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AUTHOR	Cohen, David K.
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ABSTRACT

The question considered in this essay is: Given what we know about research and development in education, what might reasonably be expected from a National Institute of Education (NIE)? Two suggestions are made. One way to figure out what an NIE might be good for is to make up a list of what's wrong with American education and dream up some plausible ways in which R&D might help. A more cautious way of proceeding might be to set out what we know about education and R&D in advance and try to infer from that what might reasonably be expected from a government R&D agency. Subjects discussed include: educational problems, defining goals for an NIE, the knowledge base, utilizing R&D results, and definition of research and development. Consequences for NIE include: (1) Accepting a "major problems" mandate is not likely to reduce the potential for instability in an NIE; (2) Tendency toward political instability will be compounded by the fact that evaluative criteria for educational R&D are murky; and (3) There is a formidable system of constraints and there's no simple way to avoid the problems they pose. (For related document, see AA 001 048.) (CK)

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THE NATIONAL INSTITUTE OF EDUCATION: WHAT CAN BE EXPECTED?

David K. Cohen

Center for Educational Policy Research
Graduate School of Education
Harvard University

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NATIONAL INSTITUTE OF EDUCATION
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Edited by Mary M. Dupuis, Ph.D.; Harold E. Mitzel, Ph.D., Project Director, Grant No. OEG-0-71-3636(515), College cf Education, The Pennsylvania State University.

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I. INTRODUCTION

If ever an ironic example of the American faith in education were wanted, it lies in the current enthusiasm for a National Institute of Education. An agency which would focus educational research and development on high priority school problems never would have been realized had it not been for the current wave of uncertainty over schools. One would have thought that the poor results of federal compensatory programs, the discouraging results of the Coleman Report, and the apparently minor effects of desegregation on achievement would have produced real skepticism abcut the efficacy of schooling. But while a little of this unfamiliar emotion seems to have been created, the evidence of failure seems mostly to have strengthened the conviction that with more money, or better methods, or less bureaucracy, or more responsiveness, schools could be more effective. And if the words of its planners, sponsors, and supporters can be believed, the National Institute of Education will help. By focusing research and development efforts on the nation's most pressing educational problems, it is argued, an NIE could set America's schools on a new and more productive course. The Congressional hearings on NIE are predictably laden with notions of this sort, but so are the planning documents. Thus, at the outset of a draft of the NIE Planning Team I report (March 21, 1972), we read that

> through development of a more adequate knowledge base the Institute is to help elucidate and help remove the impediments to education... In order to start out on such a task, the Institute must first try to determine what the most important problems troubling the process are . . . and devise promising strategies for their amelioration (Raizen, 1972; p. 1).

Perhaps such an argument is acceptable, but it might be equally fair to infer that research, development, and experimentation would produce no less discouraging results in the next ten years than they have in the last five. This essay is an effort to explore that puzzle. It was written in response to a request by Senta Raizen, Chairman of Team I, whose work has recently been published as *Research and Development in Education: Analysis and Program Development*. The question to be considered here is this: Given what we know about research and development in education, what might reasonably be expected from an NIE?

II. SOME ASSUMPTIONS

One way to figure out what an NIE might be good for is to make a list of what's wrong with American education and dream up some plausible ways in which R&D might help. Not surprisingly, this is the way many discussions and much of the planning have proceeded, for it provides a hopeful way of relating present problems to the possible benefits of this hypothetical agency. Still, approaching things in this fashion makes it easy to accept many assumptions which might be questionable. Many items on the lists of what's wrong change pretty often; nor is it clear that everything that is really wrong may be remediable; nor that it may be remediable by R&D.

A more cautious way of proceeding might be to set out what we know about education and R&D in advance and try to infer from that what might reasonably be expected to result from a government R&D agency. It is an intrinsically less sunny approach, for our knowledge about education and R&D is much less extensive than our ability to list problems and dream up potential solutions. But it might help in trying to figure out what actually would be possible for an NIE. The first section of this essay summarizes some of the most important knowledge about education and R&D relevant to a National Institute of Education. The realms of chief importance appear to be: the definition of educational problems or goals for an NIE; the knowledge base about schooling; utilization patterns of R&D results; and the applicability of R&D to social problems.

A. Educational Problems

Leading everyone's list of current educational problems are those which center around what is loosely called inequality. Everyone knows that poor and minority group children attend schools with fewer resources, on the average, than those which whites or advantaged children attend. Poor and minority group children perform less well on standardized tests of ability and achievement, they drop out more frequently, and they are more likely to wind up in low status jobs when they grow up. On the basis of this recital of dismal news, it has been generally concluded that American public education has failed.

It also appears that remedying this failure would be one of the first jobs for an NIE. But precisely of what does the failure consist? To say that the schools have failed is either to imply that they have not done something they were supposed to do and could accomplish, or to say that they have not done for some children what they have done for others. And since the way in which this failure is defined probably will have something to do with the structure of remedial efforts, some precision would help.

Viewed historically, all the evidence suggests that the schools are doing better than ever before. More resources are allocated to schools, more equally, than in the past. The *Equality of Educational Opportunity* survey showed—to the great surprise of its authors—that race and class disparities in facilities, curriculum, staff and materials were generally small and often non-existent. Compensatory programs implemented since the survey have improved this situation, so that schools which enroll poor and minority children often have more resources than schools in the same districts which enroll advantaged children. If this is failure, one is tempted to ask, what is success?

The most likely answer would be schools in which poor children don't perform poorly, irrespective of resource allocation. And once again, from an historical point of view there is evidence that schools are either doing better by disadvantaged children than ever before, or at least are doing no worse. For one thing, more children stay in school, and they stay for more years. Since drop-outs were disproportionately from poor or minority group backgrounds, reducing the drop-out rate has meant reducing class and racial disparities in school retention.

Second, there is a good deal of evidence which suggests that—contrary to popular notions—it has not become harder for poor children to succeed in school. One bit of evidence on this is the fact that correlations between test scores and measures of inherited status seem remarkably stable over the last three or four decades (Blau and Duncan, 1969). A second is the fact that social inheritance now has a smaller impact on high school completion. Another is the fact that city schools seem to have been much more frankly hostile to the children of European immigrants around the turn of the century than they now are to black children. and another is the fact that children from most immigrant groups performed poorly in school (Cohen, 1970). The notion that the same schools which once helped the children of European immigrants now penalize black children seems to be fantasy; whatever their shortcomings, schools certainly are less unfriendly places now.

Despite the evidence of historical improvement, however, none of this evidence says that children have an equal probability of doing well in school. Indeed, the probability of either doing well on standardized tests or going beyond high school is considerably greater for children from advantaged backgrounds. Is this reason to conclude that the schools have failed? Do the schools help advantaged students to succeed more than they help poor children?

If we are concerned with test performance, there is no evidence that differences among children arise from differences in the way they are treated by schools. By now, in fact, there is quite a considerable body of research which suggests that variations in school resources, facilities, and programs are unrelated to differences in students' achievement scores. These results derive both from research on the impact of existing differences in the public school system (See Moynihan and Mosteller, 1972; Jencks, *et al.*, forthcoming), and from experiments in which the amount and kind of school resources and programs were varied. The results do not show that schools make no difference, nor do they show that experimental programs are all ineffective. They do show, however, that there is no systematic differential effectiveness among schools as presently constituted; schools tend to exert their effect in much the same way, and at pretty much the same rate, in a variety of places for a variety of people. This is something that might be expected from a system which has striven to achieve equality of treatment, as was thought to befit the most completely socialized social service in America. The research results also show that while some experimental programs seem to be effective

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at one time or place or in a laboratory setting, they seem to be ineffective at other times or places or when they are implemented on more than a laboratory scale.

This does not prove that schools *could* not be differentially effective, nor does it prove that the fault lies with poor children. It only shows that schools presently are not differentially effective; but it shows it in such a powerful and consistent fashion as to raise questions about the chances—given the existing stock of money and our bias towards equality—that this pattern could be transformed. It also shows that the only factor which does consistently account for differences in children's school performance is the social and economic background of their families. This does not mean that less advantaged families are to blame for failing to provide for their children's school careers, but it does suggest that being poor—which means having fewer of the socially valued social, cultural, and financial resources—typically has unhappy consequences for children. Indeed, if it did not have such consequences we would be much less disturbed by it.

If we turn to other school outcomes, there is a similar lack of evidence that differences among schools cause differences among students. Aspirations and expectations for education beyond high school, for example, seem to be totally insensitive to traditional measures of high school policy and practice, including such things as the provision of counseling resources. Or, to take another example, years of school completed beyond high school also seem quite unrelated to the resources secondary schools bring to bear upon students. As in the case of test scores, both of these outcome measures are related to the social and economic background of students' families. They also are influenced by students' IQ or achievement test scores, which we have just seen are quite insensitive to variations in school policy or resources. But perhaps the most revealing fact is that most of the variability in these educational outcomes lies within, not among, schools; the differences among schools are much less than the differences among students within any given school.

Once again, this does not prove that schools could not affect aspirations or educational attainment (new policies might prove effective), nor does it prove that they do not now have such an effect (the wrong things may have been measured). But the evidence is such that this last caveat seems rather academic. For one thing, the studies revealed as ineffective the very things about schools that everyone thought mattered; searching for other explanations after the fact seems a little lame. More important, however, is the fact that almost all of the variability in these outcomes lies within schools, not among them. This means that even if hitherto unknown school-to-school resource or policy differences which did affect aspirations and attainment were found, they would have only a trivial overall impact on the distribution of these outcomes. And affecting the allocation of aspirations and attainment within schools is likely to involve such messy things as teachers' attitudes or the principle of curricular differentiation based on ability, rather than traditional and easy-to-manipulate resources or materials.

Now, in light of this evidence on the effects of schooling, what does it mean to assert that schools have failed? Certainly it cannot mean that existing policy and practice with respect to resource allocation have the effect of damaging poor or minority students while helping their more advantaged peers. It might mean that schools ought to have provided sufficient compensatory treatment to permit less advantaged students to catch up, but the evidence on the effects of compensatory treatment suggests that such efforts don't work well enough to qualify as good policy. And it hardly seems meaningful to say that the schools have failed to do something which the evidence suggests they may be incapable of doing. Nor is it clear—quite apart from the possibility of achieving equal outcomes among groups—that the conception of social policy implicit in this notion is sensible or wise. How does one know that social services should be equally effective rather than being equally or more than equally provided? Is our social policy one of equal outcomes, or one of equal opportunity, or one of rescue from disaster? If there is such consistent evidence that non-school environments are much more powerful than school environments, would other areas of intervention than schools be more sensible?

A final way in which schools might have failed is by adopting policies and procedures like testing or curriculum differentiation based on ability. If such policies were abolished, it has been argued, race and class differences in school success would vanish. Such actions of course would assume that test scores were an inappropriate way to summarize the outcomes of schooling. On one hand, it seems evident that they are an important element in educational decisions—they influence the ability groups and curricula children are assigned to, as well as the education (if any) they have beyond high school. Researchers and developers could take this as further evidence of the importance of test scores and set to work on compensatory strategies with renewed vigor, but other responses would be possible. After all, test scores might be important for success in school only because educators decided they should be and arranged schools accordingly, not because they bear any intrinsic relationship to one's ability to perform later in occupations or to participate in society. And if the importance of test scores results only from professional conventions, it might make more sense to attack the conventions and devise policies like open enrollment, rather than trying to change the scores.

Although evidence in this area is much thinner than one would like, everything suggests that test scores do affect the allocation of status and income in adult life to a noticeable degree, but that they affect it only indirectly, by way of their influence on how long people stay in school. One might argue that this is evidence that the scores do measure an underlying set of skills important to success in society, but one might also argue that scores seem important only because employers have adopted the same conventions that schoolmen use. This doesn't help us decide whether schools have failed, or how, or what to do about it. For example, even though we know that test scores do have some relation to adult performance, does that tell us how important test scores would have to be before we would decide to allocate resources to devising compensatory strategies, rather than researching and developing policies like open enrollment, designed to minimize the role of scores in stratification decisions?

The chief complicating element is the assumption that test scores might have some "intrinsic" importance to adult performance. In fact, though, the importance of test scores is largely based on convention. After all, this society could decide not to permit scores to influence decisions about allocating status, wealth, or power. Such a decision could produce a society in which scores correlated not at all with any index of adult success. It might also produce a society that was less efficient, or more productive, but these, after all, are nothing more than values which can be expressed in policy. In effect, the question of whether IQ is "important" ultimately has more to do with value judgments about the sort of society one prefers for moral or aesthetic reasons than it does with any "intrinsic" importance of IQ.

In fact (as I will argue in greater detail further on), we know precious little about whether test scores are important because of relatively trivial professional conventions about what should be important in school, or because of relatively fundamental social conventions about what should be important in decisions about the allocation of status, wealth, and power in adult life. This ignorance, combined with the rather astringent results of research concerning the effects of schooling and the fundamental moral dilemmas surrounding any decision about the importance of any educational outcome, raises the gravest questions about whether schools have "failed," or if they have, what they have failed *at*, and what the most promising remedies might be. And therefore, it is extremely difficult to imagine how intelligent decisions about resource allocation in educational development and dissemination could be made. One could argue convincingly that until we have a much clearer conception of whether there is a problem, and if there is, precisely of what it consists, it would make sense not to invest resources in developing and disseminating "solutions." If nothing else, the last ten years have shown the demoralizing effect of creating solutions to problems under conditions of such scientific and social uncertainty that either the solution turned out not to work or after a few years the problem turned out to be something quite different.

B. Defining Goals for an NIE

For other reasons, however, it seems unlikely that an NIE's first priority would be to carry out the basic research required for a sensible definition of the agency's goals. The goals of government agencies usually are only marginally affected by research findings, and this seems just as likely in a research and development agency in education. Some of the reasons for this have less to do with the nature of education or research than with the nature of man and government: political and economic interests can always be expected to affect the allocation of public resources for reasons which have more to do with the control of resources than with the satisfaction of abstract programmatic or ideological goals.

There are, however, other reasons for skepticism which have more to do with the nature of education. More than any other realm of domestic policy, educational decisions are governed by considerations of morals, culture, and deep political values. Of course, such considerations bear on other realms of social policy, but they seem to dominate schooling more.

Perhaps the chief reason for this lies in the character of schooling. Schools, after all, are the places in which children are believed to acquire political values, to undergo moral development, and to learn what is essential in their culture. Not surprisingly, these are matters that adults feel strongly about, for the content of socialization in these areas amounts to nothing more or less than a society's mythology of itself, an embodiment of its aspirations and a statement of its identity. Since there is a good deal of variety in the political and moral culture of various population subgroups, there often is a good deal of disagreement about what schools should do and how they should do it. Despite the professionalization of child-rearing in industrial societies and the consequent estrangement of families from schooling, feelings still run deep.

All of this is given greatly added force by the fact that the socialization function is raised to public status in the American school system, which means that any discussion of educational goals or of the content of the schooling process is at least implicitly a discussion of what political values and whose conception of culture should be given public approval.

This accounts for the passion with which so much public debate about schooling is invested and the tendency towards innocuous materials and ideas in public education. However, it does not wholly account for the fact that moral and cultural considerations have more to do with setting priorities in education than in most other realms. After all, people feel strongly about life and death, but they generally don't tell doctors what to do or expect to decide how hospitals should be run. One contributing factor is the rather weak knowledge base in education—certainly weaker than in fields like agriculture or health. There is a much higher probability that doctors will be able to treat successfully a physically ailing child than that teachers will be able to treat successfully an educationally ailing one. Part of the reason for this is that the scientific knowledge underlying medical practice is more developed and better organized, and another reason is that clinical practice—which typically has no rigorous scientific base—is more extensively organized and is codified and transmitted more effectively in health than in education. The result is that school professionals are less well protected from their clients by special knowledge and competence; to the extent that the authority of professionals rests on the qualification of special knowledge, educators are more vulnerable to influence by the values of one or another client group.

This vulnerability is enhanced (again in comparison with fields like health) by the simple fact that education is nearly a fully socialized service. It is therefore governed, rather than being delivered through an essentially private system of service provision. Education professionals are much more likely to be subject to shifts in political fashion and to have to accommodate themselves to whatever values happen to dominate their state or local government.

Perhaps the most important consequence of this is that educational goals—and the entire matter of education itself—must be consensual to an extraordinary degree. This accounts for the pablum-like character of most educational materials, and it has much to do with the defensive posture of the education professions, vis-a-vis clients. They would hardly spend so much time insisting that schooling is non-political if it were not in fact profoundly—and in their view dangerously—political.

Therefore, an NIE probably would not be able to set its priorities with the same degree of independence from the opinions of officials and publics as seems to be the case in health or agriculture. And this situation is oddly exacerbated by the deep-seated notion that in one way or another education is the best way to solve most social problems. This idea is one of the reasons education generally enjoys the more or less consistent warm regard of legislators at all levels of government, and it also is one reason why the proposal for an NIE has been well received in the Congress. But it also explains why education and training occupied such a predominant role in the anti-poverty legislation of the last decade; were schooling not believed to be the "balance wheel of the social machinery," remedying the schools' "failures" might not be seen as the solution to the problem of poverty. And it suggests that as long as this attitude persists, a National Institute of Education will be regarded as one of the government's leading social problem-solving agencies. Were it not so, proposals for an NIE probably would never have come so far.

Setting goals for a National Institute of Education, then, seems likely to occur under a combination of relatively difficult circumstances. Because of Americans' faith in the redeeming power of education and their conviction that most problems are soluble, various social problems are likely to be defined as school problems. But, as the earlier discussion of inequality suggests, this definition may be based on such a vague understanding of the social processes involved as to be confusing and misleading. As a result, priorities will be pressed on an NIE, or embraced by it, which it may well be unable to satisfy.

C. The Knowledge Base

A third element which would shape expectations for an NIE is the character of existing knowledge about education. In part, of course, this is important simply because a grasp of what is known is useful in any decision about what ought to be known. But the character of existing knowledge in education is probably far more important for what it tells us about the possibilities for development and dissemination: is there enough knowledge, or knowledge of the right sort, to permit effective development work? Is there a body of fundamental knowledge in education which can serve as the basis for development and dissemination? My discussion of these questions will focus on the effects of schooling, since that seems to be a central concern in NIE planning.

The state of knowledge in this area can best be summed up by saying that nothing resembling a respectable body of theory exists and that most of the empirical research tends to contradict the assumptions on the basis of which both research and school policy has proceeded for the last half-century or so. Owing to the absence of any coherent or articulated theories of the schooling process, research in education has had an *ad hoc* character, focusing on isolated problems or the consequences of particular interventions. Despite the accumulation of enormous quantities of research, therefore, it has tended to be repetitive and fragmented, rather than cumulative. As a result, it has provided a poor basis for development and dissemination.

Perhaps the most telling example of this situation is the condition of our knowledge about the outcomes of schooling. The quasi-theory on which much educational policy depends asserts that the school outcome of chief importance is technical competence to perform the highly specialized tasks which characterize work in a complex industrialized society. The principal educational indices of such competence are degrees and years of schooling completed on one hand, and tests of achievement and ability on the other. The assumption is that both indices are good proxy measures for future job performance and productivity, that societies with greater educational attainment and higher scores will be more productive; and that the more congruent the hierarchy of work is with the hierarchy of school attainment, the more efficient social organization and action will be.

As I suggested earlier, however, the evidence on these notions is ambiguous. It does show that educational attainment (years of school completed) is a moderately important predictor of occupational status, and it shows that test scores are a moderately important predictor of years of school completed. But the crucial element in the theory is that these are mere proxies for later productivity and job performance, and here things begin to break down. There is no evidence of an independent relationship between test scores and occupational status; scores seem to have their impact exclusively through their influence on educational attainment. This, of course, raises the suspicion that the relationship between scores and schooling and status results only from the adoption of a common series of conventions by schoolmen and employers. We know that school professionals acting on the assumptions set out in the theory use tests to group and track students and to assign them different educational careers, which are then signified by differentiated degrees. Employers, acting on the same assumptions, then use the degrees as simple ways of differentiating among workers. Test scores could therefore appear to affect occupational status without there being any necessary relation between scores and job performance.

This suspicion is hard to check fully, because it is impossible to arrange an alternative occupational structure with different criteria for assigning jobs and then determine if one system is more productive or efficient than another. But it is possible to check partially, by inspecting the impact which cognitive skills seem to have on job performance within broad occupational categories. If no relationship appeared within these categories, we would be dubious about the importance of scores to worker performance. And the existing research tends to produce this doubt, for it shows that cognitive skills have no impact on either productivity or other measures of job performance for workers within occupational groups. Instead, it appears that workers' attitudes and motivation seem to be much more important determinants of their performance.

But this research doesn't really bear directly on the possible impact of scores on occupational status since it examines the association only within job status categories. This can be partly remedied by asking whether test scores seem to have any impact on occupational status within broad categories of educational attainment. If we found that those college graduates with higher test scores wound up with higher status jobs, we would be inclined to think the theory was roughly correct. But once again the evidence tends to confound the theory: college graduates with high test scores seem to wind up in no more prestigious or remunerative occupations than graduates with low scores. This seems to hold whether the group of graduates in question hold A.B.'s or more specialized professional degrees.

While this evidence does not conclusively prove that the prevailing assumptions about the outcomes of education are wrong, it does cast grave doubt on them. And for this reason it raises equally grave questions about the evaluative criteria for R&D products. If educational attainment and test scores turned out to be related to adult success only because of a system of relatively superficial conventions, would it make sense to continue to use these outcomes to measure the impact of R&D efforts–or any other educational policy, for that matter? If these educational outcomes are dubious, are there others which might be more reliable?

Existing knowledge provides little help in answering these questions, a situation which suggests some useful areas for the investment of R&D monies. Research on the long term effects of schooling and on other outcomes which may be important (such as attitudes and motivation) would be hard to surpass as a source of guidance in the future allocation of educational resources. The results of such research would not solve all the problems of setting priorities for educational R&D, because to know what the outcomes of education are is not necessarily to know that they are desirable or that alternative outcomes should not be pursued. But our understanding of such moral and political questions might be improved by more complete information on how the schooling system works.

Problems on this score are not limited to the long-range effects of schooling. There also is serious confusion about the ways in which schools affect children's development in the short run. The assumption which has dominated research and policy for the last half-century or so is that there is a relatively uncomplicated relationship between children's academic development and their exposure to teachers, teaching materials, and the various ancillary services schools provide. The quality of these resources has been defined roughly in terms which appealed to an emerging semi-profession: better schools were those with better teachers, smaller classes, more specialized staffs, and higher expenditures. Better teachers had more degrees, more specialized professional training, and more of a claim on the esoteric knowledge which would differentiate them from parents, voters, and board members.

As I noted earlier, recent research has called these notions into serious question. The *Equality of Educational Opportunity Survey* (Coleman, 1966) and various reanalyses of the Survey data showed that none of the school resources traditionally assumed to affect achievement did so; evaluations of various compensatory programs produced similar results. What is more remarkable, however, is that these conclusions have been foreshadowed in the educational research literature ever since it began to accumulate. Literally hundreds of studies of the effects of school resources and policies on students' achievement have been carried out since the 1920's, and they generally showed that things like spending more money, training teachers more, or consolidating school districts usually produced achievement little different than what would have been expected without these policies. Indeed, so much evidence of this sort accumulated that before long it became common among educational researchers to refer to the pattern of negative results. Nonetheless, the research continued to accumulate and to have absolutely no impact on policy (Stephens, 1967).

One reason for this, of course, was the unwillingness of education professionals–researchers among them–to admit that the things that would improve the lives of professionals in schools might not improve the minds of their students. But I suspect that another lies in the power of the metaphor which underlies the notion that resources should affect achievement. For behind the identification of "more" with "better" was the idea of production, a metaphor which has had a powerful grip on the American imagination since late in the last century. It was around that time, in fact, that schooling began to be conceived in terms of productive efficiency, and central to the notion of production is the idea of a regular relationship between resource inputs, production processes, and finished products.

Indeed, the notion of schooling as a production process is so deeply rooted that patterns of thought simply resist the evidence that it may not be so. The main result of the Coleman report, for example, was not to raise questions about whether production was the right metaphor, but rather to send researchers scurrying for unmeasured "inputs" or alternative measures of school outcomes. Thus the recent fascination with teacher expectancies, students' sense of "fate-control," and the like. It is worth asking, however, whether the notion of production is sensible: is it the most appropriate way to conceive of the process of schooling?

It is hard to think of a more important question, because if anything characterizes work on the process of schooling it is the absence of any general conceptual framework which more or less squares with the evidence. This is not likely to make coherent R&D easy: in fact, the absence of such a framework makes it likely that investments in educational R&D will produce a continuing accumulation of generally unrelated and non-cumulative results.

It is, however, much easier to bemoan the lack of theory than it is to remedy it. And while this is hardly the place to pretend to solve the problem, there are other ways to conceive of the schooling process. One notion, for which there is a good deal of evidence, is that the schools are nothing more than an elaborate sorting mechanism, which selects students on the basis of essentially noncognitive and behavioral criteria, allocating them to curricula and assigning them degrees which lead to different occupational futures. The "effect" of schooling is therefore to be seen in the assignment and certification routines, and in its ability to legitimate this system of discrimination in the eyes of the society and the minds of the individuals involved. On this view it simply is not important whether or not schools affect students' cognitive development. The important point is that the schools' main function is to provide rough screening for the occupational structure.

Even if we are concerned with cognitive outcomes, there are other possibilities than the factory metaphor. One which presently seems promising centers around the notion of exposure. At the root of this idea is the supposition that schools are not like factories (which are very specialized and relatively powerful environments), but that they are rather like climates (which are diffuse and generalized environments). In the first case, it would make sense to conceive of the effects of schooling in terms of how variations in inputs and production processes affect the quality or quantity of outputs. In the second case, however, it makes more sense to think in less complex terms; here the main determinant of outcome differences would be the duration of exposure to schooling and the extent of exposure to other environments.

Conceiving schools as generalized climates suggests several possible results. One is that major variations in the duration of exposure, rather than variations in resources or pedagogy, would be most likely to affect outcomes. Given the general uniformity and weakness of school environments, there would be no regular differences in the effectiveness of schools or teachers. The main difference would not be what kind of environment students were exposed to, but whether they were exposed, and how long. The metaphor also suggests that competing environments (family, neighborhood, etc.) which were either more intense or of greater duration would have more influence on educational outcomes. Finally, it seems likely that the only interventions likely to produce any substantial change in school outcomes would involve quite radical shifts in the intensity of the school environment, rather than increments of existing resources.

Evidence on all of these points is hard to come by, precisely because of the uniformity and universality of schooling. But there is some fragmentary data on the effects of differences in exposure. The few studies of the relations between years of schooling and test scores show that (allowing for the children's early IQ), those who stay in school longer seem to have somewhat higher scores. There also has been a rough investigation of the effects of differential exposure to school for children from various social and economic backgrounds. The study suggests that learning rates for advantaged and disadvantaged children are roughly the same during the academic year (when all the children are being exposed to the school environment) but tend to diverge sharply during the summer months (when the character of the environment becomes less similar due to the absence of school) (Hayes and Grether, 1969). Both findings support the notion that exposure to schooling affects outcomes.

An enormous amount of research shows that particular resources and educational policies have no differential effects upon school outcomes, something which the notion of climates would lead us to expect. But the really important question is not whether particular resources are ineffective, but rather whether there is any underlying consistency in the effectiveness of schools or teachers. The climate metaphor would suggest that given the uniformity and diffuseness of the schooling process, there would be no consistent differences in the effectiveness of either schools or teachers. A few studies allow one to examine whether teachers are differentially effective, by comparing annual test gains for successive classes of children taught by the same teachers. These studies show that at best (allowing for differences in the children) there is only a small relationship between the gains produced from one year to the next. The same teachers do not seem to be consistently more effective, year after year, than others (Rosenschine, 1970).

Similar findings turn up when we consider the consistency of effects at the school level. When school-to-school differences in children's achievement gains from one year to the next (allowing for differences in their ability) are compared, there seems to be no consistency in schools' effectiveness in producing gains. While most of any annual gain is accounted for by students' earlier scores, there is a modest residual gain which might represent the "value added" by the schools if there were regular differences in schools' effectiveness. If some schools were better "producers" than others, presumably they would regularly create larger gains than others. The evidence, however, does not support this view. There is no substantial correlation between the residual gain scores–i.e., schools which produce a larger residual gain one year produce a small gain the next (Acland, 1972). The rank order of "effective" schools changes so much from year to year as to produce no discernible pattern whatever.

The absence of any consistent differences in the effectiveness of schools or teachers certainly suggests that it may be inappropriate to conceive of schools as factories. There is little evidence of regularities in productivity which might be related to variations in inputs or production processes. The evidence also lends some credibility to the idea that the interventions most likely to produce outcome changes are those which involve radical changes in the intensity of the school environment. While there is only fragmentary evidence on interventions of this sort, it seems to support the notion advanced here. The results of recent experiments indicates that only school interventions which transform classrooms into highly structured and quite intense training situations do succeed in making at least temporary changes in children's test scores (Bissell, 1970).

It would be foolish to argue that this "proves" that climate is the proper metaphor on which to build a theory of the schooling process. Much more theoretical and empirical work would be required to approach that point. The discussion does, however, reveal our limited understanding of the schooling process, the weakness of existing research, and the poverty of the factory metaphor. This, in turn, suggests cautions about how useful a basis existing research on schooling will be for development activities. Most of the planning documents for NIE, for example, suggest that a major R&D priority for the new agency must be to improve the schools' productive efficiency for disadvantaged students. But if the discussion above is even roughly correct, the entire conception of schooling on which these recommendations are based may be mistaken. And what good could come from R&D efforts based on a mistaken conception of how schools work? At best, the results would probably be an NIE which reproduced the same pattern of fragmented and non-cumulative research and intervention which has characterized education for the last fifty years. And there could be much less happy consequences–for example, more programs which "prove" that poor children cannot learn. Perhaps NIE's highest priority should not be development and dissemination aimed at making schools more "productive," but rather research and experimentation designed to elaborate and test alternative theories of the schooling process.

D. Utilizing R&D Results

The idea for a National Institute of Education did not exactly well up from the grass roots and impose itself on the federal government. Quite the contrary, it arose from experiences unique to the national government itself. Disillusion with the Office of Education's research program; the "failure" of several major federal education programs; and the resistance of state and local school systems to reform: each of these elements in the recent federal experience with education was thought to imply real problems at the roots of the educational system. The failure of the OE research program has been laid at the door of the educational professions and the priorities they imposed on educational R&D; the failure of Title I has been ascribed to the inability of schools to succeed with poor children; the resistance to reform has been chalked up to narrow professionalism, bureaucracy, and centralization.

One can see how such judgments could arise from the federal government's recent efforts; but if all this is true, why would an NIE help? If this diagnosis of the schools' problem is correct, why would any school system adopt the products of an NIE? Is there any reason to think that an NIE would find much of a market for its ideas, or is the NIE proposal only another manifestation of the same reformism which produced the "failures" in federal education policy which the NIE is supposed to help correct?

The question is a little unfair, but it does suggest the importance of reflecting on the potential demand for the "products" of an NIE. One's expectations for such an agency would depend in good measure on the likelihood that its work would find a receptive audience. This is particularly important, because almost all the proposed agendas for NIE focus on solving major problems in American education. They stress such things as improving the schools' productivity, changing the character of professional roles, and changing the schools' administrative and political structure. Would there be much demand for such innovations?

The answer varies a bit with the innovations involved, but in general one should expect severely limited demand. In the case of innovations concerned with improving institutional performance, there are considerable barriers to their creation and acceptance. One is the absence of a coherent veiw of the schooling process and its outcomes. This means that we lack a stable conception of how a particular innovation ought to work, or why; we lack, therefore, any real basis for evaluating its impact. To give an obvious example, efforts to improve compensatory education are really impossible to evaluate, because we have really no idea whether they should "work," or if they did how much of an effect would be "big," or what the effect would ultimately mean for children. And if our understanding of the schooling process is so incomplete, how can one expect innovation to be anything more? The absence of a stable foundation for the development and dissemination of innovations removes one important incentive for changing educational practice. Or rather, it creates a situation in which the incentives for changing practice in education resemble nothing so much as the incentives for changing fashion.

A second barrier to the adoption of innovations involving institutional performance is the absence of any incentives within the system of public education tied to such performance. One reason for this has to do with the character of the incentives and constraints within which educational professionals work. The public schools have evolved as a public employment system, and for the last three or four decades the values of that system have been dominated by ideas about merit and performance originally derived from the Progressives. The criteria of entry and advancement in such systems generally center on such things as performance on objective examinations, length of years in service, and number of degrees from educational institutions. And if rewards like salary and professional status are handed out on these grounds, it should be no surprise that competition among teachers within systems would also focus on those criteria for advancement. Needless to say, neither teachers' degrees, their salaries, their status, nor their exam scores have much to do with their students' performance.

There also is competition among schools and school systems, but it is not focused chiefly on school outcomes. Good schools are those which stand high on the ladder of professional values, which involves things like overall expenditures, teachers with advanced degrees, quality of the facilities, and so on. There is some competition among schools related to outcomes: the emphasis, however, is not on making better students but on having them. The better schools and school systems are not those which take their students farther from where they began, but those whose students do better at the end—and that is very much a function of how well they did at the beginning.

A third obstacle to innovations concerning institutional performance is the dominant system of ideas concerning the responsibility for school performance. The ideas of educators on this point are oddly mixed, for while they insist that investment is more and better resources is important because it will help children perform better, they also identify school performance problems with the clients, not the institutions. Indeed, the schools have long operated on the assumption that the source of failure in school lies with the child or his family. This is manifest in the schools' information system, which contains an absolute avalanche of data on the performance of students and little or nothing on the performance of schools. Pupils are tested, rated, graded, and grouped, they and their families are apprised of these things, and precautions are taken to make sure that any deficiencies are noted. There are, however, no report cards on schools.

A final barrier to the adoption of innovations related to institutional performance is the absence of effective countervailing forces interested in the application of performance standards. This is not to say that some checks and balances are not present: at local levels, schools are politically accountable to the electorate; there are independent accrediting organizations; and state agencies have extensive responsibility for overseeing local school systems. Nonetheless, these countervailing mechanisms have little effect on schools, insofar as outcomes are concerned. The oversight exercised by state education departments, accrediting agencies and professional associations focuses almost entirely on school resources. More important, all of these agencies are staffed by people drawn from the educational professions and especially from local school systems; they share a commitment not only to professional standards of quality and prevailing ideas about the responsibility for school outcomes, but also to the sanctity of local control. As a result, these "countervailing" institutions serve only to reinforce the very ideas and practices which stand opposed to outcome standards.

The result of all this is to effectively insulate schools from any professional pressure for evaluation in terms of outcomes. The only remaining source of such pressure would be the electorate and its representatives on school boards, but lay control is very effectively constrained by a system of defensive mechanisms established over the years by the educational professions. The administrators who control the operation of school systems at the upper levels have developed a system of distinctions between policy and practice which keeps board members' hands pretty well out of the machinery. In addition, there is a highly developed ethic that education is non-political, which usually insulates it pretty well against partisan controversy and all but the harshest ideological pressures. And teachers have so effectively developed the notion of their autonomy in the classroom that even other school professionals are kept pretty remote from what actually goes on there.

In addition to all this, the sort of laymen who find their way onto boards of education usually do so through some district-wide selection process, and this usually insures that the people selected have enough other things to worry about to keep them from badgering the professionals. And even if they didn't, since the professionals serve as the school board's sole staff, there is no danger that board members would have any independent sources of information or power. The only other source of such independence, of course, would be stable party organizations concerned with the schools, but the ethic of non-partisanship and the absence of patronage assure that political parties would have no continuing concern with schools. Thus, even lay people who would like to impose outcome standards on schools have no effective machinery at their disposal.

This discussion also helps explain why many other innovations would be unlikely to gain general acceptance. Changing the character of roles in schools is a type of innovation which has often been mentioned in discussions of an NIE; the ideas include such things as broadening the conception of teacher to include older children and other adults, or de-emphasizing the passive role typically associated with students in secondary schools, or trying to turn teachers into "children's advocates." It is not so hard to see how changes of this sort can be carried out at the margins of the school system. Private schools, experimental situations, and programs funded with federal monies specialize in such endeavors. But given the increasing control which the educational professions exert over certification and job tenure, the ethic of teachers' autonomy in the classroom, and the power of existing assumptions about the essential passivity of childhood, it is nearly impossible to see how such innovations would be much in demand within the main body of the school system. Or rather, it is hard to imagine much demand in the absence of a hopeless shortage of teachers, a revolution in our conception of childhood, or a turn-about in the conception of professionalism which has dominated education since the turn of the century. But at the moment it looks as though the old ideas of professionalism are gaining greater recognition, and that the supply of teachers will exceed demand for the foreseeable future. There are some indications of marginal shifts in what adults think about children, but these seem to be confined to a small segment of the urban upper-middle class, they are quite unstable and uncertain, and they tend to be expressed in non-public educational institutions.

Not surprisingly, this generally bleak picture has led many to conclude that the demand for basic innovations in the school system will remain marginal unless the system's incentive structure is changed. While it is possible that social crisis may change incentives—one could make a persuasive argument that most fundamental social innovation occurs only in times of profound crisis, like war or depression—planners generally don't regard unpredictable crises as reliable instruments of policy. Thus they have focused attention on other ways of changing the incentives, such as performance rewards, tuition vouchers, decentralization. This intuition is probably correct, but the argument that an NIE should develop, test, and promote innovations designed to change the schools' incentive structure has rather grim implications for most of the other things such an agency might do while the school system is being overhauled. It suggests that the demand for substantive innovations would be small until innovations in the incentive structure were implemented.

E. What is Research and Development?

Finally, one must ask whether research and development would help to improve education, and if so, how? I have tried to answer some parts of the question already in earlier sections of this essay: R&D is not likely to help if educational problems are mis-defined, if the knowledge base in education is not greatly strengthened, and if there is only minimal demand for R&D products. But all these earlier arguments make assumptions about research and development which are by no means self-evident. One is that R&D is well enough understood to make its application to education worth the effort. The other is that education is the sort of activity to which a problem-solving process like research and development ought to be applied.

Perhaps the nastiest question is whether anyone really knows how to make R&D work in education. R&D, after all, refers to a diverse collection of problem-solving strategies which have grown up around the efforts of industry and government to devise new products, weapons systems, medical technology, agricultural practices, or the like. Many of these efforts have been spectacularly successful, and as a result R&D has gained an enviable reputation. Oddly enough, though, the reasons for this success are not well understood. Or perhaps it would be more accurate to say that we don't know whether there are any predictably successful strategies for producing innovations. There are arguments about the best sort of organizational environments for R&D, arguments about whether organizations or innovative lone wolves really are the important thing, arguments about whether basic research or development is really most important, and arguments about whether R&D or marketing is the crucial factor in the success of new products (See Parker, 1971). Different arrangements seem to work at different times and in different places, and quite often no arrangements seem to work. This has led some to conclude that R&D can only be approached "contextually," others to hope that the underlying pattern remains only to be discovered, and others to wonder if there is any predictable secret of success (See Schalock and Sell, 1972).

If this is the case in fields which have reached great peaks of scientific and technical accomplishment, we would expect matters to be a little less hopeful in education. And when we turn to the literature on educational R&D in search of some discussion of how this problem-solving process should be applied, we come away nearly empty-handed. While there have been some illuminating arguments about the merits of various approaches, their effect is mostly to reveal the poverty of experience available to help in making choices among R&D strategies.

This is painfully evident in the ongoing debate over which part of the research/development/dissemination/evaluation chain is most important. One argument concerns the relative importance of R&D, with many maintaining that R&D in education will only be successful if development gets priority. For support one can point to the defense and aerospace fields, in which priorities on development have generated both technical innovations and new basic knowledge. One difficulty with this sort of example, however, is that it is drawn from fields in which development rests on an already mature scientific and technical base. If much basic knowledge already exists in a field, it is easy to see how giving development priority might make sense, but does the argument transfer to education, in which the knowledge base is so weak? The obvious rejoinder is that since there is no easily understood path from basic knowledge to applications, it makes sense to focus on the required applications. But while it certainly is true that the route from theory to practice is often obscure, can one imagine fruitful applications in the absence of a solid knowledge base—however mysteriously the two connect?

What one searches for vainly in this argument are concrete examples in education which might give it empirical content. Is "Sesame Street" an example of the primacy of development, or would it have been a disaster without existing psychological research and theory? Or does it show that the argument about the primacy of R&D is an example of asking the wrong question? One doesn't know the answers because such questions have only been applied to pieces of paper in essays, not to the experience of educational R&D.

Another argument concerns the importance of dissemination. While most commentators seem to think that overall priorities should be set by either researchers or developers, some argue that priorities should be heavily influenced by the clients of R&D—school professionals, parents, and the like. In one way this idea makes perfect sense—after all, if the people who are supposed to use R&D products help to define the needs and develop the solutions, they might be more inclined to adopt the results. But in another sense, the notion has an Alice-in-Wonderland quality, for if the problems an NIE is supposed to solve are as serious as the arguments for NIE suggest, it seems unlikely that defining problems and developing solutions within the system would produce much of a departure from existing practice.

There have been other important debates about how to approach educational R&D, but there is no point in rehearsing them here, for they too are mostly without empirical content. This is not a condemnation—such literature is scarce in fields with a much longer and richer history of R&D; one couldn't expect more in education when it has only been about ten years since the passion for R&D began rising. While some efforts have been made to institutionalize R&D in the Regional Laboratories and R&D Centers, if anything was learned from these efforts it hardly ever was committed to paper. And efforts to explore past experience with R&D in education in order to learn something about the merits of various approaches are only just beginning.

Understandable as this may be, it does have some troublesome implications. One is most clearly evident in the discussions of how an NIE ought to proceed. The consensus seems to be that such an agency ought to employ every known approach to R&D at the same time, on the principle, one assumes, that a few of them are bound to turn out well. While this may seem the wisest course in view of our ignorance, it is not exactly encouraging. If we know too little to choose among alternative approaches to R&D, do we know enough to say with any confidence that some of them *will* be helpful?

One would be inclined to answer in a mildly affirmative tone, were it not for two circumstances. The first is that the emphasis in all discussions of NIE is on improving educational practice—on development, dissemination and evaluation, rather than on research. This means that the proposed NIE would try harder to affect students, parents, teachers, and governments than to affect researchers, universities, and consulting firms. This emphasis on educational practice ought to make us more circumspect, if for no other reason than the fact that making mistakes with small children and governments probably would have more dire consequences than making mistakes with universities, professors, or consulting firms.

Second, this sense of caution is greatly reinforced by my uncertainty about whether R&D is really an appropriate way to go about solving many of the school problems which it is proposed that NIE should solve. I have already discussed the alleged unproductiveness and failure of the schools, and tried to show that the view of schooling on which such efforts rest may be incorrect. If the work went on anyway, it could have many unhappy effects, over and beyond the likely failure of the work itself. It could, for example, promote the idea that schools are deliberately cheating children; the failure of even more efforts to improve productivity could further the idea that poor children can't learn, and that poverty is a result of individual inferiority; and more examples of government enterprises which failed to achieve their objectives would not strengthen anyone's confidence in government.

In a sense, though, this is the least difficult of the problems concerning the applicability of research and development to education. Suppose for a moment that the factory metaphor were partly correct, and that there was some relationship between what schools did and how well students learned. Does that provide a sufficient rationale for an NIE to develop and disseminate techniques to improve the schools' efficiency? At one level it is easy to give an affirmative answer—for if schools were like factories, then improving their effectiveness could make a difference for many students. But that easy answer is deceptive, because it is not self-evident that schools should be like factories, or that fully efficient factories would be better than partly efficient ones. One could argue that schools only produce what society needs, but society's definition of its needs in this case is nothing but a definition of the conventions about what it values. An R&D agency which places the weight of the government behind efforts to improve the effectiveness with which schools follow those conventions is making a moral and political judgment of no little consequence.

This problem becomes more acute the less one thinks that the schools' job is to produce "outputs," and the more one thinks their job is to teach values, to keep children out of their parents' hair, and to pass along whatever seems essential in the culture. If it is hard to see how R&D will help improve the schools' productivity, it is harder still to understand the role of R&D when the essential transactions are moral, political, or cultural. It is not easy to imagine what worse or better ways to teach morals and politics might be–it may be that methods change with changing tastes and values and that little more can be said. It is, of course, not hard to imagine government support for the development of curricula, or the preparation of materials, or for new teaching techniques–this goes on all the time, and it is viable precisely because all the government does is provide the money to isolated development efforts. The results are not really seen as the solution of any problem–unless it is the problem of occasionally providing some educators with the wherewithal to change what they do or how they do it. This suits the educators' changing ideas about what should go on in schools, but the federal government doesn't decide that some approaches to moral or political education are better than others, nor does it test the results or persuade people to adopt the new methods.

It is quite likely that efforts to depart from this fragmented, sporadic, and non-cumulative R&D process—and to substitute more systematic and uniform efforts to "improve" schools–might be very sticky when it comes to the political, cultural, or moral aspects of education. But since it is quite unlikely that anyone would seriously contemplate doing anything but giving professionals free money to change things occasionally, the worst one can say is that R&D as commonly understood would simply be irrelevant to this rather large sector of the schooling process.

This may not be the case, however, with proposals that an NIE should develop and disseminate solutions to problems related to incentives. Most discussions of NIE assert that it should experiment with tuition vouchers, performance reward systems for students and teachers, and decentralization or community control. These are nothing more or less than proposals for the federal government to experiment with alternative forms of local government–rather a tall order. I will return to this further on, but it is important to note the enormous problems of deciding what better governments are and the rather trivial role which even the finest research would play in such decisions. It is very difficult to regard such decisions as part of an R&D process.

In summary, then, the arena for educational R&D seems a bit constrained. On one hand, the conception of schooling to which R&D seems most suited—the idea that schools produce things–may well be incorrect and, therefore, an inappropriate basis for development and dissemination. On the other hand, the areas of education which seem to loom largest in actual practice–the transmission of culture and values—seem to be of such a character as to defy any focused and cumulative federal problem-solving efforts. And the improvement of school governance seems rather a delicate area for sustained federal intervention. None of this suggests that the usual conception of R&D is broadly applicable to education. It appears that many of the problems which it is hoped NIE would help solve may not be amenable to the sort of sustained, organized and cumulative process of developing, testing, and implementing innovations which we associate with R&D.

III. CONSEQUENCES FOR NIE

This analysis is hardly exhaustive, but it does offer some sense of the situation in which an NIE is likely to find itself. Since the main issue in all this is the character of R&D policy in education, one is less interested in the analysis than in what it implies for the actions of an NIE. And the word which best sums up the likely situation is instability. This is not because the forces likely to influence NIE priorities are different than those which influence R&D in other fields: professional interests, public and governmental concerns, detached judgments about research or development requirements, and fashion operate in all cases. But the balance of these forces varies from one field to another, and an R&D agency in education would be more vulnerable to pressures which would distract it from basing priorities on careful consideration of R&D needs. The best example of this lies in the agency's mandate: NIE is supposed to commit major resources to the solution of pressing problems in education—especially the problem of inequality—and it is to do this without much thought about whether it would have the capacity to produce the solutions. One might well ask why the agency is not beginning by finding out whether the problems are really school problems, or whether schools could solve them, rather than assuming that the answers to those queries were known to be affirmative? The answer, of course, has something to do with the fact that the planners correctly perceive that one major reason for NIE's acceptance is the notion that it will help solve those problems. In effect, then, NIE begins on a rather weak foundation.

The nastiest part of this situation, though, is that accepting a "major problems" mandate is not likely to reduce the potential for instability in an NIE. The earlier sections of this paper surveyed some of the problems the NIE seems destined to tackle and pointed out several of the barriers to solving them with an R&D agency. Some of the difficulties may arise from mis-definition: perhaps the problems NIE is to solve are not really school problems or are not soluble by schools. Other difficulties arise from the weak knowledge base in education and others arise from the absence of incentives to utilize R&D results.

Oddly enough, though, the enormous intellectual uncertainty which surrounds the schooling process has only heightened the anxiety to show that R&D really could work. And the desire to demonstrate good reasons to invest in educational R&D make it likely that an NIE would give high priority to producing quick solutions to educational problems. Among the reasons for such a course of action, naturally, would be the desire to assure continued support for the agency within government and especially in the Congress.

Concern for political support would not only be reflected in efforts to find some quick victories. It also is reasonable to suppose that high priority would be given to supporting professional activities traditionally supported by R&D funds—training, research, and development within universities and schools. While there are lots of good reasons for such support, one important motive would be the need to maintain relationships which could be translated into political support for the agency. This situation is not without irony, for to the extent that an NIE is unable to build a firm base of support within the government by solving school problems, it may become more dependent on the educational professions. And to the extent it depended on the professions for political support in the appropriations process, an NIE could be hampered in efforts to meet one of its other objectives—namely, to change the character and content of the professions. In addition, the pressure to demonstrate successes could easily have unhappy consequences for the quality of an NIE's work. Thus, erecting a "major problems" agenda for the NIE may interfere with one of its other objectives—namely, to improve the quality of R&D in education.

The tendency toward political instability will be compounded by the fact that evaluative criteria for educational R&D are murky; this is partly because we do not well understand the economic and social outcomes of schooling, and partly because many of the outcomes are moral and political, about which Americans will always disagree. The fog surrounding this area will not make it easy for the Congress or the executive branch to arrive at balanced judgments about an NIE's effectiveness and this may well give even greater impetus to efforts to produce quick successes.

The instability of an NIE is also likely to be somewhat increased by the fact that Americans' perception of school problems seems to change with fair regularity. This means that many of the problems it sets out to solve may well have faded from view by the time solutions are developed. One recent example of this is the general loss of interest in the reformation of science education not long after the post-Sputnik furor generated major curriculum development efforts. Another is the shift in black attitudes over the past several years, away from an interest in test scores. Many educational problems are not like heart attacks, which will continue to trouble people until someone invents a pacemaker. They often seem more like temporary fixations, which wax and wane rather quickly. And while this provides almost endless entertainment for people who write about education, it may put an NIE in the unhappy position of having no long-term appreciation for some of its efforts.

It is impossible to know what effect these constraints will have, but it would be no surprise if the instability of the NIE could lead either to excessive dependence on the educational professions, or to hasty efforts to demonstrate success, or both. It is worth noting that both elements have been present in the Office of Education's management of research and operating programs. Inventing a new institution does not dissipate the forces which have shaped federal education policy. The tension between the schools' relative impotence and the weak knowledge base in education on one hand, and the generally exaggerated hopes for education on the other has plagued education for some time, and until some elements in this configuration change, life for the NIE is likely to be difficult.

This is a formidable system of constraints, and there is no simple way to avoid the problems they pose. One step worth exploring, however, might be for the NIE to begin not by trying to solve problems which arise in the effort to make existing policies work, but by asking whether the assumptions on which those policies rest are correct. In this view, one of the first tasks of an R&D agency in education should be to carefully work out a basic research program designed to help us learn more about the schooling process. This is not to say there are no other things an NIE ought to do at the same time, but perhaps major efforts to develop and disseminate solutions to problems as poorly understood as those discussed earlier in this essay should be restrained.

Such a course could help the NIE to make the most effective use of its resources, and it might also help to avoid the pitfalls of R&D work which is not self-conscious about its assumptions. But it would imply an effort to hold in abeyance the assumptions about problem-solving explicit in the planning materials and instead to give high priority to the development of a research program aimed at a more adequate comprehension of what schools do. That implies a research program which elaborated a comprehensive set of models of the schooling process, which tested the validity of those models by research and experimentation, and which modified the models in light of evidence. Work of this sort would be reasonably likely to produce a more coherent view of the character of the schooling process, and knowledge about education might become more cumulative. It might even spare the grief associated with mis-directed government problem-solving efforts, and reduce the chances of major blunders in development and dissemination or mis-definition of NIE's role. But it would also require a considerable re-direction of the NIE away from quick problem-solving and that seems unlikely.

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