TEACHING THE MENTALLY RETARDED[1]

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Introduction

I. Focus of this Chapter

Our central focus is concerned with research on teaching the mentally retarded. For reasons to be discussed in the next section, we adhere to a broad operational definition of empirical study that includes both formal experimentation as well as other types of observational systems. Frankly, we have viewed with concern an almost total commitment to experimental and quasi-experimental approaches applied even to very complex and "dirty" field problems that cannot be studied satisfactorily in the laboratory--i.e., outside the natural setting. As Shulman (1970) remarked.

> If the object of such research (educational) is the development of coherent and workable theories, researchers are nearly as far from that goal today as they are from controlling the weather. If the goal of educational research is significant improvement in the daily functioning of educational programs, I know of little evidence that researchers have made discernible strides in that direction.
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> (p. 371)

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[1]The authors are grateful to Dr. Harriett Blank and Professors Arthur Blumberg, Thomas Green, Samuel Guskin, Samuel A. Kirk, Horace Mann, James J. McCarthy, Maynard Reynolds, Seymour B. Sarason, Paul R. Salamone, and Howard Spicker for reading earlier drafts of this chapter and offering us valuable and constructive comments and suggestions. We are also indebted to Mrs. Virginia Andrews, Mrs. Mary Kishman, and Mrs. Nancy Spekman for their generous assistance in preparing various manuscript drafts of this chapter.

The problem of relevancy has been particularly troublesome in the field of mental retardation. With some rare exceptions--few of which might be called research on teaching (e.g. Edgerton, 1967, Goffman, 1961)--research in mental retardation has followed traditional lines of experimentation, survey analysis, and test construction and validation. With rare exceptions, participant observation procedures and other phenomenological approaches to systematic data collection and analysis have not been applied to the general study of mentally retarded children or, specifically, to their school lives.

The above remarks are not meant to suggest antagonism to the value and promotion of formal experimentation in the field. Our concern is with the extent to which traditional models have determined the kind of research that is being conducted--rather than, coversely, models determined by the nature of problems studied. Further, we are concerned that such traditional research models also determine the kinds of independent variables that are selected for study and influence the scaling of independent variation.[2] To state this another way, researchers are confronted by

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[2]Presenting a compelling argument, Shulman (1970) encourgaged educational researchers to leave the safe and sterile atmosphere of the conventional laboratory for the classroom setting. Because the current "...gap between such studies ("conclusion-oriented") and needed educational applications is simply too great," what is needed is another form of investigation "... to bridge that gap and create the basis for educational theory." (p. 377) Shulman concluded that, in view of its complexity, "...it might be in the long range interest of both psychological theory and education to ignore those theories for the moment and proceed along a relatively atheoretical path in the study of education." (p. 383)

In a personal communication, Samuel A. Kirk, pursuing a line of reasoning similar to Shulman's speculated that "...one of the research approaches that could get at process and some of the things that you are talking about is through idio-graphic studies. I always think of the report by Itard which has become a classic...I think that in our field we need more rather intensive studies of cases, how they learn, what obstacles there are to learning, in order to understand their processes. When we have enough hypotheses from this kind of approach, we could do research. As you indicated, we tend to jump on comparison of methods, without making a real analysis of what ought to be done."

problems connected with the assignment of children to treatments and, to further complicate this, of teacher to treatments. This problem becomes formidable when the researcher attempts to effectively deal with triads of teachers, children, and methods. Therefore, when one designs an experiment that includes children (who vary) and teachers and, possibly, some other adults (who vary) in classrooms, the notion of homogeneity of variance that assumes there is similarity of the way a treatment occurs in different classes with different teachers and different children is questionable. In attempting to deal with group comparability, some researchers have utilized random procedures (or substitute methods) to gain group comparability in the assignment of children, teachers, and methods. Unfortunately, although this may solve certain theoretical problems if the randomization procedures are maintained--which they rarely are in field studies--other problems are hardly dealt with and certain new ones are created.[3]

Our review of recent literature relating to how, and under what conditions, the mentally retarded learn, reveals continued major emphasis on experimental studies that attempt to control various independent factors. This research has assessed differences among several independent methods of teaching the mentally retarded. Investigators have designed research utilizing randomization procedures in which groups of children learning

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[3]Accompanying the randomization strategy is the assumption that factors which do not interest the researcher, or with which he cannot deal, will "randomize out," i.e. will equalize across groups. Although, as we stated above, this may provide a theoretical solution for the researcher--if the randomization procedure does not break down--it is entirely possible that those variables with which the researcher has attempted to deal through randomization may be the very factors upon which the research might have profitably focused.

to read under one method are equal to those assigned to another method. Further, the researcher randomly assigns teachers to each group, hopefully to insure that one teacher would be more or less as well adjusted to his group and method as any other teacher would be to any other group and method; and, lastly, to give greater assurance to the assumption of group equality, other pertinent variables would be measured to check the randomization. Consequently, the researcher is in a position to claim that these two variables, children and teachers, were held constant for all of the groups studied.

The above research strategy is based on the belief that the method of teaching (or the curriculum or the curriculum organization) is the most significant independent variable. In such studies, the kinds of children and the personalities of the teachers are considered to be intervening variables that have importance, but are peripheral to the experimental comparison being made. Therefore, controls are employed to equalize the other potentially independent variables. One objective of this chapter is to present a rationale that is a reversal of the above example. By this, we intend to discuss the possibilities and values that may obtain by assigning specifically--for the purposes of field research on teaching--major independent variables which relate directly to teachers and children, and intervening variables which relate to method and curriculum content. Although this approach is suitable for the study of classroom situations, per se, it appears to be especially appropriate for the study of the educational environment of mentally retarded or most other disabled children.[4] In those special programs, the "usual"

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[4]In personal communication, James J. McCarthy echoed support of the above assumption, "...during the past months, the thought has been occurring to me that the real differences in treatments have often hinged on affective variables (e.g. motivation) and, therefore, we ought to put our research effort there. To your view that most research on teaching the retarded ignores 'processes,' I can only say, amen!"

curriculum goals are, generally, subordinate to ones pertaining to interpersonal relationships. Emphasis is not primarily on achievement, and methods of teaching are not generally considered to be of greatest importance and are, in fact, deemphasized. This is another way of saying that the independent variables which should be given most attention in such settings--teachers, children, and their interactions--have not been subjected to careful measurement and control.

There are several technical reasons for experimenting with only one or two fairly discrete variables at a time; on the other hand, there are as many reasons for analyzing the complex interactions of children in natural settings (Shulman, 1970, p. 383). In the latter case, classroom situations can be manipulated in order to provide the observer with limited structure in a natural setting. Data obtained could then be used to compare programs and curricula for children, in order to enhance the possibility of favorable behavioral changes, which will depend upon prior maximization of the principal sources of variance--teachers and children. Through such study, we begin to confront the following questions: What anomalous behaviors are displayed by the children and how are they connected to the evolving class atmosphere? What are the specific effects of various procedures upon individual and group behaviors?

To continue this line of reasoning, much attention has been given to the proposition that the teacher-child relationship is critical to the teaching process, suggesting the importance of not only the "how" of teaching, but the relationship that develops between the teacher, on the one hand, and both individual children and the total group, on the other (Rosenthal & Jacobson, 1966). An example of this phenomenon is the "Hawthorne Effect," one that

persistently appears in psychological and educational experiments and which seems to be more consistently related to improved performance than any particular method or curriculum. Stated another way, the excitement generated by a research project (i.e. the "Hawthorne Effect") is an experimental side effect that appears to have more research significance than so-called main effects. Therefore, one assumption the researcher might consider is that something like the "Hawthorne Effect" is necessary to the development of a significant interaction. Yet another way of stating this is to specifically design "Hawthorne Effects" as sources of independent variation in research on teaching.

Although we believe that something akin to the "Hawthorne" is necessary, we also believe that, in itself, such an effect is not sufficient. There are other questions to be answered. How do children spend their time in classrooms? Do they attend to what is going on? How is their attention monitored? How are they dealt with when they succeed and when they fail? What kinds of questions do they ask? What kind of questions are asked of them? Questions such as these--and a good many others--must be studied and answered if we are to learn more about behavior and how it can be affected. Yet, rarely do we pose such questions; rarely do we judge a teacher's effectiveness, for example, by other than an estimate of her acquisition of knowledge concerning her "subject" or her "teaching".

In spite of the aforementioned neglect and ignorance, there is sufficient evidence to reconsider this particular pervasive focus on teaching. To begin, variables in the usual educational situation are of such a nature as to discourage the rigorous experimentalist from dealing with them. The classroom situation is antithetical to an experiment that demands rigid

application of certain a priori determined conditions. Personalities of teachers and children, social interactions, and creative processes are examples of difficult-to-measure factors that must be dealt with if we are to do more than produce sterile descriptions of curricula. As stated earlier, since these factors cannot be measured easily, or perhaps not at all with presently available techniques, they are not usually included in the design of an experimental study.[5] For purposes of clarification, we may discuss these factors in terms of the "process" and the "substance" of classroom life.

"Process" refers to the way in which relationships are initiated, develop, and endure among individuals, and the extent to which they exist. "Substance" is concerned with that well defined content of relationships which can be tested formally. In studies of children in school, "substance" has received considerably more attention than "process". Thus, in terms of what is here called "substance," an extensive body of literature provides hypothetical and empirical constructs that describe how children differ from one another and how individual children's test scores change. However, the literature is not at all clear on how to produce changes most efficiently, especially in dealing with children who have cognitive or other disorders. In terms of the present discussion, "process" has received less attention because it is less amenable to study. This is to say that the measurement of children's abilities ("substance") is less difficult than the measurement of their social interactions and motivations. It is understandable that psychologists and educators have concentrated on variables that are relatively

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[5] In recent years, the work of Amidon, Bales, Flanders, and Medley & Mitzel, among others, have developed interesting and potentially illuminating observational systems. However, with very rare exceptions, these newer observational approaches have been noticeably excluded from the design of research dealing with teaching the handicapped. What such exclusion suggests is impossible for us to determine.

easy to measure, even though such variables may be of trivial importance to learning.

For example, an intelligence quotient is a good predictor of academic success. However, academic success is a function of both "substance" and "process" variables. The latter, being difficult to measure, are more or less ignored. Why then is IQ such a good predictor of academic success if it measures essentially the "substance" and not the "process"? It is probable that "process" variables affect IQ in the same way that they affect academic, success, and the predictive efficiency of the IQ is, to a greater or lesser extent, due to indirect measurement of the "process". Therefore, it is important for those engaged in research on teaching to explore not only the components of the IQ but those of academic success as well. Such exploration calls for intensive investigation of the total field of child behavior with minimum attention to conventional aptitudinal criteria and maximum attention to "processes". Although this is neither a new nor profound idea, it remains conspicuously absent from research and evaluation programs. Such a focus is clearly a reversal of what generally takes place in research on teaching.

In summary, the focus and rationale of this chapter suggest the development of research strategies that are in harmony with discovering and evaluating what actually occurs in classrooms. It is further concluded that such research assign, as bases for comparison, the variability that exists among and between interactions rather than among and between either teachers or children. Possibly, this orientation to research on teaching offers a solution to what Blackman (1969) described as the serious and ambivalent dichotomy between those so-called logical positivists who prefer experimentation as the method of proof and those who view education essentially as an art form, one which

would lose its color and vitality if the movement to fractionate the teacher-pupil interaction achieves its apparent goal.

II. Chapter Overview

During the past decade, more textbooks, monographs, research studies, and journal articles concerning the mentally retarded have been published than in all the previous history of Man's efforts to describe and understand this group of people. As in other fields, and in spite of valiant efforts by individuals and organizations to catalogue and retrieve information and to prepare bibliographies and reviews, it is impossible for even the most diligent scholar to "keep up" with all of the literature in this field. Fortunately, in recent years, a number of superior substantive reviews have been published. In the past, The Review of Educational Research regularly devoted one of its issues to "Exceptional Children" and the reader will want to examine the still timely analyses of Dunn and Capobianco (1959) and Blackman and Heintz (1966). In 1963, the Council for Exceptional Children published Kirk and Weiner's Behavioral Research on Exceptional Children. The chapters by Heber, on the educable retarded child, and Charney, on the trainable retarded child, present a valuable collection of abstracts that have recently been updated by the contribution of Spicker and Bartel in Johnson and Blank's (1968) Exceptional Children Research Review. Several other important reviews of research on teaching the mentally retarded should be noted for readers wishing to pursue this literature beyond this chapter's limits, dictated by the ever-present compromise between space allocations and chapter focus: Guskin and Spicker (1968), Kirk (1964), McCarthy and Sheerenberger (1966), and Quay (1963). Lastly, among the many related books that have been

published in recent years, the following are particularly noteworthy in that each presents comprehensive reviews of literature that, in direct and tangential ways, are relevant to our central concern. Ellis (1966a, 1966b, 1968), Jordan (1966), Phillips and Easer (1966), Robinson and Robinson (1965), Sarason and Doris (1968), and Schiefelbusch, Copeland, and Smith (1967).

The review of literature to be presented in this chapter will not attempt to duplicate, or even elaborate upon, the aforementioned reviews. Rather, we will deal briefly with only recent literature pertaining to teaching the mentally retarded and, beyond that, discuss the general research in this area in terms of our hypotheses relative to the study of teaching and our theoretical formulations that have obtained from both the evaluation of prior work and our own research experiences.

The remainder of this chapter will be concerned with, first, the continuance and elaboration of our earlier discussion of research on teaching. Secondly, a selected critical review of the most recent relevant research has been divided into three sections: studies concerning variations in home and community settings, studies concerning variations in educational atmosphere, and studies dealing with variations in children and teachers. Lastly, the chapter will conclude with a discussion of the nature of research on teaching, the importance of hypothesis-generating studies, and possibilities for the development of new scientific traditions that may enable field researchers to deal with heretofore insuperable problems in the study of teaching and its effects.

Research on Teaching

I. A Polemic

Nearly all research on teaching-learning is plagued by a paradox: on the one hand there is the need to support generalizations about teachers, children and methodologies; and, on the other hand, lies the problem of individualization, i.e. which children work best with which teachers and under what methodological conditions (Vale & Vale, 1969). The need for generalizations produces research which attempts to structure supposedly categorical uniformity over qualitatively different inputs. Independent variation is assumed to exist *a priori* as is the case with comparative studies of methodologies, curricula, or teacher styles. Such propositions set forth the premise that, given discrete independent variation of particular teacher, methodological, or curriculum variables, there will be measurable differences in output as inferred from either the later measurement of observed behavior or through the use of standardized or specially constructed tests which specifically measure independent variation that is a function of independent variables. This approach lends itself to the study of many different classes and teachers who may be assigned to points on a scale of independent variation. This assignment can be random or it can be *ex post facto* in terms of the given characteristics of a general environment, teacher, classroom, physical facility, curriculum, or chosen pedagogy.

A second major approach implicitly assumes that class variation is secondary to individual variation and that the primary research unit should be either an individual child or an individual child with a specific teacher or class. Without doubt this leads to a far more tedious research procedure

and does not lend itself to the random assignment of children to treatments. This molecular approach suggests that the search for what promotes difference must center on the longitudinal dynamic interaction between specific children, their teachers and their peers; further, it implies that between-class differences will not be as important as variations of children within particular classes.

This is not to say that there will never be uniform class (i.e. classrooms of children) differences but, rather, that such differences will be relatively rare since they would be dependent upon uniform application of specific kinds of subject matter and goals across groups of children of widely varying abilities, interests, values and motivations. The factor that is brought into bold relief when we study the education of mentally retarded children, as opposed to children in regular classes, is the impossibility of applying uniform academic goals. This reasoning follows from those factors leading to the placement of mentally retarded children in special classes, the structure and continuity of those classes and the powerful variations--vis-a-vis the handicapped--that exist between different school systems, classes and teachers.[6] The extent to which within-class variation is trivial

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[6]For example, an impressive literature fails to demonstrate the superiority of either special curricula, administrative organization or special methods in the educational treatment of the retarded (Blatt, 1958; Cain & Levine, 1963; Cassidy & Stanton, 1959; Goldstein, Moss & Jordan, 1965; Hottel, 1958; Kirk, 1958; Mullen & Itkin, 1961; and Wrightstone, Forlano, Lepkowski, Sontag & Edelstein, 1959). We speculate that these rather consistent findings are due, in part, to the nature of "special child" identification and, in part, to the pervasive effects of such identification, which together provide both extraordinary variation between and within each class as well as an equally extraordinary variation between those classes and so-called regular classes.

will depend upon the existence of powerful and uniform differences between classes, which is by way of saying that no matter what the differences are within classes they are not nearly as important as the communality that exists over groups of children in their abilities, goals, and acceptance of basic educational assumptions regarding why they are in school, what they hope to achieve, what rules they have to attend to, and their sources of gratification.

Our argument is not that there is a categorical difference between regular and special education with regard to critical sources of variation; rather, the fact that special education consists largely of children who are rejects from the regular system suggests that greater within-variation and less uniformity of behaviors and attitudes than generally encountered in typical academic situations will be found here. However, it should be added that gross models used to compare different teaching methods or curricula have failed as badly in studies of regular classes as they have in studies of special classes (Gage, 1963).

An illustration of this phenomenon (i.e. the effects and importance of within- and between-class differences) may be found in research related to home and community effects on learning. Where there is relatively little variation between homes and within a community with regard to academic progress, one can expect school inputs and processes to contribute strongly to output variance, and, it may not be necessary to be specially concerned with out-of-school variables. (However, this assumes that variation in academic behavior of children includes success and failure. There are schools where there is no important variation--virtually everyone succeeds or everyone fails.) On the other hand, where there is significant effective variation--i.e. effective in the sense that what takes place in the home and in the

community will alter school behavior significantly--it is necessary to consider out-of-school environmental factors seriously, to measure them carefully and, perhaps, to contribute to independent varation in them (i.e. to actively manipulate) in order to more adequately assess change.

It is our contention that special classes, in general, and special classes for the mentally retarded, in particular, are heavily loaded with effective sources of variation other than those pertaining to academic activities in the school. With regard to constitutional variation (including genetic factors) which is relatively constant within educationally relevant time periods, we must, at the present time, consider ourselves to be more or less ignorant and must, therefore, remain open-minded. The literature on stability and change in children from various social classes does not offer a solid foundation from which to theorize about educational programs. Consequently, our position about the potential and probability for change in children has to be derived from other than (or in addition to) a strictly experimentally designed empirical base. This has been incisively demonstrated in the debate that has taken place recently between Jensen (1969) and Deutsch (1969), Kagan (1969), and others.

If constitutional (physiological) variation is eliminated from a total input-process-output design for the study of teaching--and where the primary criteria for the appropriateness of input are based on the careful measurement and description of process rather than presumptions about capacity (IQ) or potential and, secondly, where we can assume the importance of community, home and non-academic school variables in the process of change--it becomes imperative to assess research on teaching the mentally retarded in terms of the aforementioned questions and assumptions.

II. Goodness of Fit

In numerous ways, individuals function differently. Research attempts to record these ways and explain the whys. For some researchers, description is an end in itself. However, the history of social science has, at least, one certainty about it; description always leads from and to something. There is no "unbiased description". For example, when several groups are given IQ tests, almost invariably they will have different means. Are these objectively derived differences? We believe not! A good deal went into the development of the IQ test, selection of items, and procedures for administering the test. The testing format is, itself, a very special structure for communication. Tests are validated in specific ways using specific criteria. They are developed to do something. The narrower that something is, the easier it is to validate the test; however, the test becomes more biased when used with other groups at other times.

We often talk about variability. What makes the greatest difference? It is heredity or environment? It is school or home? Latin or home economics? Discipline or therapy? If a child has a problem, what (or who) had most to do with it? What is the main, most significant, most pervasive cause? What is the best, very best, way of undoing the problem? Does the answer to the first question (cause) lead to the answer to the second (undoing)? Does what is wrong indicate what should be done?

Eventually the question is: What should we do? And, how do we obtain that answer? Does it depend on who does it, or where it is done, or how much time there is? It is wishful thinking to expect that there is a clear relationship between what exists, why it exists, and what to do about it. Useful reductions are impossible, at least in the usual sense. Perscriptive education is a reduction. Therapeutic education is a reduction. Montessori,

Frostig, Kephart, Cruickshank, Bereiter, A. S. Neill all offer reductions. They say this is what to do with children who present or behave in this manner. Whatever this is, there is the assumption that this can be identified, described and distinguished from something other than this.

What contributes to difference? Children are poor, come from families who have inadequate housing, food, medical services, space--are crowded into cities (or rurally separated)--and they do not do well in school! Or on tests! Or on the cello! Often, they are migrants, emigrants, or immigrants. And, they do not speak Standard English. They are different. They do not fit well.

A lot of confusion exists about what people should do, how they should do it, and when it should be done. Who are to judge? Are the judges' values my values? Or yours? How can it all be put together: poverty, delinquency, migration, retardation, language, values, disability, learning? Or, can't it? Is it psychology, sociology, anthropology, or epistemology? Some individuals in some groups do not fit. The first problem is to decide about fit: individuals who do not fit, groups that do not fit, and individuals who do not fit groups that do not fit.

There are several differences to being an individual who does not fit (I-no-fit) rather than in a group that does not fit (G-no-fit). Special education "rides" the I-no-fit local. Black power "rides" the G-no-fit express. The new field of learning disabilities has eptiomized the I-no-fit way (Blatt, 1969a). Find out what is wrong, then treat it! The patient subsequently will get better. Mental retardation has always been in the I-no-fit category, but it was a strategic error to assign the retarded to it. Either in special classes, institutions or at home many do not have the skills to make it on their own.

A G-no-fit means there is something wrong with the society, or the culture, or with the G--or everything. What do you want your child to become? Or yourself? Or Lee Harvey Oswald? But whatever it is, it has little to do with the child, with you, or Lee Harvey.

With any problem there are I-no-fit and G-no-fit alternatives. For example, we can examine juvenile delinquency. According to the I-no-fit strategy, the delinquent can be treated individually (or in groups) as a sick, ill-advised, or alienated person requiring rehabilitation, therapy, education, counseling or, possibly, vocational training. A G-no-fit policy leads to a dilemma. Do we categorically change G? Or the rest of society? Are delinquents to be understood and treated as a collection of individuals who have something superficially in common with each other? They all have done something illegal? Therefore, should we impose or prescribe a common treatment? Enter, G-no-fit analysis. It is absurd to talk about a thousand or ten thousand adolescents getting the same treatment. The G is at issue. But that either leads backwards--lock them all up, vengeance, punishment, retribution--or to an examination of who does not fit what, and when. Whoever and whatever does not fit has to apply to the total G. Whatever is to be done has to apply to the total G. How can we speak in these terms without descending to an absurd reductionism? In other words, if the G-justifying generality cannot apply to G, maybe there is a generality that can uniformly be applied to non-G. What is it that can be said about non-G that connects it to G--that forces G to be G-no-fit? What does non-G do, think, believe, feel, worship or deny that operates on G? This is not simply a question of prevention versus treatment. The kind of prevention or treatment will depend on which no-fit track is being used.

The learning disabilities movement has pushed for the identification of a particular kind of child--perceptually impaired--who is supposed to be different from mentally retarded or emotionally disturbed children. Each of these children is to receive individualized assessment and treatment. This appears to be a bastard no-fit strategy. But, in reality, it is not! It is I-no-fit "all the way". The G is supposedly identified but it is always quite clear that it is really I that does not fit and must be dealt with. Again, we ask what makes a difference? Are children with learning disabilities going to be any different if we view them as different from mentally retarded or emotionally disturbed children? Or is the real difference going to center around the goodness or badness of fit? To what extent do we change individuals, or at least try to change them, and to what extent do we change groups and structures? For example, programs that change the structure of services for mentally retarded children that go beyond the requirements of any given individual child or adult who has been designated as being mentally retarded are clearly G-no-fit programs. The greater the inclusiveness of the G--therefore, including diverse disability groupings--the more it leans in the direction of G-no-fit. The introduction of more refined diagnostic categories is a push in the I-no-fit direction. This is certainly justified, at times, by the special needs of some disabled individuals and some disability groups. For example, a special diet for a child who has been identified as being phenolketonuric is the appropriate I-no-fit strategy. However, in our view, this is a proper exception to, not regularity of, our philosophical and clinical orientation.

III. Curriculum and Learning

The preceding section leads to a primary concern, one whether any particular educational strategy--be it related to methodology, teacher, peer group or curriculum--"takes" in more or less the same way as an innoculation does or does not take. It is easy to establish whether an innoculation was administered, but there is considerable uncertainty in knowing whether or not it accomplished its purpose, i.e. before its effects can be verified by long term follow-up. Thus, in the case of our analogy, it is one thing to judge whether an innoculation has "taken" by examining the individual some time after it was given; it is another to analyze whether or not it "took", in terms of its effect. The latter circumstance involves questions about whether the innoculation influenced susceptibility or, on the other hand, whether the individual was susceptible but never in contact with the disease-producing germ.

Similar questions exist with regard to educational input, process, and output. The input can be there for various groups of children and it might or might not "take" depending upon personnel, timing, and method of application. If there is reasonable evidence that, in fact, it did "take", it still does not mean that it will affect output. For example, it might or might not generalize to other situations and materials. Or, appropriate situations and materials may not present themselves and, therefore, although the process originally "took", follow-up will offer no evidence of this.

Most research on teaching the mentally retarded (and, for that matter, most research on teaching) tends to concern itself with input and output phases but to ignore process. At best this can seriously decrease the power of a study and, at worst, it can destroy entirely the meaning of such research because of the "noise" that exists in a system that results in an error-ridden

process that often has an overwhelmingly negative effect on children for whom there is a misfit between their needs and the educational situation.

IV. Teachers and Teaching

The model used here assumes that research on teaching covers a finite period of time where certain individuals and groups are exposed either naturally, or through manipulation, to ordinary or extraordinary interventions, with measurements taken at various points during this period. Criteria for effectiveness can consist of a sequence of measures, a final measure, or a series of measures in the last phase of the period. Studies can concern themselves with any one or all of the following stages: input, process, and output. There are not meant to be mutually exclusive but, rather, useful for raising provocative research questions.

Input includes teacher, child, facility, methodological and curriculum variables that are given sources of independent variation and that may or may not be affected by the interventional process. Input variables may or may not be measurable, even though they can be conceptually described. They may or may not include non-school variables such as those concerned with home, community or other external conditions and processes which are operating upon children, teachers and schools at the time of the intervention. They necessarily include the choice of sampling unit to be studied, whether it be individual child, teacher-child diads, classroom, schools, or other units, either defined externally or in terms of input characteristics.

Process variables are concerned with what takes place during the intervention and the ways in which input variables are modified as a result of the intervention. They include the quantity and quality of verbal and social interactions, the ways in which materials and activities are used by children,

the ways children and teachers spend their time, and the interrelationships that exist between school and non-school activities. Process variables can be conceived both in terms of teaching and learning, or of what we might call the teacher-learning process. They can be the end product of a study--namely, to affect process by certain input--or they can be a means to producing stipulated goals.

Output is the effect(s) produced by a given intervention, with given inputs, and with either certain assumptions made or certain conditions ascertained about processes. Output can be measured with standardized or specially constructed tests, observational scales, or by measuring behaviors in subsequent extra-experimental situations. The strength of inferences about the relationship between input and output will depend upon the extent to which processes are identified, measured, and included in the data analysis.

Studies of the effects of educational interventions must be concerned with the extent to which observed behaviors are child-specific or situation-specific. Child-specific behaviors will be relatively unchanged by situational variation, whereas situationally specific behaviors will vary for any given child as he enters into different kinds of situations. Inputs that do not attend to situational variance will necessarily have marginal effects on children. But, it is unlikely that the differential effects of situations will be identified unless considerable attention is paid to such process measurement and input variation which permit attention to specific and systematic situational variation.

This is not to say that child-specific behaviors are accepted as being immutable but, rather, the existence of situational variations suggest strategies of teaching which attempt to recreate elements of other situations in

which desired behaviors are known to exist. If a child's behavior varies with different adults, and this information is critical for generating effective interventions, it is unwise to leave to chance the study of factors which are closely associated with, or cause, behavioral variation, particularly with children who have repeatedly demonstrated situational failures. To assume that all situational failures are, also, child failures is both dangerous and misleading. Similarly, an excessive preoccupation with child-specific behaviors, without careful recognition of their implications for teaching, can only reinforce the expectation that the total child is child-specific and that educational programs can be little more than holding operations which keep children occupied and, hopefully, happy.

Review of Research

As mentioned earlier in this chapter, this review has been arbitrarily divided into three sections. Further, it claims neither depth nor does it include all possible variables that deserve consideration. It is designed to augment more comprehensive reviews and, secondly, it is included to illustrate both the kinds of research programs currently receiving support and the status of the field with respect to the nature and correlates of teaching the mentally retarded. Lastly, because there have been several recent substantive reviews (e.g. Guskin & Spicker, 1968 and Spicker & Bartel, 1968), this section will be brief and will focus on subsequently published literature--our purpose being to provide a basis for discussing research trends, interests, and strategies.

I. Variations in Home and Community Settings

Review of recent literature indicates that little attention has been given to studying the effects of the home and community on learning ability and achievement. This is surprising, in view of enormous support to compensatory education and the documentation, during the last ten years, of a strong, persistent, and pervasive relationship between socio-economic class and educational achievement. The authors' own research with so-called "high risk" children (Blatt & Garfunkel, 1969) found, on secondary analysis, a significant correlation (.52) between family organization and family (sibs) school behavior. That finding is consistent with the Coleman report (1966), Hurley (1964), and unpublished follow-up data from our aforementioned study. With such modest exceptions as the few studies describing the effects of family counseling or community recreation programs (e.g. Pmphrey, Goodman, Kidd, & Peters, 1970), there appears to have been little research activity in this area. Further, there is an equal paucity of studies that seek to illuminate or modify the attitudes of community groups or individuals toward the handicapped. Although several studies did report parents' attitudes toward their mentally retarded children, only one recent study was located which attempted to assess general community beliefs (Meyers, Sitkei & Watts, 1966).

The dearth of research dealing specifically with variables of home and/or community--especially those studies that bear directly on social, emotional and cognitive aspects of school behavior--is particularly discouraging in view of what we had thought to have been deep interest in this area. Most related research, little as it has been, was concerned with intelligence as the critical, and usually as the only, independent variable. There has been a growing acceptance of the importance of home and social class factors, but

these are not taken very seriously. Witness the design of Coleman's survey on Equality of Educational Opportunity (1966) and of evaluations of compensatory education, including Head Start. It is not that variables from home and community are not used. They are usually present in most current research studies, but are visibly trivial. That is, they do not have particular meaning or importance or contribute very much to the researcher's understanding of the problems confronting him. Asking parents of Head Start children questions about how they feel towards their children, Head Start, and their community, does not deliver revealing data. It amounts to using a teaspoon to do the work of a steam shovel. Similarly, attention to socio-economic status does not, in itself, attend to the relationship between poverty and the ways that poor families or families with mentally retarded children deal with schools.

Our review of literature indicates either the general belief that the home and community have little influence on school-related development or---as is more probably the case---the belief that current experimental research capacities and techniques do not lend themselves to the adequate examination of that multitude of interrelated variables connected with families and communities. To be sure, experimental methodologies have not been as useful or productive as the so-called "soft" approaches of Edgerton (1967), Glasser and Strauss (1962), or the general model of participant observation as described by Bruyn (1966). However, there are other reasons--perhaps more important--why scant research attention has been given to home and community variables:

1. It is easier to use well established instruments, with known reliabilities, short administration time, and presumed conceptual clarity. As soon as one gets into other methodologies, it usually requires months of observation.

2. Apparently, there is a degree of satisfaction in doing relatively "clean" research, even if it may not have important meaning or relevancy.

3. Possibly, a covert factor is related to whatever biases researchers have against the concept of "change". To discover that others can and have changed means that the researcher could have changed. He could be somebody other than what he is. Expectations for change are tied up with the lives of the expectors as much as with those for whom they have greater and lesser expectations. Designs, variables, procedures, and analyses are certainly influenced by these expectations.

4. If retarded individuals (or any other group) are studied in environments that are maximally different from what they are used to (certainly not necessarily a special class), and criteria are selected that are tied up with that difference and, furthermore, if those criteria have not been operationalized to demonstrate reliability (short-term consistency) and stability (long-term consistency) as a major function, but rather have been intentionally constructed to get at change (even at the sacrifice of predictability), then we can expect to be able to document change. (See Blatt & Garfunkel, 1969). Most special classes do not radically alter children's lives and most homes do not change very much. But there are variations between homes and between communities that are probably much more compelling

than formal educational variation--including school, teacher, methodology and materials.

II. Variations in Educational Atmosphere

Our review confirms the continued popularity of so-called efficacy studies, curriculum studies, and evaluations of teaching methodologies. The abundance of research of this type is disconcerting, in light of frequent expressions in the literature relegating such research to positions of minor value with little possibility for shedding either new light on tired questions or generating new hypotheses for the study of heretofore puzzling problems. Kirk (1964) expressed the belief of many educational researchers with his comment that research on efficacy of special classes will yield little return in relation to the effort and resources required. Insofar as studies of special methodologies or curricula are concerned, the literature discloses the near universal failure to reject the null hypothesis, i.e. no difference between various experimental and control groups of children (Blatt, 1967).

What have we learned from these efficacy and methodology studies? Or, how may we interpret their relatively uniform findings? We have concluded that the accumulation of evidence leads to a clear rejection of even the legitimacy of the form and content of these two questions asked rhetorically. The special vs. regular class dichotomy is not a defensible independent variable. Although there may be powerful exceptions to this hypothesis, the regularity of data findings suggest strongly that children's experiences are not systematically different in a consistent way if they are in one or the other class. A child can have individual attention, warmth, support, friends and an exciting program in either class. Furthermore, his home can vary

independently of the kind of class he is in. For many children, the home contributes so potently to variance that it may well drown out any differences connected with educational programming.

The most recent efficacy studies are in the familiar tradition. Welch (1965) compared the effects of segregated and partially integrated school programs on self concept and achievement of educable retarded children. She found that those educable children who remain in a regular classroom one-half day were significantly less self-derogatory than those who were completely segregated, i.e. had no contact with typical youngsters while in school. Further, the partially integrated children improved significantly in reading in contrast with the academic achievement of the comparison group. Grounded along similar theoretical lines, Zito and Bardon (1969) investigated differences in achievement motivation between two groups of Negro educable adolescents, one group in a special education program and a second group in a regular school program. A third group, adolescent Negroes of typical intelligence in regular classes, comprised the remainder of the study sample. The results indicated that retarded adolescents were more influenced by success than failure and, further, that their achievement motivation was comparable to that of typical subjects from similar socio-economic backgrounds. Insofar as comparisons between special and regular class youngsters, the special class experience appears to have made these adolescents more cautious in setting goals and more likely to anticipate failure while the regular class children anticipated success and, in fact, showed greater achievement. In a study similar to Johnson's (1950) now-classic sociometric research on friend selection, rejection, and acceptance of mentally retarded children in public schools, Rucker, Howe and Snider (1969) administered a sociometric instrument

in 30 regular junior high school classes. The results of their investigation, designed to measure the social acceptance of the educable mentally retarded participating in both academic and non-academic regular classes, supported the conclusion that retarded children enrolled, at least half time, in regular junior high classes were less accepted than their non-retarded peers. Further, these children were equally rejected in non-academic and academic classes.

Other recently reported "efficacy" research has dealt with such matters as the effectiveness of cooperative programs between special education and rehabilitation departments (Bloom, 1967), off-campus work placement for the educable retarded (Howe, 1968), the effectiveness of special education on perceptual-motor performance (Krop & Smith, 1969), and integration vs. segregation as related to success expectation and achievement (Schwarz & Jens, 1969). Each of the above studies, although relatively well controlled, has added little more than new layers to the massive ambiguity surrounding such questions as they concern curriculum design, administrative organization, and the efficacy of special interventions or treatments.

Preschool studies are being reported with increasing frequency, due--at least in part--to the favorable conditions vis-a-vis federal and state support of both service programs and research in this area. Guskin and Spicker (1968), Spicker and Bartel (1968), and the present authors (1969) have all reviewed this rather impressive literature. Since the work of Skeels and his associates, to the most recent studies, several theoretical threads reappear and, if for no other reason than their consistency and frequency, may be noteworthy. There continues to be marked interest in the study of so-called "cultural-familial" mentally retarded children and their families. More broadly, there is a significant escalation of interest in studies concerning the correlates of social

class and intelligence. However, whereas during the first decades of this century "cultural-familial" cases were viewed as a specific etiological grouping of genetic origin, they tend now to be viewed as part of that much larger group labelled "culturally deprived" (Blatt & Garfunkel, 1969).[7] Insofar as genetic processes are concerned, the argument of Jensen (1969) and his adherents is by no means original. Even before Goddard's infamous "Kallikak" study, and through all of the decades to the present, there has been general agreement in the psychological and educational communities that genetic processes represent an important source of influence on the biological foundations of intelligence (see Blatt & Garfunkel, 1969, or Sarason & Doris, 1968, for discussions of this history). However, there has also been recognition, which is now increasing remarkably, that far too little is known about the nature of intelligence--except, perhaps, that it is vastly more complex than what is indicated by the IQ score--to justify anything more than the formulation of hypotheses and sheer speculations about the role played by multiple genetic factors (Blatt, 1970 and Bodmer & Cavalli-Sforza, 1970). As we have stated elsewhere (Blatt & Garfunkel, 1969), the nature-nurture controversies of the

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[7]Although the "technical" definition of cultural-familial mental retardation is stated somewhat differently (Heber, 1959, pp. 39-40), substantively it suggests at least five characteristics which have long been descriptive of these individuals: (1) by traditional methods of evaluation their intelligence is subnormal, (2) the intellectual level and social adequacy of at least one parent and one sibling appear also to be subnormal, (3) there is no discernible central nervous system pathology giving rise to the subnormality, (4) they were born into, and reared in, a cultural milieu which is "inferior" to other strata of our society, and (5) they represent a disproportionately large part of the case load of many social agencies.

past are being superceded by the realization that earlier positions (either nativist or environmental) were oversimplifications which served certain polemicists' personal opinions far better than they clarified the problem. This important shift in viewing the nature-nurture controversy as neither settled nor understood--together with the emergence of cultural deprivation as a major political, economic, social, and educational problem in our society--seem to have set the stage for systematic research and social action on environmental changes that might prevent intellectual deficits.[8]

To date, relatively few well controlled studies bear directly on the effects of planned intervention on the intellectual development of culturally deprived or "cultural-familial" mentally retarded children (see Sarason & Doris, 1968, for a perspective on this problem). The accumulated research in this area varies greatly in methodological sophistication and quantity of descriptive detail about sample selection, differences in contrasting environments and control of bias in collection of data. Although findings generally tend

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[8]For a full discussion of classification and terminological problems in mental retardation, tied so intimately to each shifting nature-nurture "fashion," see Blatt & Garfunkel (1969) and Heber (1959). Traditionally, mental retardation was defined as a constitutional condition of the central nervous system, existing from birth or early age, incurable, and irremediable, oftentimes resulting in the inability of the individual to profit from ordinary schooling. This traditional definition was joined to a classification system that utilized arbitrarily determined I.Q. scores to categorize levels of intellectual capacity; e.g. 25-50 I.Q. was in the "trainable" category; 50-75 I.Q. was in the "educable" category. More recently (Heber, 1959), a new, and widely used, definition and classification manual was developed by a committee of the American Association on Mental Deficiency. This new manual defined mental retardation as subaverage general intellectual functioning, originating during the developmental period and associated with impairment in adaptive behavior. This definition did not assume a constitutional condition as a necessary requirement for mental retardation (e.g. in "cultural-familial mental retardation," p. 39-40). It referred to function rather than, as is traditional, to capacity, and it did not preclude possibilities for prevention, cure, or amelioration of mental retardation and its associated consequenced.

to suggest that planned interventions have the predicted effect of increasing intelligence test scores, these studies have neither produced compelling data nor have they permitted us to draw other than the most cautious conclusions concerning the correlates of intelligence. The three most recent preschool studies, not previously discussed in the aforementioned reviews, have had little more success than their predecessors in contributing to either educational theory or practice. Using groups of disadvantaged children of average intelligence, Karnes, Hodgins, and Teska (1968) compared the effects of traditional and highly structured experimental preschools. Kodman (1970) observed the effects of a special enrichment program designed for Appalachian children and, the third study, conducted at the University of Washington's Experimental Education Unit, dealt with behavior modification procedures for Head Start children (Haring, Hayden & Nolen, 1969). All three studies reported significant changes in the predicted directions. However, each employed very small samples and, with the exception of Karnes and her associates, there was little attempt to deal with the bedevilling problems of internal validity. Of the three, Haring et al. was most encouraging, first because the investigators were able to meaningfully depart from the tradition of IQ change as the major dependent variable and, secondly, because their design permitted the systematic study of teacher-child interactions and the modifiability of behavior.

In spite of the educational community's current interest in programmed materials, text books and, further, in elaborate new "hardware" systems to promote pupil learning, only a handful of studies relating to the education of mentally retarded children have been reported in recent years that dealt with assessing the potentialities of these newer educational technologies. Of those reviewed, Blackman and Capobianco's (1965)--the most sophisticated

in terms of research design and conduct--reported disappointing results with a carefully developed teaching machine program in reading and arithmetic. Other studies by Bradley and Hundziak (1965), Rainey and Kelley (1967), and Miller and Miller (1968), reported greater possibilities with time telling programs, programmed textbooks, and a unique method for teaching word recognition and discrimination, respectively. However, both Bradley and Hundziak's research and the Millers' study of "symbol accentuation" should be considered exploratory, in view of both their small samples and limited research objectives.

Several other methodology studies are worth mentioning. Cawley and Goodman (1969) hypothesized that trained teachers, employing a well planned program, could effect significant improvement in the arithmetic problem solving of mentally retarded children. Utilizing two control and two experimental groups--three of these classes for the retarded and one a regular class--it was demonstrated that, when teachers were trained during a two week workshop, mentally retarded children improved significantly. Rouse (1965) found significant gain scores resulting from the involvement of educable mentally retarded children in a training program designed to enhance their productive thinking. However, Budoff, Meskin, and Kemler (1968) were unable to improve productive thinking scores in a general replication of Rouse's experiment. Working with 30 institutionalized retarded children, Bradley, Maurer, and Hundziak (1966) demonstrated the effectiveness of milieu therapy and language training in incrementing psycholinguistic functioning. In a study of the effects of group counseling on educable boys, Mann, Beaber, and Jacobson (1969) found that those who received counseling exhibited anxiety reduction and improved self-concept, deportment, and school grades. Lastly, Vergason (1966) compared the

effects of a traditional and an auto-instructional method on retention of sight vocabulary. Although there were no differences between groups after one day, superior retention for words learned by the automated self-instructional procedure was found during several follow-up periods.

Elsewhere, we have reviewed and discussed an almost endless sea of studies relating to physical performance and capacity of the retarded child (Blatt, 1956, 1958, 1969b). For good and sufficient reason, few of the traditional strength, motor ability, and physical ability studies are currently being reported. Replacing the physical fitness comparison and survey research of three and four decades ago is a renewed interest in perceptual-motor training and performance. Certainly, this interest is a reflection of a major educational movement--learning disabilities--which is now, literally, sweeping the country and, obviously, has broad and important implications for the field of Mental Retardation (Blatt, 1969a). Kahn and Burdett (1967) found that, by utilizing practice and reward schedules, mentally retarded adolescents improved significantly in motor skills. Employing specially designed training programs, both Lillie (1968) and Ross (1969) reported similar results, i. e. with training, mentally retarded children improved in motor proficiency. Edgar, Ball, McIntyre, and Shotwell (1969) reported gains in adaptive behavior after a program of sensory-motor training with a small group of organically impaired retarded children. However, Alley (1968) was unable to demonstrate significant effects resulting from a systematic perceptual-motor training program. Lastly, both Corder (1966) and Solomon and Pangle (1967) found that physical education programs significantly influenced the development of retarded children. However, most of these studies suffer from one or more serious design problems: samples that are too small or ambiguous, very short term treatments,

and dependent variables that seem unrelated to the experimental treatment (e.g. Corder, 1966, with an experimental sample of 8 boys, designed a 20 day program of physical education, using the WISC as a dependent variable).

Since the theoretical work of B. F. Skinner in the 50's, the field of behavior analysis and modification has gained increasing attention and importance. A perusal of the literature in mental retardation generously testifies to the prominence and influence the operant conditioning movement has had in this field. Although much of the work reported emanates from the laboratory, an increasing literature, anchored in the field, can now be regularly found in journals dealing with the education and treatment of the mentally retarded. Much of this literature is concerned with the severely retarded and the modification of such self-help skills as toileting, dressing, and eating. A number of other studies have been successful in extinguishing destructiveness, aggression, and self-abuse. The following reports are examples of behavior studies that have succeeded in modifying the performance of mildly and severely retarded children--some institutionalized and others in the community: Bensberg, Colwell and Cassell (1965); Broden, Hall, Dunlap, and Clark (1970), Doubros (1966); Karen and Maxwell (1967); McKenzie, Clark, Wolk, Kothera, and Benson (1968); and Siegel, Forman, and Williams (1967). Undoubtedly, a great deal more can be said concerning the influence of this movement on the development of theory and practice in the field. There appears to be almost no possibility for other than increased activity in this area and prominence and support for its advocates, in spite of shortcomings and limitations inherent in the concept of behavior modification and, secondly, increasing misuse of this potentially important area by its unsophisticated advocates (Macmillan & Forness, 1970).

III. Variations in Children and Teachers

The preponderance of research dealing with the learning characteristics and behavior of mentally retarded children originates in the laboratory and emanates from the experimental tradition. Experimenters continue to be interested in the laboratory examination of: paired-associate learning (Baumeister, Hawkins, & Davis, 1966, Hawker & Keilman, 1969; Milgram & Riedl, 1969; and Ring, 1965); short-term recall (Baumeister, Hawkins & Holland, 1967; and Gallagher, 1969), discrimination learning (Riese & Lobb, 1967); curiosity behavior (Morgan, 1969); learning transfer (Gerjuoy & Alvarez, 1969); and attention (Follini, Sitkowski, & Stayton, 1969).

The contrast between the great number of basic research studies and the scarcity of field or applied studies is remarkable. Except for the organizational efficacy and methodology studies, there is almost no recent research to report in the latter area. Lovell and Bradbury (1967) observed the learning of English morphology in educable retarded children. Huber (1965) studied the relationship of anxiety to the academic work of retarded institutionalized children. Levine, Elzey, and Paulson (1966); Laing and Chazen (1966); and Jacobs and Pierce (1968) reported on the social status of retarded children in various in-school or school-excluded settings. Lastly, a number of personality-type studies--reminiscent of the familiar comparison and status reports of the 30's and 40's--have appeared from time to time during recent years, neither adding to our knowledge nor worthy of further discussion here.

We found but four studies dealing with teachers, their prestige, turnover, and characteristics: Knox (1968); Meisgeier (1965), Sharples and Thomas (1969); and Sparks and Younie (1969). Finally, we found but two studies (Jones, Marcotte, & Markham, 1968 and Strauch, 1970) that dealt with the attitudes typical children have toward the retarded.

DISCUSSION

During the 1969-70 academic year, the Council for Exceptional Children asked both authors to participate in a unique experience involving the organization of what they termed an "invisible college." Due to limitations of time and resources, and because the Council needed some fairly reliable data concerning the kind of research that is currently being conducted and who is doing it, a core of key researchers in special education were interviewed by telephone to ascertain their opinions concerning current research efforts, issues, and controversies. Eventually, the consensus on several topics are to form the base for convening the "invisible college." A total of 55 telephone interviews were conducted, the interviewers asking each participant to:

1. Identify projects they found interesting and significant;
2. Describe their own work;
3. Identify the "hottest" controversy in the field;
4. Identify technical or methodological problems delaying research efforts;
5. Name the creative mavericks.

In the general field called "Special Education," the categories of behavior modification, early childhood, strategies in special education, curriculum development in mental retardation and innovations in personnel training were the most frequently cited. Pupil characteristics, methods and materials, and speech, language, and communication disorders were mentioned with lesser, but impressive, frequency. Although the above survey assessed research interest in a much broader area than ours, "mental retardation," these findings accurately reflect how we would respond to such questions as they might deal specifically with the field of mental retardation. Our brief critical

literature survey revealed the great, and increasingly influential, position no enjoyed by those engaged in behavior modification research. When--for the purposes of categorization--reinforcement, applied behavior change, and classical conditioning studies are grouped together, they probably constitute the greates percentage of articles on mental retardation to be found in current major journals. Further, although the Council for Exceptional Children has a somewhat different constituency and mission from the American Association on Mental Deficiency or other organizations focused specifically on mental retardation, literature reviews in our field would, undoubtedly, disclose the majority of basic studies concerned with: verbal learning, discrimination, reinforcement and applied behavior change, and--to a lesser extent--generalization and motor learning. (See Gardner, Solomowitz & Saposknek's paper "Trends in Learning Research with the Mentally Retarded," unpublished but reproduced in the Council for Exceptional Children Planning Report for Information Analysis Products, 1969).

Our literature survey, and the results of the Council's telephone study, have both indicated that the preponderance of published research in mental retardation is experimental. Most studies of teaching have used traditional designs, whether they were efficacy studies, follow-up studies of children in special and regular classes, studies of different reading approaches, or studies of different curriculum approaches. Although these kinds of studies are more amenable to design modifications which may account for diadic variation, we believe that there are more appropriate ways to study teaching-learning in classroom or tutorial situations. Guskin and Spicker (1968) commented upon what, to us, is the most important lesson we could learn about the effects of our current style of research with the handicapped, i. e. our resea

has contributed almost nothing of value for the educational practitioner and (we may add) for the educational theoretician. It is well known that researchers, especially doctoral students, engage not in what they want to do but what they are able to do, not in what is important but what is possible, not in what is risky but what is safe and gives assurance of completion. People do what can be supported and most of us engage ourselves in activities that are comfortable and appreciated by others. Possibly, the most accurate judgment we can make about the research in mental retardation now being published is that this is what the people in the field want or, possibly, there is not anything else known that they can or wish to substitute for their current mode of activity.

We conclude that:

1. There is nothing inherent in mental retardation--or in any disability--to produce handicap. Further, it is not the primary responsibility of the behavioral sciences to determine the validity of the aforementioned statement, but to make it valid. We have supported far too many studies purporting to demonstrate differences between groups or the disorders of one type of child in contrast with another. All these years, we should have promoted and encouraged research that sought to make it come true that a child would learn after participation in a special program or curriculum. To state this another way, we are less than enthusiastic about the possibility that "all or nothing"--either we find something (significant) or we find nothing--research has anything to offer, to our understanding of

the handicapped or to pragmatic solutions to their learning problems. As an aside--we believe an important aside--in such "all or nothing" studies, one can see important and, perhaps, insidious relationships between the needs of research design and programming. That is, it is certainly seductive to randomly assign groups of children to treatments in order to see whether those treatments are effective, disregarding questions concerning the desirable way to develop educational programs for children.

2. The above leads directly to our second recommendation, viz., the study of particular methods, for the purpose of demonstrating their efficacy, is rather fruitless and whatever is demonstrated will eventually be contradicted by subsequent research. Such "all or nothing" studies of methodologies prove little. By "all or nothing," we mean studies that compare the efficacy of one method with that of another or compare the superiority of one type of individual with that of another.[9] As methods do not exist outside of a psychoeducational setting, and as they are implemented by unique groups of human beings, only a naive researcher could conclude that the demonstrated superiority of his method has

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[9]Or, as Campbell and Stanley (1963) incisively concluded,"...we must increase our time perspective, and recognize that continuous, multiple experimentation is more typical of science than once-and-for-all definitive experiments... we should not expect that 'crucial experiments' which pit opposing theories will be likely to have clear-cut outcomes." (p.3)

direct and specific transferability to other educational settings.[10] Our research preference is to study children, and how they change, in different educational environments. We believe it is more defensible, and will make a greater difference, to generalize about children interacting with each other and with adults in situations than it is to generalize about procedures. It is from evaluations of varieties of methods, with varieties of children in more or less formal and informal settings, utilizing teachers with heterogeneous backgrounds, that hypotheses will be generated that will lead to viable theories concerning human development and learning. It appears to us that, in this kind of strategy, theory construction shifts from methodological concerns to those involving human interactions.

3. Every researcher is confronted with a decision concerning the number of variables and sample size to be studied. Consequently, in light of limited resources, manpower, and time, to the degree that the researcher does not restrict

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[10] On the other hand, we are not ready to suggest that there is nothing but uniqueness in an educational setting. There must be possibilities for building generalizations for, if "knowledge" is an objective, we must be concerned with the degrees of non-uniqueness. Unfortunately, as we stated above, the numerous dimensions of child-teacher interactions have been neglected and, consequently, hardly understood.

variables of the study, he will have to restrict his sample, or vice versa. Our recommendation is to restrict sample size rather than number of variables. In studying the complex problems of the handicapped, on the one hand, and teaching them on the other hand, the restriction of variables to be studied and accounted for may lead to a distorted impression of results that either mislead the researcher or tell him very little about that which he has so diligently attempted to investigate. Therefore, although it is desirable to use as large, unbiased, and representative a sample as possible--especially if one is interested in the generalizability that a study may provide--in respect to the aforementioned realities and compromises that must be made, we cannot help but recommend that the research payoff will be greater if compromises are made with sample size rather than number of variables.

4. Leading from the above discussion is our recommendation that a great deal more work is needed before we truly comprehend the varieties and nature of educational settings for the mentally retarded. Education and psychology are now just beginning to appreciate the dictum that, before the researcher attempts to manipulate variables, he should describe the natural setting. What are so desperately needed today are studies describing how and under what conditions handicapped children are admitted to school programs, how and under what conditions they perform in such programs, their attitudes and the attitudes

of their instructors, and the interactive effects of such programs on those children, their families, and other involved children.

5. Finally, the enormous current interest in specialized educational strategies--e.g., Montessori, Bereiter, Special Classes, Head Start, token reinforcement, compensatory education, operant conditioning, and various learning disabilities programs--is testimony to the wide acceptance of a view of learning that places high value on teachers and learners rather than on teaching and learning. All of those strategies are attractive, in part because they are self-contained and can be discussed, described and set up as independent variables. Similarly, single dimensional teacher differences as a factor in differential learning places us in a comparable trap. Such distinctions as structured versus non-structured, directive versus permissive, child-centered versus teacher-centered, do not appear to make much of a difference other than that which is specifically tied to the behaviors under consideration. It would appear that other factors in teaching and learning are more important, that they cannot be simply described by the aforementioned methodologies or style labels, and that they are best studied by looking at differential process.

In this chapter, the authors presented the position that before we can adequately measure and understand quantitative differences in children and their teachers, we will first have to deal with and understand qualitative

differences and processes. Our goal as educational researchers is to examine the components of the teaching-learning interaction. We conclude that, to accomplish this goal, individual components cannot be amputated; that is, as we amputate, we both change the natural setting and destroy much of any understanding we might have gained from a more holistic view. To extend this analogy further, the surgeon might more easily examine and operate on the brain if it could be removed from the skull. However, notwithstanding modern medicine and its miracle workers, that trick is not yet possible. We, in the behavioral areas, seem not to believe that the variables we study and minipulate are more complex and less well understood than the surgeon's.