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ABSTRACT

Teachers do not intentionally use or consider research findings in making teaching decisions. This lack of use is usually blamed on research deficiencies; e.g., educational research does not address the realistic demands of classroom teaching, research findings are inconsistent, and research articles are difficult to obtain and read. A major difficulty is that teachers and researchers have different orientations, as well as different reference groups which set and apply divergent standards of professional achievement. For example, the American Educational Research Association encourages research aimed at the interests of academicians, not teachers. There are many problems to implementing any solutions. One proposed solution is to shift from a research to a research and development orientation. But such a shift would conflict with the researchers yearning to be social scientists and with the academic standards often applied in judgments of their work. Another solution is to encourage research by teachers. But, without relief from teaching demands, where would teachers find the time and energy for such efforts? Making research reports more readable is difficult because of the over-sophistication in statistical analysis. Despite the complex problems, dialogue between teachers and researchers might move educational research toward greater usefulness. (RM)

MAKING RESEARCH USEFUL TO TEACHERS*

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Concern over the lack of use, or even perceived usefulness, of educational research is not a recent phenomenon. Whether educational research has influenced practice has been a recurring question since the beginning of the "scientific movement in education" in the twentieth century (Clifford, 1973; also see Gage, 1978). Although there are few hard data on impact (Clifford, 1973, p. 3), it is generally assumed that educational practitioners do not often use research in their professional decisions.

Educational research has the potential for use at several levels of educational decision-making: in the making of national, state, or local educational policy; in school district judgments about curricula; in school-wide decisions about curricula or student management; in decisions by individual teachers about what to teach and how to teach it, as well as how to otherwise interact with their classes and individual students.

Research findings are used occasionally at the national level, with research sometimes even funded--e.g., studies of compensatory education (Welsh, 1972; Kennedy, 1978)--for use in policy-making. Whether the result has been improved policy is not the point here, although it has been argued that the same methodological problems that have plagued researchers' efforts to build educational theory--e.g., the inability to

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assign subjects randomly to treatments, poor instrumentation, and inappropriate analyses (e.g., Campbell & Boruch, 1975; Page & Keith, 1981)--may have produced misleading evidence for policy-makers.

At the local levels of decision-making, where decisions are likely to be made by educators, research seems to be less highly regarded as relevant evidence. For example, the stages in the adoption of social studies material which Hahn (1977) discussed can be taken to include recourse to research evidence only by a great stretch of the imagination, although benefits observable by the users are stressed. And, Boag and Massey's (1981) case study of two teachers' reactions to new social studies materials also notably lacks any reference to their use of research evidence. The National Science Foundation studies of status in social studies in 1976 indicated that social studies teachers are rarely aware of research findings, much less users of them (Shaver, Davis, & Helburn, 1980).[1] It is the lack of use of research by practitioners, especially social studies teachers, which is the focus of this paper.

For the purpose of this paper, it is important to distinguish between the "influence" of research and the "use" of research. Use has an intentional element to its meaning which influence does not. Research findings may influence thought and action even though they are not consciously used to guide practice. Clifford (1973, pp. 25-6), for example, in considering the "impact" of research on teaching, discussed "cultural diffusion"--the process by which ideas generated through research become a part of the common knowledge of the society

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[1] Interestingly, conceptualizations of teacher decision-making as a basis for research (e.g., Borko, Cone, Russo, & Shavelson, 1979) and reviews of research on teacher thinking (e.g., Clark & Yinger, 1979) do not include recourse to research evidence as a factor.

or of a subgroup (e.g., social studies teachers). An example would be the far-reaching influence of standardized tests (NRC . . ., 1982), an effect which some (e.g., Jencks & Crouse, 1982) would argue has been deleterious. In social psychology, Gergen (1973) has also emphasized the influence of research through cultural diffusion, as have recent defenders of social science research (e.g., Mosteller, 1981; Prewitt, 1981; Academy . . ., 1982). Research may indirectly affect what happens in school; however, this paper is concerned with use--the intentional consideration of research findings by teachers in making teaching decisions.

Why Don't Teachers Use Research Findings?

As Boag and Massey (1981, p. 39) have pointed out, there is a strong tendency in the educational literature to assume that teachers are rational problem-solvers, systematic decision-makers for whom one only has to provide the appropriate evidence to affect practice. Discussions of reasons for the lack of use of research findings by teachers are no exception to this rationalist assumption. The result is an emphasis on the nature of research and research findings, as illustrated by the following list extracted from an extended analysis (Shaver, 1979c) and a recent synopsis (Banks, 1982) of reasons for educational practitioners' inattention to research:

> (1) Educational research does not address the realistic demands of classroom teaching and is too often aimed at issues not of interest to teachers.
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> (2) Educational research is too rarely aimed at specific schooling problems or based on available technological knowledge (also see Schutz, 1979).
>
> (3) Educational research is based almost totally on the assumption that practice needs to be improved, with little effort directed toward finding out what is now being done well.

(4) It has been assumed that tests of statistical significance provide much more information than they do about the importance of results, their causes, and the likelihood that they will re-occur in either future research or classroom practice.

(5) Too little attention has been paid to the practical or educational significance of findings, i.e., to whether a result is of sufficient magnitude to be deemed important by practitioners.

(6) Too few research findings are replicated to establish their reliability and/or their limits.

(7) It is erroneously assumed that research findings can be converted directly into practice, and researchers neither direct enough attention to drawing out implications for practice in their reports (see also, Clifford, 1973, p. 35) or doing research to test implications.

(8) Research articles are neither easy for teachers to obtain nor for teachers to read.

(9) Research findings are frequently inconsistent and contradictory.

(10) The proliferation of research reports in education and social science fields related to teaching decisions (not to mention research reports in social science and history related to social studies content) makes it impossible to keep in touch with potentially relevant findings.

(11) Efforts to integrate and synthesize the research literature have been conceptually and methodologically inadequate.

(12) Research findings rarely have clear implications for teaching because differing assumed facts and underlying values may lead different people to conclude that the same finding suggests quite different educational practices.

(13) Too much educational research is polemic in nature, done for the doctrinal, or ideological, purpose of sustaining or debunking a position, making it difficult to sort out implications for practice (also see Bereiter, 1982; Berlak & Berlak, 1981, pp. 10-18).

(14) Social science research (of which educational research is taken to be a part) is based on an overly simplistic view of human behavior and, in particular, of instruction and learning, including the belief that human behavior fits the assumptions of regularity and predictability which are basic to the physical sciences.

(15) Social science research (including educational research) results in tentative theories and generalizations that rarely provide teachers with specific guidelines for practice.

As noted above, the assumption often seems to be that if we could correct research deficiencies, then teachers (and other practitioners)

would apply the findings in their professional decision-making. In fact, we know little about teacher decision-making, at least from research (Clark & Yinger, Note 1). But what we do know from anecdotal evidence and personal experience should lead us to question the model of teacher as rational decision-maker. And there is no reason why our expectations for teachers' decision-making should be any different than for others. People generally have as many, if not more, non-rational as rational elements in their decision-making (see Janis & Mann, 1977). That is another reason why research findings may be ignored.

Elementary and secondary school teachers may not use research findings in making teaching decisions; but, then, do professors of education? I conducted a small, informal survey, asking some professors about their use of research in making instructional decisions. The initial reaction was typically incredulity at the question. But, no, they reported, while they may discuss research findings as part of the content of their courses, they rarely refer to them, certainly not as part of a consistent decision-making model, in making decisions about content, presentation, and noninstructional teacher-student interactions.[2] So another possible reason for lack of use of research findings by teachers may be the lack of models of such use during their preservice or inservice training.

The "rational problem-solver" model assumes that teachers are, at heart, philosophers. That view leaves much teacher behavior--or, more appropriately, human behavior--unexplained. I have, for example,

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[2]Elsewhere (Shaver, Note 2), I have commented on a parallel tendency for professors to propose that prospective teachers use conceptions of teaching, such as Dewey's proposals for experience-based curricula (Shaver, 1977), but not to apply such conceptions in their own teaching.

been baffled by the lack of reaction by social studies teachers to the perennial finding (e.g., Shaver, Davis, & Helburn, 1980) that students predominantly report social studies to be uninteresting, if not downright boring. But, teachers' inattention to that finding should not be surprising when it is so easy to observe other intelligent people ignoring research evidence--for example, findings that connect cigarette smoking with risk of cancer or use of seat belts with reduced injuries in car accidents. If smokers or nonseat belt users can so easily deny the likelihood that they may be victims, it should not be difficult to see how teachers can ignore the problem of lack of interest (after all, in doing so, they continue a social studies tradition, one which they likely experienced as students) or believe that while the findings may be generally true, their students find social studies interesting.

Psychological explanations of human behavior, such as Festinger's (1957) theory of cognitive dissonance, which presents hypotheses about the reactions of individuals to dissonant information, are probably more applicable to understanding teacher thinking (and the thinking of other people) than is the rational decision-making model. But the point is that whether one is considering research on teaching thinking (Clark, Note 3) or contemplating how to increase teachers' use of research findings, the conception of teacher thinking from which one proceeds will have a significant impact on the outcome. Cognitive dissonance is, of course, only one of the nonrational, psychological factors in teacher thinking that may work against the use of research findings (Janis & Mann, 1977).

Another important consideration is frame of reference. Teachers tend not to be interested in intellectualizing what they do, and less

so in presenting those intellectualizations to others in papers. Their professional interests are on the more personal level of the influences exerted and the satisfactions gained through encounters with students. That is why they teach.

Teachers operate in a social context that includes not only the general social, cultural, and political setting, but their own definition of a meaningful classroom situation (Boag & Massey, 1981), and their perceptions of how important others--their principal, fellow teachers, students, parents, other community people--define their role. The expectations embodied in these dimensions of social context are powerful determinants of what happens in classrooms and schools (Shaver, Davis, & Helburn, 1980). That teachers ought to consider research findings in making teaching decisions is rarely among the expectations.

What Might Be Done to Increase Teachers' Use of Research Findings

The constraints on increasing teachers' use of research findings, of course, are not only the ones I have noted above in regard to proposed shortfalls in research and the psychological and social factors that affect teacher decision-making and behavior. Researchers, too, are bound by their interests and intellectual orientations, by their psychological needs, and by their social context, including the way in which they define their academic situation. Just as public school teachers have frames of reference oriented toward teaching young people and, within that context, meeting the expectations of persons or groups they deem relevant (rarely university professors or researchers), so do researchers (who are most often university professors) have their frames

of reference, oriented toward the production of knowledge[3]--or, in any event, publications--and meeting the expectations of the persons or groups they deem relevant (rarely public school teachers). It is important to keep in mind the existence of an educational research establishment. It is represented formally by the American Educational Research Association (AERA), which is now a lobbying force as well as a professional organization. AERA--and within NCSS, the College and University Faculty Assembly (CUFA)--provides a social system which provides, along with the criteria for tenure and promotion at universities, strong reinforcers for research aimed at the interests of academicians, not teachers.

A major additional difficulty, then, is that teachers and researchers have different orientations (based on psychological and intellectual predilections, as well as education and experience), as well as different reference groups which set and apply quite divergent standards of professional achievement. Any attempt to deal with the "usefulness of research" issue, including efforts to promote dialogue between teachers and researchers, must start from that basic recognition: That is, not only do teachers and researchers view the world differently (see Shaver, Davis, & Helburn, 1980), but change in either may not be only a matter of intellectual persuasion, as difficult as that may be, but a matter of modifying personality characteristics and complex social contexts which control and limit. If proposals to increase the use

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[3] Whether educational research can produce the scientific knowledge to which many researchers aspire is an important issue that is relevant to the topic of this paper. In so far as researchers pursue that goal, usefulness to teachers will not be a criterion for selecting research problems (Kerlinger, 1977). Also, many of the reasons for the lack of usefulness listed on pages 3-4 have their roots in what I perceive to be confused efforts to be scientific. I have developed arguments about educational research as science elsewhere (Shaver, 1979b, 1979c, 1982, Note 4; Shaver & Norton, 1980a, b).

of research findings are to be realistic, they must be made against that background.

In any event, with some twenty reasons for the lack of use of research findings by teachers alluded to on the previous pages, it might seem that a number of proposals for amelioration of the situation would be obvious. That is not so, however, especially if the proposed solutions are to be subjected to the criteria that they must have a high likelihood of being implemented and of resulting in teacher use of research. The formulation of such solutions is an extremely difficult task, so much so as is to almost defy imagination. The reasons for lack of use of educational research findings form a strong, tangled web of intellectual, psychological, and sociological threads. Any effort to assault the total web at once is not only difficult to conceive, but doomed to failure. There also does not appear to be one "key thread", the pulling of which would lead to the unraveling of the rest. In fact, the threads appear to be so knotted and sticky that it would be impossible to extricate any one for individual attack. Discussion of a few of the problems of implementing solutions to some of the reasons for nonuse may help to illustrate the complexities involved.

Drawing Implications from Research

One possible solution to the lack of use of educational research findings by practitioners is that researchers direct their efforts toward producing implications for practice (reason #7 on page 4). This suggestion--which involves both that research should be done that is intended to have an impact and that research reports should contain more elaborate sections on practical implications (Clifford, 1973, p. 35)--seems reasonable on the surface, but poses serious questions. Schutz (1979)

argues that research cannot be converted directly into educational practice; indeed, that efforts to do so are "akin to alchemy" (p. 6). The development of educational practice is, he argues, a matter of technology not of research. Put in different terms, it is an R&D task, not a task for research in a scientific mode, although there is a symbiotic relationship between the two.

If the lack of practical implications in research reports is not simply the result of researchers' inattention to such concerns, then a shift from a research to an R&D orientation among many researchers might be in order. But such a shift would conflict with the researchers' yearning to be "social scientists" and with the academic standards often applied in judgments of their work. Moreover, some would argue that such a shift would be self-defeating in the long run, in that it would pull researchers from the important task of developing a base of knowledge (Kerlinger, 1977).

Those who argue for impact-directed research must contend with the complicated question of whether educational research can be scientific[4] and, if so, whether scientific research can be practical. If Kuhn (1970, pp. 19, 164) is correct, relative isolation from society is important in efforts to build knowledge scientifically. Otherwise problems are too often pursued, not because of their scientific interest, but because of their social urgency, regardless of whether adequate methodologies for investigating them are available.

But, in any event, should researchers be expected to draw specific implications for practice from their studies? Reason #4 (page 4) refers to the difficulty of such judgments. The "is" of research

[4] See footnote 3.

findings does not translate automatically into "oughts" for classroom instruction. Whether stated or not, the link involves factual and value assumptions (Phillips, 1980). What, for example, should teachers do when research suggests that some teaching competencies identified by teachers are negatively correlated with learning outcomes or produce results with self-concept and achievement measures that are inconsistent (Coker, Medley, & Soar, 1980)? As another example, I have argued (Shaver, 1979c), that research findings in regard to students' political knowledge translate into educational implications only if one assumes certain values--e.g., in order to judge whether research results indicate a learning "deficit"--and facts--e.g., about the outcomes of increased instruction and about how such instruction should occur. In the latest research to capture educators' imaginations, time-on-task seems to be established as a potent learning variable (Borg, 1980). But Rosenshine (1979), while acknowledging that the pattern of findings indicates that "time spent engaged in relevant content appears to be essential for achievement. . ." (p. 47), also urges "caution in implementing the results into teacher education programs or into evaluative checklists for teachers". We should, he believes, avoid "adopting another round of dicta on 'teachers should'" (p. 32; also see Peterson, 1979).

Rosenshine's concern about moving to conclusions about teacher behavior fits well with Clark and Yinger's (1979) argument that the results of research can be applied only by individual teachers who themselves make the adaptations to the "unique combination of personalities, constraints, and opportunities" present in each class (p. 231-2). Why should we assume that researchers have the omniscience to draw practical implications applicable to all classroom teachers?

Meeting Teaching Realities

But if research does not often address specific schooling problems, much less the realities of schooling that are of concern to teachers (reasons #1 and 2, p. 3), what is the teacher to adapt? This question turns attention back to the dilemma of research aimed at impact versus research aimed at knowledge building (which, of course, may not be a clearcut dichotomy, Shaver, 1979b), a dilemma that is not easy to resolve. Even if educational researchers were willing to accept the assumption that research should address specific schooling problems and were willing to shift to that orientation, difficulties would remain. R&D research at the school district level is expensive (Schutz, 1979), and it is dubious that funds would be available for it. Certainly, school district budgets have little room for research costs, and federal and state funding was hardly adequate before the current austerity. Moreover, R&D aimed at problems identified at the district level may not speak to classroom realities as seen by individual teachers in the district. Even if so, dissemination of the R&D results and training will be necessary, followed by adaptation from classroom to classroom. This sounds much like the situation now existing in which research findings are little used. R&D efforts and dissemination of the results, too, will be hindered both by lack of resources and by lack of teacher awareness and interest.

To meet the problem of adaptation, research by individual teachers might be encouraged (Shaver, 1979a), and not only from a statistical-experimental orientation (Nelson & Cornbleth, 1982). But are teachers likely to have the interest to find the time and energy for such efforts--especially without relief from the demands of teaching, such as would be

provided by reduced classloads? The prospect that school boards will stretch their budgets to accommodate time for teacher research is even more unlikely than widespread teacher interest in doing the research. And, of course, one must ask about teacher competencies to do valid research without considerable training and/or collaborative experience, especially if they are to choose from an array of methodologies (Nelson & Cornbleth, 1982). Is poor research by teachers better than none? I think not. The failure of the action research movement of the 1950's to sustain itself (Clifford, 1973, p. 21) speaks to the difficulties involved in teachers doing their own research.

Another proposed solution is to involve teachers as partners in research so that the identification of problems, methods and designs, and interpretation of data will reflect classroom realities (e.g., Shaver, Davis, & Helburn, 1980). This idea is being tried at Michigan State University's Institute for Research on Teaching. But one must wonder how widespread the effects will be, in light of the same time and energy concerns as can be raised in regard to teachers doing their own research. Being a research partner also demands time and effort. Moreover, the intellectual thrust and financial base for research lies with the university researchers, making it probable that they will be the dominant force in structuring the research. It is not likely, because of role perceptions and financial reasons, that the research thrust and financial base will change from universities to school districts. Then, too, those teachers who get involved as research partners are likely to be those with orientations similar to the researchers'. Moreover, as is the case with supervisors who go into

graduate education programs (Shaver, Davis, & Helburn, 1980), their belief systems will come to be even more like the university researchers' as they work with them--thus becoming less like other classroom teachers and defeating at least one purpose of having teachers as research partners. Of course, the question must also be raised whether affiliation with teachers, to the extent that research is thereby reoriented toward practical school realities, may not be to the detriment of efforts to build generalized knowledge about education.

Availability and Readability of Research Reports

In considering how (or even, whether) to make research reports more available to and readable by teachers, we again come face to face with the two different worlds of teachers and researchers. We might consider strategies to bring teachers in contact with research reports, such as having such reports available in schools. But would teachers read the reports? Even if the time were available for them to do so, why should they choose to devote the time to reports which contain conflicting findings and which are, in any event, perceived as irrelevant to the demands of teaching (a deficit which, if my analysis is correct, would be difficult to correct, and the correction of which might well interfere with knowledge building in the long run)? In my experience, teachers do not flock to research sessions at NCSS annual meetings, yet NCSS members are likely to be more professionally concerned than social studies teachers in general.

Even if teachers wanted to read research reports, which should they read? The ERIC system was intended in part to help make research available to school people. But it has also added to the proliferation

of research reports already taking place in the journals. Surely to expect teachers to peruse ERIC or educational research journals for research findings pertinent to teaching is as unrealistic as to expect them to keep up with all of the social science and historical research that might be relevant to the content of their courses.

Perhaps more reviews of the research literature, better done, would help (reason #11, page 4), if they could be made available to teachers and if teachers would read them. The introduction of review techniques such as meta-analysis (Glass, McGaw, & Smith, 1981) may increase both the number and quality of reviews. Whether the findings are available to be summarized and whether implications of assistance to individual teachers can be drawn are serious questions. As a reader of the Review of Educational Research, I see little there of interest to teachers. One purpose of the Research Section of Social Education established in 1969 was to encourage the preparation of reviews of research of assistance to social studies teachers. Such reviews did not materialize, suggesting either that the research is not available or that social studies professionals lack interest in the task.

Teachers find research articles difficult to read in part because of the statistical analyses that are presented. Unfortunately, the availability of faster computers leads educational researchers to more and more complicated multivariate analyses that become more and more removed from reality. The situation is not yet as bad as in economics where complex multiple regression models often have little connection with the real world (see Leontief, 1982), but it is getting worse. It is often forgotten that the computer adage, "Garbage in; garbage out",

refers to statistical analysis programs as well as to data. In my experience, researchers are even less likely to examine the appropriateness of statistical packages than they are to scrutinize the meaningfulness of their data. So research reports are often not only difficult to read without the type of statistical training teachers cannot be expected to obtain, but the findings often reflect the over-analysis of inadequate data.

Statistical and Practical Significance

The push toward over-sophistication in statistical analyses is not the only problem. The continued reliance on inferential statistics, despite cogent attacks (e.g., Carver, 1978) on their meaningfulness and the frequent failure to meet the underlying assumptions (Shaver & Norton, 1980a, b), has distracted educational researchers from the traditional scientific strategy of replicating findings (Campbell & Jackson, 1979; also see Lewin, 1981). It has also meant that research results have been stated in terms that are not meaningful for practitioners.

Unfortunately, there has been a lack of attention to practical or educational significance--to whether findings are of sufficient size to merit attention by practitioners (e.g., Shaver, 1979c, Note 4). But, then, if researchers are not dealing with issues of interest to teachers, is it not a contradiction in terms to speak of the "educational significance" of their results?

Teachers are likely to be convinced by effects they can observe (Hahn, 1977). Even if measures of practical significance, such as estimates of the variance accounted for by treatments, were more frequently reported, they are far removed from "observable effects". So a basic block to use of research findings would remain.

It has been the production of effects observable in the classroom, rather than results reported in terms of test scores, which has made the application of operant conditioning research findings so attractive to teachers of the mentally handicapped--results that came, ironically, from a field in which researchers have viewed themselves as scientific and eschewed practicality (Greer, 1982). The implications for educational research are not clear. It may mean that usefulness would be enhanced in the long run by turning to theory-guided laboratory research, which would certainly limit even more the discourse between teachers and researchers. Or, if Greer (1982) is correct, a major shift in epistemology is called for--a move from a quantitative, mentalistic approach to behavioral analysis; from asking how the student's mind thinks to dealing with relationships between student behavior and contingencies controllable by researchers and teachers.[5] In any event, at this point in time, the production of observable effects does not fit either the technology of educational research with its emphasis on quantifiable "objective" tests with little attention to the meaning of differences in scores (Shaver, Note 4) or the desire to build knowledge through generalizable, not teacher-specific findings.

Educational Research as Science

A recurrent theme is the extent to which the desire to be scientific in educational research may conflict with the production of useful research findings. I have already alluded to the inherent

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[5]Although Greer's claims about the applicability of behavioral analysis to complex types of human learning are exaggerated, the theme of his article is compelling: How can the educational research establishment be brought to confront its own unproductivity?

contradiction some see (e.g., Kuhn, 1970; Kerlinger, 1977) between a social service orientation and the goal of building knowledge through research. Concerns have also been expressed about the extent to which the researchers' social values permeate and influence research (Popkewitz, 1978) and, concurrently, about the ideological nature of much educational research and the extent to which ideology leads to polemics that interfere with linking theory and practice (Bereiter, 1982).

Ironically, however, it also happens that the research findings which are accepted are those that fit the educational ideology of practitioners (Clifford, 1973). My impression is that research findings are important to school practitioners primarily to the extent that they do fit their ideology. Certainly, the research on the effects of the jurisprudential approach (Oliver & Shaver, 1974, Appendix) have been rarely cited and have seemed to be of little interest to those who have found the approach compelling. How that curricular approach fit their view of what social studies should be was the critical factor. The match between teacher perspectives and program perspectives is crucial, not the availability of technical evidence that supports program effectiveness (Shaver, Davis, & Helburn, 1980; Boags & Massey, 1981).

To the extent that educational researchers want their findings to affect practice, they must heed educational ideology; to the extent that they want to be scientific, ideology must be shunned. So another dilemma arises from the web of reasons that affect the use and perceived usefulness of research findings by teachers.

A fundamental question also needs to be raised about epistemology as it relates to usefulness. The thrust of educational researchers who see themselves as social scientists is toward building generalizations

that will hold across teachers, students, and settings, in the hope of building scientific theory (see, e.g., Pinar, 1978). Serious questions about that goal have been raised by writers such as Gergen (1973), Cronbach (1975), and Snow (1977).[6] They have argued that social science research results pervade the culture, bringing about changes that invalidate findings and make it impossible to accumulate findings into scientific theory. They also maintain that the interactions among personological and ecological variables are extremely complex and difficult to ferret out--which may explain many of the apparently inconsistent findings in the literature,[7] but which also makes the amassing of findings over time and place a formidable, probably impossible, task.

Critics such as Gergen, Cronbach, and Snow accept the premise that human events are lawful (see, e.g., Cronbach, 1975, p. 123). Their quarrel is with the idea that the assumed regularities can be formulated into nomothetic scientific theory. In an insightful essay, a professor of sociology (Perrow, 1981)[8] has challenged even the assumption of the lawfulness and regularity of human behavior. Perrow contends that there is considerable natural disorder and unpredictability to human life. Yet, the social scientist's goal is to build rational

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[6] See footnote 2.

[7] Cronbach (1975, p. 121) points out that interactions can even be a problem in research with lower animals. One series of studies found puzzling differences from laboratory to laboratory in the responses of mice to hexobarbital. It turned out that bedding and drug interacted, with red-cedar or pine bedding producing different responses than birch or maple bedding.

[8] A later version of this article is in the Phi Delta Kappan, 1982, 63, pp. 684-88.

designs that explain human behavior, and thus eliminate disorder. This drive to give "sensible accounts" of human phenomena, to build theory, leads social scientists to disregard "happenstance, accidents, mysteries, illogicalities, and above all, fate" (p. 4) as unforseeable determiners of human lives. Not so the ancients, Perrow points out. Although they did try "to make sense out of things", they also accepted the limits of rationality in discerning patterns, if indeed such existed. As Perrow notes: "Count no life happy, the chorus repeats in the Greek tragedies, until it is over; one can never know what the unpredictable gods have in store" (p. 4).

I am reminded of Robert Burns' less imposing statement in his ode, To a Mouse:

> The best laid schemes o'mice and men
> Gang aft a-gley;
> An' lea'e us nought but grief and pain,
> For promis'd joy.

The humanities are often considered to be important in social studies as means to help students understand their place in humankind. The humanities can also help teachers comprehend their own teaching situation, as a rational counterforce to the social science assumptions embedded in most educational research.

What is important in the critiques by Gergen, Cronbach, and Snow is the recognition of the difficulties of accumulating findings about human behavior to allow scientific prediction and control. Perrow's attack on over-rationalism adds the dimension of the unforseeable and the unintended in individual lives. Together they suggest a different representation of the teacher's life than that of social science, based as it is on the assumption of predictable regularities.

Calling attention to fate and the unpredictable is not intended as an argument against trying to be rational. It is meant to call

attention to the bounds of rationality, and to the limits of educational research based on the assumption of regularities. Nor is it the intent to imply that teachers should pay no attention to research. Findings may often serve the heuristic functions of helping to understand the classroom and of suggesting alternatives. But educational researchers need to be more sensitive to the potential limitations of the positivist empirical approach, with its rationalistic assumptions, when it is applied to human affairs. The anthropological methodologies that are becoming popular in education (e.g., Dobbart, 1982; Spindler, 1982) are no panacea either, as their users strive to develop generalized propositions about the structures and processes of classrooms. Ironically, too, data produced by absorption in the life of one or a few classrooms may prove to be as inadequate a basis for generalization as traditional historians have, appropriately, perceived their data to be (Perrow, 1981).

Teachers know that their classroom lives, like their lives out of the classroom and like the lives of others, have large elements of predictability. Otherwise, life would not be manageable. But they also know that their lives are engrained with chance and surprise. Is the bottom line, then, that the set of assumptions which govern educational research do not square with human life broadly, or classroom life specifically? Is there serendipity in teachers' disinclination to use, even their unawareness of, the findings of educational research?

Conclusion

Phillips' (1980) recent summary of the past and the future of educational research is typical of such synopses in its pessimism:

> The wise educational practitioner ought not to hold his or her breath waiting for new, reliable, far-reaching breakthroughs by researchers. A skeptical, if not cynical, attitude toward research seems to be justified. (p. 17)

There are serious reasons to believe that the quest implied by the title of this paper, "Making Research Useful to Teachers," is not likely to be a fruitful one. My underlying theme has been the difficulty, if not the impossibility, of fulfilling the promise of that title. Throughout, the emphasis has been on complexities and contradictions, on the web of circumstances and epistemological difficulties that appear to bind and confound the educational research enterprise. No tenable suggestions for enhancing the usefulness of educational research for teachers have been offered, nor will they be offered in conclusion.

Nevertheless, the tone of the paper is not meant to be negative, but hopefully provocative. The premise is that dialogue and thought between teachers and researchers which might move educational research toward greater usefulness, or at least make clearer the dimensions of the problem, will not proceed productively until the participants share a sense--not just intellectually, but emotionally--of the seriousness of the situation. The underlying assumption is John Dewey's (1933), that thinking is based on "perplexity, confusion, or doubt" (p. 15). It is not yet time to consider how, or even whether, to close the gap between the worlds of teachers and researchers, because the problem itself is not yet adequately identified or accepted as important by those involved. Individual researchers can continue their work within the educational research enterprise without addressing the question; classroom teachers have little reason to believe that continued lack of attention

to educational research findings, much less to the state of the field, will have negative effects for them or their students.

The engagement of teachers within NCSS, as atypical as they may be of social studies teachers in general, in open, frank interchange and discussion with university researchers about the usefulness of educational research will not just happen. Those interested in such dialogue might first address the question:

> Is the lack of use, perceived usefulness, even awareness, of educational research findings by social studies teachers a problem? If so, to whom?

The discussion may go no further. There may be only a clearer recognition of the boundaries between the worlds of teachers and researchers and of the lack of shared interests and concerns--to use another metaphor, of the extent to which teachers and researchers "march to different drummers". But if some sense of mutual perplexity and/or doubt is aroused, along with sufficient commitment to make dealing with the problem somewhat compelling, discussion might focus on questions about possible explanations for the lack of usefulness to teachers of research findings, such as:

> Are the hindrances identified in this paper real or the determining ones?
>
> Are the interrelations among the obstacles as complex and inextricable as suggested?
>
> Are there feasible steps to counter the present situation?

Careful attention to three overarching questions may help to keep the above discussion from becoming too entangled:

> Is the state of educational research as dire as many observers believe?
>
> Is there an inevitable conflict between the research goal of producing scientific theory, or at least generalizations that will hold across populations and settings, and the research goal of producing findings with direct implications for classroom practice?

Are the assumptions of regularity and predictability that undergird research in the physical and biological sciences appropriate to the study of human behavior in general, and, to schooling-classroom research in particular?

Teachers' lack of awareness and use of research findings is a complex and difficult matter. If this paper helps even to formulate productive questions, it will have served its purpose.

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