CHAPTER TWO

Research and the Purposes of Education

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Education research is a field of inquiry in which most work is applied and whose results, in the United States, are regularly the object of public interest and political passion. One possible reason for the interest and passion is the subject, which includes matters of considerable public importance: the effects of teaching, the nature of learning, the costs and benefits of schooling, and the schools’ role in reducing poverty, increasing productivity, and improving economic competitiveness. Yet interest and passion seem to vary among nations, for the results of similar inquiries are more placidly received in corporate and consensual societies, where there is less argument and more deference to authority. Citizens in such nations appear tacitly to delegate most decisions about the ends and means of schooling to specialists in professions, ministries, and schools. Another reason is history, or habit: Americans have disagreed about schooling ever since we have had schools; educational argument seems to be a national pastime. Seventeenth-century Calvinists vehemently argued about how to instruct the young, and the habit has persisted despite or perhaps because of increasing religious and cultural pluralism.

Still another reason for interest in research on schools is that our political system was carefully arranged to open it to popular access and to restrain government

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influence in education and other domestic matters. It is difficult to imagine a political system better suited to engendering arguments about schools, of which Americans seem so fond, or to open schools to the influence of those arguments. All manner of information about schools is used in and generated by the ensuing disputes, research included. Ours is a nation of part-time populists, who regularly dispute, subvert, and overturn the decisions of government officials and professionals in education. The past fifteen years of school reform, and argument about it, are the latest in a nearly unbroken era of reform and controversy since World War II. The growing movement for charter schools and choice is only the most recent evidence of efforts to restrict professional and government influence in schooling. Research has been widely used in both, and both developments have generated a good deal of research.

RESEARCH AND THE SCHOOLS’ PURPOSES

If it is not news that American arguments about schools have been a steady accompaniment to research on them, it may be a bit more surprising to find that political dispute and systematic inquiry often have been handmaidsens. Many researchers consider politics a distraction from the more dispassionate methods of social science, but in the United States at least, they often seem to be allied. One case in point arose in the 1840s, near the very beginning of the common school crusade, when allies of Horace Mann struggled with conservative school administrators for control of the Boston schools. Mann’s colleagues campaigned for a more humane and progressive pedagogy, more equal schooling for African Americans and the poor, and a more rational school administration. Several of these Boston liberals won election to the Grammar School Committee and then tried to launch what may have been the first organized education research program in U.S. history. They composed a new and intellectually demanding city-wide examination to assess the more progressive academic goals they thought schools should pursue. Members of the committee descended on the schools, where they administered the exam to all students, interviewed the grammar school masters, and observed lessons. They analyzed the data and composed a school-by-school report in which they depicted students’ performance in most schools as dismally weak. The central problem in students’ performance was their lack of deep knowledge and a parallel inability to reason about the questions they had been asked. Committee members portrayed the grammar masters and their antiquated, mindless, fact-and-memorization pedagogy and passion for discipline and control as the villains of the piece. Mann’s allies proposed a new curriculum and pedagogy that would be more intellectually ambitious and engaging for students. They also argued that a more rational school system would employ research, including regular assessment of students’ performance by a new central school agency, to monitor schools and inform cor-
effective action (Cohen and Barnes, 1995; Mann, 1868, 1951; Parsons, Howe, and Neal, 1845; Messerli, 1972; Katz, 1971).

Education research in the United States thus seems to have been born in a major political battle over whether public education should become more humane, equal, and rational or God-fearing, authoritarian, and traditional. Disputes over schools’ purposes were central to all the arguments, and research on how schools achieved their purposes was a central element in the reformers’ program and arguments about it.

Disputes about schools’ purposes, and related entanglements with research, became even more common six decades later in the progressive era. Educators, reformers, and public officials struggled to adjust public education to their various pictures of how a more urban, populous, diverse, and industrial society would work. Many researchers saw the schools’ purposes as a matter of helping industrial capitalism work better and aligned themselves with business-oriented reformers. E. L. Thorndike, George Strayer, and others envisioned America as a developing social meritocracy of ability and achievement married to a developing economy of efficiency. They sought to create schools that would help to align the society with the economy by educating young members of one for suitable positions in the other. In a small 1911 volume, Individuality, Thorndike wrote, “Specialization of schools is needed not only to fit pupils for special professions, arts, trades, and the like, but also to fit the schools to original differences in the pupils” (Jončich, 1962, p. 125). Applied research, in the form of school surveys, student testing, and curriculum evaluation, was central to their program. Thorndike cautioned education reformers not to “expect too much of education.” Rather, “like good men of science they should measure and know, and like good engineers” allow a factor of safety (p. 133).

These researchers believed that more efficient and meritocratic schools would better serve America, both by using modern management to improve systems’ performance and by matching students’ studies with their social destinies. Many educators also seemed to bet that such work would offer them a way to improve their academic positions, to do more consequential public service, and thus to improve their social situation. Schools would be guided by research on them and their students and, thus informed by science, would better serve the developing industrial order. Thorndike, Strayer, and others worked in an era in which the professionalization and institutionalization of social research had begun in earnest, developments from which they benefited and to which they contributed.

John Dewey, Lucy Sprague Mitchell, George Counts, and others of a more radical persuasion worked the same issues but from a very different perspective. They wanted a more equal and cooperative America, and they wanted schools to combat the evils of industrial capitalism and help build a more humane society. They opposed testing, curriculum tracking, and the influence of business on schooling. But research was not central to their program, for they relied more on argument than on inquiry and evidence. George Counts (1922,
1929) took a few opportunities to collect evidence on the operation and effects of the existing schools. The Eight-Year Study was a major effort to assess the effects of progressive schooling. But this work aside, empirical research did not figure prominently in the progressives' rhetoric of criticism or reconstruction.

Institution building was more prominent, but that was limited as well. With the brief exception of the Dewey School and a few longer-running progressive private schools, they did not try to invent examples of the schools for which they argued. Bank Street, Wheelock, Pacific Oaks, and Lesley colleges recruited and educated professionals for service in either mainstream or alternative schools, but they were a few drops in a huge and growing bucket of conventional practice. There is no evidence that progressives tried to turn these colleges into a broader national strategy to change schools by changing the people who worked in them.

Opportunities to link ideas about the schools' purposes, professional service, and research thus were more rare in this dissenting tradition than in the line of work that Strayer and Thorndike pioneered. For better or worse, professional service for the left-wing progressives came to be dissent accompanied by tiny islands of progressive practice, rather than efforts to inquire systematically into the problems of the public schools or the operation of alternatives. Dewey wrote extensively about education, and he offered thoughtful arguments for more equality and more challenging instruction in public schools. But his educational writings defined no agenda for further inquiry. Although his writing about the schools was salient to issues of great interest and importance, he rarely suggested lines of further inquiry, experimentation, and research. Even his Laboratory School, which he did bill as an experiment, did not seem to be systematically documented, with the exception of the Mayhew and Edwards (1966) volume many years later. Neither he nor any of his associates published any evidence on the school's effects for students.

As a result, left-wing progressives on the problems of the dominant school regime, the creation of educational alternatives on a large scale, and research on their effects never gained a foothold in either university research or the public domain. Dewey's ideas never became a regular part of the research and graduate education mainstream, which is one reason for their modest effect in public education. Dewey's very different conception of the purposes and practices of schooling remained outside the central organizations and research agendas of public education. Graduate research and education in education were instead largely defined by Thorndike's views, his agenda for inquiry, and his graduate students.

Research and public affairs became more entangled after World War II, as social research grew along with the phenomenal expansion of higher education, and school criticism and reform became a regular feature of public life. The era opened with a sustained chorus of university-led complaint about the purposes and methods that professional educators had embraced in life adjustment education and their mind-numbing effects in the schools. That attack on the schools'
purposes was soon followed by the U.S. Supreme Court’s decision in *Brown v. Board of Education of Topeka* (1954), which announced that the entire Jim Crow educational regime in the South was illegitimate and henceforth illegal (see *Brown v. Board of Education of Topeka*, 1951; and also *McLaurin v. Oklahoma State Regents for Higher Education*, 1950; and *Sweat v. Painter*, 1950). If ever there was a significant change in the purposes of American schooling, it was that move from state-sponsored racism.

Academic researchers played a central role in both developments. Scientists and humanists were the chief critics of public education in the postwar decade; Arthur Bestor, James B. Conant, and Admiral Hyman Rickover were among the leading figures, and the professors took a central role in the invention of new and intellectually much more demanding purposes for public schools. Their agent for this effort was curriculum, but the purpose was to shift the aims and content of schooling radically, at least for the more able students. Social scientists also played a central role in *Brown*: they brought evidence and argument to bear for the plaintiff’s claims and, as the desegregation cases grew, played an increasingly important role in efforts to define, create, and criticize a racially neutral school system (see, for example, the effects of segregation, Appendix to appellants’ briefs in *Brown*, 1952).

By the late 1960s social science had become the chief means for investigating the effects not only of desegregation but of Great Society school improvement programs, social policy more broadly, and the schools themselves, especially their effects on academic performance and achievement later in life. That development brought research closer to the center of political arguments about the purposes and effects of educational and social policy and about the fairness of American society, where it remains. One reason for that development was that research revealed, with growing force and clarity, how much education counted for occupational and economic performance later in life. If schooling counted for the things about which most Americans cared deeply, then research on schooling was no frill. Another reason for the growing importance of research was that just as Americans became more sensible of their growing entanglement with economies overseas (courtesy of competition with the Japanese over cars in the 1970s and 1980s) social researchers began to report how poorly American schoolchildren stacked up against their European and Asian peers. The message was that American students did poorly in a world perspective, but the medium was sociology, economics, and psychology. Social scientists reported on the differences among national school systems, explained the sources of the differences, and offered advice about what might be done (Stevenson and Stigler, 1992).

If the links between politics and research on schools’ purposes have often been close, they have not been simple. In some cases research that bore on the schools’ purposes was closely and deliberately tied to political arguments. That certainly was the case with Mann’s allies, much of the work of Strayer and
Thorndike, some of George Counts’s work, and many more recent studies of issues related to school segregation and other public policy matters. But in some other cases, research has had a more inadvertent bearing on public affairs. One increasingly common case in point is when conventional disciplinary work, like research on human capital formation, becomes salient to public affairs because social or political developments highlight issues of social investment.

Political concerns and research are directly linked in both of these cases, but as research has grown in volume and autonomy, more and more of it has been done within the academy. Although it bears on matters of public concern, it is increasingly carried out and communicated in specialized scientific channels. The growth of the academy thus has increased the difficulty of members’ communication with the society that supports them. Research and public affairs have become a species of parallel play; concerns and arguments within research roughly mirror those in the larger society, without contact between the two.

THE EFFECTS OF SCHOOLING

One of the chief ways that researchers have contributed to the conversation about schools’ purposes has been to issue dismal reports on how schools affect learning—or fail to affect it. Horace Mann’s allies stirred up a hornet’s nest of local controversy with their report that most grammar schools failed to educate most students. Joseph Meyer Rice did something of the same sort nationally at the end of the nineteenth century with his studies of how students in city schools across the country performed on the tests that he devised and administered. Some might say that Mann’s colleagues and Rice were not professional researchers, but these men did have some specialized knowledge, saw it as crucial to school improvement, and tried to invent more.

There were many more inquiries into the effects of schooling topics during the nineteenth and twentieth centuries (see Cohen and Neufeld, 1981). But what contemporary researchers would recognize as systematic inquiry into the effects of schooling began only after World War II, as survey research, mathematical modeling of social processes, and computerized information processing enabled large-scale data collection and analysis in which the attributes of many schools, students, and educational resources could be related. James Coleman’s 1966 *Equality of Educational Opportunity Survey* (EEOS) usually is seen as the first major effort of a scientific sort.

Although Coleman did not set out to create a dispute, his work nevertheless had some of the same effects—stirring up controversy by poking it with a stick of evidence—as had the work of Mann’s colleagues and Rice earlier. His study became central to political arguments that raged well past the report’s publication. The study was sponsored by the U.S. Office of Education (USOE), pursuant
to an obscure subsection of Title IV of the 1964 Civil Rights Act. With the leadership of Alexander Mood, an educational statistician then serving as assistant commissioner for educational statistics in USOE, Coleman and his colleagues had undertaken the congressionally required national survey of schools and students to report on patterns of inequality. But with Mood's agreement they also collected evidence that would enable them to analyze the relations among students' family backgrounds, schools' educational resources, and students' achievement.

Much to everyone's astonishment, the analysis revealed less inequality in access to resources than had been expected. Differences among schools in the allocation of such resources as libraries, teachers' experience and education, per-pupil expenditures, science labs, and other facilities were much more weakly related to students' race and class than had been expected. A larger surprise was that differences among schools in those same educational resources were weakly related to differences among schools in student performance. Differences among schools' libraries, their teachers' experience and education, their per pupil expenditures, science labs, and other facilities had little or no effect on differences in students' achievement. Although teachers' verbal ability was related to their students' performance, the most powerful predictors of students' performance were their parents' educational and social backgrounds, in comparison to whose effects school resources were trivial. Schools' social and economic composition was also related to students' performance: disadvantaged students who attended school with more advantaged students did better than those similarly situated who attended school with others who were disadvantaged.

Coleman's report was startling and corrosive, for the results were starkly opposed to long-established professional opinion and public belief about schools. His report raised fundamental questions about public policy, including the wisdom of investment of more dollars in schooling (Mosteller and Moynihan, 1972; Moynihan, 1968). Despite the astonishment, the results had not come out of scientific left field. Many small studies in earlier decades had shown that the school resources conventionally thought important were weakly and inconsistently related to student performance. A large and quite compelling study—Project Talent—had been undertaken nearly a decade earlier with no public fanfare and produced similar results. Project Talent was a longitudinal study of students and secondary schools that enabled researchers to weigh the effects of school resources on students' achievement near the end of high school, given knowledge of students' achievement at the beginning of high school. The analyses were published in blandly inconspicuous scientific reports that offered even more convincing evidence about the weak differential effects of educational resources on student achievement.

The EEOC confirmed this large, and largely unknown, study, but because it was a government report and because the civil rights movement and other policy changes had increased interest in liberal social policies, Coleman's study...
became a central element in political dispute about education. Although it had been intended chiefly to report on the extent of inequality in the schools, it helped to usher in a period of much more vivid public controversy about the schools' effects on learning and the implications for educational policy.

Coleman's report on the effects of schools' social composition encouraged political liberals, but everything else about the report discouraged them. That effect was fed by a small eruption of disappointing evaluations of Project Head Start, Title I of the 1965 Elementary and Secondary Education Act (ESEA), and other Great Society school improvement programs. The evaluations were undertaken or sponsored by social scientists who went to Washington, D.C., in the New Frontier and Great Society eras. They hoped to improve the effectiveness of government programs by improving knowledge of their effects. Although they were more technocratic than political in their intentions, the evaluation reports followed on the heels of the Coleman study, during the slow collapse of President Lyndon B. Johnson's ambitions for domestic reform and the growth of the conservative movement in American politics. The combination created something of a crisis in education and education research, for accumulating evidence seemed to suggest that conventional educational resources and ameliorative interventions were weakly effective at best (McLaughlin, 1975). These reports were coupled with persistent evidence that what counted most for students' performance was their families' social and economic status, their race, and their own incoming school achievement. That led many to conclude that researchers were claiming that schools "made no difference," an idea that was reinforced in 1972 by the publication of *Inequality* by Christopher Jencks and several colleagues (1972).

No one had claimed, or could have claimed, that schools made no difference. For one thing, no researcher had compared the performance of American students to that of their peers in another nation that was in every respect similar, save that there were no publicly provided schools. For another, there was the evident fact that students learned a great deal in school; algebra and French do not spring spontaneously to the minds of American adolescents. Coleman's, Jencks', and other studies showed only that when schooling was nearly universally provided and when the allocation of educational resources among them was relatively equal, differences among schools in those resources did not seem to be strongly related to differences among schools in students' performances. Although there were great differences in average achievement across schools, and especially troubling achievement differences between schools that enrolled the children of affluent and poor parents, differences in the educational resources that most people thought significant, like money spent or the education of teachers, were at best weakly related to student performance differences.

But even on a cautious interpretation of the research, it seemed to suggest that allocating more money to schools that enrolled many more poor children
would be unlikely to improve achievement, a conclusion that was powerfully reinforced by the disappointing evaluations of programs like Title I of the 1965 ESEA, which entailed just such compensatory resource allocation. Conservatives used the evidence to call for cutbacks in Title I and other liberal programs. That troubled liberals, but the evidence was even more troubling because it appeared to suggest that schools did not serve, in Horace Mann's often-quoted phrase, as "the balance wheel of the social machinery." Rather than reducing the educational effects of America's larger social and economic inequalities, schools appeared to transmit and perhaps harden them. (On this point, see the varying views of Bowles and Gintis, 1976; Jencks and others, 1972.)

In reaching that conclusion, commentators forgot that no one had done research on that nonexistent-comparison America that was in all respects similar to the one Coleman and others investigated, save that public schools did not exist and schooling could only be purchased in the market. Had such miraculous research been done, investigators could have estimated both the effects that having a public school system had on the inequalities in knowledge and skill with which students entered school and on the economic and occupational consequences of schooling. This point was lost in the arguments, even though a moment's thought would have revealed that in societies in which schooling could only be purchased in the market, differences between affluent and poor students in access and achievement would be much greater than in societies in which nearly everyone attended school.

One reason that these developments were so startling was that the research challenged the American faith in the power of exposure to school. Since the eighteenth century we had thought that social environments were a powerful influence on the development of mind and that better environments could cure all manner of mental and moral problems. It was thought that schools, asylums, prisons, libraries, and other formal institutions could undo the damage done by irresponsible families, slum neighborhoods, papist doctrine, family poverty, and other social evils (Rothman, 1971; Katz, 1971; Cohen, 1985; Ravitch, 1974; Kaestle, 1983). But research and evaluation seemed to challenge that faith directly. In addition, they questioned the commonplace educational resources—teachers' education, books, science labs, money spent on schools, and the like—that educators had long told Americans would make better schools better. These were the indicators that parents and teachers used to make judgments about the quality of education—and the resources for which Americans regularly paid state and local taxes.

It seemed incredible that everyone should have been so completely incorrect, especially when educational opportunity played such a central part in American ideas about equality and social policy. On some accounts, equal educational opportunity was the American substitute for social policy in employment, welfare, and family support (Katznelson and Weir, 1988; also Wier, Orloff, and
Skocpol, 1988), so if schools did offer equal opportunity, then little else need be done. But if educational opportunity was as unequal as the research seemed to suggest, something was very wrong with the idea that America offered equal opportunities for accomplishment and with the view that schools were the central agent of equality.

However incredible, the subversive findings turned out to be influential, and in surprisingly different ways. Some conservative commentators embraced the results and used them to support their skepticism about Great Society programs and liberal social policy. President Nixon and his advisers—who were no friends of social research, regarding researchers as a generally hostile group of liberals—later used the results as part of the justification for proposals to turn Title I of the 1965 ESEA into a revenue-sharing scheme. They argued that state and local officials would know better what to do with the money than remote federal bureaucrats.

Less predictably, President Nixon accepted the plan of Daniel Patrick Moynihan, at that time the president's chief adviser for domestic affairs, to improve research and proposed to create the National Institute of Education (NIE) to do more research on the issues that Coleman and others had raised. The idea made friends on both sides of the congressional aisle, and became law. For the first time in U.S. history, a federal agency sought to improve education by investigating how schools worked. Studies that had seemed to create a crisis in liberal social policy and the reputation of education research somehow also helped to set a more focused and prominent mission for education research and to promise a rosy future for research funding.

The NIE did not have a happy life, and its funding soon looked less rosy. But the subversive research that contributed to its invention also helped to stimulate a contrary line of inquiry, much of it funded by the NIE. Beginning in the early 1970s, research on schools, teaching, and learning began to prosper. Many researchers moved from a focus on schools to teachers, in an effort to figure out whether some teachers were unusually effective and why. David Berliner, Jere Brophy, William Cooley, Tom Good, and Gaea Leinhardt were among the leading figures in this line of work, and Brophy summarized the evidence in the mid-1980s. There were unusually effective teachers, as judged by the magnitude of students' gains on standardized tests, and their practice seemed very different from that of their less effective peers. More effective teachers seemed to plan lessons carefully, select appropriate materials, make their goals clear to students, maintain a brisk pace in lessons, check students' work regularly, and teach material again when students seemed to have trouble learning (Brophy and Good, 1986). They also spent much more time on instructional tasks (Cooley and Leinhardt, 1985; Berliner, 1979). Such teachers had coherent strategies for instruction and deployed lessons, books, and other resources in ways consistent with the strategies. They believed that their students could learn and that
they had a large responsibility to help. They had definite objectives and organized instruction to achieve them. Although typically quite traditional and didactic, these teachers' lessons were well thought out, well organized, and well paced. Typically the teachers used conventional tests and texts but as part of a well-crafted strategy to improve children's learning.

Many other teachers did not have coherent strategies for instruction and deployed resources in a scattered and inconsistent way. They had vague objectives, and their lessons were not well thought out or well organized. Classroom work typically was badly paced, and teachers either did not regularly check to see how students were doing or, if they did check, did not make many midcourse corrections to accommodate students' responses. John Goodlad (1984) reported that more than half of elementary students told the members of his research team that they "did not know what they were supposed to do in class." Teachers of this sort did not exert themselves to make educationally fruitful connections with students; many acted as though they believed that their responsibility was only to "present the material" and let students get it if they could. When such teachers worked with children from disadvantaged circumstances, they often acted as though students could handle only watered-down instruction.

Other researchers did similar work at the school level, probing connections between schools' collective characteristics and student performance. Ronald Edmonds began by trying to figure out whether some schools produced unusually large achievement gains, and when he found some, he tried to identify what distinguished them from run-of-the-mill schools. Edmonds and others reported that faculty members in unusually effective schools shared a vision of the purposes of instruction, agreed that their school's purpose was to promote students' learning, and believed that they were responsible for helping students to learn. Teachers had a strong commitment to students' academic success, and their principals' leadership helped to create and sustain these beliefs and practices (Edmonds, 1979, 1984).

Critics soon pointed out crucial methodological problems with this line of work, including interannual variation in effectiveness (Rowan, Guthrie, Lee, and Guthrie, 1986) and problems in reasoning from schools with unusually high performance to more ordinary schools (Purkey and Smith, 1983). The methodological problems might have kept researchers away if they had closely followed their methods textbooks, but they pursued Edmonds's insight anyway, embroidering and deepening it (Purkey and Smith, 1985). Despite the problems, his work offered an appealing way to depict the differences between strong and weak schools.

This line of inquiry gained credibility as evidence accumulated. A group of researchers collaborated on the most comprehensive study (Bryk, Lee, and Holland, 1993), changing this tradition of work as they capitalized on it. They found that more effective high schools were more likely to have teachers who shared
a commitment to their students and believed that they had an obligation to help students achieve academic success. Faculty members in these schools were collegial, had extensive contact with students in and out of class, and had high morale. Students were likely to study the same curriculum, for there was little curriculum tracking. Anthony Bryk and his colleagues portrayed effective high schools as communities in which students and teachers took responsibility for each other academically and socially. Although students in such schools had higher achievement, what is especially noteworthy is that achievement differences between advantaged and disadvantaged students decreased over the high school years. That reversed the pattern in typical secondary schools, which were much more fragmented and anomic. Teachers, staff members, and students in these weaker schools shared no common vision of instructional purposes and made no common commitment to students' success, and there was no common curriculum. There was instead low morale, with little collegiality or contact among teachers and students outside class, and achievement differences between advantaged and disadvantaged students increased over the high school years.

This sketch of recent research on the effects of teaching and schooling reveals some surprising features of research on schools' purposes. One is that seemingly dismal reports on the schools' achievement of their purposes had constructive as well as some worrying consequences, because one element in research decisions is a dialectic among researchers. Research that seemed so negative in the context and afterglow of the Great Society stimulated researchers to ask questions they might never have asked had the troublesome work never been done. Another is that opponents of research sometimes advance it when inquiry about the purposes and effects of schooling suits their purposes. Richard Nixon's electoral victory reminded educators and researchers that support for schools and ameliorative social programs might not continue forever on faith. More evidence that social programs were ineffective could encourage changes in the politics and economics of education that would damage research and schools. The conservative turn in U.S. politics, which seemed an enemy of investment in schooling, offered researchers incentives to investigate schools' effectiveness, a development that was repeated in the 1980s and continues today.

Thus the previous two decades saw both unparalleled dispute about schools' purposes and effects and a remarkable flowering of research on those matters. One reason for the flowering was that research came to seem more useful across the political spectrum. Liberals found it increasingly useful because, for the first time, research on the purposes of schooling raised fundamental questions about the schools' effects and effectiveness, and public discussion of the research alerted them to the importance of such knowledge. Political conservatives increasingly found education research to be a handy tool as cross-national studies reported dismal comparisons between U.S. and other nations' schools, and
national surveys revealed similar patterns. Because these findings reinforced conservative critiques, they supported expensive national and cross-national studies in the 1980s that they might earlier have brushed off as politically irrelevant or a frill. But as conservative proposals for charter schools have been adopted, conservatives have found it useful or necessary to support or submit to the same sorts of research and evaluation that they earlier urged on public schools and liberal programs (Berman and others, 1998; Manno and others, 1997; Witte, 1994; Greene and others, 1996; Greene and Peterson, 1996).

**EFFECTS OF RESEARCH ON THE EFFECTS OF SCHOOLING**

Research on the schools’ purposes and effects has had larger impacts as well. One is that research on schooling plays a larger role in public discourse about education now than it did thirty years ago. It figures more prominently in executive branch discussions of schooling and in congressional deliberations. It is used increasingly to justify and attack programs, and it has a more prominent role in foundation reports and the actions of state governments. In these ways and others, research has helped to change the ways that Americans think about schools and how they act on them.

These changes did not occur because particular studies shaped particular decisions, for social research typically is not influential in that way. The changes are instead associated with the growing role of social research as a language for advocating, explaining, and justifying ideas and decisions. This influence of research might be regarded as linguistic: research shapes the ideas, claims, and evidence that are in good currency in political discourse by shaping the vocabulary and syntax of thought and speech. Research on the purposes of schooling has helped to alter Americans’ ideas about those purposes and the schools’ part in achieving them.

One key example of such influence is the increasing focus on results in public and professional discussion of schooling. Before the 1960s public debate about schools was focused much more on the allocation of resources, the availability of access, and the content of curriculum. There was little systematic attention to results, even though they were tacitly assumed to be implied in resources allocated and curriculum used.

The initial and absolutely fundamental contribution of Coleman, Jencks, and other researchers was to raise deep questions about that assumed connection; by the late 1960s resources no longer could be assumed to ensure student performance. That was a fundamental rupture in inherited ideas about schooling, and it occurred as the result of efforts to model schooling processes mathematically, in input-output terms. One might say that the formal models only made explicit what educators and citizens always had assumed—that students in better schools
did better because their schools were better. But the process of trying to fit an input-output model to data on school resources and student performance enabled Project Talent researchers and Coleman explicitly to test an assumption that never had been held up for inspection on any scale. The result dramatically called attention to the lack of those direct connections between resources and results that nearly everyone, including the researchers, had expected to find.

The rupture had several effects. One was to help bring school outputs out into the open as the chief focus for both schools and research. Many critics and commentators responded to the research and evaluations by arguing that if conventional resources did not produce the desired school outcomes, something else should be done. The late 1960s and early 1970s thus saw the first eruption of efforts to focus schools explicitly on results, in proposals for performance contracting, outcome accountability, and program evaluation. None had the desired effects, but all were part of an increased effort to reorient schooling to results.

In fact, the continuing failure to find tight connections between resources and results created potent incentives, in and around the research enterprise, to figure out what was wrong, in order to explain why the connection was weak and to figure out how it might be strengthened. The apparent ineffectiveness of schools and programs created opportunities for skeptics, conservatives, and advocates of efficiency to call for scientific evaluation determining whether schools and interventions improved students' achievement. That rhetorical stance and the ensuing evaluations helped to keep the focus on results. The broken connection also put liberals, service providers, and program advocates on the defensive, increasing the incentives for them to invest in research and acquiesce in the novel idea that intervention programs, which had once seemed self-evidently desirable, should be evaluated to see if they improved results (McLaughlin, 1975).

One consequence was progressive public and political attention to what had been termed "accountability." In the 1970s state governments began to collect and publish data on student performance, often school by school—an idea that would have been treated as unthinkable just a decade before. The motives for these initiatives were mixed, but one was to encourage schools to pay more attention to student outcomes by making the information public and allowing citizens and educators to compare and draw their own conclusions. The concern with accountability has grown since then. Most states now have well-established accountability systems, and a few state and local agencies have quite sophisticated means of collecting and publishing data on schools' performance. These are premised on the sense that schools should be responsible for students' performance and the belief that schools should be penalized if they fail to produce results. Recent state school reforms in Texas and Kentucky are perhaps the clearest expression of those ideas.

Another stimulus for the attention to results was research on the role of schooling in status attainment and economic performance. Decades of research
had shown that, on average, students who stayed in school longer got better jobs and earned more money. Some recent work suggests similar payoffs to student achievement (Jencks and Phillips, 1999). Researchers also reported evidence that the occupational and economic importance of schooling has increased, probably because many jobs require more knowledge and skill than formerly. This work grew more sophisticated and convincing as it used longitudinal rather than cross-sectional data and offered evidence pointing to the causal links among school attainment, income, and occupational status.

These developments added resonance to the sense that results were the appropriate way to think about schools; they also drew public attention to results. Reports of schools’ performance are by now a regular feature in newspapers, magazines, and other media. Newspapers publish annual state assessments; critics argue about the meaning of the reports, and they lock horns about the appropriateness of particular assessment instruments. Concern about the schools’ production of results never has been higher, and arguments about results have never been more intense. The movement for standards-based reform is only the most recent and significant manifestation of results-oriented thinking about schools, for a central idea in the movement is that schools should be responsible for achieving explicit outcomes, based on clear academic standards (Resnick and Resnick, 1989). This movement has helped to intensify attention to results as reformers and researchers try to conceptualize and design assessments that will more faithfully represent standards for school performance and offer students more opportunities to display their thinking. One mark of the change is that among the many arguments about standards-based reform, there has been little dissent from the idea that school improvement should focus on school outcomes.

Social research thus has increasingly shaped the language used to understand and discuss social and educational problems. It has especially focused attention on students’ academic performance, as that is measured in formal assessments. And these changes in public discourse have been accompanied by others, also drawn from the vocabulary and syntax of social science. Input-output models of social processes, a key feature of the new language arising from modern economics and sociology, have helped to frame the focus on results by picturing schools as agents of production. The idea of schools as learning “factories” is at least as old as Thorndike and Bobbitt, but only in the past three decades has there been an analytic apparatus that social scientists can use to model schooling processes formally in input-output terms and thus help reorganize political and public debate around such models.

To argue that research has had a linguistic influence is not to imply that this is a matter of mere talk. Language often shapes behavior by influencing how we read and define situations, what problems we see, how we define solutions, and what action we believe is possible. Franklin D. Roosevelt’s language had a
significant effect on American politics and social policy, in part because he spoke about the Great Depression in ways that recognized privation and legitimized ameliorative action. Hitler’s language had a powerful effect on German behavior in the 1930s and 1940s, in part because he used it effectively to mobilize national pride, prejudice, and a sense of collective identity and to legitimize violence against Jews, dissenters, gypsies, and homosexuals. President Clinton’s success in using language to portray congressional Republicans as dangerous enemies of education and health care in 1995 and 1996 assisted his legislative efforts at the time, his subsequent campaign for reelection, and the 1998 congressional elections. The language of the Supreme Court’s decision in Brown v. Board of Education of Topeka had an enormous effect on Americans’ behavior in part because it accepted that black Americans had been right all along about the injustice of Jim Crow, that white America had been wrong, and that remedies were in order. The Court thus legitimized black expression, which had long been politically buried, and led to a range of actions from lunch counter sit-ins to voter registration.

Changes in the language of social research seem to have had similar consequences. For instance, when Title I of the ESEA was initially passed in 1965, the legislative focus was on channeling more educational resources to the schools that disadvantaged students attended. Advocates may have assumed that students’ achievement would improve, but there was only the most modest attention to results (McLaughlin, 1975). When Title I was reauthorized in 1994, however, the legislation focused heavily on results: it now calls for states to set explicit standards for student performance, for Title I no longer aims to channel resources to schools but to improve student performance. The legislation also proposes to hold schools and districts responsible for performance. States are required to hold local schools and districts “accountable” for student performance, offer assistance to schools failing to improve that performance, and close schools or reassign staff if they do not succeed. That is a remarkable shift, in both the nation’s single most important federal public school program and the very idea of government responsibility in school improvement. With Title I and Goals 2000, the Clinton administration declared that student outcomes were the purpose of school improvement and that government was responsible for those results. Although the administration’s position capped developments over several decades, it was a momentous change.

The change was not isolated. The past decade has seen an unprecedented migration of researchers into school improvement efforts, all seeking to improve student achievement. Robert Slavin, an education researcher at Johns Hopkins University, used a program of research to define, test, and develop Success for All, an elementary school intervention designed to improve achievement for disadvantaged students. Henry Levin, an economist of education at Stanford University’s Graduate School of Education, developed the Accelerated Schools
Program, with roughly the same purpose. James P. Comer, a Yale University psychiatrist, developed an intervention program for young children that seeks to improve education for disadvantaged students by improving family–school connections and social service coordination. Bryk, an education researcher at the University of Chicago, has taken a central role in the Chicago school reforms, including the development of an intervention program to improve literacy performance in low-income schools. E. D. Hirsch, a professor of English at the University of Virginia, has developed an intervention program designed to improve performance by focusing on traditional conceptions of academic subjects.

The entry of these and other researchers into school improvement is unprecedented, in both the researchers’ focus on results and their effort to work in practice. They have begun to redefine the role of research in schooling by redefining the researcher’s role. Of course, some academics previously tried to improve schools by doing research on learning and teaching, developing curricula, and writing texts, but few worked directly as school improvement. Dewey and Thorndike wrote texts and devised tests. The researcher-intervenors discussed here assumed that changing schooling will require more than such isolated academic instruments as tests, texts, or a laboratory school. If our view of the influence of social science is roughly correct, then the increased focus on results (due in part to changes in the language of research itself) has enabled research to become a more direct agent of school improvement and the researcher to become a more direct agent of change.

For an account of this sort to be convincing, however, it also must explain why Americans might have been receptive to the language of research. The supply of research alone could hardly be the cause of more influence, since research journals are not required reading for citizens and officials. A more plausible explanation lies in the increased importance of expertise in many enterprises and occupations and the expanding number of channels for the use of expert knowledge. For instance, nonprofit foundations were staffed in the 1950s by well-educated amateurs who had little specialized professional preparation. Now these agencies hire specialists with advanced degrees who were educated as researchers and speak that language. Foundations increasingly use research in their deliberations, in various task forces, and in reports that they hope will influence public opinion.

The growth of analytic agencies in and around government was a similar development. There were only a few state and federal agencies of this sort in the 1950s and even fewer nongovernment think tanks. Both sorts of offices have multiplied since the mid-1960s and are staffed by a new class of knowledge professionals who have specialized professional degrees in various social sciences. They conduct and manage research and other advice based on research to consumers in and out of government. The existence of these agencies and the presence of social science–trained professionals have encouraged a larger role for
research in conversations within executive and legislative agencies and in relations between such agencies and other groups.

The demand for social research thus can be traced at least in part to a larger population of specialized analytic agencies and professionals whose work concerns social problem solving. More agencies now define their mission as social problem solving through analysis, and more people are paid to speak and write about social problems and public policy. These professionals have learned to speak the language of social science and use it to frame problems and possible solutions. They read research and consult researchers because they have learned that is where authoritative knowledge is found. And they work in agencies whose mission and survival depend on the use of research. Both the agencies and their staffs have been supported partly by university social science departments and policy analysis programs that specialize in educating professionals to work in just such agencies. Postgraduate education of that sort was entirely unavailable in the 1950s, but the programs have prospered since then, sending thousands of students to work in analytic offices, government, private agencies, foundations, and community agencies.

**SCIENTIFIC PROGRESS?**

Thus far we have told a tale of increasingly sophisticated understanding of and agreement on how schools work. Beginning from research that seemed to undercut schools’ claims to effectiveness, knowledge about teaching and schooling has produced consistent evidence of educational effectiveness. One distinctive thread in the new work has been the broadened definition of educational resources to include teachers’ goals and strategic actions, their professional commitments and knowledge, the knowledge that they have and the skills that they deploy, and how these enable them to guide students’ use of materials and facilities. Researchers have also included in educational resources various collective attributes of schools and classrooms, such as leadership, shared goals, and collegiality.

These shifts mark a continuing movement in scholarly interest away from conventional conceptions of educational resources—such as teachers’ qualifications and school facilities—toward particular instructional practices and organizational arrangements and the knowledge, skill, and culture that these entail. Educational inquiry also increasingly defines resources in terms of their salience to outcomes, rather than assuming that connection. None of this denies the significance of instructional materials, facilities, and teachers’ formal qualifications; it implies, rather, that their salience depends on how teachers deploy them. Materials, facilities, teachers’ formal qualifications, and the like are only
potential resources in this view. Whether they become actual depends on what teachers make of them, and that depends on teachers’ knowledge, skills, and professional commitments, as well as on the organizations in which they work.

This new picture of educational resources is not simple. The differences that researchers discern in schools’ and teachers’ effectiveness are subtle and relatively difficult to manipulate because the resources are complex and because manipulating professional action, knowledge, and norms is a more complex undertaking than increasing salaries, building better schools, and revising standards for hiring. The new ideas about resources are less easy for policymakers or members of the public to grasp than money, bricks, and mortar, nor do they lead easily to clear policy initiatives. Worse, knowledge about schooling has become more subtle at the same time that public life in the United States has become more simplistic; as research complicates our view of schooling processes, American politics has grown more addicted to partisan and ideological sound-bite sloganeering.

That contrast is particularly important because the recent research seems to imply more professional and less political influence on schools. If key educational resources center on professional action, norms, and knowledge, then inventing a more accomplished professionalism will be one key to school improvement. Such professionalism can be supported and encouraged by public agencies, but it can be done only by the professionals in question. One of the most prominent policy innovations of our era has been the National Board for Professional Teaching Standards (NBPTS). This private professional group has devised standards and examinations for school teachers in an effort to raise standards for teaching and place governance of the standards and examinations in the hands of professionals rather than government.

American politics does not offer many openings for such work. Policy initiatives come and go in a brief and unstable issue-attention cycle, in which state and federal policy goals, programs, and issues change regularly. There is little time for careful analysis and policy formation or for learning from experience with previous initiatives. The politics of education reveals increasingly deep division about the purposes of schooling. Although the recent research seems to imply more deference to professionals, politicians seem inclined toward more ideologically inspired intervention, and there are sharper partisan differences over education policy. The NBPTS has thus far secured significant state and federal support, but its growing success has been attacked.

Research cannot solve these problems, for they are political and professional in origin. But researchers could make a more subtle contribution to solving them if they produced more substantial results and presented them with more convincing authority. Attention to and support for research in economics or health care has not visibly suffered from the recent increases in ideological warfare and
partisan conflict, and it is just at this juncture that the frailty of social science
progress in education becomes apparent. Our earlier account stressed the impor-
tant work that has been done since the early 1970s, but it did not mention
equally important work that has not been done. For instance, studies of effect-
tive schools seem simply to have stopped after producing suggestive results. No
one devised and tested interventions based on the research, nor did anyone then
organize field trials of the surviving designs. Similarly, research on effective
teaching did not progress very far from studies of the unusually effective teach-
ers. Brophy organized research and experiments that tested some of the ideas
derived from his field studies, but the logical next steps for this entire program
of inquiry, like larger experiments with well-defined treatments, were not tried
(Mosteller, 1972). The same could be said of work that William Cooley and
Gaea Leinhardt and other researchers began. Sponsors and researchers instead
moved on to new topics, leaving suggestive but incomplete work behind. The
research opened up important new lines of inquiry, but because the studies
were typically small and used varied methods, it is unclear how far anyone can
generalize from them. These limitations also mean that there are no general esti-
mates of the magnitude of effects on students’ achievement relative to other
influences. In addition old issues have resurfaced as several researchers claim
to have shown that traditional resources do influence student performance, as
do, according to evidence from an experiment, sharp reductions in class size
(Hedges, Laine, and Greenwald, 1994; Mosteller, 1995; Mosteller, Light, and
Sachs, 1996). The researcher-initiated interventions sketched a few pages ear-
lier, however, are reported to have produced little of the expected effects on stu-
dent achievement (Millsap et al., 1997).

Researchers have improved understanding in the sense that they have opened
up new vistas, raised more fundamental problems, and offered suggestive evi-
dence. But they have not produced convincing theoretical formulations, con-
sistent and compelling findings for the new ideas, or convincing support for
courses of action (see, for example, Cohen and Weiss, 1977, on school deseg-
geration). Moreover, although researchers agree on many matters, they have not
yet reached scientific consensus. Some critical issues, like the cumulative effects
of teaching on students’ performance, have gained little attention, and some old
issues remain undecided.

How this bit of history turns out will depend partly on developments in the
next decade or two. If researchers organize to encourage more cumulation of
knowledge on the issues discussed here, more convincing advice based on solid
evidence might be possible. But that would require researchers to create stronger
social guidance for topic choice in investigations of schooling and stronger
Canons about acceptable research methods that would encourage more compa-
rability in studies. It would also require incentives for cumulative research pro-
grams and support for research, development, and assessment of interventions, including experiments of several sorts.

Such things are technically possible, but they would be quite difficult to arrange. One reason is that they would be very costly, requiring many times the amount of money now invested in education research and development. There have been precious few sustained efforts to link research and improvement, let alone to link them in ways that are well conceived, carefully managed, adequately supported, and closely tied to sound evaluative research. One reason has been the pitifully weak support for educational research and development, much bemoaned among researchers. But a more fundamental reason is that the education research and development field is so diverse and diffuse, and intellectually so weakly governed, that education researchers never have come together to devise a rational agenda and seek support for it.

Another reason that more cumulative research would be difficult to arrange is that it would require more consensus about methods of inquiry in education research than now exist. Creating consensus would cut across many competing grains in the field. Cumulation of knowledge is a serious problem in large areas of social research, partly because the enterprise cannot depend on replicated studies to winnow out weak hypotheses. Historical change and context variation change relationships and threaten validity in social studies of nearly any sort. Few competing hypotheses are entirely invalid, so many competing explanations coexist. The relative absence of such threats in the physical and biological sciences means that there are real individual incentives for investigators to replicate others' studies and real intellectual payoff in such work. But in social science these incentives are reduced by historical and situational variation, the partial validity of many hypotheses, and the ensuing incentives for researchers to make reputations by focusing on one of several hypotheses, thus dispersing knowledge. Scientists can achieve more cumulation only by the sort of self-conscious priority setting and guidance of research decision making that we sketch here.

Even if some greater consensus were achieved, success in such an enterprise also would require persistent good judgment in the collective guidance of education research—something that would be difficult to arrange even for a few years, let alone over the longer term. It is much easier to rely on a combination of very general guidance and individual incentives in the marketplace of ideas. That works in sciences in which knowledge structures are strong, but in social studies of schooling it has had the perverse effect of encouraging a species of small-scale buccaneering individualism that reduces collective understanding and knowledge cumulation. One could argue that the rather fragmented character of research fits with the divided and episodic nature of politics and public discourse about schooling in the United States. Politics and public discourse
may support fragmentation within research and decrease the possibilities for cumulation by offering researchers incentives to contribute in fragmented and episodic rather than cumulative ways. The irrationalities that some researchers have discerned in the relationship of public discourse, politics, and research may be as much an artifact of the culture of American politics as evidence of inherent incompatibility.

CONCLUSION

This account of research on the purposes of schooling offers a somewhat surprising view of the relationship of research on schools' purposes, politics, and policy. The past three decades have been a period of growing political and ideological conflict about schools, yet also of broadened interest in and attention to education research and in improving research. The improvements are encouraging, but researchers have not capitalized on that work with efforts either to deepen and confirm findings or to make more rational decisions about research programs. We see hopeful signs, including growing attention to differential effectiveness among teachers and schools and more sophistication in the investigation of such matters. There also has been the beginning of a tradition of research-based, knowledge-generating clinical intervention in schooling. That sort of endeavor has been missing from the education research enterprise for nearly the entire century of its existence, crippling its capacity to change the social allocation of resources to schools, observe the consequences for students and educators, and inform practice and policy. The lack of systematic intervention that is linked to careful research also has contributed to the scattered and frequently inconclusive character of research and the inability to decide what had been solidly learned from a very important tradition of deliberate inquiry. If some elements of a more fruitful approach have appeared, our consideration of them should help to reveal how much remains to be done.

References


