918	33,956	13,978	28,675	19,502	9,384	10,163
	(799)	(806)	(312)	(311)	(1,433)	(2,574)	(2,523)	(529)	(726)	(1,082)	(2,835)	(843)	(1,210)	(288)	(2,500)	(1,483)	(1,280)
1 week after payment	11,708	11,534	8,093	8,126	12,206	9,674	9,914	11,040	12,753	9,032	14,383	13,434	11,267	13,354	18,140	8,606	8,917
	(1,550)	(1,492)	(133)	(126)	(1,826)	(557)	(3,650)	(1,405)	(1,242)	(2,007)	(2,051)	(1,308)	(2,781)	(636)	(4,264)	(1,293)	(2,196)
2 weeks after payment	5,550	5,416	3,291	3,323	5,449	6,028	4,362	2,759	6,993	3,503	5,178	5,206	5,660	5,321	−93	2,964	6,153
	(1,371)	(1,277)	(119)	(109)	(1,653)	(1,905)	(957)	(542)	(1,205)	(927)	(3,852)	(745)	(2,371)	(153)	(1,136)	(1,541)	(2,609)
3 weeks after payment	5,064	5,048	2,022	2,045	6,399	2,343	3,021	4,712	2,370	3,598	2,693	2,604	4,048	3,343	−36	3,218	7,225
	(930)	(1,002)	(86)	(82)	(2,286)	(756)	(1,019)	(1,095)	(762)	(2,345)	(2,570)	(637)	(1,263)	(315)	(4,778)	(1,887)	(1,948)
4 weeks after payment	2,872	2,778	1,481	1,498	2,307	3,497	2,220	1,686	2,661	5,263	3,286	1,194	3,800	1,701	5,424	2,415	3,234
	(922)	(938)	(108)	(108)	(1,497)	(1,224)	(1,085)	(668)	(616)	(2,546)	(1,200)	(1,473)	(1,335)	(450)	(1,551)	(1,765)	(1,802)
5 weeks after payment	4,942	4,825	1,159	1,165	5,797	6,301	1,523	3,445	3,800	2,636	1,678	−19	7,222	1,320	270	−1,647	9,213
	(1,908)	(1,892)	(89)	(92)	(2,442)	(2,790)	(726)	(801)	(1,582)	(1,492)	(1,399)	(532)	(4,068)	(263)	(3,510)	(1,309)	(3,578)
Family Sizes					1	2	3	4									
COVID-19 Income Shocks									$< 15\%$	$15-50\%$	$> 50\%$						
Liquidity Constraints												Yes	No				
Demand-Deposit Balance														Low	Low	High	High
Total Wealth														Low	High	Low	High
Week FE	Yes		Yes														
Week*Prefecture FE		Yes		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size (millions)	98.1	98.1	98.1	98.1	44.3	23.3	15.4	11.8	30.0	5.1	3.1	11.8	30.1	44.0	5.1	5.0	44.0

Notes: The standard errors are in parenthesis and clustered at prefecture level.

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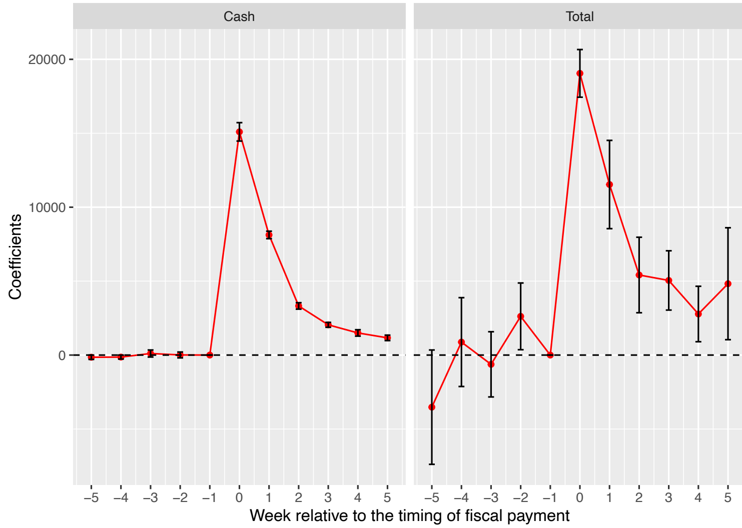
Figure 2: Responses to SFB Payments: Full Sample



Notes: The figure plots the estimated coefficients $\hat{\gamma}^k$ for $k \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$. Estimates are from Columns (2) and (4) in Table 2. Note that γ^{-1} is normalized to 0. Bars indicate 95 percent confidence intervals. Standard errors are clustered at prefectural level.

5.2 Discussion on Identifying Assumptions

A key identifying assumption in our event-study design is the parallel trend in withdrawal amounts between households that differ in the timing of SFB deposits. Although our assumption of a parallel trend is not directly testable, we provide supporting evidences.

First, we examine the lead coefficients on the event study design. Most of the coefficients before SFB payments are statistically insignificant (right panel of Figure 2) or are precisely estimated as zero (left panel). These estimates imply absence of any pre-trend in household consumption, suggesting that a parallel trend assumption is likely to hold. This finding aligns with our discussion in Section 3.3 and Appendix A on the plausible exogeneity of payment timings within a narrow time window.

Second, we investigate the robustness of our results by including week-by-prefecture fixed ef-

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fects. The concern here is the correlation between the timing of SFB payments and regional macroeconomic shocks, which arise from differing industry composition and the spread of COVID-19. Columns (1) and (3) in Table 2 control only for week fixed effects and Columns (2) and (4) account for week-by-prefecture fixed effects to address this concern. We are reassured that the magnitude of our coefficients are statically and economically robust. Our discussions will be based on the results of week-by-prefecture fixed effects.

5.3 Heterogeneous Impacts of Fiscal Payments

We explore heterogeneous responses in consumption to SFB payments by dividing account holders into sub-samples. Table 2 and Figure 3 summarize the corresponding coefficients and standard errors of groups categorized by (a) family size, (b) COVID-19 income shocks, (c) liquidity constraints, and (d) demand deposit balances and total asset holdings. In the Appendix, we report the results for the sub-samples defined by quartiles of demand-deposit balance (Table B.2 and Figure B.1), total wealth (Table B.3 and Figure B.2), and monthly salary (Table B.4 and Figure B.3), respectively. Hereafter we base our discussion on the specifications of total outflows as the dependent variable with control for prefecture-by-week fixed effects.

(a) Family Size We first consider the heterogeneity in family size to check the validity of normalizing spending to a per-person amount. Family size is identified by the amount of SFB payments because the payment per person is fixed at 100,000 Japanese Yen. Regression results for single-, two-, three- and four-person families are shown in Column (5), (6), (7), and (8), respectively, in Table 2. The coefficients and standard errors are also plotted in Panel (a) of Figure 3. Larger families tended to respond slightly more during the week of the deposit, though there is not much difference in expenditure per person.[18]

[18] Our bank account data do not have detailed information about family structure such as the number of children.

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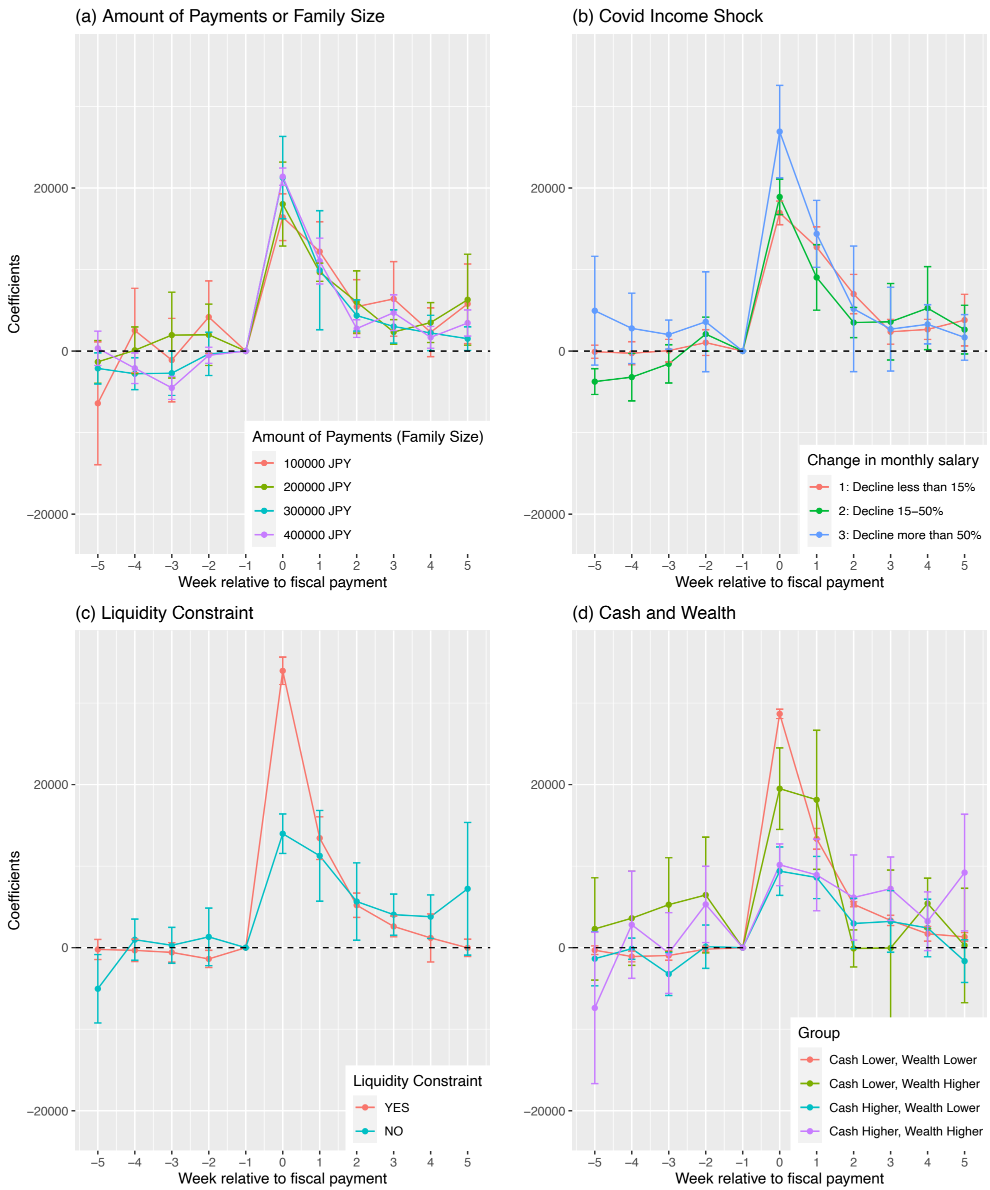
Figure 3: Heterogeneous Responses to SFB Payments



Notes: The figure plots the estimated coefficients $\hat{\gamma}^k$ for $k \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$. Note that γ^{-1} is normalized to 0. Bars indicate 95 percent confidence intervals. Standard errors are clustered at prefectural level.

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(b) Income Shock attributable to the COVID-19 Crisis We show how the income shock attributable to COVID-19 affects responses to SFB payments. Columns (9) and (10) in Table 2 presents results for account holders whose monthly income fell by less than 15% and between 15% and 50%, respectively. Column (11) shows coefficients of groups that experienced drops of 50% or larger. Coefficients and standard errors appear in Panel (b) of Figure 3. The most seriously affected group has a modestly higher MPC. However, differences among the three groups are unclear. That may be partly because the Japanese government's COVID-19 fiscal package includes temporary leave benefits, which likely stabilize jobs and benefit small businesses by compensating for significant income loss among the self-employed.

(c) Liquidity Constraint Columns (12) and (13) in Table 3 and Panel (c) of Figure 3 show differential responses to SFB payments by liquidity-constrained households and others, respectively. We observe a higher spike among the former during the week of payments. Households not constrained by liquidity react moderately but sustain more spending during the second to fifth weeks after SFB payments. These findings might be because liquidity-constrained households need cash for daily living, whereas other households buy non-necessities without rushing. Standard errors for the former group are small after the payment, showing that households facing liquidity constraints react homogeneously to SFB payments.

(d) Wealthy Hand-to-mouth Another dimension of household heterogeneity is the joint distribution of liquid and illiquid assets. Columns (14)–(17) of Table 3 and Panel (d) of Figure 3 display the event study analysis by high/low total asset holdings and demand deposit balance. Immediate responses during the first and second weeks after receiving payment were sharp among groups with lower cash savings. The jump evident for the group with higher assets is also steep. That is, MPC is substantial among the "wealthy hand-to-mouth." Those households likely need cash for daily

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transactions. However, MPC is almost irrelevant to total asset holdings. Our results accord with the theoretical implications by Kaplan and Violante (2014) and Kaplan et al. (2018).

6 Conclusion

This study examines households' responses to a large-scale and universal cash payment program in response to the COVID-19 pandemic in Japan. We obtain causal estimates under a natural experimental design created by randomized timings of cash transfers. Moreover, high-resolution bank account data help to deliver precise and robust results. We find a sizable MPC and significant heterogeneity in financial status.

Unlike past recessions characterized by macroeconomic demand/supply shocks, the current COVID-19 crisis is characterized with heterogeneous sector-level shocks (del Rio-Chanona et al., 2020), particularly concentrated in the service industry. That heterogeneity might amplify these shocks through input-output links among industry networks (Baqaee and Farhi, 2020). Household MPCs might also be biased toward non-services. Guerrieri et al. (2020) theoretically predict that such a biased consumption pattern significantly reduces the multiplier effects of fiscal policies. Although service workers supposedly have higher MPCs, they earn less from consumer spending stimulated by cash transfers. These secondary or higher-round effects are crucial policy considerations warranting further study. Multiplier effects throughout economic networks could be discerned by estimating MPCs by worker's occupation/industry and by items consumed. In future research we intend to identify worker information from their bank accounts and decompose expenditures into categories by linking our dataset with credit card data.

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Appendix

A Additional Discussion about Timing of SFB Payments

As discussed in Section 2.2, the timing of SFB payments was driven by the administrative delays and can be regarded as unpredictable. This Appendix supports that claim by regressing the week of SFB deposits against demographic variables and geographic indicators.

Specifically, the dependent variable is the week in which a bank account receives an SFB payment. Independent variables include age, a female dummy, wealth, cash saving, COVID-19 shock indicators, and a dummy for liquidity constraint. We also add prefecture dummies and municipality indicators in some specifications.[19]

Estimation results for the timing of payments appear in Table A.1. Columns (1) and (4) are the specifications without geographic dummies, Columns (2) and (5) include prefecture dummies, and columns (3) and (6) add municipality dummies. The R-squared values are 0.027 and 0.083, respectively, in Columns (1) and (2). With municipality dummies, the R-squared reaches 0.29. While geography predicts timings of payments to some extent, there remains substantial variation in timing that is not explained by geography.

Turning to demographic variables, coefficients are estimated precisely given the large sample size. However, the magnitudes of these coefficients are small and correlate weakly with the timing of SFB deposits. For example, an account holder 10 years older than the average individual will receive an SFB payment only 0.1 weeks earlier. As such, our analysis suggests that the account holders' region of residence drives the timing of SFB payments. Little statistical evidence suggests households endogenously manipulate the timing of payments.

[19] The prefecture is Japan's largest unit of local government. There are 47 prefectures in total. Municipalities are the lower unit of local government in each prefecture. The total number of municipalities is 1,741 as of October 1, 2018

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Table A.1: Correlation between Timing of Payments and Demographic Variables

	Dependent variable:					
	The week of deposit					
	(1)	(2)	(3)	(4)	(5)	(6)
Age	−0.013	−0.016	−0.015	−0.011	−0.016	−0.015
	(0.002)	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)
Female	0.344	0.282	0.258	0.332	0.255	0.243
	(0.030)	(0.013)	(0.013)	(0.056)	(0.029)	(0.019)
Wealth	0.00001	0.00001	0.00001	−0.00001	−0.00000	−0.00000
	(0.00001)	(0.00000)	(0.00000)	(0.00001)	(0.00000)	(0.00000)
Saving	0.0001	0.0001	0.0001	0.0001	0.0001	0.0001
	(0.00003)	(0.00002)	(0.00002)	(0.00001)	(0.00001)	(0.00001)
Family size	−0.257	−0.277	−0.272	−0.125	−0.146	−0.141
	(0.020)	(0.014)	(0.013)	(0.020)	(0.013)	(0.011)
Monthly salary in 2019				0.002	0.001	0.001
				(0.0004)	(0.0002)	(0.0001)
COVID19 shock 1				−0.102	−0.099	−0.088
				(0.011)	(0.012)	(0.005)
COVID19 shock 2				−0.115	−0.118	−0.152
				(0.014)	(0.020)	(0.015)
Liquidity constraint				−0.577	−0.576	−0.512
				(0.017)	(0.013)	(0.012)
Constant	28.033			27.417		
	(0.130)			(0.157)		
Observations	2,798,149	2,798,149	2,798,149	1,194,378	1,194,378	1,194,378
R^2	0.027	0.083	0.286	0.026	0.092	0.306
Prefecture FE		Yes			Yes	
Municipality FE			Yes			Yes

Notes: The dependent variable is the week in which a bank account receives the fiscal payment per person. Independent variables include age, a female dummy, wealth, an SFB payment dummy, COVID-19 shock indicators, and a dummy for liquidity constraint. Columns (1) and (4) are specifications without geographic dummies. Columns (2) and (5) include prefecture dummies. Columns (3) and (6) add municipality dummies. The unit of wealth and amounts of SFB payments are 10,000 JPY. Standard errors are in parenthesis and clustered at prefecture level.

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B Additional Tables and Figures

Table B.2: Regression Results by Quartile of Savings in Demand-deposit Accounts

	1st	2nd	3rd	4th
5 weeks prior to payment	−246	103	−1,051	−12,450
	(760)	(397)	(955)	(8,063)
4 weeks prior to payment	−293	−976	122	4,969
	(711)	(810)	(598)	(5,869)
3 weeks prior to payment	527	−1,254	1,014	−2,862
	(612)	(570)	(615)	(4,493)
2 weeks prior to payment	987	−63	433	9,130
	(300)	(431)	(572)	(4,124)
Week of Payment	36,816	18,587	12,110	8,022
	(219)	(626)	(861)	(2,653)
1 week after payment	16,301	11,256	7,911	9,804
	(1,089)	(763)	(1,462)	(3,946)
2 weeks after payment	5,879	3,513	1,736	9,912
	(324)	(296)	(930)	(4,268)
3 weeks after payment	3,136	2,719	3,004	10,585
	(940)	(724)	(842)	(3,359)
4 weeks after payment	3,162	870	2,185	4,074
	(278)	(698)	(788)	(2,888)
5 weeks after payment	2,041	265	2,578	13,653
	(234)	(1,174)	(895)	(5,674)
Sample Size	24,409,070	24,624,880	24,527,545	24,521,210
Week FE				
Week*Prefecture FE	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at prefectural level.

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Table B.3: Regression Results by Quartiles of Total Asset Holdings

	1st	2nd	3rd	4th
5 weeks prior to payment	−1,145	351	−181	−12,798
	(189)	(564)	(454)	(7,560)
4 weeks prior to payment	44	−2,003	−711	6,323
	(644)	(972)	(835)	(5,536)
3 weeks prior to payment	−844	−1,547	2,295	−2,478
	(198)	(524)	(683)	(4,454)
2 weeks prior to payment	−116	−210	468	10,340
	(253)	(439)	(438)	(4,493)
Week of Payment	36,248	17,094	12,558	9,606
	(259)	(525)	(593)	(2,741)
1 week after payment	15,099	10,524	7,131	12,516
	(465)	(865)	(669)	(4,467)
2 weeks after payment	6,418	3,642	4,263	6,638
	(172)	(406)	(1,957)	(3,134)
3 weeks after payment	3,694	2,891	2,896	9,958
	(186)	(452)	(1,422)	(3,317)
4 weeks after payment	2,063	1,385	3,514	3,437
	(412)	(550)	(413)	(3,168)
5 weeks after payment	1,043	923	2,266	14,288
	(220)	(517)	(919)	(6,259)
Sample Size	24,462,550	24,540,670	24,540,495	24,540,880
Week FE				
Week*Prefecture FE	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at prefectural level.

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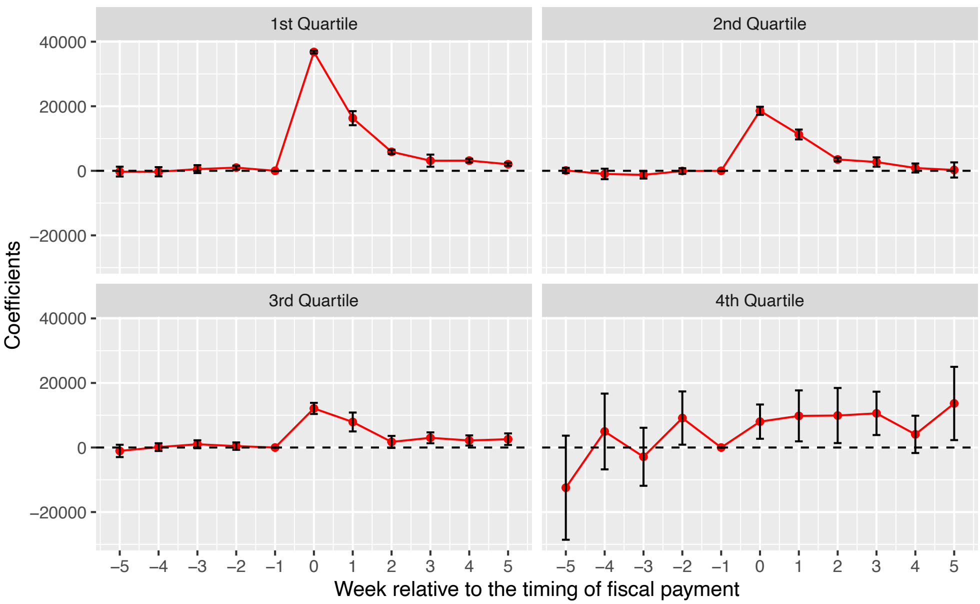
Table B.4: Regression Results by Quartile of Monthly Salary

	1st	2nd	3rd	4th
5 weeks prior to payment	−246	103	−1,051	−12,450
	(760)	(397)	(955)	(8,063)
4 weeks prior to payment	−293	−976	122	4,969
	(711)	(810)	(598)	(5,869)
3 weeks prior to payment	527	−1,254	1,014	−2,862
	(612)	(570)	(615)	(4,493)
2 weeks prior to payment	987	−63	433	9,130
	(300)	(431)	(572)	(4,124)
Week of Payment	36,816	18,587	12,110	8,022
	(219)	(626)	(861)	(2,653)
1 week after payment	16,301	11,256	7,911	9,804
	(1,089)	(763)	(1,462)	(3,946)
2 weeks after payment	5,879	3,513	1,736	9,912
	(324)	(296)	(930)	(4,268)
3 weeks after payment	3,136	2,719	3,004	10,585
	(940)	(724)	(842)	(3,359)
4 weeks after payment	3,162	870	2,185	4,074
	(278)	(698)	(788)	(2,888)
5 weeks after payment	2,041	265	2,578	13,653
	(234)	(1,174)	(895)	(5,674)
Sample Size	24,409,070	24,624,880	24,527,545	24,521,210
Week FE				
Week*Prefecture FE	Yes	Yes	Yes	Yes

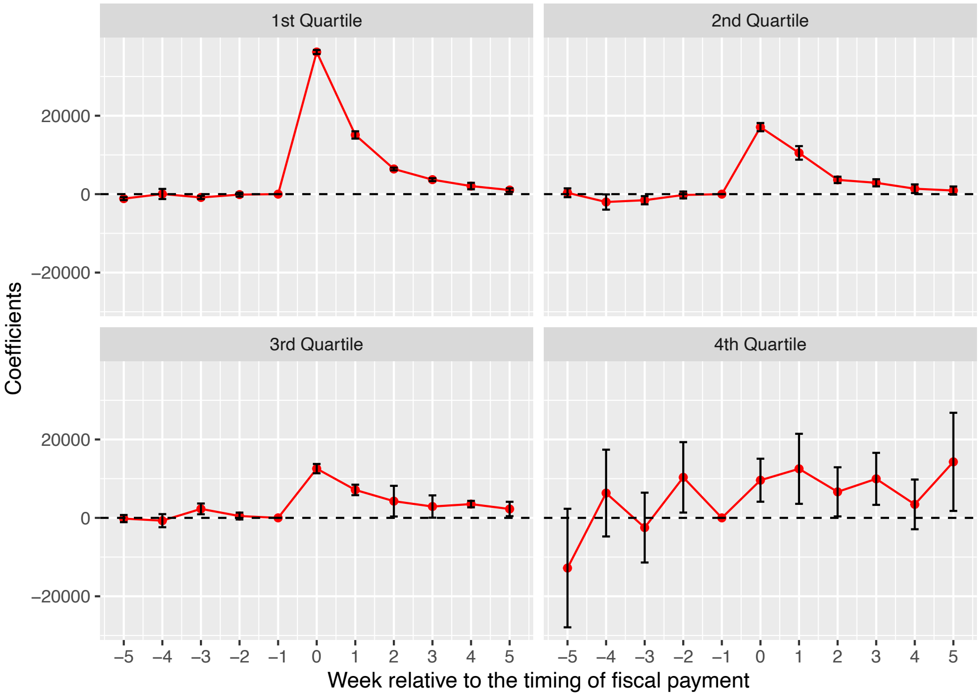
Notes: Standard errors are clustered at prefectural level.

Figure B.1: Heterogeneous Responses to SFB Payments by Quartile of Savings in Demand-deposit Accounts



Notes: The figure plots estimated coefficients of $\hat{\gamma}^k$ for $k \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$. Note that γ^{-1} is normalized to 0. The bars indicate 95 percent confidence intervals. Standard errors are clustered at prefectural level.

Figure B.2: Heterogeneous Responses to SFB Payments by Quartile of Total Asset Holdings



Notes: The figure plots estimated coefficients of $\hat{\gamma}^k$ for $k \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$. Note that γ^{-1} is normalized to 0. The bars indicate 95 percent confidence intervals. Standard errors are clustered at prefectural level.

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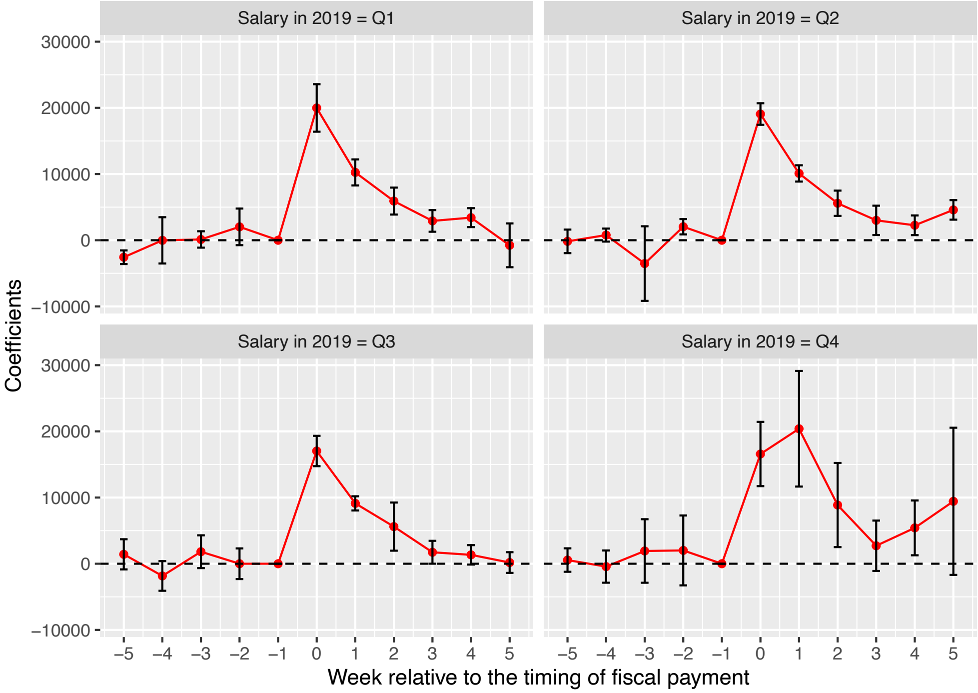
Figure B.3: Heterogeneous Responses to SFB Payments by Quartile of Monthly Salary



Notes: The figure plots estimated coefficients of $\hat{\gamma}^k$ for $k \in \{-5, \ldots, -1, 0, 1, \ldots, 5\}$. Note that γ^{-1} is normalized to 0. The bars indicate 95 percent confidence intervals. Standard errors are clustered at prefectural level.

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The effect of mandatory child care center closures on women's labor market outcomes during the COVID-19 pandemic

Lauren Russell[1] and Chuxuan Sun[2]

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The COVID-19 pandemic has had a dramatic effect on women's labor market outcomes. We assess the effects of state-level policies that mandated the closure of child care centers or imposed class size restrictions using a triple-differences approach that exploits variation across states, across time, and across women who did and did not have young children who could have been affected. We find some evidence that both of these policies increase the unemployment rate of mothers of young children in the short term. In the long term, the effects of mandated closures on unemployment become even larger and persist even after states discontinue closures, consistent with a permanent child care supply side effect.

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1 Introduction

The economic downturn ushered in by the COVID-19 pandemic stands in stark contrast to previous recessions because it has disproportionately affected women. Alon et al. (2020b) show that for every recession between 1948 and 2009, men's unemployment rates have increased more than women's or the effects have been relatively equal. The 2020 recession is the first recession where the unemployment rate for women has risen significantly more than the unemployment rate for men.

Many have hypothesized that two primary factors are responsible for the dramatic effects on women's employment rates in the US: the concentration of women in sectors and occupations disproportionately impacted by the pandemic and changes in child care availability (Alon et al., 2020b; Dingel et al., 2020; Collins et al., 2020). There are a prior reasons to believe that changes in child care availability will disproportionately affect mothers. Alon et al. (2020a) use time-use data to show that mothers spend more time on childcare than fathers in two-parent households. They also point out using US Census Bureau data that single mother households are much more common than single father households (Alon et al., 2020a). Dingel et al. (2020) document that 32 percent of the US workforce has a child under age 14 and 9.4 percent have a child under age 6. They conclude therefore that child care center closures will affect women's employment much more than men's employment but do not directly quantify the extent to which child care availability drives employment effects.

Estimating worker fixed effects models using US Current Population Survey data, Collins et al. (2020) show that mothers with children aged 13 or younger reduced their work hours by five times as much as father's between March and April 2020. However, it is not clear what portion of this decline is due to differences in the type of occupations chosen by mothers and fathers as opposed to child care responsibilities.

Heggeness (2020) provides some direct evidence on the effects of child care availability on mother's labor market outcomes. Using a differences-in-differences approach, she estimates effects of early public school closures and stay-at-home orders on women's unemployment, labor market attachment, and hours worked. She finds that mothers in early closure states

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were significantly more likely to have a job but not be working as a result of early shutdowns but found no immediate impact on labor market detachment or unemployment. Because the analysis focuses on parents of school age children and the effect of school closures, the results do not shed light on loss of child care for parents of young children - those five and under.

Compared to school age children, very young children require more intensive care (Drago, 2009). While school age children may be capable of completing some tasks independently (such getting dressed, retrieving and eating a snack, or entertaining themselves), younger children require around-the-clock supervision and attention. Loss of child care for very young children during hours that would otherwise be used for paid work may have an even more dramatic effect on mothers' labor supply outcomes than loss of public school for a school-age child.

Prior to the pandemic, 24% of children aged 5 and younger received center-based care from a day care center, preschool, prekindergarten or other early childhood program, and 60% participated at least one weekly in some type of non-parental care arrangement including home-based day cares or care arrangements with a relative (U.S. Department of Education, 2016). By mid-March and early April, 16 states had mandated the closure of child care centers, potentially limiting the ability of parents to access child care. Another twenty states imposed class size restrictions, typically allowing classes to contain no more than 10 children.

In this paper, we assess the effects of these mandatory child care center closures and class size limits on mothers' labor supply outcomes, including unemployment, detachment from the labor force, shares of women who are employed but not working, and actual hours worked. In contrast to Heggeness (2020), we are able to estimate longer-term rather than just immediate effects of closures. Specifically, we are able to track employment outcomes six months after closures or class size restrictions were first implemented. Our triple-differences approach exploits variation across states, time, and womens' motherhood status.

Ultimately, we find that state-level mandates that forced closure of child care centers or

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imposed class size limits had important effects on unemployment rates of mothers of young children aged 0 to 5. We estimate that in the short-term (the first one to three months at the beginning of the pandemic when closures were actually in effect) closures increased unemployment rates by 2.7 percentage points, on average, with this effect being marginally statistically significant at the 10% level. However, unemployment effects persisted and grew larger in the months after the closures were rescinded. Post-closure, states that reopened child care centers and shifted to class size restrictions had unemployment rates that were 4 percentage points higher than they would have been had closures never been implemented. Similarly, we estimate that post-closure, the three states that reopened child care centers without class size restrictions had unemployment rates were 6.6 percentage points higher than they otherwise would have been.

We also find statistically significant effects of class size restrictions. Our estimates indicate that states that kept centers open but mandated smaller class sizes increased the unemployment rates of mothers of young children by 2 percentage points. States that implemented class size restrictions tended to keep these in place much longer than closures, so we have less precision to estimate effects once class size restrictions were relaxed. The point estimate is similar in magnitude to the effect when class size restrictions were in place, but the confidence interval is very wide and still includes zero.

Though we lack data to directly test how child care availability changed by state, it's plausible that early financial pressures directly caused by mandated closure or class size restrictions caused some centers to close their doors permanently. The Center for American Progress has estimated that meeting pandemic-related state guidelines would increase operating expenses for child care providers by 47%, on average (Jessen-Howard and Workman, 2020b). Most of these increased costs would take the form of personnel costs to comply with reduced class size requirements as well as increased sanitation costs (Jessen-Howard and Workman, 2020a). During mandatory closures, some centers also tried to continue paying staff even when centers were closed, meaning that centers were trying to make payroll when revenues were at best reduced, or at worst, nonexistent. Even if centers could meet budget

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shortfalls for a month or two, it's unlikely they would be able to absorb such cost increases in the long term, potentially leading to permanent closures and a contraction in the supply of child care.

A survey by the National Association for the Education of Young Children found that nationally, 18% of child care centers were closed in July 2020 as a result of the pandemic, even though all states had officially allowed child care centers to reopen by that time, which is consistent with this type of permanent supply side response (National Association for the Education of Young Children, 2020). The survey also predicted that closures would become more widespread in the months that followed. Forty percent of respondents said they were certain that they would close permanently within the year without additional public assistance (National Association for the Education of Young Children, 2020). Unfortunately, such support has not materialized.

All of this evidence suggests that as the pandemic stretches on, the supply of child care has became more constrained. Our evidence indicates that this has had the notable downstream effect of increasing unemployment rates for women of young children.

2 Mandatory Child Care Center Closures

A prominent aspect of the COVID-19 crisis is that it has involved stay-at-home orders, some of which forced the closure of child care facilities. In March-April 2020, 16 states issued orders that forced child care businesses to close, though most included an exemption which allowed centers to stay open if they served the children of essential workers.

The other 34 states (plus DC) allowed childcare businesses to stay open, according to Child Care Aware of America, an organization that works with state and local childcare resource and referral agencies (Quinton, 2020). However, among these 34 states, 20 imposed class size limits designed to increase social distancing and reduce the risk of COVID transmission without a classroom. Table 1 identifies the states that ordered the closure of child care businesses, states that allowed child care centers to remain open without class size limits,

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and states that allowed child care centers to remain open but imposed class size limits. Even though Alabama initially ordered child care centers to close, this closure remained in effect only for one week between March 19, 2020 and March 27, 2020 at which point the state allowed centers to reopen with a class size limit of 11. Therefore, in our analysis we classify Alabama as a class size limit state rather than a mandated closure state. Note that most states that mandated child care center closures in the early months of the pandemic later transitioned to class size limits once centers were allowed to reopen.

Table 1: States with Mandatory Child Care Center Closures

State	**Policy**	**Date Closures Effective**	**Date Reopening Allowed**	**Notes**
Alabama	Closed, with option to provide emergency care	March 19, 2020 (modified March 27, 2020 to allow operation with 11 or fewer children in each room)	March 27, 2020	When reopened, group sizes limited to 11. By May 21, 2020, no group size restrictions in place.
Alaska	Option to remain open			Effective April 24, 2020 group sizes limited to 10 children; by May 21, 2020, no group size restrictions in place.
Arizona	Option to remain open			
Arkansas	Option to remain open			Effective July 17, 2020, group sizes limited to 10 people, including staff
California	Option to remain open			Effective August 25, 2020, cohorts limited to no more than 14 children
Colorado	Option to remain open			Required to operate with groups of 10 or fewer from April 1, 2020 - June 4, 2020
Connecticut	Option to remain open			Effective March 16, 2020, limited group sizes to no more than 14 children
Delaware	Closed, with option to provide emergency care	April 6, 2020	June 15, 2020	When reopened, group sizes limited
District of Columbia	Option to remain open			

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Florida	Option to remain open			Effective March 24, 2020, all gatherings of 10 or more people prohibited; Office of Child Care Regulation Guidance for Child Care Providers identified no specific group size restrictions on June 5, 2020
Georgia	Option to remain open			Starting March 23, 2020, centers must limit class sizes to no more than 10 (including staff)
Hawaii	Closed, with option to provide emergency care	March 23, 2020	May 7, 2020	Starting on March 19, 2020, no group could be larger than 10, including the staff person. By June 9, 2020, no group size restrictions in place.
Idaho	Option to remain open			
Illinois	Closed, with option to provide emergency care	March 20, 2020	May 29, 2020	When reopened, class size limits imposed (varied depending on age group)
Indiana	Option to remain open			Effective March 20, 2020 - May 22, 2020, no more than 20 children can reside in a classroom
Iowa	Option to remain open			
Kansas	Option to remain open			
Kentucky	Closed, with option to provide emergency care	March 20, 2020	June 8, 2020 (Family-based care); June 15, 2020 (Center-based care)	Effective June 8, 2020, class sizes limited to 10 children; class size limits relaxed to 15 children for 2 years+ on September 1, 2020
Louisiana	Option to remain open			Effective March 22, 2020, any gatherings of 10 or more prohibited; Child Care Guidelines released on July 13, 2020 that explicitly limited class sizes (varied depending on age group)
Maine	Option to remain open			
Maryland	Closed, with option to provide emergency care	March 26, 2020	May 16, 2020	When centers reopened, no more than 15 total persons per room
Massachusetts	Closed, with option to provide emergency care	March 23, 2020	Effective June 8, reopning plans to be submitted for reveiew beginning June 15, 2020	Centers must apply to re-open; forms first reviewed June 15, 2020; class size limitations imposed upon reopening until July 25, 2020
Michigan	Closed, with option to provide emergency care	March 23, 2020	June 1, 2020	When reopened, highly recommended that group sizes be kept to 10 or fewer children
Minnesota	Option to remain open			
Mississippi	Option to remain open			
Missouri	Option to remain open			

Montana	Option to remain open			Effective April 1, 2020, class sizes limited to 10 children
Nebraska	Option to remain open			Effective March 18, 2020, class sizes limited to 10 children per class; effective May 4, 2020, some child care facilities permitted to have up to 15 children per class
Nevada	Option to remain open			
New Hampshire	Option to remain open			Centers must comply with health and safety guidelines; limited class sizes to 10 or fewer
New Jersey	Closed, with option to provide emergency care	March 25, 2020	June 15, 2020	Effective June 18, 2020, group sizes limited to 10 (not including staff)
New Mexico	Option to remain open			Effective March 23, 2020, gatherings limited to 5 people; effective August 14, 2020, class sizes limited to 10-20 (depending on age group)
New York	Closed, with option to provide emergency care (NYC); Option to remain open (NY State)	NYC: April 3, 2020		Effective June 26, 2020, class sizes limited to 15
North Carolina	Closed, with option to provide emergency care	March 25, 2020	May 8, 2020	
North Dakota	Option to remain open			Effective April 1, 2020, class sizes limited to 9 children and one staff person. Effective July 21, 2020, classes limited to 15 people per room (children plus staff)
Ohio	Closed, with option to provide emergency care	March 26, 2020	May 31, 2020	When reopened, reduced group sizes; effective August 9 returned to normal group sizes
Oklahoma	Option to remain open			Effective March 24, 2020 - June 17, 2020, group sizes limited to 10
Oregon	Closed, with option to provide emergency care	March 25, 2020	May 15, 2020	Effective March 16, 2020, class sizes limited to 10 children (8 for children under 24 months old)
Pennsylvania	Closed, with option to provide emergency care	March 19, 2020	May 8, 2020 (24 northern counties); May 15, 2020 (12 remaining counties)	
Rhode Island	Closed, with option to provide emergency care	March 29, 2020	June 1, 2020	When reopened, group sizes limited to stable groups of 20 children; effective June 29, 2020, programs could seek DHS approval to increase group sizes
South Carolina	Option to remain open			
South Dakota	Option to remain open			

Tennessee	Option to remain open			Effective March 15, 2020, "all effort should be made to limit congregation of children and class sizes to 10 or less."
Texas	Option to remain open			Effective May 18, 2020, class sizes limited (varied depending on age)
Utah	Option to remain open			Effective March 25, 2020, class sizes limited to 10; effective April 29, 2020, class sizes limited to 20
Vermont	Closed, with option to provide emergency care	March 17, 2020	June 1, 2020	Effective September 1, 2020, class size restrictions that vary by age group
Virginia	Option to remain open			Effective March 18, 2020, class sizes limited to 10; effective June 2, 2020, class sizes restrictions relaxed (new restrictions varied by age group); effective September 25, 2020, no class size restrictions
Washington	Option to remain open			Effective June 26, 2020, classes could contain no more than 22 children and adults
West Virginia	Closed, with option to provide emergency care	March 25, 2020	May 11, 2020	Effective May 9, 2020, class sizes limited to 10 (including staff)
Wisconsin	Option to remain open			
Wyoming	Closed, with option to provide emergency care	March 19, 2020	April 28, 2020	When reopened, class sizes limited to 10 (including staff); restrictions updated on June 8, 2020 to keep total persons to 10 or less in each room; effective September 16, 2020, class size restrictions removed

Notes: Most information comes from cross-referencing sources from the Hunt Institute (2020), the Food Industry Association (2020), and Child Care Aware of America (2020). State-specific news articles and government orders were also consulted. For more details, see full data appendix.

Even in states that did not officially mandate stay-at-home orders or class size limits, child care centers were deeply affected. Some centers voluntarily closed their doors due to health concerns, and others voluntarily decreased class sizes in accordance with state recommendations to allow for more social distancing. Some parents decided not to send children to child care centers, even if centers were open in their area (Quinton, 2020). Therefore, even the states where childcare businesses technically had the ability to operate as normal during the early months of the pandemic, parents may have experienced decreased child care access.

Between March 21 and April 2020, the Bipartisan Policy Center and Morning Consult

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conducted a national survey of 800 parents with children under age 5. They found that 60% of child care programs were fully closed (Bipartisan Policy Center, 2020). Unfortunately, these aggregate data do not report data separately by state, so it is impossible to directly compare the share of child care centers closed in states that mandated closure versus those that did not during the earliest months of the pandemic. The aggregate statistics reported by the the aforementioned National Association for the Education of Young Children survey, which found that 18% of child care centers were closed in July 2020, suggests that some but not all of these centers had reopened once states relaxed their closure policies in April, May, and June.

Though many mandates to close child care centers were sometimes part of a more general stay-at-home order, state-imposed child care center closures are not perfectly correlated with other types of closures such as public school closures (Heggeness, 2020). Some states that closed public schools explicitly allowed child care centers to remain open (Hunt Institute, 2020; Food Industry Association, 2020; Child Care Aware of America, 2020). In the analysis that follows, we investigate the independent effect of mandatory child care center closure policies and class size limit policies on the labor market outcomes of mothers of young children.

3 Data Description

We use three data sources for our analysis: state-level information on child care center closure policies, the Household Pulse Survey, and the Current Population Survey. Our data on child care center closure policies, including dates of announcement/implementation and dates of reopenings, come primarily from cross-checking three sources: Hunt Institute (2020); Food Industry Association (2020); Child Care Aware of America (2020). However, we also consult state-specific news articles and press releases to confirm this information. The online data appendix reports specific language from these orders and a complete list of sources for each state.

The Household Pulse Survey, a survey launched in April 2020 specifically to shed light on COVID-19 related issues, is administered by the US Census Bureau. The short 20-minute survey consists of questions related to employment status, spending patterns, food security, housing, physical and mental health, access to health care, and educational disruptions (US Census Bureau, 2020; Centers for Disease Control and Prevention, 2020). The weekly survey provides a "near real-time snapshot" of COVID-19 experiences because there is only an 8 day lag between when respondents fill out the questionnaire and when the results are reported Centers for Disease Control and Prevention (2020). Although the survey has the advantage of asking questions most relevant to effects of the COVID-19 pandemic, data were first collected only after state-level mandates for child care center closures. Therefore, we are unable to use the Pulse Survey data for our main triple-differences analysis. The data also fail to identify specific ages of children for respondents, so we cannot isolate reporting to parents of children aged 0 to 5, the population for whom child care is relevant.

Instead, we rely on the basic monthly files from the Current Population Survey, a monthly survey of about 60,000 households sponsored by the US Census Bureau and the US Bureau of Labor Statistics (Flood, Sarah and King, Miriam and Rodgers, Renae and Ruggles, Steven and Warren, J. Robert, 2020). Sampled households are in the survey for four consecutive months, are out for eight months, and then return for another four consecutive months before leaving the sample permanently. A new group of respondents starts in each calendar month at the same time another group completes its rotation.

Our microdata correspond to January 2019 to September 2020, though most of our analysis focuses on September 2019 to September 2020. We limit the sample to people aged 18-64, inclusive, to focus analysis on the working-age population. We drop anyone living in group quarters or working in the armed forces. We drop New York from our sample because New York City had a child care center closure policy while the rest of the state did not, so it is impossible to assign either treatment or control status to the state. We also drop any individuals whose reporting of age, sex, and race is inconsistent across the months where they report data to the CPS. Our triple-differences analysis uses the subset of data corresponding

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to women with children aged 0 to 5 and women without any children.

4 Aggregate Effects of the Pandemic on Women's Employment and the Importance of Child Care

Before presenting our analysis of the causal effects of state-level child care closure policies, we begin by presenting descriptive statistics on women's unemployment and the reported importance of child care access across all states during the pandemic period. Figure 1 uses CPS data to show unemployment rates of men and women pre and post-pandemic. Prior to the pandemic, unemployment rates of both men and women aged 18-64 hovered around 3-4%. Then unemployment rates increased dramatically between February 2020 and April 2020, peaking at 15.4% for women and 13.1% for men. Consistent with Alon et al. (2020b)'s analysis, we find the increase is much larger for women – an 11.2 percentage point increase – compared to 8.5 percentage points for men between February and April. Unemployment rates for both men and women declined between April 2020 and September 2020 but still remain at very high levels relative to the pre-pandemic period. As of September 2020, the women's unemployment rate still exceeded the men's unemployment rate by 0.5 percentage points.

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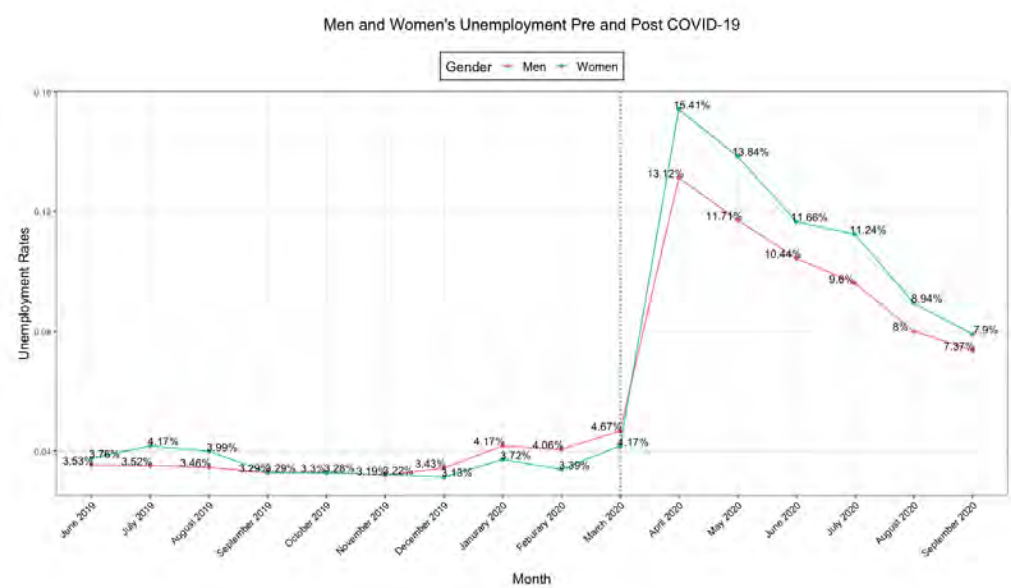
Figure 1: Men and Women's Unemployment Pre and Post COVID-19



Source: IPUMS-CPS, University of Minnesota, www.ipums.org

Notes: Authors' tabulations. The sample consists of people aged 18-64 in the labor force.

4.1 Importance of Child Care Access

Next, we use the Pulse data to investigate how many women are reporting that child care issues are a significant driver of their unemployment (Figure 2). [1] For these figures, we limit our sample to parents with children aged 18 and under.

Throughout the data collection period of April 23 to September 28, a significant number of parents are reporting that they are not working and that this is due to child care issues. The fraction of mothers reporting not working due to COVID-19 related child care issues is significantly higher than for fathers. For example, in the July 16-July 21 survey, 11% of mothers versus only 3% of fathers were not working due to COVID-19 related child care

[1]For more analysis of these data, see Heggeness and Fields (2020).

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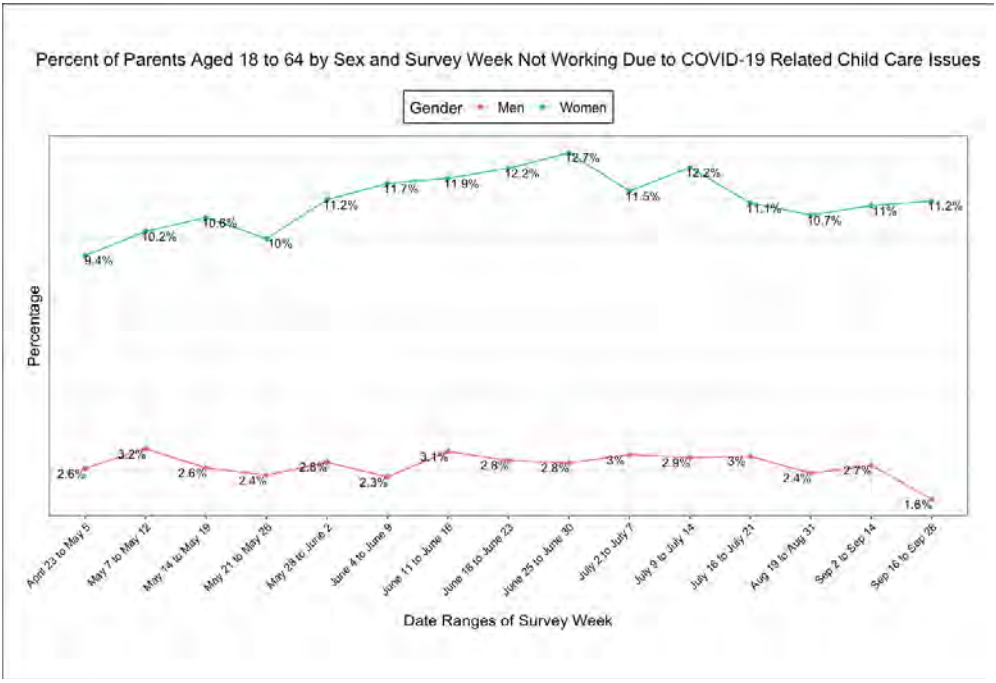
issues.

Figure 2: Percent of Parents Not Working Due to COVID-19 Related Child Care Issues

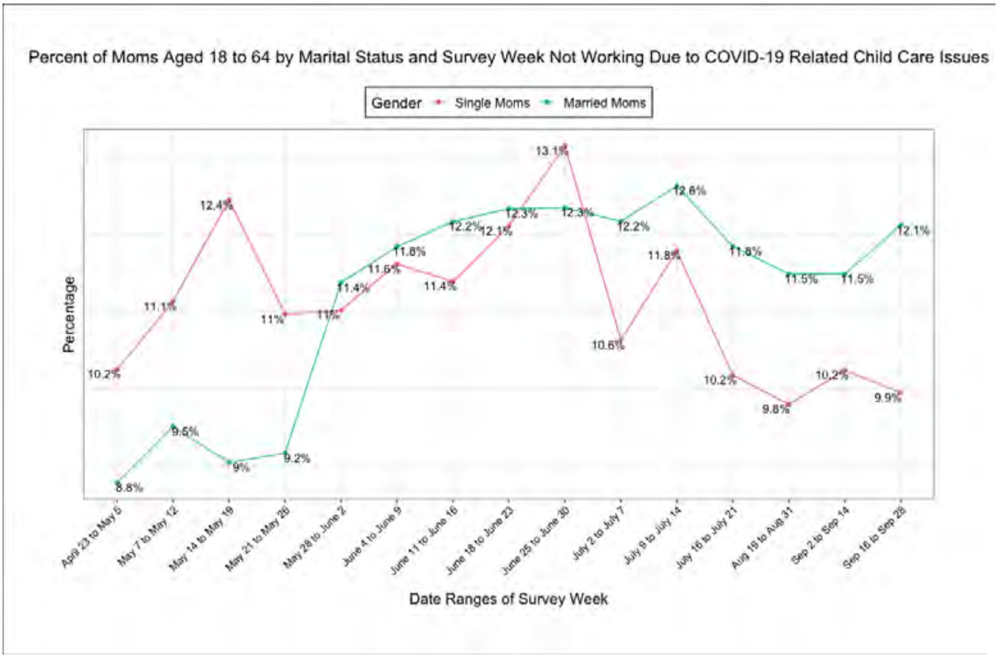


Source: Household Pulse Survey Public Use File, United States Census Bureau, www.census.gov/programs-surveys/household-pulse-survey/

Notes: Figure displays the percent of parents aged 18-64 who have at least one child under 18 and report they are not working due to COVID-19 related child care issues among all respondents to the Pulse Survey. Group quarter observations are dropped, and composite weights are used.

We also extend previous descriptive work by investigating differences by characteristics of these mothers. Figure 3 reports the percent of single and married mothers not working who cited COVID-19 child care issues as the cause. In April and May, single mothers were more likely than married mothers to report not working due to COVID-19 related child care issues. By May and June, single and married mothers were reporting similar rates. By July, August, and September married mothers were more likely than single mothers to report not working due to COVID-19 related child care issues.

Figure 3: Percent of Mothers Not Working Due to COVID-19 Related Child Care Issues



Source: Household Pulse Survey Public Use File, United States Census Bureau, www.census.gov/programs-surveys/household-pulse-survey/

Notes: Sample includes mothers who have at least one child under 18. Group quarter observations are dropped, and composite weights are used.

We also investigated heterogeneity in child care issues as a driver of unemployment by race/ethnicity. We find in Figure 4 that race/ethnicity is not a strong predictor of which mothers report that they are not working due to child care issues.

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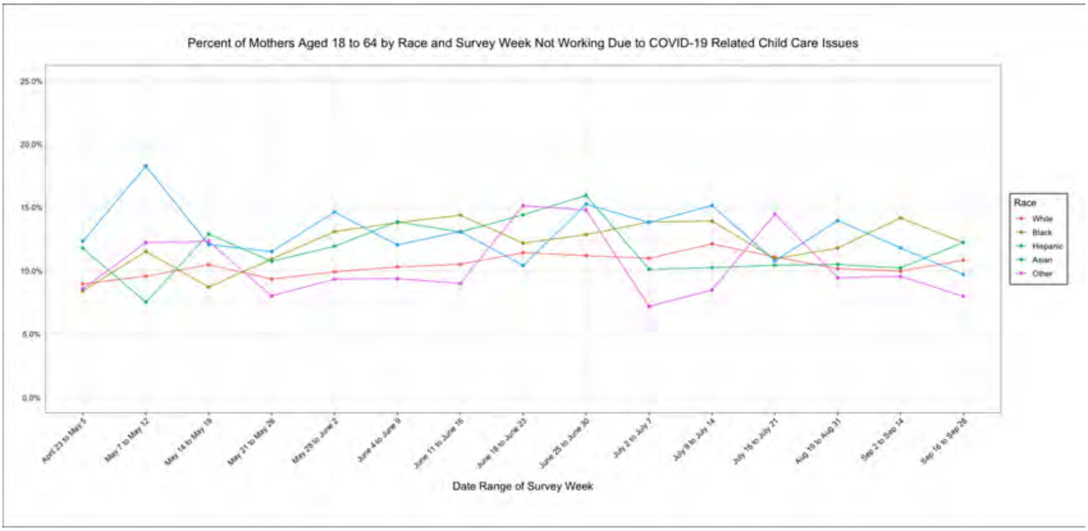
Figure 4: Percent of Mothers Not Working Due to COVID-19 Related Child Care Issues



Source: Household Pulse Survey Public Use File, United States Census Bureau, www.census.gov/programs-surveys/household-pulse-survey/

Notes: Sample includes mothers who have at least one child under 18. Group quarter observations are dropped, and composite weights are used.

5 Effects of Mandatory Child Care Center Closures

Though these descriptive statistics reveal that in the aggregate child care access is important for mothers' labor supply, it is not known whether state-level child care closures or class size restrictions, as opposed to voluntary closures of child care centers or loss of home-based care provided by acquaintances, friends, or relatives, had an independent impact on mothers' labor market outcomes.

5.1 Triple-Differences Empirical Strategy

To study the effects of state mandated child care center closures and class size restrictions on the employment of women during the pandemic, we use a triple-differences strategy.[2]

[2]For a derivation of the triple-differences estimator and a complete discussion of its identifying assumptions, we refer readers to Olden and Møen (2020).

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Our empirical strategy uses three dimensions of variation: cross-state variation in which states implemented mandates, cross-time variation in when mandates were implemented, and cross-worker variation in whether a woman had young children who would potentially need child care.

One challenge in estimating the effect of child care center closures is the decision to close all child care centers may not be quasi-random. While we find evidence that women's employment was on parallel trends prior to the start of the pandemic for states that did and did not implement closures, it is possible that states that mandated the closure of child care centers were hit harder by the pandemic at the time the decision was made to close child care centers. Thus, women's employment could decline more in these states for reasons unrelated to child care availability. For example, prior work has shown that women tend to be over-represented in sectors and occupations that were impacted most severely by the pandemic (Alon et al., 2020a).

If these child care closure mandates are correlated with pandemic severity, a differences-in-differences analysis may conflate impacts of the pandemic on job availability with impacts through child care availability. By including women without children in the analysis, we are able to isolate the child care availability effect. We omit women of older children from the analysis because these mothers also experienced changing family obligations as many schools and universities were closed or switched to remote learning formats.

We start by estimating triple-differences event study models with leads and lags 6 months before and 6 months after closure and class size restrictions implementation:

$$y_{ipst} = \gamma_{st} + \theta_{pt} + \mu_{ps} + \sum_{j=-6}^{6} \beta_j Closure^j_{pst} + \sum_{j=-6}^{6} \Delta_j Restriction^j_{pst} + X_{ipst}\delta + \omega_i + \varepsilon_{iast} \quad (1)$$

In this regression equation, y_{ipst} is a labor market outcome for woman i in state s and month t who either is or is not a parent (p) of a child aged 0-5. Recall that because we omit parents of older children from the analysis sample, any observation that is not a parent of a child aged 0-5 is a non-parent. We control for state-specific shocks that vary over time γ_{st} and include interactions for parent and time effects θ_{pt} and parent and state effects μ_{ps}. The matrix X_{ipst} includes a rich set of individual-level controls including age, marital status, education, and occupation fixed effects and a control for whether there is another adult in the household. The panel structure of the CPS also allows us to include person fixed effects (ω_i). We cluster standard errors at the state level.

The CPS survey is conducted on the 19th of each month and asks respondents questions about the previous week. Because all of our closure and restriction policies were effective after March 12, April 2020 is the first month where labor market outcomes in the CPS could have been directly affected by these mandates. Accordingly, for our event studies, the omitted month is March 2020, a month prior to when closures or restrictions could have first impacted labor market outcomes.

It is important to keep in mind that closures were rescinded after one month in Hawaii, North Carolina, West Virginia, and Wyoming, after two months in Illinois, Maryland, Michigan, Ohio, Oregon, Pennsylvania, Rhode Island, and Vermont, and after three months in Delaware, Kentucky, Massachusetts, and New Jersey. Therefore, no state in the +4 to +6 months still had closures in effect, though we still plot these coefficients to investigate whether there were longer-term effects on labor market outcomes that persisted after policies were relaxed.

To account for potentially different effects in months where closures or class size restrictions were in effect vs. time periods where they had been relaxed, our triple-differences regression takes the following form:

$$\begin{gathered} y_{ipst} = \gamma_{st} + \theta_{pt} + \mu_{ps} + \beta ClosureInEffect_{pst} \\ + \Psi ClosureDiscontLimitImposed_{pst} + \Lambda ClosureDiscontNoLimit_{pst} \\ + \Delta LimitInEffect_{pst} + \Pi LimitDiscont_{pst} + \\ X_{ipst}\delta + \omega_i + \varepsilon_{iast} \end{gathered} \tag{2}$$

Our set of five treatment indicators captures every possible treatment status in the post-

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policy period. $ClosureInEffect_{pst}$ equals 1 if person i was a parent of a young child in state s where child care center closures were mandated in month t. $ClosureDiscontLimitImposed_{pst}$ equals 1 for post-closure months once centers were allowed to reopen if class size limits were imposed at that time. $ClosureDiscontNoLimit_{pst}$ equals 1 in post-closure months once the closure policy was discontinued if no class size limits were imposed. Similarly, $LimitInEffect_{pst}$ equals 1 if person i was a parent of a young child in state s where child care centers were subject to class size limits in month t. $LimitDiscont_{pst}$ equals 1 in months after class size limits were discontinued.

The identifying assumption for our triple-differences estimator is that there is no contemporaneous shock that differentially affects the outcomes of the treatment group (mothers with young children) compared to the control group (women without children) in the same state-months as state-mandated child care center closures or child care class size limits.

5.2 Results

Figures 5 and 6 show the results of the event study specification for four labor market outcomes: labor force detachment, unemployment, being employed but not working, and actual hours worked last week. The plots show evidence of parallel trends, lending credence to the identifying assumption. There are no obvious effects of closures on labor force detachment, being employed but not working, or actual hours worked last week. By contrast, there is an obvious jump in unemployment after closures are implemented, and this effect persists and is statistically significant in the fourth month to sixth after the closure is implemented.

The results for class size limits in Figure 6 show a similar pattern. There are no discernible effects on labor force detachment, being employed but not working, or actual hours worked last week, but there is a statistically significant increase in unemployment at the time class size limits go into effect. Unlike for closures, the negative employment effects seem to dissipate over time and are not statistically significant by three months after implementation.

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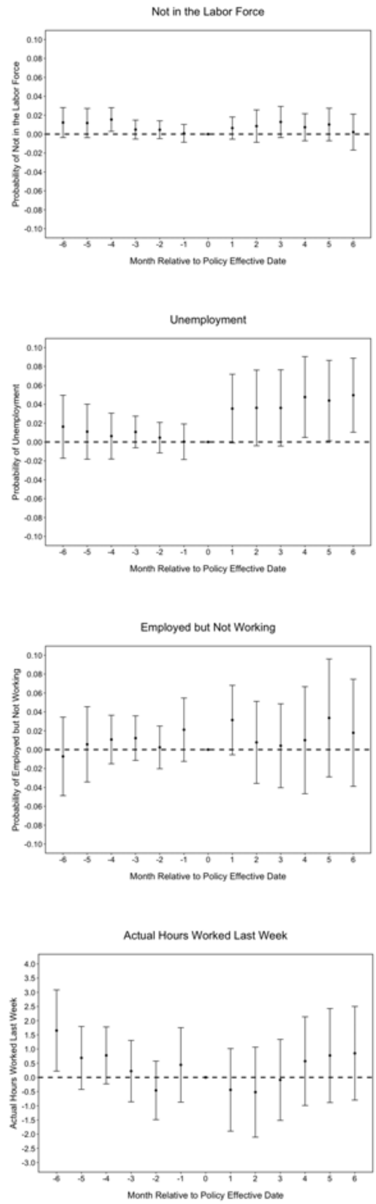
Figure 5: Triple-Differences Event Studies for Child Care Center Closure Policies



Source: IPUMS-CPS, University of Minnesota, www.ipums.org

Notes: Results from estimating equation 1 as described in text and then plotting the coefficients on the closure policy time relative to implementation indicators: β_j.

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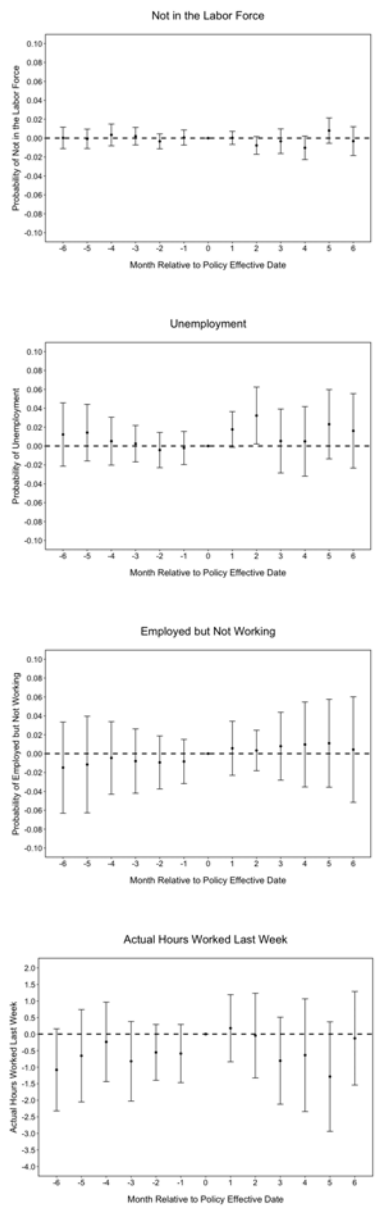
Figure 6: Triple-Differences Event Studies for Class Size Limits



Source: IPUMS-CPS, University of Minnesota, www.ipums.org

Notes: Results from estimating equation 1 as described in text and then plotting the coefficients on the class limit policy time relative to implementation indicators: Δ_j.

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Table 2 shows the triple-differences estimates. The first point estimate in column 2 indicates that closures increased unemployment rates of mothers with young children by 2.7 percentage points in months when a closure was actually in effect. (Note that these were the early months of the pandemic.) The second point estimate indicates that in post-closure months where closures were discontinued but class size limits were imposed (later months of the pandemic), unemployment rates were 4.0 percentage points higher than they otherwise would have been. The third point estimate indicates that in post-closure months where closures were discontinued and no class size limits were imposed, unemployment rates were 6.6 percentage points higher than they would have otherwise been.

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Table 2: Effect of Mandated Child Care Center Closures and Class Size Limits on Women's Labor Market Outcomes

	Labor Market Outcome			
	(1) Not In the Labor Force	(2) Unemployed	(3) Employed But Not Working	(4) Actual Hours Worked Last Week
Post Closure with Closure in Effect x Mother of Child 0-5	0.004	0.027*	0.001	-0.82
	(0.005)	(0.016)	(0.013)	(0.44)
Post Closure with Closure Discontinued But Class Limits x Mother of Child 0-5	-0.000	0.040**	-0.015	-0.38
	(0.006)	(0.017)	(0.014)	(0.47)
Post Closure with Closure Discontinued & No Limits x Mother of Child 0-5	-0.004	0.066***	0.017	0.41
	(0.005)	(0.016)	(0.023)	(0.48)
Post Class Size Limits with Limits in Effect x Mother of Child 0-5	-0.002	0.020**	0.004	0.18
	(0.004)	(0.009)	(0.012)	(0.48)
Post Class Size Limits x Limits Discontinued x Mother of Child 0-5	-0.002	0.018	0.006	0.04
	(0.006)	(0.023)	(0.013)	(0.91)
Number of Individuals	92,956	68,250	64,671	63,194
Number of Observations	275,896	191,204	179,010	170,519

Source: IPUMS-CPS, University of Minnesota, www.ipums.org

Notes: Results from estimation of equation (2) as described in the text. All regressions include occupation fixed effects, age fixed effects, marriage status fixed effects, control for at least one other adult in the household, person fixed effects, and all the double interactions (state by month fixed effects, mother of young child x month fixed effects, and state x mother of young child fixed effects). Standard errors are clustered at the state level.

The estimates also show an effect of class size limits on unemployment rates of mothers of young children: +2.0 percentage points in months where limits were in effect with this effect statistically significant at the 5% level. There is no statistically significant effect in post-class limit months once limits were discontinued, though the confidence interval cannot rule out effects as large as during months where the limits were actually in place.

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5.3 Robustness

Because mothers of very young infants may have taken maternity leave and been unaffected by changes in child care center availability, we assessed the robustness of our results to defining mothers of young children as those with children aged 1-5 rather than 0-5. Table 3 shows that our results are robust to this change in the young mothers definition, though the effect of mandated closures in the early months when such policies were actually in effect is similar in magnitude but no longer statistically significant.

Table 3: Robustness Check Dropping Mothers of Infants

	Labor Market Outcome			
	(1) Not In the Labor Force	(2) Unemployed	(3) Employed But Not Working	(4) Actual Hours Worked Last Week
Post Closure with Closure in Effect x Mother of Child 1-5	0.004 (0.005)	0.024 (0.017)	0.012 (0.015)	-0.59 (0.45)
Post Closure with Closure Discontinued But Class Limits x Mother of Child 1-5	-0.002 (0.006)	0.047** (0.018)	-0.005 (0.021)	-0.18 (0.62)
Post Closure with Closure Discontinued & No Limits x Mother of Child 1-5	-0.002 (0.006)	0.063*** (0.017)	0.014 (0.020)	0.64 (0.43)
Post Class Size Limits with Limits in Effect x Mother of Child 1-5	-0.001 (0.004)	0.026*** (0.010)	-0.004 (0.010)	0.41 (0.45)
Post Class Size Limits x Limits Discontinued x Mother of Child 1-5	-0.002 (0.007)	0.013 (0.023)	-0.008 (0.016)	-0.29 (1.03)
Number of Individuals	90,287	66,395	62,915	61,623
Number of Observations	265,562	184,762	172,965	165,710

Source: IPUMS-CPS, University of Minnesota, www.ipums.org

Notes: Results from estimation of equation (2) as described in the text except parents (mothers of young children) are defined as those with a child aged 1 to 5. All regressions include occupation fixed effects, age fixed effects, marriage status fixed effects, control for at least one other adult in the household, person fixed effects, and all the double interactions (state by month fixed effects, mother of young child x month fixed effects, and state x mother of young child fixed effects). Standard errors are clustered at the state level.

We would have liked to directly examine the number of women reporting that they are unemployed because of child care issues, but the CPS does not ask a question with response choices that would allow us to investigate this. The only reasons respondents can cite for being unemployed include (1) looking for first jobs, (2) re-entering after an extended work absence, (3) have left a job, (4) temporary job ended, (5) laid off, or (6) left job for another reason. None of these has a definitive link with child care issues. The Pulse survey is also poorly suited to investigating whether mothers of young children in states with closures or mandates were more likely to report being unemployed due to child care issues as the data cannot be disaggregated to include only mothers with young children.

Instead, we take advantage of a child care question asked on the March 2020 Annual Social and Economic Supplement (ASEC). Specifically, the question asked whether paid child care was needed for each child in the household. We define a mother has requiring paid child care for a child aged 0-5 if there is any child in her household aged 0-5 for whom "paid child care is needed." We have 4,550 mothers with a child aged 0 to 5 who responded to both the ASEC and appear in the March basic monthly file. Among those mothers, 34% have at least one child who needs child care which is consistent with estimates from the U.S. Department of Education (2016).

A challenge of using this question for our analysis is that it is only asked once per year. We impute whether a mother needs child care in other months where she participates in the CPS panel by carrying this March response forward and backwards in time. Recall that sampled households are in the survey for four consecutive months, so if this household appeared in the CPS in February, March, April, and May, we use the March response and assign that same value to this household (mother) in February, April, and May. Then, we re-run our triple differences analysis, redefining the treatment group as mothers of children aged 0-5 who expressed a need for paid child care. The control group is the same as before - women without any children.

We would expect this analysis to be somewhat less informative than our preferred analysis previously presented. We are not able to look at effects of the closure and limitation

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policies past June 2020 because we do not have any treatment group coverage in August or September (more than four months after March). Moreover, though it is reasonable to assume that if a mother required paid child care in March, she also required it in other months, that assumption could be incorrect if there were changes in her outside options (availability of informal child care arrangements). We also have less statistical power due to smaller sample sizes. Nevertheless, if the results are truly driven by child care access, labor market effects of child care policies should be somewhat larger when estimating the triple differences specification on this sample.

In fact, this is generally what we find in Table 4. Though we lose statistical significance of some estimates due to larger standard errors, the point estimates, especially for the effects of class size limits, are somewhat larger than in the main specification. In contrast to the results before, we now find a statistically significant effect on a woman being employed but not working when class size limits are in place, and an increase in actual hours worked last week when closures were discontinued but class size limits were imposed. The latter effect may seem counterintuitive but is actually consistent with Heggeness (2020) who found that for women who continued working, women in early closure states reported working more weekly hours than those in late closure states, perhaps because these women were compensating for reduced hours worked by other household members or because they faced increased work loads due to changing operations during COVID.

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Table 4: Robustness Check With Mothers Who Need Paid Child Care as of March 2020

	Labor Market Outcome			
	(1) Not In the Labor Force	(2) Unemployed	(3) Employed But Not Working	(4) Actual Hours Worked Last Week
Post Closure with Closure in Effect x Mother of Child 0-5	0.002 (0.007)	0.023 (0.030)	0.018 (0.027)	1.17 (1.13)
Post Closure with Closure Discontinued But Class Limits x Mother of Child 0-5	0.049 (0.037)	0.039 (0.033)	-0.040 (0.027)	4.13*** (1.31)
Post Closure with Closure Discontinued & No Limits x Mother of Child 0-5	0.023 (0.024)	0.118* (0.063)	0.012 (0.022)	-0.16 (1.21)
Post Class Size Limits with Limits in Effect x Mother of Child 0-5	0.019* (0.011)	0.044* (0.024)	0.054*** (0.020)	1.23 (0.76)
Post Class Size Limits x Limits Discontinued x Mother of Child 0-5	0.007 (0.030)	0.062 (0.039)	-0.004 (0.044)	4.39* (2.34)
Number of Individuals	74,900	55,435	52,578	51,542
Number of Observations	222,238	155,847	145,887	139,839

Source: IPUMS-CPS, University of Minnesota, www.ipums.org

Notes: Results from estimation of equation (2) as described in the text except parents (mothers of young children) are defined as those with a child aged 0 to 5 who needed paid child care in March 2020. All regressions include occupation fixed effects, age fixed effects, marriage status fixed effects, control for at least one other adult in the household, person fixed effects, and all the double interactions (state by month fixed effects, mother of young child x month fixed effects, and state x mother of young child fixed effects). Standard errors are clustered at the state level.

6 Conclusion

In the aggregate, the COVID-19 pandemic has had a substantial effect on women's labor supply outcomes, especially relative to men's. In this paper, we examine whether state-level policies that forced the closure of child care centers or regulated class sizes specifically had a discernible impact on labor supply outcomes for mothers of young children. We find that these policies did, in fact, increase unemployment rates of mothers of young children in

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these states. Unfortunately, the negative effects do not dissipate once states reopen child care centers, consistent with permanent effects on child care supply in these states.

These results underscore the importance of access to reliable child care in promoting equitable labor market outcomes for men and women. Especially at a time when 4.5 million child care slots are at risk of disappearing, emergency funding and longer-term solutions to support child care centers are desperately needed or mothers of young children may continue to experience persistent and permanent employment losses in the future (Jessen-Howard and Workman, 2020a).

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Rightly blamed the 'bad guy'? Grandparental child care and Covid-19[1]

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This study explores the link between regular grandparental child care and Sars-CoV-2 infection rates at the level of German counties. In our analysis, we suggest that a region's infection rates are shaped by region-, household- as well as individual-specific parameters. We extensively draw on the latter, exploring the inner- and outer-family mechanisms fueling individual contact frequency to test the potential role of regular grandparental child care in explaining overall infection rates. We combine aggregate survey data with local administrative data for German counties and find a positive correlation between the frequency of regular grandparental child care and local Sars-CoV-2 infection rates. However, statistical significance of this relationship breaks down as soon as potentially confounding factors, in particular the local Catholic population share, are controlled for. Our findings do not provide valid support for a significant role of grandparental child care in driving Sars-CoV-2 infections and rather suggest that the frequency of outer-family contacts driven by religious communities might be a more relevant channel in this context. Our results cast doubt on simplistic narratives postulating a link between intergenerational contacts and infection rates.

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3 German Youth Institute and CESifo.

Introduction

Since the beginning of the pandemic, many studies have analyzed the driving forces behind the spread of Sars-CoV-2. This includes the role of social contacts for the prevalence of Sars-CoV-2 and the disease Covid-19 caused by the virus. In this context, the links between contacts within the family, potentially increased risks of infection and Covid-19 mortality have been investigated e.g. by Aparicio and Grossbard 2020, Arpino et al. 2020, Balbo et al. 2020, and Bayer and Kuhn, 2020. In our analysis, we suggest that a region's infection rates are mainly shaped by the two region-specific parameters infection path and spatial distance and the two individual-specific parameters vulnerability and contact frequency. We extensively draw on the latter, exploring the inner- and outer-family mechanisms fueling contact frequency to test the potential role of regular grandparental child care in explaining overall infection rates. We study these relationships for Germany combining aggregate survey data with local administrative data and find a positive correlation between the frequency of grandparental child care and local Sars-CoV-2 infection rates. However, statistical significance of this relationship breaks down as soon as potentially confounding factors, in particular the local Catholic population share, are controlled for. Our findings suggest that the frequency of outer-family contacts driven by religious communities might be a more relevant channel of Sars-CoV-2 infections than grandparental child care.

Due to substantially higher mortality rates of the older persons infected with Sars-CoV-2, early studies in 2020 already pointed at the vulnerability of certain regions due to their demographic characteristics (Kashnitsky and Aburto 2020) as well as the prevalence of intergenerational relations (Balbo et al. 2020). Aparicio and Grossbard (2020) present evidence that the frequency of intergenerational co-residence in US states is positively related to Covid-19 fatalities per capita. Similar results are found by Bayer and Kuhn (2020) in a cross-country analysis with 24 countries. However, Arpino et al. (2020) cannot confirm these findings. They provide a comprehensive analysis of aggregated data on intergenerational family relations from the SHARE (Survey of Health, Ageing and Retirement in Europe) survey linked with information on registered Sars-CoV-2 test data and case fatality rates as published by national health agencies in several European countries. They do not find a robust relationship between infections or case fatality rates and their key variables of interest on the family level, including the frequency of intergenerational contacts, the share of intergenerational households, and the prevalence of grandparental child care in a region or country.[1]

With data for Germany, a comprehensive analysis of the potential link between the extent of grandparental child care support and Sars-CoV-2 infections has not yet been conducted. The SHARE data used in Arpino et al. (2020), for example, do not allow for an analysis on the fine-grained local level because location information of respondents is only made available on the level of federal states. Moreover, previous studies have not convincingly investigated the role of potentially confounding factors when analyzing the correlation between grandparental child care support and Sars-CoV-2 infections. Our study aims to fill these gaps and contribute to the existing literature with an analysis for the case of Germany.

We study a potential relationship between grandparental child care support and Sars-CoV-2 infection rates in Germany combining survey data and registered infections at the level of German local administrative units (counties; German "Kreise"). We draw on a comprehensive register of Sars-CoV-2 infections registered by the local German health authorities (Gesundheitsämter) since the beginning of the pandemic and link these data with aggregated survey data on grandparental child care support at the

[1] Dowd et al. (2020) argue that results from these aggregate level analyses should not be taken as evidence against a link between intergenerational relations and Sars-CoV-2 infection risks. However, this is not the scope of analysis in Arpino et al. (2020). Moreover, due to the lack of data on infections and the frequency/intensity of family contacts on the individual level, all studies to date rely on aggregate survey and/or administrative regional data.

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local regional level. We also provide additional micro foundations for our analysis regarding intergenerational support based on rich individual level survey data.

Theoretical considerations and hypotheses

We restrict our attention to registered Sars-CoV-2 cases at the county level among those 60 years or older relative to the population of those aged 60 or older. The reason for the age restriction is that we are interested in whether child care responsibilities lead to higher risk of infections among the older, grandparent population. In our main analysis, we use March 23, 2020 as a reference point when counting all infections among those aged 60 years or older in the respective county.[2] March 23 was the date when first official policy restrictions were announced in Germany as a response to the accelerating spread of Sars-CoV-2.

We theorize that beyond individual vulnerability in terms of health status, which we capture with the individual's age group affiliation, *three mechanisms* might have shaped an individual's exposure to the Sars-CoV-2 virus on March 23, 2020 and may therefore have driven the number of infections in the population aged 60 and over at the county level at that time (see **Figure 1**).

The first mechanism refers to the *infection path*, since due to the high infectiousness of the Sars-CoV-2 virus, the moment in time when the first infection is measured takes the respective county to a higher level of virus dissemination, measured by the increase in infection numbers (e.g. Frieden and Lee 2020, Bouffanais and Lim 2020). Second, the *spatial distance* of people, captured by the settlement structure, moderates the infection path such that (c.p.) densely populated areas will reinforce and sparsely populated areas will decrease the dissemination path of the virus. The mechanism behind settlement structure is that spatial distance to other human beings is more easily kept in more spacious areas (e.g. Rader et al. 2020). Third, and of utmost importance to this paper, the *frequency of social contacts*, which can be subdivided into intra-familial and extra-familial contacts, is decisive. Although this mechanism is mutually linked to settlement structure and infection path, we argue that it is by itself a function of individuals' daily lives which are shaped by individual and household context-related characteristics as well as macro-level norms, institutions and economic activity. Concerning the extra-familial sphere, the job context as well as non-job activities should shape an individual's interpersonal interactions. Inside the family, interactions refer to individuals of the same generation (intra-generational), as well as to upward intergenerational (the children generation which might be engaged in eldercare for their parents), and downward intergenerational (the grandchildren generation which might be subject to grandparental care) interactions. It is this last type of intra-family contacts we are interested in in this paper.

[2] We also conducted our analysis using infection rates in the total population as dependent variable; those results are slightly weaker but qualitatively similar and not statistically significantly different from results in our main analysis. They are available upon request.

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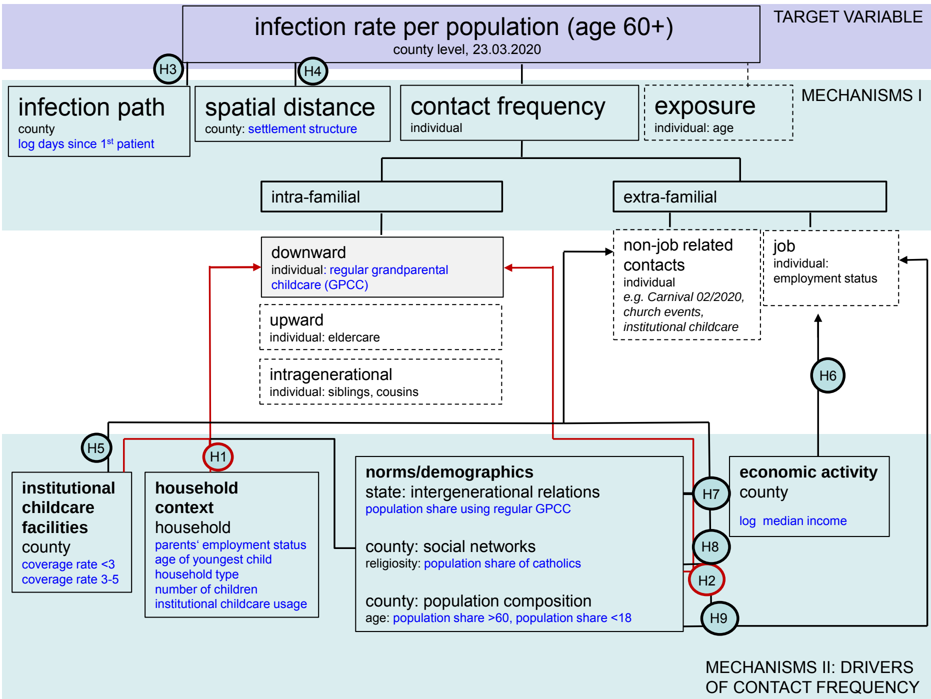
Figure 1. Illustration of causal mechanisms explaining infection rates at the county level.



Source: own illustration.

Since grandparental child care (GPCC) is only one source of social contacts, our **hypotheses,** which structure our empirical investigation, will focus on potential drivers of GPCC and, at the same time, on extra-familial sources of social contacts which might be confounding variables when analyzing the link between GPCC and the observed overall infection rates in the elderly population on the county level. We argue that **grandparental child care** (GPCC) should be shaped by the household context, institutional child care facilities available on the local level as a substitute for within-family care (**H1**), and norms, i.e. reciprocity norms and the value attributed to intergenerational relations (**H2**).

In more detail, we rely on the **extensive margin of regular GPCC** in our main analysis, exploiting the information on the use of regular child care. Starting with the *household context* (**H1**), we expect that working parents, especially working single mothers, should increase the likelihood of GPCC, since grandparents could compensate for scarce parental time resources in this case (Hank and Buber, 2009). Compared to small children under three for which substitutes for parental care are more difficult to find, the presence of a (youngest) child aged 3-5 should be related to more grandparental care (Hank and Buber, 2009). However, grandparental child care should become the less frequent the higher the number of children is (Jappens and van Bavel, 2012). Institutional child care usage at the individual level should decrease the need for grandparental care (Albertini and Kohli, 2013). Further, concerning norms, we expect that Catholic denomination is related to stronger intergenerational family ties and should therefore increase the propensity of grandparental child care (**H2**). A central foundation of Catholic social thought is the so-called subsidiary concept emphasizing the role of intra-family solidarity for social support (e.g. Gundlach 1964, Althammer 2013). Thus, we expect Catholic denomination to be positively associated with the prevalence of social norms fostering within-family support, e.g. GPCC.

Regarding **infection rates at the county level**, we postulate that the *infection path* in terms of (log) days since the first patient increases the number of infections since the virus has had more time to disseminate (**H3**). We further hypothesize that infection rates increase with *population density*, measured in four settlement types (**H4**). Moreover, we suggest that children enrolled in public child care as well as parents engaged in job-related social contacts could provide a source of infection for the (grand-)parents they interact with. Therefore, we use *institutional child care coverage rates* for children below 3 and 3-5, respectively (**H5**), as well as (log) *median income* as a proxy for economic activity (**H6**) as explanatory variables in the infections equation.[3] We thereby rely on previous studies suggesting a positive linkage between economic activity and infections (e.g. Adda 2016, Rader et al. 2020) and institutional child care, respectively. The intuition behind income is that a higher economic added value is related to more job-related contacts. Remote work, especially work from home, has been shown to be more likely offered to and used by the highly educated workforce (Alipour et al. 2020) at the top of the earnings distribution. Since in March 2020 remote work was not as prevalent as it is today, this counter effect was probably not strong enough to outweigh the opposite (sales and revenues driven) positive linkage between income and infections.

Due to a higher risk exposure of elderly people who take care of their grandchildren we expect that regular GPCC should be associated with higher infection rates (**H7**). However, we doubt that GPCC is the true source of this phenomenon. Rather, we suggest that influential third variables drive the association between GPCC and infections at the county level. We thereby follow Arpino et al. (2020) who argue that a stronger focus on within-family ties and a correspondingly lower weight of extra-family ties could serve as a shield protecting the elderly against the virus. As pointed out by the authors, elderly people with close relationships to their children and grandchildren might rely less on social contacts outside the family which might potentially involve even bigger threats of infections. Second, strong intergenerational relationships might affect family life; members might be more careful regarding social interactions outside the family; in some of these cases grandparents might also live close to their children and grandchildren or even in the same house. Third, there is also evidence that family ties and interactions can have a positive effect on psychological wellbeing and health, decreasing the risk of an infection (see e.g. Cohen 2020). In sum, these arguments would motivate a negative association between regular GPCC and infection rates.

Beyond the indirect channel via GPCC, Catholic denomination might impact infections directly via a networks mechanism related to the private (non-job) sphere. Religious activities and other ritual activities do not occur unless a sufficient number of followers is reached at the local level. In particular, religious events, e.g. worships and religiously motivated celebrations with a high number of participants such as weddings, are suspected to drive infection numbers (Lee et al. 2020, Salvador et al. 2020). Moreover, though not directly associated with religiosity in contemporary Germany, German Rhineland regions, i.e. Rhineland-Palatinate and North Rhine-Westphalia where many Catholic people live, are well known for their Carnival processions which take place in February. Thus, via the regional bracket, counties with a high population share of Catholics might exhibit higher infection numbers via the non-job networks channel (**H8**). An extensive literature has studied the role of religiosity for extra-familial social networks (see e.g. for Germany: Traunmüller 2009).

Moreover, due to a higher contact frequency among younger people, a high population share of minors and a low share of elderly people (aged 60 or over) should increase infection rates. In sum, population composition by age should play a role, too (**H9**). As a further control variable we include the share of

[3] Regarding institutional child care it is debated whether children in institutional care could also be a driver of infections in the population. Recent studies argue that infected but asymptomatic children are likely to be a source of further Sars-CoV-2 contagions; Hippich et al., 2020, Laxminarayan et al., 2020.

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foreign nationals per county as well as a dummy variable for eastern German counties, which belonged to the former GDR.

Empirical analysis

Data and descriptive statistics

For our subsequent analysis in the light of the hypothesized mechanisms we combine different data sources. We draw on individual-level survey data from the 6th wave of KiBS (Kinderbetreuungsstudie) which is administered by the German Youth Institute (Alt et al. 2020, Aust et al. 2018). The data was collected in 2017 and involved 36.800 interviews conducted among reference persons ("Auskunftspersonen") of children in the target population below the age of 15 living in 249 selected counties in Germany. We apply survey weights as described in Alt et al. (2020) correcting for non-response bias based on a two-stage weighting procedure. First, observations are weighted according to administrative statistics information on the distribution of children according to age groups in the residence state. Second, the weighting also accounts for non-response behavior of reference persons according to different institutional child-care arrangements. However, our main results are not sensitive to using the weighted or the unweighted sample in our analysis. The variable of interest in our analysis is the indicator whether grandparents provide regular child care to their grandchildren. KiBS contains information on whether grandparents provide regular child care support for a child. Alternative answer options in the survey questionnaire were: only irregular use, no use.[4] If a respondent chose "regular grandparental child care support", he/she was asked for how many hours this support was provided in a regular week. In our analysis, we use the survey information of whether a child receives regular grandparental child care as a binary variable at the individual level as well as in form of weighted respondent shares at the county level. For our micro-founded analyses, we draw on further individual-level information from the KiBS survey such as a child's age, the household composition and labor force participation.
Second, we use county level data on Sars-CoV-2 infections in Germany, as collected by the local health authorities (Gesundheitsämter) and published by the Robert-Koch Institute, RKI (2020). As central information from these data we use case counts per 100,000 inhabitants at the county level (at different points in time). In our main analysis we use infection register data from before the first policy restrictions were announced on March 23, 2020 to circumvent any differential effect of these restrictions which could be correlated with our variables of interest. In addition to the total recorded Sars-CoV-2 cases, the RKI has also published corresponding figures by age groups. From the RKI data, we additionally calculate the time that has passed since the first Sars-CoV-2 case record in a given county.

Third, we use administrative data on sociodemographic and economic characteristics at the county level as provided by the interactive database INKAR (Indikatoren und Karten zur Raum- und Stadtentwicklung) of the Bundesinstitut für Bau-, Stadt- und Raumforschung.

In the Appendix, we present some descriptive statistics for the key variables of our analysis (see Tables A1-A4 in the appendix for detailed information). **Table A1** depicts the shares of GPCC aggregated from the KiBS data for the 16 federal states and four county types distinguishing rural and urban counties. The data shows that GPCC is less common in northern than in southern states in Germany and also less common in the so-called city-states, Hamburg, Berlin and Bremen. This holds for all regular GPCC as well as for GPCC that covers more than 7 hours per week. **Table A2** reports these shares separately by children's age groups (0-3 vs. 3-5 vs. 6-14) revealing that regular GPCC support is most frequent in the

[4] See Appendix B1 for the exact wording of the question in the questionnaire.

age group 3-5. Table A2 additionally shows the share of intense regular GPCC (more than 7 hours per week) per child age group. However, we argue that not intensity but regularity is decisive for infection transmission on the micro level, and a significant share of families using grandparental child care is required to establish a related link between GPCC and infection rates on the macro (county) level. We therefore adhere to our sample specification of families with 0-14 year old children, focusing on regular GPCC in our main and intense GPCC in our sensitivity analysis.

As aforementioned, the use of institutional child care arrangements in a county might be an important factor in this context if grandparents step in in case no other child care possibilities are available. Column 4 in Table A1 shows that institutional child care coverage for children between 3 and 5 is generally very high (>89%) in all states and county types. In contrast, it is considerably lower for children below the age of three (column 3) The coverage in cities (Städtischer Kreis) tends to be lower than in rural areas and large metropolitan areas (Kreisfreie Großstadt), but the variation between different county types is not as large as between federal states. In general, coverage rates for under-threes are higher in Eastern compared to Western Germany.

Table A3 denotes total infection numbers and numbers per 1,000 inhabitants by German federal states for March 23, 2020. Additional to our focus group of elderly people aged 60 and over, figures for the total population are presented. Concerning the elderly, infections per 1,000 inhabitants vary between 24.2 (Mecklenburg-Western Pomerania) to 185.6 (Baden-Wuerttemberg). With respect to the total population, cases range from 81 in Bremen to 4,723 in Bavaria. **Table A4** depicts total infection numbers and numbers per 1,000 inhabitants by German federal states for 30 September, 2020 which is used as an alternative reference point in our robustness checks.

Main regression analysis

Obviously, GPCC is only a small piece of the puzzle explaining overall infection rates in the target population of elderly people. Therefore, our empirical strategy consists of two strands. First, we build on the theoretical underpinnings of grandparental child care. This micro foundation will undergo an empirical test relying on the KiBS data outlined above. Via its aggregated form on the county level, the population share using regular GPCC, the outcome variable on the micro level will enter the infections equation on the macro (county) level as the second strand of our empirical design.

Starting with the micro foundation of regular GPCC, **Table 1** presents the results of linear probability regressions with the weighted sample of 33,259 children in the target population of those aged 0 to 14 in Germany reached by the KiBS survey. The dependent variable is whether a child in the sample is regularly taken care of by a grandparent. Indeed, the results in Table 1 document that the population share of Catholic population on the county level is strongly positively associated with the probability that the grandparents are involved in child care support, confirming our hypothesis **H2**. Depending on the specification, this roughly means that a 10% increase in the Catholic population share in a county is associated with a 0.4 to 0.6 percentage point increase in the probability that the grandparents of a child are regularly involved in child care.

Moreover, these micro-level results reveal that children between the age of three and five are more frequently taken care of by their grandparents; children aged six to ten and particularly those aged ten to 14 are less likely to receive regular grandparental care compared to the reference group of children under the age of three. As to parents' labor force participation status, the reference category is defined as couples pursuing a traditional male breadwinner model, i.e. the male partner works and the female partner is out of the labor force. Relative to this reference group, single mothers in as well as out of the

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labor force are more likely to receive child care support from the grandparents of their children. For single fathers this is only the case if they are in the labor force (the group of single fathers in the sample contains only 60 observations). For couples that pursue a dual earner model and couples where only the female partner participates in the labor market it is also more likely that grandparents of their children provide regular child care support. This analysis shows that certain family types, e.g. single mothers and dual earner couples rely on support by grandparents relatively more often than traditional male breadwinner families. In case of an available and used institutional child care arrangement, grandparents are less likely to be involved in regular child care support. In sum, the household context variables meet our expectations (**H1**).

In column 2, we additionally include information on the county settlement structure (rural or urban), in column 3 we include federal state dummies. Our previous findings are robust to the inclusion of these additional variables.

Table 1. Analysis of micro-level characteristics associated with regular grandparental child care support.

Catholic population share	0.0619***	0.0406***	0.0528***
	(0.0104)	(0.0107)	(0.0172)
Child age 0-2 (reference category)	-	-	-
Child age 3-5	0.0314***	0.0276***	0.0276***
	(0.00736)	(0.00736)	(0.00737)
Child age 6-10	-0.00776	-0.0140**	-0.0143**
	(0.00661)	(0.00664)	(0.00665)
Child age 10-14	-0.127***	-0.132***	-0.132***
	(0.00675)	(0.00677)	(0.00677)
Single mother, working	0.129***	0.129***	0.128***
	(0.0168)	(0.0168)	(0.0168)
Single mother, not working	0.0761***	0.0714**	0.0705**
	(0.0289)	(0.0289)	(0.0289)
Single father, working	0.103*	0.105*	0.109**
	(0.0537)	(0.0536)	(0.0536)
Single father, not working	-0.0381	-0.0158	-0.00985
	(0.113)	(0.113)	(0.113)
Couple, male breadwinner (reference category)	-	-	-
Couple, female breadwinner	0.0316**	0.0340**	0.0342**
	(0.0143)	(0.0143)	(0.0143)
Couple, dual earner	0.131***	0.131***	0.130***
	(0.00555)	(0.00555)	(0.00555)
Couple, not working	0.00214	0.00439	0.00501
	(0.0154)	(0.0153)	(0.0154)
1 child in household (reference category)	-	-	-
2 children in household	-0.0161***	-0.0164***	-0.0162***
	(0.00546)	(0.00545)	(0.00546)
3+ children in household	-0.0552***	-0.0547***	-0.0547***
	(0.00641)	(0.00640)	(0.00641)
Institutional child care attendance	-0.0460***	-0.0417***	-0.0416***
	(0.00503)	(0.00505)	(0.00508)
Rural county – sparsely populated (ref.)	-	-	-

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Rural county		0.0176**	0.0164*
		(0.00850)	(0.00885)
Urban county		0.00386	0.00394
		(0.00752)	(0.00877)
Urban county (single metropolitan area)		-0.0330***	-0.0357***
		(0.00757)	(0.00848)
State fixed effects			X
Observations	33,259	33,259	33,259

Note: Weighted OLS regressions (Linear Probability Model),
*** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses.

We now turn to the question whether the presented data reveal any relationship between regular grandparental child care support and higher rates of Sars-CoV-2 infections in a county.

Table 2 reports results of OLS regression estimations at the county level linking aggregated information on regular grandparental child care support for children aged under 15 from KiBS at the county level[5] with regional administrative statistics and the infection rates as introduced above. Using the logarithm of cumulative infection rates in a county on March 23 as the dependent variable, we subsequently introduce various regional variables to test our hypotheses and account for potentially confounding factors in the correlation analysis. We present population-size-weighted estimates. Results are qualitatively similar when running the regressions without population weights.

Column 1 shows a statistically significant positive relationship between log Sars-CoV-2 infection rates and the percentage share of grandparental child care in a county, supporting **H7**. In this specification, we only additionally account for the days since the first registered Sars-CoV-2 case in a county (which are significantly positively associated to infection rates, supporting **H3**). These otherwise unconditional correlation results could be interpreted as confirming the hypothesis that more frequent contact of the old-age population is associated with higher risk of infection (e.g. Aparicio and Grossbard 2020). In columns 2-5 we introduce further variables to our empirical model. Counties with higher median income and metropolitan counties exhibit higher infection rates in the old-age population, providing support for our hypotheses **H4** and **H6**. Child care coverage rates and the population shares of non-Germans and elderly people, respectively, lack significant associations with county-specific infection rates.

Most importantly, when including the share of Catholic population in column 5, we observe that this variable is highly positively correlated with our dependent variable (confirming **H8**) and that it absorbs the effect of grandparental child care.[6] That is, H7 has to be discarded in this specification. This finding shows that there is no valid link between grandparental child care and infection rates on the county level as soon as further region-specific characteristics are controlled for. Therefore, regional variation in grandparental care cannot be blamed as a driver of regional variation in infection rates. Although affiliation to Catholic religion increases the likelihood of grandparental child care, the Catholic effect is

[5] We only include counties with at least 30 individual-level observations. Results are qualitatively similar without minimum number of observations restriction or when restricting to at least 20 or 50 observations per county. Average number of underlying individual-level survey observations per county in the analysis presented in Table 2 is 184. As described above, we apply survey non-response weights as described in Alt et al. 2020.

[6] When including state fixed effects in the specifications in Table 2 the coefficient for grandparental support also becomes insignificant in columns 1-4. This suggests that the positive relationship between grandparental support and Sars-CoV-2 infections in these columns is rather driven by the variation between states. The estimation results including fixed effects are available upon request.

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obviously not limited to the family sphere but drives infection rates even if its within-family effect is accounted for. We motivate this out-of-family effect with social networks related to religious beliefs. Our regression results in Table 2 support this suggestion.

The population share of minors is significant in the second and even more so in the third specification. We do not find any effect of the population share of those aged 60 and over. That is, hypothesis **H9** can only partially be confirmed and **H5** cannot be confirmed at all on the basis of our data.

Table 2. Regressions explaining log registered infections 60+ per 100.000 (March 23, 2020) at the county level.

	Log registered infections per 100,000. (60+)				
Log days since first case	1.478***	0.929***	0.916***	0.947***	0.914***
	(0.168)	(0.186)	(0.183)	(0.184)	(0.174)
Share of regular grandparental child care (below age 15)	1.658**	1.607**	1.531**	1.594**	0.481
	(0.708)	(0.683)	(0.671)	(0.682)	(0.651)
Log pop./km^2		-0.0587	-0.123**	-0.0544	-0.0903
		(0.0506)	(0.0587)	(0.0710)	(0.0712)
Log median income		1.747***	1.273**	1.442*	1.176*
		(0.581)	(0.622)	(0.751)	(0.708)
East Germany		-0.0903	0.0179	-0.0844	0.311
		(0.149)	(0.149)	(0.220)	(0.273)
Urban county			0.362***	0.311**	0.286**
			(0.131)	(0.137)	(0.128)
Population share age 60+			-1.842	-1.748	1.969
			(2.606)	(2.767)	(2.798)
Population share under 18			7.371	9.052*	10.38**
			(4.698)	(5.187)	(5.005)
Share institutional child care (below age 3)				0.00186	0.00246
				(0.00716)	(0.00943)
Share institutional child care (age 3-5)				0.0164	0.0164
				(0.0142)	(0.0154)
Foreigner share				-1.551	0.478
				(1.706)	(1.637)
Catholic population share					1.375***
					(0.257)
Observations (counties)	197	197	197	197	188

Note: Only counties with at least 30 individual observations in KiBS survey data, population-weighted coefficient estimation.
*** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses.

In our next step, we build on the findings from Table 2 and particularly on the interpretation, that Catholic denomination fuels infection rates both via a GPCC and a social networks channel.

On the one hand, following from **H1** which has been confirmed in our GPCC estimations, a higher share of Catholics in the population should increase GPCC, which in turn proved to increase infection risks in our infection regressions (Table 2, colums 1-4). Following this reasoning, we could expect a stronger/significant relationship between regular grandparental support and infection rates in these counties.

On the other hand, as discussed in the empirical strategy section, we suppose that the out-of-family channel requires a sufficient population share of Catholics to become effective: To celebrate religious events with non-family members and to establish religion-related rituals, a threshold of peers with the same attitudes is required. As aforementioned, due to the region commonality, we subsume Carnival

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processions in this category. We cannot measure the non-job networks channel directly, but we suggest that in counties lacking a significant share of Catholics, the out-of-family mechanism should be relatively weaker than the within-family mechanism, attributing regular GPCC a higher role in this environment. Conversely, in counties exhibiting a significant share of Catholics, we expect the regular GPCC effect to be relatively weaker and the out-of-family channel to be relatively stronger. To disentangle the GPCC from the Catholic out-of-family channel and to verify which argumentation is supported by our data, our next task is to test H7 again, this time in two different social environments, distinguished by the prevalence of Catholic denomination. While this factor entered Table 2 as a 'shift effect', it is allowed to interact with all independent variables in the following regressions. To this end, we divide our sample in two subsamples with the first comprising counties with a population share of Catholics below 20% and the second with the remaining counties. We selected a threshold of 20% Catholic population share as this divides our sample roughly in half. Since Eastern Germany lacks representation in the first group, we restrict both subsamples to West German counties. The first group comprises counties with only minor shares of Catholics spans counties in Northern Germany. In 2011 for example, Schleswig-Holstein exhibited a Catholics share of 6.4%, while Lower Saxony stood at 18.3% (Frerk 2018a). The second group comprising counties with Catholic population shares of 20% and over concentrates in Southern and Western Germany. In 2015, the population share of Catholics stood at 39.3% in North Rhine-Westphalia, at 42.2% in Rhineland-Palatinate, at 59.8% in the Saarland, at 34.5% in Baden-Wuerttemberg and at 51.2% in Bavaria (Frerk 2018b). We use the model specification presented in column 4 of Table 2 and run our OLS regressions based on the two aforementioned subsamples.

Table 3 reports the results. As can easily be seen, regular GPCC is not significant in either of the two county groups. Apparently, neither in South-Western Germany nor in the rest of the country, regular GPCC was significantly related to infection rates in March 2020. Even in the counties where Catholics form a relevant population subgroup, regular GPCC does not significantly relate to infection rates among the elderly population. If anything, the data point to the importance of extra-familial ties: The regression coefficient for regular grandparental support is larger in those counties with a Catholic share of less than 20%. However, the coefficients lack significance.

A cautious interpretation of this finding would be that it further strengthens our interpretation derived from Table 2, column 5: The 'bad guy' role of GPCC is lost as soon as relevant third variables come on stage.

Table 3. Regressions explaining log registered infections per 100.000 inhabitants 60+ (March 23, 2020) at the county level **(West-Germany)**.

	Log registered infections per 100,000. (60+)			
	Cath.pop. <20%		Cath.pop. >=20%	
Log days since first case	-0.0394	-0.104	1.185***	1.098***
	(0.474)	(0.486)	(0.229)	(0.225)
Share of regular grandparental child care (below age 15)	1.684	1.953	0.188	0.0981
	(2.131)	(2.182)	(0.869)	(0.843)
Log pop./km^2	-0.0685	-0.124	-0.0648	-0.0472
	(0.195)	(0.212)	(0.106)	(0.103)
Log median income	3.332*	3.528*	1.023	1.128
	(1.845)	(1.881)	(0.960)	(0.932)
Urban county	0.464	0.533*	0.0529	0.194
	(0.294)	(0.312)	(0.201)	(0.203)
Population share age 60+	7.084	5.723	-6.007	-2.544

	(6.591)	(6.921)	(4.714)	(4.769)
Population share under 18	-0.609	-2.524	3.914	8.906
	(13.05)	(13.44)	(6.468)	(6.571)
Share institutional child care	0.0330	0.0305	-0.0136	-0.0136
(below age 3)	(0.0234)	(0.0239)	(0.0135)	(0.0131)
Share institutional child care	-0.0396	-0.0291	0.0361	0.0423*
(age 3-5)	(0.0344)	(0.0377)	(0.0246)	(0.0239)
Foreigner share	-4.019	-2.164	-2.908	-0.735
	(4.974)	(5.660)	(2.474)	(2.546)
Catholic population share		-2.505		1.222**
		(3.538)		(0.480)
Observations (counties)	40	40	97	97

Note: Only western German counties with at least 30 individual observations in KiBS survey data, population-weighted coefficient estimation.
*** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses.

Sensitivity analysis

We conduct two *robustness checks*. The first refers to our dependent variable in the GPCC equation. Deviating from the standard for the main analysis defined above, we use the share of regular GPCC for children aged 0-14 that covers **more than 7 hours per week** to check whether grandparental child care intensity makes a difference for our results.[7] See **Table A5** in the appendix for the detailed results. Table A5 replicates the model specifications of Table 2, the only difference lies with the specification of the GPCC variable, using the intensive margin instead of the extensive margin of GPCC. The main take-away from the results is that, different from the extensive margin, regular GPCC is insignificant in all specifications when the intensive margin is used: The prevalence of regular grandparental child care of more than 7 hours a week does not significantly relate to a county's infection rates among elderly people. This is intuitive since, as aforementioned, for virus dissemination, the frequency of contacts should play a higher role than intensity in terms of duration.

The second robustness check concerns the reference point in time regarding registered infections. Deviating from the standard defined above, we base our analysis on **30 September, 2020** and check the stability of our results against this change. For our regressions, we use the variable specifications denoted in Table 2. **Table A6** in the appendix provides detailed regression results. Analogous to the results derived for March 23, 2020, the parameter of regular GPCC turns insignificant as soon as the population share with Catholic denomination is taken into account. Regarding the other independent variables, results resemble those of March, too: Metropolitan areas and a high population share of minors are positively associated with infection numbers. Different from March results, the population share of the non-German population is now positively related to infection rates, too. Differences to the results in our main analysis could be driven by the changed regional distribution of hot spots between March and September, 2020 (see Table A4 in the appendix): during that time, the virus had been disseminating further. By the end of September, parts of central and northern Germany (e.g. some Hessian regions and the city-state of Hamburg) exhibit rather high infection dynamics. By contrast, states in North-Eastern Germany (i.e. Mecklenburg-Vorpommern, Sachsen-Anhalt, Brandenburg and Schleswig-Holstein) still feature lower infection rates. These states are characterized by rather low foreign population shares (see Table A1), which might have driven the positive linkage of foreign population share to infection rates by the end of September.

[7] We also conducted the regressions at the micro level with the dependent variable GPCC (more than 7h per week). The results are qualitatively similar to our main analysis and available upon request.

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Conclusion

This study explores the role of regular grandparental child care (GPCC) for Sars-CoV-2 infection rates at the level of German counties. Our results show that a significant association between the two vanishes as soon as the Catholic population share is accounted for. Although Catholic religion increases the likelihood of grandparental child care in a family, our regressions of infection rates on a range of county-related covariates show that the Catholic effect drives infection rates even if the population share using GPCC effect is accounted for. We motivate this out-of-family channel with social networks related to religious beliefs. Our suggestion is tentatively confirmed by regressions based on two subsamples of counties, differing in their Catholic population share. Even in the subsample with a notable prevalence of Catholics, the GPCC parameter lacks significance. However, its effect size is lower, tentatively confirming our network hypothesis assigning out-of-family mechanisms a relatively higher role in these environments.

Our main result still holds when we use September 30, 2020 instead of March 23, 2020 as a point of reference for the infection rates: Here too, the significance of regular GPCC is lost as soon as the population share with Catholic denomination is accounted for. By contrast, drawing on the intensive instead of the extensive margin regarding GPCC does not yield any significant association between GPCC prevalence and infection rates in any of the specifications. This does not contradict our main finding but is intuitive since for virus dissemination, the frequency of contacts should play a higher role than their intensity in terms of duration.

In sum, our findings cast doubt on simplistic narratives postulating a link between intergenerational contacts and infection rates. At least our data does not provide valid support for a significant role of grandparental child care in driving infections. Rather, our findings support previous evidence highlighting the decisive role of third variables which have to be taken into account. Exemplified with Catholic denomination, we show that region-specific third variables may enforce the postulated (intra-family) mechanism but at the same time fuel out-of-family channels that limit or, in our case, even eliminate the statistical relevance of grandparental child care. In principle, a protective effect of intra-familial ties would also be possible, but our data is not suited to investigating this. Our data does not provide reliable support for a 'bad guy' role of grandparental child care.

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Appendix A

Table A1. Share of children receiving regular grandparental child care and institutional child care in per cent (columns 1-4), population share catholic in per cent (column 5), population share w. foreign nationality in per cent (column 6) in 2017.

State	Share of regular grandparental child care (below age 15)	Share of regular grandparental child care >7hrs/week	Share institutional child care children 0-3	Share institutional child care children	Share Catholic population	Share foreign population
Baden-Wuerttemberg	16.9	6.5	27.8	92.2	36.3	15.1
Bavaria	16.6	7.2	26.5	89.7	53.8	12.7
Berlin	12.4	4.6	43.8	90.4	8.8	17.6
Brandenburg	14.1	5.5	55.5	92.3	3.3	4.4
Bremen	13.4	5.8	25.9	82.8	10.9	17.4
Hamburg	12.5	3.7	43.5	85.9	9.9	16.2
Hesse	16.2	7.7	29.2	90.5	24.3	15.7
Mecklenburg-Vorpommern	11.9	5.6	55.8	93.8	3.2	4.3
Lower Saxony	14.5	6.3	29.2	90.3	17.5	9.0
North Rhine-Westphalia	15.2	7.6	25.7	88.8	40.9	12.8
Rhineland-Palatinate	16.8	7.6	30.0	93.7	44.2	10.6
Saarland	16.9	8.7	27.7	91.2	61.9	10.7
Saxony	16.1	6.3	50.6	93.7	3.6	4.6
Saxony-Anhalt	16.6	8.2	57.0	91.7	3.4	4.7
Schleswig-Holstein	14.2	6.0	31.3	89.3	6.0	7.7
Thuringia	18.3	9.0	53.4	94.7	7.7	4.5
Rural county – sparsely populated	16.5	7.5	37.6	91.5	27.7	6.2
Rural county	18.5	8.7	34.3	91.1	31.2	7.4
Urban county	17.2	8.0	28.2	90.8	36.9	11.5
Urban county (single metropolitan area)	13.2	5.3	34.8	89.5	27.4	17.3
Total	15.6	6.9	32.6	90.6	30.1	11.7

Source column 1-2: KiBS Survey, wave 6, 33,259 observations (weighted, s. Alt et al. 2020), lower number of observations due to missing information for weight calculation; column 3-6: Data from INKAR, 2020. Reference year for all data: 2017.

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Table A2. Share of children receiving regular grandparental child care in per cent in 2017 (by age group).

	Share of regular grandparental child care					
State	age 0-3	age 0-3, >7hrs/week	age 3-5	age 3-5, >7hrs/week	age 6-14	age 6-14, >7hrs/week
Baden-Wuerttemberg	15.8	9.1	22.0	7.8	15.6	5.0
Bavaria	15.9	9.5	23.0	10.2	14.5	5.4
Berlin	11.6	3.9	15.8	6.0	11.4	4.3
Brandenburg	11.9	4.4	20.3	8.9	12.7	4.8
Bremen	14.1	7.8	13.5	5.1	13.0	5.1
Hamburg	11.4	4.3	17.5	5.6	11.0	2.8
Hesse	14.5	9.4	18.8	7.5	15.9	7.1
Mecklenburg-Vorpommern	8.4	3.5	13.2	6.4	12.7	6.1
Lower Saxony	16.6	9.2	17.8	7.4	12.7	4.9
North Rhine-Westphalia	19.3	12.5	17.1	7.9	13.0	5.6
Rhineland-Palatinate	16.1	8.3	22.4	11.3	15.1	6.2
Saarland	17.7	11.1	23.5	10.9	14.4	7.2
Saxony	12.9	4.8	21.2	7.8	15.5	6.1
Saxony-Anhalt	14.8	7.0	19.7	9.4	16.2	8.1
Schleswig-Holstein	15.7	7.7	19.6	7.8	11.9	4.7
Thuringia	15.7	6.0	21.6	11.1	18.1	9.6
Rural county – sparsely populated	17.0	10.1	20.5	9.7	15.3	6.3
Rural county	20.7	12.5	25.0	11.3	16.0	6.8
Urban county	17.3	10.6	22.3	9.2	15.6	6.8
Urban county (single metropolitan area)	13.7	7.0	16.4	6.7	11.5	3.7
Total	15.9	8.9	19.6	8.3	14.1	5.6

Source: KiBS Survey, wave 6, 33,259 observations (weighted, s. Alt et al. 2020), lower number of observations due to missing information for weight calculation.

Table A3. Sars-CoV-2 infections and associated deaths by federal state according to RKI registers from March 23, 2020.

State	Infections (total)	Infections (per 100,000 inhabitants)	Infections (age 60+)	Infections (per 100,000, age 60+)
Baden-Wuerttemberg	15,330	138.5	4,114	185.6
Bavaria	18,251	139.6	4723	178.2
Berlin	2,901	79.6	454	64.9
Brandenburg	1,055	42.0	266	44.0
Bremen	312	45.7	81	56.1
Hamburg	2,578	140.0	473	139.6
Hesse	3,576	57.1	811	63.1
Mecklenburg-Vorpommern	425	26.4	94	24.2
Lower Saxony	4,767	59.7	1,265	72.7
North Rhine-Westphalia	15,674	87.4	3,777	100.9
Rhineland-Palatinate	3,069	75.1	671	76.4
Saarland	946	95.5	224	96.6
Saxony	2,288	56.1	585	55.5
Saxony-Anhalt	779	35.3	220	38.3
Schleswig-Holstein	1,274	44.0	377	57.0
Thuringia	886	41.3	221	40.8
Total	74,111	89.0	18,356	107.8

Source: registered Sars-CoV-2 infections, RKI (2020), own calculation of infections per 100,000 population by age group based on INKAR, 2020.

Table A4. Sars-CoV-2 infections and associated deaths by federal state according to RKI registers from Sept. 30, 2020.

State	Infections (total)	Infections (per 100,000 inhabitants)	Infections (age 60+)
Baden-Wuerttemberg	49,203	444.5	570.6
Bavaria	67,761	518.2	615.4
Berlin	14,326	393.1	314.1
Brandenburg	4,251	169.2	181.0
Bremen	2,385	349.2	272.3
Hamburg	7,750	420.9	474.1
Hesse	18,788	299.8	291.8
Mecklenburg-Vorpommern	1,168	72.6	72.4
Lower Saxony	20,025	250.9	259.0
North Rhine-Westphalia	69,283	386.4	383.1
Rhineland-Palatinate	10,629	260.2	254.0
Saarland	3,296	332.8	454.3
Saxony	7,151	175.4	206.5
Saxony-Anhalt	2,613	118.3	118.4
Schleswig-Holstein	4,723	163.0	177.2
Thuringia	4,056	189.3	258.9
Total	287,408	339.4	383.3

Source: registered Sars-CoV-2 infections, RKI (2020), own calculation of infections per 100,000 population by age group based on INKAR, 2020.

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Table A5. Regressions explaining log registered infections 60+ per 100.000 (March 23, 2020) at the county level.

	Log registered infections per 100,000 (60+).				
Log days since first case	1.428***	0.892***	0.885***	0.914***	0.889***
	(0.169)	(0.187)	(0.184)	(0.186)	(0.174)
Share of regular grandparental child care, below age 15, **>7hrs/week**	1.189	1.175	1.127	1.221	0.433
	(1.061)	(1.031)	(1.009)	(1.020)	(0.969)
Log pop./km^2		-0.0709	-0.135**	-0.0668	-0.105
		(0.0511)	(0.0592)	(0.0719)	(0.0712)
Log median income		1.792***	1.339**	1.484*	1.161
		(0.587)	(0.628)	(0.759)	(0.709)
East Germany		-0.0909	0.0211	-0.0529	0.345
		(0.151)	(0.151)	(0.222)	(0.271)
Urban county			0.373***	0.318**	0.293**
			(0.133)	(0.138)	(0.128)
Population share age 60+			-1.509	-1.462	2.304
			(2.632)	(2.796)	(2.807)
Population share under 18			7.689	9.066*	10.34**
			(4.744)	(5.243)	(5.009)
Share institutional child care (below age 3)				0.000228	0.00172
				(0.00719)	(0.00938)
Share institutional child care (age 3-5)				0.0183	0.0168
				(0.0143)	(0.0154)
Foreigner share				-1.448	0.705
				(1.726)	(1.635)
Catholic population share					1.439***
					(0.260)
Observations (counties)	197	197	197	197	188

Notes: Minimum number of individual-level observations per county in KiBS survey: 30, regression estimates weighted based on population size.
Standard errors in parentheses,
*** p<0.01, ** p<0.05, * p<0.1.

Table A6. Regressions explaining log **registered infections 60+ per 100.000 (Sept. 30, 2020)** at the county level.

	Log registered infections per 100,000 (60+).				
Log days since first case	1.223***	0.556***	0.538***	0.486***	0.477***
	(0.169)	(0.181)	(0.176)	(0.176)	(0.163)
Share of regular grandparental child care, below age 15	1.597**	1.932***	1.851***	1.609**	0.570
	(0.715)	(0.664)	(0.646)	(0.651)	(0.609)
Log pop./km^2		0.0567	0.00316	-0.0670	-0.126*
		(0.0492)	(0.0565)	(0.0678)	(0.0666)
Log median income		1.132**	0.679	-0.0193	-0.421
		(0.565)	(0.599)	(0.717)	(0.662)
East Germany		-0.280*	-0.154	-0.0346	0.338
		(0.145)	(0.143)	(0.210)	(0.255)
Urban county			0.344***	0.377***	0.349***
			(0.126)	(0.130)	(0.119)
Population share age 60+			-1.316	-0.625	2.216
			(2.510)	(2.641)	(2.616)
Population share under 18			10.30**	9.030*	9.768**
			(4.524)	(4.952)	(4.678)

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Share institutional child care (below age 3)				-0.00710	-0.00752
				(0.00684)	(0.00882)
Share institutional child care				0.00466	0.00622
(age 3-5)				(0.0136)	(0.0144)
Foreigner share				3.339**	5.649***
				(1.629)	(1.530)
Catholic population share					1.158***
					(0.240)
Observations (counties)	197	197	197	197	188

Notes: Minimum number of individual-level observations per county in KiBS survey: 30, regression estimates weighted based on population size.
Standard errors in parentheses,
*** p<0.01, ** p<0.05, * p<0.1.

Table A7. Regressions explaining log registered infections per 100.000 inhabitants 60+ (Sept. 30, 2020) at the county level **(West-Germany)**.

	Log registered infections per 100,000 (60+).			
	Cath.pop. <20%		Cath.Pop. >=20%	
Log days since first case	-0.214	-0.257	0.771***	0.714***
	(0.463)	(0.478)	(0.182)	(0.181)
Share of regular grandparental child care, below age 15	0.740	0.917	0.520	0.461
	(2.083)	(2.144)	(0.691)	(0.679)
Log pop./km^2	-0.325*	-0.362*	0.0185	0.0300
	(0.190)	(0.208)	(0.0845)	(0.0831)
Log median income	1.395	1.525	-0.161	-0.0916
	(1.803)	(1.848)	(0.764)	(0.751)
Urban county	0.642**	0.687**	0.0310	0.123
	(0.287)	(0.307)	(0.160)	(0.164)
Population share age 60+	5.415	4.518	-5.201	-2.935
	(6.443)	(6.798)	(3.751)	(3.841)
Population share under 18	5.139	3.877	2.205	5.473
	(12.76)	(13.20)	(5.148)	(5.293)
Share institutional child care (below age 3)	0.0199	0.0182	-0.0211*	-0.0211*
	(0.0229)	(0.0235)	(0.0107)	(0.0108)
Share institutional child care	-0.0257	-0.0188	0.0176	0.0217
(age 3-5)	(0.0336)	(0.0371)	(0.0195)	(0.0193)
Foreigner share	6.279	7.502	0.0666	1.489
	(4.862)	(5.560)	(1.969)	(2.051)
Catholic population share		-1.651		0.800**
		(3.475)		(0.387)
Observations (counties)	40	40	97	97

Notes: Only western German counties with at least 30 individual observations in KiBS survey data, population-weighted coefficient estimation.
*** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses.

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Appendix B

The exact wording of the question on grandparental child care support in KiBS wave 6 is as follows:

In welchem Umfang wird Ihr Kind von den Großeltern betreut? (*How often is your child looked after by his or her grandparents?)*

Regelmäßig, mit ___ Stunden in einer typischen Woche (*regularly, with __ hours in a typical week*) … ___
Nach Bedarf (*as required*)…… ___
Gar nicht (*not at all*)………… ___

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