

## Teacher Effectiveness and the Limits of Psychological Explanation

*Mention teacher effectiveness to a layman nowadays and you will probably hear in reply a snort — of shall we say discontent? Here we are at the latter end of a century characterized by widespread, earnest, academic routine research in education, and the payoff from it all in applied schooling commands little respect, either in the public at large or among teachers themselves, or with other academics. Sanders reviews the recent history of direct research assaults on teacher effectiveness, rejects the general tendency to blame its failures on methodology, and calls into question the underlying assumptions of such research as to rationale and paradigm. The history itself seems to confirm that psychology's business is with what goes on in a student, what he does when he learns; what a teacher does when he teaches is a matter of what goes on in a social structure. The implication is that a different research approach is called for, and a different discipline. The paper asks a question for which other papers in this issue may have an answer.*

The sociologist C. Wright Mills charged that a certain style of empirical social science research, which he dubbed “abstracted empiricism,” was prone to a categorical error known as psychologism.

‘Psychologism’ refers to the attempt to explain social phenomena in terms of the facts and theories about the make-up of individuals. Historically, as a doctrine, it rests upon an explicit metaphysical denial of the reality of social structure.

... psychologism rests upon the idea that if we study a series of individuals and their milieux, the results of our studies in some way can be added up to knowledge of social structure.<sup>1</sup>

Recently, Phillip Schlechty has argued that the empirical research in education similarly “... places an undue reliance on the study of the psychological determinants of behavior.”<sup>2</sup> More particularly, Schlechty contends that “one of the fundamental difficulties with existing modes of thought about teaching is the insistence that the appropriate units of analysis are the behavior of in-

dividuals and idiosyncratic psychological change.”<sup>3</sup> The present paper examines the relevance and implications of this generic epistemological criticism for that troubled sub-species of educational research known as teacher effectiveness research.

As Mills saw it, one of the inherent consequences of psychologism was “thinness of results.”<sup>4</sup> And never, certainly, have results been any thinner than those of teacher effectiveness research.

A review of the teacher effectiveness investigations during the first three-quarters of this century reveals a woeful record of unfulfilled hopes and unrejected null hypotheses.

The hundreds and hundreds of studies designed to isolate a defensible index of instructional skill have, almost without exceptions, failed to yield the anticipated dividends.<sup>5</sup>

Even the staunchest advocates of teacher effectiveness research have admitted that “in the large, these studies have yielded disappointing results: correlations that are insignificant, inconsistent from one study to the next, and usually lacking in psychological and educational meaning.”<sup>6</sup> “Although,” as Doyle observes, “optimism is more apparent in recent writings (Flanders & Simon, 1969; Gage, 1977; Good, Biddle & Brophy, 1975; Rosenshine, 1976a, 1976b), a general perception of low productivity would seem to prevail.”<sup>7</sup>

### **It must be the methodology**

Apart from Mills’ “thinness of results,” the additional circumstantial evidence that teacher effectiveness research is prone to be overly psychological or ‘psychologistic’ in its basic epistemological stance is the simple fact that most of this research has been designed and executed by psychologists. “Whether for good or for ill, the main burden of instructional research has been carried by psychologists.”<sup>8</sup> This circumstance is probably not just happenstance. For one thing, the fundamental question that legitimates this research, namely, “What kinds of teacher classroom behaviour promote superior student learning?”, seems to ask an exclusively psychological question by trading upon, as it does, those two super-concepts of modern psychology, Behaviour and Learning. Furthermore, the question seems to pose a straightforward, empirical question that is virtually devoid of any misleading theoretical preconceptions. That is, the question asks for little more than a patient search for those observable teacher behaviours that are functionally related to (that is, correlated with) observed increases in student achievement.

As a consequence, perhaps, of this apparent conceptual-theoretical austerity of teacher effectiveness research, its advocates have commonly assumed that its meagre productivity is in the main the result of methodological or statistical problems endemic to empirical research. David Berliner, for example, has claimed that the chief impediments to the study of teacher effectiveness are

“problems of instrumentation, methodology and statistics.”<sup>9</sup> More recently, Jere Brophy, in his assessment of current trends and future prospects, focuses almost entirely upon the methodological issues confronting the study of teacher behaviour and its effects.<sup>10</sup> Brophy does acknowledge that “most of this research is heavily empirical, guided by no systematic theory and, in fact, very little theory at all.”<sup>11</sup> He does not, however, consider this to be a shortcoming of this research — quite the contrary. He instead defends the heavy empiricism as the best means of accumulating data uncontaminated by “the premature and overly abstract and grandiose theories that have plagued the field to date.”<sup>12</sup> (Parenthetically I must interject: whatever problems teacher effectiveness research has had, abstract, grandiose theorizing has certainly *not* been one of them!)

The belief that the difficulties of teacher effectiveness research are rooted in methodology is intrinsically optimistic. Methodological difficulties imply a class of problems or puzzles that one can reasonably expect, or at least hope, to solve with persistent effort and ingenuity. This explains why, as Fenstermacher points out, “criticisms of research design characteristics or data analysis techniques are more likely to be attended to than criticisms that expose deeply rooted assumptions or unanticipated ethical implications.”<sup>13</sup> Technical defects are usually repairable. Thus, the shared conviction that the inadequacies of teacher effectiveness research are largely if not wholly methodological has no doubt given proponents the self-sustaining optimism they have needed to confront the discouraging empirical returns from this research.

This prevailing characterization of teacher effectiveness research as an essentially atheoretical, empirical, fact-gathering enterprise beset with methodological defects may also partially account for its advocates’ occasional impatience with their critics. To harp on the “woeful record” (as Popham has called it) is to risk being dismissed as a pessimist spreading “lugubrious views” and “dismal generalizations”<sup>14</sup> or, still worse, as an opportunistic member of the “The Cult of Criticism” trying to exploit “a popular formula for writing best-selling books about education.”<sup>15</sup> The clear *ad hominem* tone of these rejoinders reflects perhaps just how committed these investigators are to the belief that the problems of effectiveness research are at bottom methodological. It is as if they look upon the persistently null findings as constituting a stern, projective test of empirical outlook and commitment. Those critics who wish to dwell upon the “woeful record” simply betray their constitutional pessimism. They are merely predisposed to “see” effectiveness research, like the ambiguous container, as half empty when it is equally obvious that it is half full. And given this perceptual stalemate, one might as well get on with the patient search for the determinants of teacher effectiveness and hope for the best.

### **Whatever happened to teacher personality?**

While teacher effectiveness research has never succumbed to extravagant theorizing, it has been influenced and organized by a persuasive descriptive con-

ceptualization: the so-called “presage-context-process-product” paradigm first suggested by Mitzel.<sup>16</sup> In a general way, this paradigm has defined the psychological research on teacher effectiveness as the search for stable, empirical relationships among antecedent teacher characteristics (presage variables), intermediate classroom behaviours (process variables) and consequent pupil outcomes (product variables). The paradigm also acknowledges that the generality of any of these empirical relationships may be qualified by certain immediate, situation-specific circumstances — what Mitzel apparently called “Type 2 Variables, Contingency Factors,”<sup>17</sup> but what are now called “context variables.” Context variables have received much less attention from investigators than the other three categories of variables.<sup>18</sup>

In Mitzel’s original article, his three-way classification of ‘presage,’ ‘process,’ and ‘product’ denoted *criteria* of teacher effectiveness.<sup>19</sup> Only later did these terms come to refer to variables. This may, of course, be nothing more than a trivial shift in terminology. The original language, however, does suggest an earlier recognition of plural criteria and, by implication, of alternative and competing *concepts* of teacher effectiveness. Such competing conceptions have not emerged. The various reviews and retrospectives on this research have been unanimous in condemning presage criteria as both a false start and a dead end. “The presage conception of teacher effectiveness was the obviously mistaken view that effectiveness could be a consequence of certain personality traits or characteristics possessed by the teacher, and research was geared to identifying those traits.”<sup>20</sup> The death knell for this view was sounded by the now famous Getzels and Jackson review of teacher personality in the *First Handbook of Research on Teaching*, which concluded that “despite . . . a half century of prodigious research effort, very little is known for certain about the relation between teacher personality and teaching effectiveness.”<sup>21</sup> In stark opposition to the conventional wisdom, teacher personality seemed to be unrelated to teaching effectiveness.

It is important, however, to remind ourselves in what sense presage variables such as teacher personality, background characteristics, training, and so forth are unrelated to teaching effectiveness. What “unrelated” means in this context is that presage criteria or variables do not predict or correlate with pupil outcomes, that is, product criteria. This contextual meaning should also remind us of the singular, “dog-wagging” influence pupil outcomes, especially tested effectiveness research has in recent years become nearly coextensive with so-called “process-product” or “process-outcome” research.

It should be made plain, however, that the failure of presage variables to predict (or correlate substantially with) pupil outcomes is hardly unique to these variables — as the “woeful record” clearly shows. Thus, the grounds for downgrading the importance or relevance of presage variables in teacher effectiveness research cannot entirely be their lack of empirical support. The additional grounds are that such variables are viewed as lacking conceptual

plausibility in the psychological model of teaching. That is, a teacher's personality or background characteristics are necessarily *causally remote* from the chief criterion of teacher effectiveness — pupil outcomes. Teacher traits and predispositions can only influence pupil outcomes *indirectly* through the medium of the teacher's classroom behaviour, that is, via process variables. Thus, the presage conception of teacher effectiveness was discarded as the psychological equivalent of "action-at-a-distance." Partly as a consequence, effectiveness research has in recent years become nearly coextensive with so-called "process-product" or "process-outcome" research.

### Well, a teacher's behaviour then?

The more or less explicit rationale that underlies process-outcome research is "the idea that teacher behaviour is the 'cause' and student learning is the 'effect' (which) is the basic model out of which much educational research is done."<sup>22</sup> This idea, in fact, is so ingrained in teacher effectiveness research that the empirical process-outcome correlations are construed as all but equivalent to cause-and-effect relations. It is not that researchers do not recognize the distinction between correlation and causality, but rather that the distinction tends to be seen as a logical nicety that could perhaps be waived in this research context. Consider, for example, Brophy and Evertson's cautious, yet confident prediction:

Bear in mind that all the data presented here are *correlational*. That is, they show that a teacher characteristic is associated, either linearly or non-linearly, with student learning gains. In most cases, follow up experimental research probably will reveal that the relationship is *causal*, that the teacher characteristics cause relatively poor or good learning gains.<sup>23</sup>

In addition, the process-outcome research has promoted a rather uncomplicated, one-way view of instructional causality. As Doyle observes,

Greater teacher enthusiasm or more complex questions, for instance, are implicitly thought to cause increased student achievement, even though the opposite interpretation — that teachers ask complex questions and are more enthusiastic with higher achieving students — is equally permissible from the type of evidence available in most studies. Isolating teacher variables would seem, however, to oversimplify the picture of causality in classrooms.<sup>24</sup>

The most recent conceptual amendment to the psychological model of teacher effectiveness has been the positing of an intervening variable between teacher behaviour (process) and pupil achievement (outcome). As David Berliner explains,

This complex variable is called Academic Learning Time (ALT). Although the relationship is probably not linear, the accrual of ALT is expected to be a strong correlate of achievement.<sup>25</sup>

Besides academic learning time, there have been several other characterizations of this intervening variable, such as “opportunity to learn,” “engaged time,” and “time on task.” But their common conceptual significance is that they represent a category of *pupil* behaviour that is *antecedent* or *causally prior* to tested achievement. Or to put it another way, such variables emphasize the point that individuals mediate their own learning. The introduction of these intervening variables underscores Schlechty’s earlier insistence that “only indirectly can teaching be conceived of as a cause of learning. The cause, if one chooses to use the term, is more appropriately located within the learner himself.”<sup>26</sup>

The additional significance of these proxy measures of pupil mediating responses, such as ALT, is that they seem to mark a final stage in what has been a steady contraction of the psychological model of teacher effectiveness. The model initially took as its explicit locus of explanation the teacher’s personality and background characteristics: predisposing psychological structures. Attention then shifted to the teacher’s literal, observable classroom behaviours, and now finally to the pupil’s mediating or, as it were, self-instructional behaviours. An argument, I think, could be made that this new, revised psychological teaching model reduces more or less to old psychological learning theory. Indeed, considering the explanatory significance assigned to pupils’ mediating responses in this model, Smith has argued that educational psychologists should now re-order their research agenda:

I would suggest that research on learning is even more critical and should have higher priority . . . Research on learning processes of pupils should be considered as an alternative to research on teacher effectiveness.<sup>27</sup>

Given that standardized measures of academic achievement have stood as the empirical *sine qua non* of teacher effectiveness in this tradition, the current enthusiasm for academic learning time as “a strong positive correlate of achievement” is understandable. But that there should be a strong positive correlation between academic learning time and academic achievement does not seem especially surprising. It seems little more than an obvious deduction from the more general empirical law, namely, that performance on a learning task increases as a function of learning trials. In short, such a correlation is but an instance of the axiomatic relation between practice and performance. Furthermore, this may explain why some of the recent “findings” of this research seem so trivially true. For example, consider the following general finding:

The message of this section is clear. The stronger the academic emphasis, the stronger the academic results. Time spent on reading and numbers is associated with growth in those areas, whereas time spent in other areas appears to detract from growth in reading and mathematics.<sup>28</sup>

I find it difficult to conceive of how time spent on reading and mathematics would not affect growth in those areas.

## **Back to Square One**

Since academic learning time and the other pupil mediating responses are, by definition, individual, learner-based variables, one would think that their prescriptive implications for teacher behaviour and teacher effectiveness are far from apparent. Nevertheless, there is remarkable consensus that these mediating learning variables imply (by a kind of backward extrapolation) a model of teaching called “direct instruction.” According to Rosenshine:

Direct instruction is a relatively new concept that has been developed independently by a number of researchers in recent years.<sup>29</sup>

If direct instruction is a “relatively new concept,” its components seem familiar enough:

Large groups, decision making by the teacher, limited choice of materials and activities by students, orderliness, factual questions, limited exploration of ideas, drill, and high percentages of correct answers.<sup>30</sup>

It is ironic, as Peterson points out, that “we seem to be traversing the same old avenue again, only this time the teacher-centered or directive method is called ‘direct instruction.’”<sup>31</sup> I suspect that those of us who remember when Flanders’ “indirect teaching” was in vogue may see the present resurrection of direct instruction as even more ironic. In fairness, the claims for the efficacy of direct instruction are acknowledged to be “contextually” restricted to the teaching of (1) basic skills (notably, reading and mathematics) and (2) pupils in the early, elementary-school grades. Be that as it may, however, Peterson for one has complained that the “recent reviews of research on teaching strongly suggest that direct instruction is the most effective way of teaching,” and that unsuspecting readers are likely to be “left with the overriding impression that direct instruction is best.”<sup>32</sup>

Assuming for purposes of argument that this impression is, indeed, overriding, then it may signal the very narrow range of instructional options that do, in fact, follow from this psychological model of teacher effectiveness. That is to say, once one settles for achievement test gains as the only reasonable measure of teaching outcome, it may be that little else *can* follow except direct instruction or the equivalent. In short, limited “outcomes” imply limited “processes.” Direct instruction, the accrual of ALT (as they say), achievement test gains, and the rest are expressive of what Philip Jackson has called “the engineering viewpoint” in education. And its basic weakness as a theory of teaching, as Jackson sees it, is that the “principle of maximizing achievement gains provides a guideline of dubious worth to the practitioner.”<sup>34</sup>

The more upbeat message in educational psychology’s renewed interest in pupil learning variables and achievement outcomes is that it marks out a pro-

blem domain that is rather more congruent with the conceptual vocabulary and modes of explanation that characterize psychology as a social science discipline: “no one is so well equipped as the psychologist to understand the processes by which learning occurs.”<sup>35</sup> But as is evident, teaching processes are *not* the immediate processes by which learning occurs. Teaching processes or “what teachers do and how this relates to the subsequent behaviour of students has to do with the structure of the social relationships, in schools and in classrooms, and it is in the understanding of such social relationships that sociology finds much of the grist for its mill.”<sup>36</sup>

## NOTES

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