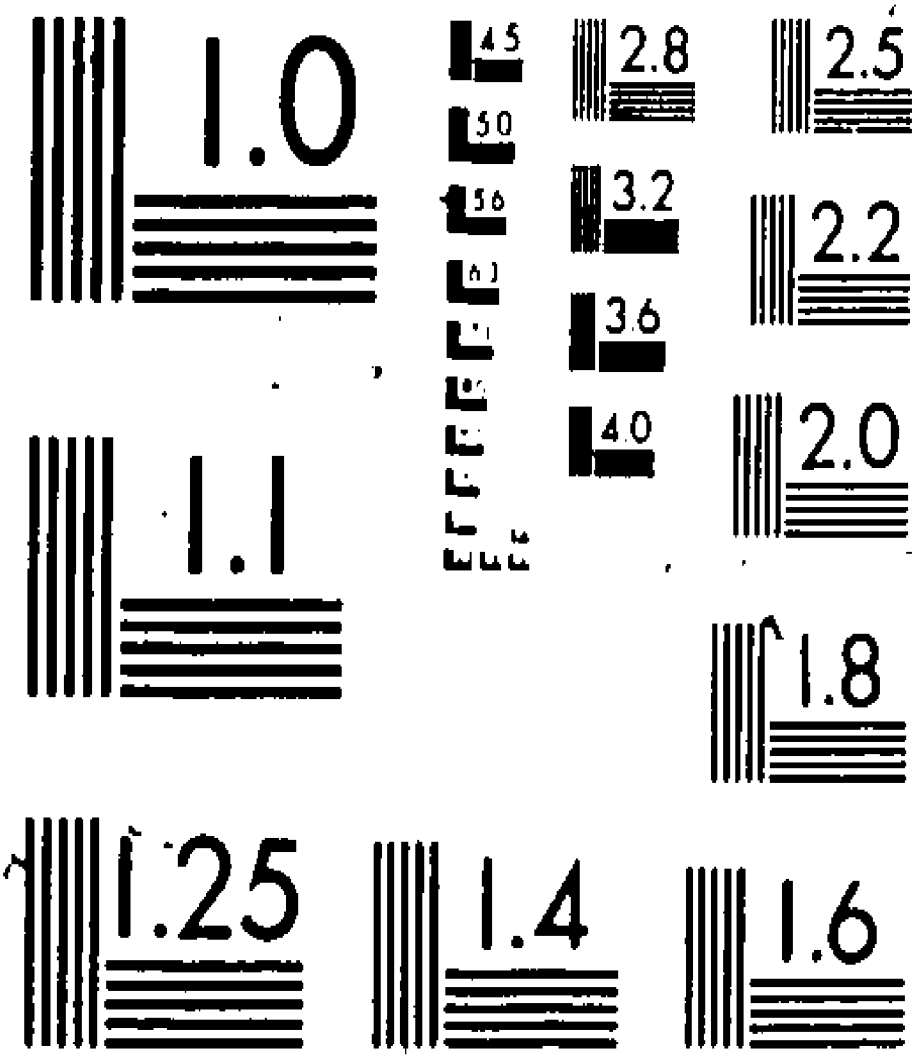

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ABSTRACT

Currently, findings of educational research are of little consequence to the curricular/instructional decisions of social studies teachers. Four basic conditions have created this inconsistency. (1) Since social studies teaching takes place among value and factual assumptions, decisions about educational practice are based more on value judgments than on research data or theory. (2) Generally, there is a lack of consistent, cumulative, definitive research findings on which to make curricular/instructional prescriptions. Most research is carried out by doctoral candidates and supervised by professors, many of whom lack commitment to research. Moreover, results that are merely statistically significant, although trivial in nature, contribute to the fragmented nature of social studies research. Research findings are sterile because they are abstractions, out of touch with reality. (3) The concerns of social studies teachers tend to be different from those of university professors who conduct and supervise most research. (4) The instruction/learning model that has dominated the design of social studies is overly simple. Therefore, social studies researchers need to reconsider the nature of their enterprise. Attention must be focused on those attributes of science which are applicable to educational research, including the role of theory in educational practice, the functionality of research strategies, and realistic goals. (KC)

ARE EDUCATIONAL RESEARCH FINDINGS USEFUL FOR CURRICULAR/INSTRUCTIONAL DECISIONS? A SKEPTICAL VIEW*

James P. Shaver

Utah State University

Paper presented at the annual meeting of the National Council for the Social Studies, Houston, November 24, 1978, as recipient of the NCSS Citation for Exemplary Research in Social Studies Education, 1977.

ARE EDUCATIONAL RESEARCH FINDINGS USEFUL FOR CURRICULAR/INSTRUCTIONAL DECISIONS?[1] A SKEPTICAL VIEW*

James P. Shaver
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A common goal--perhaps one could say, dream--among educational researchers is to impact practice in the schools. The hope is that their research will help practitioners improve the educational experiences of children and youth. Some anticipate doing a landmark study that will revolutionize schooling. More frequently, the expectation is that the studies they do will contribute to an accumulation of knowledge from which schooling decisions can be made. For some, this means trying to build and verify theory that could be applied in making curricular/instructional decisions.[2] The major focus of this paper is whether that goal, or dream, has been realized in social studies education--or is likely to be in the near future. As the title indicates, my conclusion is not overwhelmingly positive.

The Teacher as Focal Point

In such a paper, one could focus on any of a number of decision points

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[1]"Curricular" decisions are taken in this paper to refer to decisions about appropriate teaching goals and the experiences to reach them. "Instructional" decisions are taken to be those about how to teach within some implicit or explicit curricular frame.

[2]Pinar (1978) categorized curriculum researchers into three groups: "traditionalists" who are basically atheoretical and concerned with producing research findings of direct help to practitioners; "conceptual-empiricists" who believe that improvement of practice will come through the application of theory that has been empirically verified; and, "reconceptualists" who are basically theory-oriented, with the hope that future conceptualizations will provide more fruitful perspectives for approaching schooling and research about it. I presume that most social studies education researchers fall explicitly in the first category, that a few would like to be in the second category, and that very few are in the third category.

about curriculum and instruction--the curriculum developer, the methods course professor, the social studies supervisor, and/or the teacher. Of all of these, however, it is the teacher who is the key to what happens to students in social studies. The teacher not only interacts with the students each day, but in our system of education, tends to be the arbiter of what happens in his or her classroom. The notion of the teacher as the central figure in a classroom domain that is not to be intruded upon lightly by others has not been broken down by attempted innovations such as team teaching. Although teachers do lack control of the budget, and so find it difficult to make curricular or instructional changes that cost money, they do exert great power as members of textbook selection committees and, even more importantly, in determining what will happen to the students in their classes.[3]

Teachers can thwart, and have thwarted, the best intentions of social studies curriculum reformers. A recent example of reform languishing at the classroom door is the "New Social Studies" movement of the 1960's (Shaver, Davis & Helburn, 1978; Weiss, 1978; Wiley, 1977, Sec. 4). As a matter of fact, it appears that teachers tend to rely on other teachers for teaching advice, not on university specialists or district supervisors (Stake & Easley, 1978, especially Chs. 16 and 19). Teachers are probably little aware of the findings of educational research (e.g., Wiley, 1977, p. 9), and the theme of this paper is that it would probably be of little consequence to their curricular/instructional decision-making if they were. A discussion of the reasons for that conclusion follows.

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[3]For discussion and verification of "The teacher as key", see Stake and Easley (1978), especially Chapter 19.

Research and the Prescription of Practice

One of the functions of the "intelligensia" of social studies education--the "intellectual leadership"[4] is to make pronouncements about curricular/instructional policy, in the sense of prescribing appropriate courses of action for schools and teachers. Of course, individual teachers formulate that type of policy themselves as well, although in much less self-conscious and public ways. The "leaders" often try to rely on research findings and theoretical formulations in their position statements, while educational research findings and education-related theory seem to be largely ignored by teachers. Are the "leaders" justified in their attempts at reliance, and should teachers be made more sharply aware of the relevance of research findings and education-related theory to their own curricular/instructional decision-making? Such a question must be answered in terms of (a) the intrinsic role of empirical data and formulations in policy making and (b) the adequacy of the available empirical knowledge base for curricular/instructional decision-making.

Facts, Theory, and Policy

A finding, set of findings, or empirically based theory may indicate quite different policy positions to rational decision-makers, relying on different empirical knowledge or assumptions and values. In social policy studies, the principle that empirical data or theory do not necessarily prescribe policy seems to be well established. Because we can build a neutron bomb does not mean that we should necessarily pro-

[4] I put intellectual leadership in quotes because there is serious doubt how many followers there are, as I have suggested in the previous section. The small proportion of social studies teachers who are members of the National Council for the Social Studies is an illustration of the point.

ceed to do so. There are serious value questions and other empirical questions to be weighed first. That principle seems to be less well accepted among social studies "leaders", who frequently seem still to be caught up in the American spirit that "if it can be done, it should be done". The current enthusiasm of some social studies educators for getting teachers to use instructional techniques designed to enhance development through the moral stages that Kohlberg has theorized would be a good example--if there were not such serious questions about the philosophical (e.g., Peters, 1965) and empirical bases (e.g., Fraenkel, 1976, 1977; Lockwood, 1978; Shaver, 1977) for Kohlberg's theory and the instructional recommendations.[5]

The phenomenon of wanting to do what can be done as rapidly as possible has also been evident in attempts to apply Piaget's work to the making of education prescriptions. "If there are developmental cognitive stages," the American says, "then shouldn't we develop curricula and instructional techniques to move children through the stages as rapidly as possible?"

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[5] It is easy for the proponents of such movements and the critics, such as myself, to overestimate the classroom activity generated by bandwagons such as Kohlbergian moral development, Raths-Simon value clarification, or the discipline-centered teaching of the "New Social Studies". A couple of metaphors come to mind. One is social studies as an iceberg, with the tip--i.e., the activity--showing above the surface of the ocean, but with the mass of the iceberg--that is, the great majority of American schools and social studies teachers going on as before--largely unobservable and/or unobserved. The other metaphor is of social studies education as a deep lake with the wind rippling the surface. The innovations do ripple the observable surface of social studies education, but the great body of schooling below the surface remains largely undisturbed. The Case Studies in Science Education (Stake & Easley, 1978) and the NSF-sponsored National Survey of Science, Mathematics, and Social Studies Education (Weiss, 1978) provide confirming evidence for this view of surface change. The metaphors and the CSSE report also raise the question, Why are there so few deep sea and scuba divers--to extend the metaphors--in social studies education (or educational) research?

Piaget himself has referred to "the American question"--"Is it possible to speed up the learning of conservation concepts?" (Hall, 1970, p.30). His response is directly pertinent to the point of this paper. There is, he indicated, a counter-question to be asked--"Is it a good thing to accelerate the learning of these concepts?" And, there are good reasons for a hesitant reponse (Hall, 1970):

> Acceleration is certainly possible but first we must find out whether it is desirable or harmful. . . . Perhaps a certain slowness is useful in developing the capacity to assimilate new concepts. . . .
>
> [B]lindly to accelerate the learning of conservation concepts could even be worse than doing nothing. . . .
>
> It is difficult to decide just how to shorten studies. If you spend one year studying something verbally that requires two years of active study, then you have actually lost a year. If we were willing to lose a bit more time and let the children be active, let them use trial and error on different things, then the time we seem to have lost we may have actually gained. (p. 31)

Note that Piaget's own hint at a prescription in the last paragraph is tempered by considering possible gains and losses against an underlying value.

Social studies teaching takes place within a complex of value and factual assumptions that make hazardous the prescription of practice based on research and/or current theory. A paper by Judith Torney (1978), which provided the original stimulation for this paper, can be used to illustrate the point further. Torney's paper is an example of the rather careful use of research findings to develop a case for an educational prescription. But it also reflects the difficulties to which I have been alluding.

In her paper, "The Elementary School Years as an Optimal Period for Learning about International Human Rights", Torney makes several points. Citing an international study, she claims that a "deficit" exists among American fourteen-year-olds--that is, that they are more knowledgeable about domestic

political institutions and process than youth in some other countries, but less knowledgeable about and interested in international political institutions and processes (Torney, 1978, pp. 3-4). First of all, she discusses overcoming this "deficit" through schooling, but does not examine such factual questions as, Might increasing international interest and knowledge result in a decrease in domestic interest and knowledge? or such relevant value judgment questions as, Would such a redress of balance be desirable?

Later in the paper, after presenting evidence that there is considerable international consensus about human rights, Torney turns to the matter of the elementary school as an optimal[6] period of education about those rights. She discusses (p. 11) the notion of a "critical or optimal period" during which the "high degree of plasticity" in an organism makes the reorganization of behavior patterns relatively easy, and suggests that the elementary school may be thought of as an optimal time to teach about human rights. Although Torney does note the difficulty of testing such a notion empirically (p. 11), she goes on to cite "some research evidence which shows particular peaks of attitudes or abilities during the elementary school period" and "provide[s] useful information about the psychological characterists of children" (p. 11).

The research evidence has to do with the apparently rapid cognitive-social development of elementary school children and an apparent "loss of attitudinal plasticity" at about age twelve to fourteen. Torney rec-

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[6]Along with Webster's new world dictionary of the American language (2nd college edition), I take "optimal" to mean the "most favorable or desirable; best". Torney's discussion of her paper at the symposium indicated that her meaning was the same.

ognizes that different studies have found the growth and loss of plasticity to take place at somewhat different ages. Nevertheless, she concluded:

> The conclusion one may draw from research, even with differences in method and in the year in which change appears to be most rapid, is that the elementary school period is optimal for education about other nations, global issues, and international rights. (p. 15)

Her conclusion is questionable, regardless of the validity of the underlying studies. In the first place, it ignores alternate explanations of the findings she cites, such as the potential institutional effects on youth. Assuming that there is loss of openness and plasticity in regard to other peoples and countries, is it a function of normal development or of the impact of schooling? Could it be the result of a change from person-centered instruction at the elementary school to predominantly content-oriented instruction in secondary school? Moreover, a proposal to attempt to accelerate international social consciousness at the elementary age level must be met with Piaget's counter-question: "Is it desirable or harmful?" Would it, for example, interfere with the natural process of growth by not allowing adequate time for assimilation of the concept of culture and of varying cultural perspectives?

To further illustrate the difficulty of prescribing educational practice from research findings, Piaget's own theory provides a counter-argument to Torney's proposal. That is, to fully understand other countries and the concepts of international law and politics, as well as to empathize with people with whom one cannot have direct contact, may well presume the abstract thinking abilities of Piaget's Formal Operations Period. This line of reasoning suggests that the years beyond age 11 or 12 might be the optimal time for teaching about international rights, and that our attention should be directed (accepting the value judgment that

the goals Torney proposes are acceptable for American schools) toward restructuring our schools, curricula, and instruction at that level. In short, appropriate changes in education in international human rights may really be needed following elementary school to capitalize on potential cognitive development. Until we know more about cognitive growth and attitudinal plasticity and about the causes--e.g., developmental versus institutional--of both, the prescription of practice in that area using a research base seems at best a dubious proposition.

In general, then, the prescription of educational practice from research findings must be approached with caution. Given the present state of educational research knowledge, the prescriptive implications of any set of findings will rarely, if ever, be unequivocal. And, because prescriptions are based on value judgments, the examination of competing values must always intercede between data or theory and prescription.

The State of Research Knowledge

The second major consideration in any attempt to prescribe educational practice from research findings is the adequacy of the available research knowledge base. Gage (1978) notes that "most reviewers of research on teaching have concluded their reports by saying that past work has been essentially fruitless" (p. 1). Most reviews in the area of social studies education research have come to similar conclusions, as Karen Wiley's (1977) recent "review of prior reviews" indicates. Some of Wiley's observations and conclusions include:

> Many reviewers have expressed concern over the lack of a cumulative research base in social studies/social science education. (p. 165)
>
> [L]ittle or no research has focused on questions about the relative merits of different kinds of content (e.g., social science, public issues, chronological history) in achieving

> the goals of social studies. (p. 169)
>
> This area of social studies/social science education research [effectiveness and efficiency of instructional methods and techniques] appears to be fairly chaotic, at least from the perspective of extant reviews, both comprehensive and special focus. Although a large portion of research falls under this heading, this research appears to have yielded few conclusions that one can endorse with much confidence and few guidelines for practitioners. (p. 171)
>
> There has apparently been little research on the effectiveness of various types of curriculum materials. . . although there has been much research analyzing materials. . . and some research on the extent of use of various kinds of materials. (p. 197)

Wiley does not paint an entirely bleak picture (e.g., she draws positive conclusions in the areas of teaching for critical thinking[7] (pp. 174-7) and "models for factual and concept teaching", (p. 192[8]), but the overall message is clear: There is generally a lack of consistent, cumulative, definitive research findings upon which to make curricular/instructional prescriptions in social studies education; or conversely, teachers are losing little in decision-making power by their inattention to the research literature.

Wiley (1977, pp. 156-7, 177) does suggest that there may be more of a cumulative research base in existence than is evident, because reviewers have not adequately organized and interpreted "the disparate bits and pieces of research" available. She suggests that meaning and order might be teased out of the "numerous small, uncoordinated studies" by concentrating on limited topics and on research in and out of social studies conducted over a number of years.

Gage (1978) also maintains that the state of educational research know-

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[7] However, she refers to the "few scattered findings, which are tentative suggestions at best. . . ." (p. 176).

[8] Martorella (1977), to whom Wiley refers, indicates that "there are still more questions than answers" about the "instructional variables that have a significant effect upon cognitive outcomes" (p. 45).

ledge is probably not as dismal as reviewers have indicated. He calls for more valid reviews, paying less attention to statistical results and more attention to consistency in findings. He also recommends the use of post hoc statistical techniques, such as developed by Karl and Egon Pearson (Jones & Fiske, 1953) and Glass (1976), for amassing results across previously conducted studies. I would not be as harsh in my judgment of such statistical endeavors as Eysenck (1978), who objected to attempts to use Glaser's meta-analysis to "distill scientific knowledge from a compilation of studies mostly of poor design" with the comments: "Garbage in--garbage out" is a well-know axiom of computer specialists; it applies here with equal force". But I am not optimistic about the likely benefits of grand statistical analyses as a post hoc substitute for a priori attention to research strategy and design. I also am dubious that, as Wiley suggests, scattered, but relatable and valid findings are in the literature waiting to be given meaning.

Appropriate Research Models?

Concern with the lack of a research base for prescribing practice in social studies education raises the question as to why so much of the research activity in this field has been unfertile. I have explored elsewhere (Shaver, in press) some reasons for the lack of productivity of educational research, and the arguments presented there in regard to educational research in general apply to research in social studies education. Part of the problem is that the bulk of the research is carried out by doctoral candidates (Wiley, 1977, pp. 155-6) who frequently see their research only as a hurdle on the way to a degree, and who are supervised by professors who also lack both commitment to research and understanding of

research procedures.[9] The consequences are that much worthless research is done and new generations of university professors are socialized in attitudes dysfunctional to the building of cumulative knowledge. Basic to this problem are the superficial consideration and understanding of science among educational researchers and an attendant uncritical emphasis on the "test of statistical significance" approach to research as exemplifying science.[10]

The Test of Significance Approach. One outcome of the test of significance approach to educational research has been the celebration of the trivial result. Too many graduate students are socialized ("educated" certainly seems the wrong word) to believe that if a result reaches the mystical .05 level of significance it is important. Too rarely are they asked or helped to defend that level of significance, or any other, on grounds such as the potential costs and consequences of the decision to be made based on the results. A related but even more serious deficiency is that they are not encouraged to think in terms of the educational significance of their findings. Significant Pearson product-moment correlation coefficients are reported without computation of r^2 (the coefficient of determination) which indicates the percentage of variance the two variables have in common, or put another way, the percentage of variance in one variable accounted for by the variance in the other. Statistically

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[9] I avoid comment on the uselessness of most of the survey research which is carried out by so many doctoral candidates (see Wiley, 1977, p. 166).

[10] A. Guy Larkins has noted to me in conversation that there is a corollary lack of understanding of theory and its role in scientific knowledge-building that contributes to the dysfunctionality of educational research. I agree. But I am uncertain about the extent to which such accord leads to a common position on the nature of functional theory for schooling.

non-chance relationships that are really trivial educationally (an r = .195 is significant at the .05 level with a two-tailed test and 100 degrees of freedom; r^2 = .038--3.8% common variance) are greeted with glee. Even less frequently is the correlation ratio (η^2) computed following an analysis of variance to estimate the proportion of the variance on the dependent variable accounted for by the independent variable.

An example came to my eye in the last issue of Theory and Research in Social Education. A study having to do with the "Effects of Coordinated Environmental Studies in Social Studies and Science on Student Attitudes toward Growth and Pollution" (Hepburn, Shrum, & Simpson, 1978) found no significant main effect (the independent variable was study of a social studies module, a science module, both modules, or no module) for Pollution test items, but a statistically significant ($P < .01$) one for Growth test items for both ninth and tenth graders. Fortunately, the authors provided the sums of squares for their analyses of covariance so the η^2 could be computed (η^2 = main effect sum of squres ÷ total sum of square). For the ninth grade, η^2 = .0723 and for the tenth grade, .0596. (Interestingly, η^2 for the covariate, the pretest, was .3776 for the ninth grade and .4859 for the tenth grade.) Now the question can be raised whether a treatment that accounts for only 6 to 7% of the variation on the dependent variable is educationally significant, and whether the mixed results[11] and the correlation ratios justify the following claim:

> Results of this study support the notion that environmental knowledge does influence environmental attitudes. Specifically these results indicate that interdisciplinary instruction which is coordinated across social studies and science and which is

[11] In addition to the lack of significance on the Pollution test items, the science-only and social studies-and-science groups were significantly different from the control group but not the social studies-only group on Growth items at the ninth grade; and they were significantly different from the social studies-only, but not from the control group at the tenth grade.

> mainly cognitive in approach, can promote positive attitude change. . . . This suggests that environmental education should be part of both the science and social studies curricula of high schools. (p. 84. Italics mine.)

I cannot help but recoil from the rather glib leap from what appear to be trivial,[12] if statistically significant, findings to a curricular prescription.

The joy that comes from trivial but statistically significant results is often an aspect, too, of another misunderstanding of scientific knowledge-building--that is, that only statistically significant findings are worthwhile. So graduate students worry not about how their studies tie into past research, but about whether their results will be significant and thus acceptable to their supervisory committees. The notion of testing alternative hypotheses with crucial experiments and the accompanying importance of disproof to the building of knowledge (e.g., Platt, 1964) seem almost entirely foreign to educational research.

Fundamentally, this misunderstanding of the test of significance approach as science contributes to the fragmented nature of social studies research that is so often lamented (e.g., Wiley, 1977, 155-6), as does the poorly understood application of statistical analyses to "prove" the generalizability of results. The mathematical models underlying the statistical tests of significnace used to establish generalizability require random sampling from target populations--a condition that can rarely be met in educational research. Computing tests of significance without meet-

[12] The potential triviality depends on such matters as the validity of the tests, the unclear results, and whether the small percentages of variance accounted for are worthwhile, especially when weighed against the cost of implementing the study, e.g., financially and in terms of lost opportunities for other studies in the curriculum.

ing that assumption gives a false sense of security in one's results. Moreover, it has led educational researchers away from the essential scientific strategy for establishing the reliability and generalizability of findings--the replication of studies, either directly or with systematic variations.[13]

Getting at Realities. The positivist, statistical inference approach to research has led us astray in other ways as well. Such thrusts as the concern with hypothesis testing, the operational definition of variables and their assessment in forms appropriate for statistical analysis, the emphasis on formal research designs to keep the researcher from contaminating his subjects and vice versa, have led to research findings that are frequently sterile because they are abstractions, out of touch with reality. A few years ago, Larkins and I argued that a broader conception of appropriate educational research methods was needed and suggested that the participant-observer, ethnographic approach offered a promising alternative (Shaver & Larkins, 1973, pp. 1254-8). Others, such as Cronbach and Snow (1977, pp. 16-17, 390, 518) and Wilson (1977) have advocated similar approaches since that time. It has only been recently that a major ethnographic study in social studies education has been reported.

In 1976, the Education Directorate of the National Science Foundation contracted for three studies of status and needs in science, mathematics, and social studies education. One of the studies was a rather traditional, if well executed, national survey of teachers and school administrators (Weiss, 1978). Another was three twenty-year reviews of the research literature, one of which (Wiley, 1977) has been cited several times in this paper. The third was Case Studies in Science Education (CSSE), an ethnographic field study conducted at eleven sites across the nation (Stake & Easley, 1978; Stake, Easley et al.,

[13] A more extended discussion of these points can be found in Shaver (in press). Also, see Cronbach and Snow (1977, pp. 16, 22-3, 51-530, 519).

1978). That report--both the individual case studies and the synthesis chapters--indicates the rich, strong perspective on the realities of schooling and the factors that condition teaching and educational change that can come from such naturalistic studies (see Shaver, Davis & Helburn, 1978, for further comments).

As a matter of fact, the CSSE report indicates a major reason why the findings from most social studies education research would not have much relevance for the decision-making of social studies teachers, even if there were more valid, non-trivial, cumulative findings. It is that the concerns of social studies teachers tend to be much different from those of the university professors who conduct and supervise most research in social studies education. Faced with the day-to-day demands of teaching in a schooling social system which values the use of subject matter content for socialization--both to meet the demands of the school and for citizenship--teachers by and large are not much concerned with the questions about inquiry teaching, the promotion or critical thinking, the analysis of textbooks, and so on that intrigue university professors. In fact, many of the curricular/instructional ideas advocated and investigated by university professors threaten the basic classroom management techniques of teachers,[14] run counter to their reliance on the textbook as the authoritative source of knowledge and the central instructional tool, fly in the face of beliefs about the need to provide external motivation for children and to socialize them for later success in school, and are in opposition to a commitment, often unexplicated, to the teaching of understandings from history and government, not as ends in themselves, but as a means of promoting a positive image of

[14] For a summary of research that speaks to teachers' classroom needs, see Gage (1978, p. 234).

our nation that is deemed to be an important basis for citizenship. (See Stake & Easley, 1978, especially Ch. 12, 13, 16, 17; Shaver, Davis & Helburn 1978.) The utopian views of democratic political participation that dominate considerations of social studies education by the "leaders" do not appear to provide an adequate basis for research that can impact the majority of social studies classrooms.

Until social studies education researchers become more attuned to the realities of the classroom, and to teachers' perceptions of these realities, their research is not likely to be of much use for prescribing practice there. CSSE suggests how case studies can be used fruitfully to get at that reality and those perceptions. It also suggests areas of study for those in social studies education. As a basis for identifying meaningful research problems, more attention is needed, for example, to the socialization role of the school as the society's formal educational institution[15] and to the question of reasonable instructional demands on teachers in a setting of public, universal education.[16] And research directed at the factors that influence the experiences that social studies teachers provide their students--including the conservative effects of the models of non-inquiry provided by their own pre-college and college social science and history teachers, and the influnces exerted on teachers by their school-community social systems--is among that suggested by the more earthy ethnographic approach of CSSE.

Along with the indication that educational researchers have often not

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[15] See Berlak (1977)

[16] More frequent involvement of teachers as research partners, rather than as the subjects of research, is desirable if researchers are to identify research problems with greater relevance for teachers (Shaver, Davis, Helburn, 1978; Gajewsky, 1978).

addressed questions of interest to teachers is the implication from the CSSE ethnographic findings that researchers have erred one other way in their orientations. Educational researchers have failed to recognize that from a non-reform, "needs and demands of society" perspective much may be going on in social studies education that deserves praise. Jackson and Kieslar (1977) have argued that the orientation of educational researchers has been too narrow because of

> the almost total absorption with the goal of improving practice and discovering better techniques. We seldom ask whether educators might now be doing as well as can be done in many aspects of their endeavor. We might pay more attention to the possibility that educators may deserve and benefit greatly from some external confirmation of the appropriateness of much of what they are doing. (p. 15)

This discussion of the potential fruitfulness of ethnographic research is not, of course, meant to suggest that other types of research be abandoned for ethnographic studies. Nor should studies be directed only at determining what is good about current practice. However, the intent is to argue that the legitimacy of a greater variety of research methods be accepted. Also, the choice of appropriate research methodologies should more frequently be based on considerations such as appropriateness for the schooling setting and the problem to be addressed, and the state of knowledge in the area of interest (see Shaver & Larkins, 1973). These matters raise issues about the meaning of science and its relevance and demands for building sound knowledge about schooling (Shaver, in press).

Verified Theory? Another reason for the inadequacy of social studies education research findings as a basis for the prescription of practice is the overly simple model of instruction/learning that has dominated the design of studies. Most studies have compared one method of instruction against another with the hope of arriving at a general conclusion about efficacy

(see, e.g., Hepburn, Shrum, & Simpson, 1978, for this traditional approach). Complex interactions between instructional method, teacher characteristics, student traits, and situational factors have been considered too rarely. This oversight probably accounts in large part for the inconsistency in findings from one supposedly similar study to another which certainly limits the prescriptive power of the findings. Even, however, when a generalization does seem to appear from research studies comparing methods, caution in prescribing practice is in order. As Snow (1977) commented:

> . . .[T]he evaluation question is always, "Did the instruction work well for the students?" That is, for each student, not just for the few who stand in the vicinity of the group average. And an instructional treatment that is best on the average may still serve some students poorly. (p. 13)

Peter Martorella (1977) made the same point, but even more emphatically:

> All research generalizations, no matter how extensive their external validity, reflect only statistical probabilities about individuals. . . .[W]hile research apparently continues to add to our knowledge about individuals in general, it tells us nothing about any given individual. It may even distort our perspective on a particular student. (p. 44)

Such statements have serious implications for the development of general instructional theory, verified by research which can be used as a basis for prescribing the experiences to be provided individual students in social studies classes.

Snow (1977) has argued that "general instructional theory. . . is a holy grail" (p. 15). If instructional theory is to be developed at all, in his view, it must be local theory--i.e., specific to subject-matter, groups of students, and local situations--as well as time-limited (see Cronbach, 1975, pp. 122-3). This view is in direct contrast to that of many persons who advocate the "scientific" development of instructional theory--

> [modeling] their work on physical science, aspiring to amass empirical generalizations, to restructure them into more

> general laws, and to weld scattered laws into coherent theory. (Cronbach, 1975, p. 125)

The difficulty in applying the physical science theory-building model too literally is that

> rarely is a social or behavioral [or educational] phenomenon isolated enough to have [a] steady-process property. Hence the explanations we live by will perhaps always remain partial, and distant from real events. . . and rather short lived. . . . Our troubles do not arise because human events are in principle unlawful; man and his creations are part of the natural world. The trouble is. . . that we cannot store up generalizations and constructs for ultimate assemble into a network. (Cronbach, 1975, p. 123)

In broader context, the question must be asked, Is schooling-learning behavior too complex for traditional conceptionalizations of scientific theory to ever lead to bases for practice? Or, is Victor Weisskopf (1977), a physicist, correct in his implied optimism?

> The study of social relations between individuals of a given species--be it animals or men--is still in its infancy. We are groping to find appropriate methods, concepts, and languages that will enable us to arrive at formulations and conclusions that have an objective validity comparable to the natural sciences. (pp. 409-10)

Certainly, the meaning of science and the implications for building solid, cumulative knowledge about schooling generally, and in social studies in particular, have not been adequately addressed by educational researchers in general and social studies education researchers in particular. Science has made impressive strides in understanding the natural world and providing bases for engineering achievements. Whether the scientific modes of the physical sciences are applicable to instructional research; and, if so, which aspects and with what modifications, are questions that challenge the entire social studies educational research enterprise as it now operates. Yet, unless such questions are addressed, it does not appear likely that a less skeptical view of the usefulness of educational research findings for social studies curricular/instructional decision-making will be justified in

future years.

Conclusion

There are a number of reasons why social studies education research (as well as educational research generally) is not of much use to social studies teachers and others making curricular/instructional decisions in our schools. The same research findings and/or theory may suggest alternative, even conflicting, prescriptions for practice depending on other factual assumptions and one's value position; there are few cumulative findings about the effectiveness of differing curricula and instructional methods; researchers have tended not to address questions of interest to those operating in the "real life" of the school; the statistical significance model of research with its yes/no dichotomy and its inattention to explained variance and to the need to replicate findings has dominated the research; interactions among various instructional, personological, and contextual variables are so complex that prescriptions based on simple analyses of central tendency often do not apply to individuals; and general instructional theory that can provide specific prescriptions for instruction may well be not only currently nonexistent but unattainable.

Nevertheless, the skepticism in this paper about the current usefulness of educational research findings for curricular/instructional decision-making should not be interpreted as pessimism about involvement in the social studies educational research enterprise. It does, however, indicate belief in a pressing need to re-address the nature of that enterprise, if for no other reason than to avoid the ongoing waste of human and financial resources as it presently operates.

In particular, massive attention must be focused on our assumptions, or

lack of them, about how cumulative knowledge helpful in the guidance of practice can be built. We must ask and explore questions about the nature of science and those attributes of science that are applicable to educational research, about the role of theory in education and the possibilities of developing the type of theory we so desire, about the functionality of our research strategies and methodologies for building cumulative knowledge, about whether (Cronbach, 1975) our goals should really be any more than "to assess local events accurately, to improve short-run control" and "to develop explanatory concepts, concepts that will help people use their heads" (p. 126). We must become more self-conscious about our enterprise, more searching, more doubting about our traditional orientations and procedural modes.

There should be a suspension of knowledge-building research activities--as contrasted with the important, but less aspiring local "action-type" research--until we can get our epistemological house in better order. I know that such a hiatus is not likely to occur. There are too many vested interests, and the power of the Zeitgeist is too strong--even for me. If, however, even a sizeable minority would begin to address systematically the types of questions raised by the powerlessness of educational research in the face of the demands of educational prescription, the self-conscious, rational development of scientific orientations and modes appropriate to education might emerge as the new Zeitgeist--unless, of course, our efforts bring us to the menacing, but ironically humanistic, conclusion that human relations, and educational practice in particular, are too complex, subtle, and value laden to be illuminated adequately through the insights that can come through scientific types of research.[16]

[16] See Weisskopf (1977, pp. 410-11) for a provocative discussion of the limits of science for understanding human experience.

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