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ABSTRACT

The objectives of this report are stated in three broad questions that the research committee was asked to examine. (1) What are the principal lines of research being pursued at the present time that are significantly strengthening the scientific foundations of education, and what are some of their possible contributions to American education? Are there some lines of research that appear particularly promising and deserving of higher priority than they are now given? (2) Are current modes of conduct and support of fundamental research relevant to education adequate to ensure its quality and ultimate usefulness to education? If not, how might they be improved? (3) In light of answers to the above, what possible additions to or changes in policy relevant to fundamental research, if any, are recommended for consideration by the National Council on Educational Research or other appropriate bodies? (JD)

Fundamental Research and the Process of Education

A Report to the

NATIONAL INSTITUTE OF EDUCATION

by the

NATIONAL ACADEMY OF SCIENCES

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FUNDAMENTAL RESEARCH AND THE PROCESS OF EDUCATION

Final Report to the
National Institute of Education

by the

Committee on Fundamental Research Relevant to Education
Assembly of Behavioral and Social Sciences
National Research Council
National Academy of Sciences

Sara B. Kiesler and Charles F. Turner, Editors

Washington, D. C.

1977

NOTICE

The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the Councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the Committee responsible for the report were chosen for their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

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PREFACE

The Committee on Fundamental Research Relevant to Education, constituted in June 1976 by the National Research Council in cooperation with the National Academy of Education, was formed in response to a request from the National Institute of Education (NIE). One of the legislative charges to the Institute is that it seek to improve education in the United States by strengthening its scientific foundations. In light of that charge, Dr. Harold Hodgkinson, Director of the Institute, asked the Committee to recommend how that strengthening might be accomplished, by identifying promising lines of fundamental research, assessing the adequacy of federal support, and recommending changes in policy, if any, needing consideration by the National Council on Educational Research or other appropriate bodies.

This Committee did not conduct a scientific research project. We were asked, because of our experience and expertise as scientists and educators, to express some judgments about research and federal policy. We did not feel constrained--and were not asked--to suspend our initial belief in the value of fundamental inquiry for education. As persons who have committed our careers to fundamental research as well as to applied research and education, this belief was, and remains, strong. Nor did we feel it necessary to collect large amounts of new empirical data. There is much that we have learned over the years and much that others have learned. There exist numerous sources of information already available from reports, papers, and books on the topic of research and education. Our task, as we conceived it, was to review with each other our knowledge and perspectives and to learn from documents and colleagues outside the Committee. Our search for information and our discussions, while lengthy and to the point, were not so much exhaustive investigations as they were a form of shared reassessment of our judgments.

The Committee did try to solicit comments as widely as possible and to familiarize itself with all the pertinent literature. For example, we systematically reviewed previous reports and evaluations of change in education and the effects of research (see Bibliography), undertook a limited citation study of the flow of research information (see Appendix A), and collected information on the performers of basic research by examining current journals, books, and nationally distributed magazines and the recipients of research awards. Some of us examined the research cited in books that had had national impact on education.

The focus of much of our study was on research designed to understand the processes of individual learning and human development, the organization of social institutions, and interpersonal interaction. We reviewed only briefly applied research and work that translates basic research into educational materials. Our major concern with regard to applied research and development was to evaluate its scientific foundation.

We did not evaluate fundamental research on the subject matter of education, such as mathematics and physics. This decision was largely due to our limited time and expertise. During a one-day meeting, we did consult with a special mathematical and physical sciences panel of the Committee, whose members were George Pimentel (University of California at Berkeley), Henry Pollak (Bell Laboratories), Frederick Reif (University of California at Berkeley), Frank Westheimer (Harvard University), Hassler Whitney (Institute for Advanced Study), and Bernard Witkop (National Institutes of Health).

Our evaluation of research policy was focused mainly on the National Institute of Education, because of its mandate to improve the scientific foundation of education. Our review of the Institute and its programs was as comprehensive as we could make it. We examined at length and in detail the current spectrum of research support now maintained by the NIE and familiarized ourselves with its working structure. We interviewed program officials, examined budget documents and actual spending in detail, reviewed projects proposed to and supported by the NIE, and investigated provisions for maintaining scientific feasibility and quality. In addition, we reviewed the funding and management of educationally relevant research by the various governmental agencies that now offer such support. Among these are the Office of Education and the National Science Foundation. (During our deliberations the latter began a new program for research in science education.)

Our task in formulating and writing this report was made lighter than it might have been because many individuals helped. First, Philip Jackson, one of our own Committee members, deserves thanks. In an essay he wrote for the Committee, he captured our conception of the process by which research is diffused. A revised version of this essay constitutes Chapter 2 of this report.

The Director and Associate Directors of the National Institute of Education and several other past and present program officials provided much of the assistance we needed. They were both cooperative and sympathetic with our requests for information, quick answers to questions, and discussion. We especially wish to thank Dr. John Mays, who, as the Science Advisor of the NIE, was responsible for transmitting to us the largest portion of the information and background materials needed. He was invaluable as a source of historical background and citations.

Many others contributed at various stages of our work. For example, our initial search for information on both research and policy was aided by the directors of a large number of associations, who announced to educators and researchers our request for suggestions. In response to this request, we received over one hundred letters, visits, or calls. In addition, Christopher T. Cross, a knowledgeable staff person from the Congress, spoke to our Committee about congressional views. The aforementioned panel of mathematical and physical scientists commented on the first draft

of this report and met with us for a day. Dr. David A. Goslin, Executive Director of the Assembly of Behavioral and Social Sciences, and Dr. James G. March, of the NIE Council, also met with us and provided valuable suggestions. There were a large number of unwitting helpers, too, among whom we count the originators of the reports listed in the Bibliography and the authors of books on educational problems and organizations.

The revisions of this report have received careful scrutiny by reviewers within the Academy. These reviews, many of which were written in great detail, were extremely helpful; we owe our thanks to these anonymous reviewers. We are grateful, too, to Eugenia Grohman, Editor and Executive Associate of the Assembly, who helped us structure the report, and to Christine L. McShane, Assistant Editor, who critically edited the report and supervised its production.

Finally, we wish to thank Benita A. Anderson, the Administrative Secretary of our Committee, who not only typed the many versions of the report but was our primary research assistant, collecting information from journals and reports and collating by hand the large number of citations used in the study reported in Appendix A.

In conclusion, we joined the Committee with strongly held views about the nature of fundamental research and preferences regarding the lines of inquiry most worth pursuing. Another committee might have had somewhat different opinions. We tried to avoid narrowness on our Committee by reading and consulting widely, but we did not attempt a systematic survey of scientists and educators, nor did we try to achieve consensus on all aspects of the report. We do not presume to represent all our colleagues in this report nor to suggest that every part of this report represents the opinion of every Committee member. We did, in the end, find that the areas of agreement among Committee members were much greater than the areas of disagreement. As a whole, this report presents our collective judgment on those aspects of fundamental research relevant to education that we have been able to examine in these few months, and our recommendations regarding research policy represent some hard thinking and reflection on the part of each of us.

Sheldon H. White
Chairman, Committee on Fundamental
Research Relevant to Education

CHAPTER 1

INTRODUCTION

The modern conduct of education--through schools, colleges, training programs, television, publishing companies--touches every one of us in more ways, for more hours of the day, and probably with greater effect, than ever before. Possibly that is why education is so much in the news and is much of what seems to be behind the news. To many people, the quality and quantity of education seems connected with their own and their children's chances for success, important social problems such as unemployment and crime, and the nation's stature. Our social, political, and economic ills and expectations transcend education, but the perceived importance of "getting an education" to alleviate problems and achieve dreams is signifficant. Nearly all of the government's major social programs have an education component. Therefore, when scientists and scholars turned their research toward the understanding of education, people hoped for practical improvements in instruction as well as the alleviation of apparently related societal problems. The questions sometimes raised by these hopes, and nourished by the evidence of moon landings, antibiotics, and atomic energy, are how soon and with what effect does research on fundamental processes bear practical fruit? These questions find pointed expression in the phrase, "That's very interesting, Professor, but what's its relevance?"

This report on fundamental research addresses the issue of relevance by aiming at three questions that we, a committee of scientists and educators, believe are useful for a serious discussion of national research policy for education: "What do you mean by relevance?" "What kinds of fundamental research have potential relevance?" "How can federal policy strengthen fundamental research relevant to education?"

Our answers to those questions take the following form: first, what makes fundamental research relevant is the improved knowledge it generates, which in turn is a condition for more useful views of how education takes place, new visions of what is educationally possible, stronger commitments by those who educate, and improvements in instruction and educational institutions. Second, the kinds of fundamental research that have potential relevance derive from a broad range of inquiry focused on basic questions concerning how people mature, learn, and interact and how social institutions affect them. Third, federal policy for fundamental research

relevant to education should be designed or redesigned to improve the quality of work of those who conduct research and their working environment, to enlarge the scope of fundamental inquiry, and to provide adequate resources for its development.

These conclusions derive from our views of research and how it is administered. We believe that fundamental research relevant to education is basically a development of ideas for explaining how and why education occurs across places, time, and groups of people. The quality of this development is reflected in the validity of the new concepts and understanding that gradually diffuse to educators and the public, where it stands its ultimate test: the degree to which educators, students, and citizens find the new ideas more useful, more sensible, than the old ways of thinking. In turn, the quality of fundamental research depends heavily on the standards of those engaged in it and on their resources for systematic observation and careful analysis, building upon the work of others, responding to emerging possibilities, and examining the many realms in which basic educational processes occur. These resources depend on two factors insufficiently represented in the practice of federal policy today--commitments to financial support and flexible management that encourage self-directed fundamental inquiry.

Definitions

Defining the subject matter of this report proved to be difficult. The colloquial definition of education is imprecise--to some it means schooling, and to some it means more than that. Our discussions of fundamental research relevant to education hinged on our agreeing on a definition of education itself.

In Western society, the classical definition of education is intellectual development, or learning. The Latin origins of the noun, education, convey the notion of a leading out of ignorance. Plato recommended geometry as a course of serious study, not for the practical advantages it might afford in battle or everyday life, but rather because "geometry will draw the soul towards the truth and create the spirit of philosophy and raise up that which is unhappily allowed to fall down . . ." (*The Republic*, Book 7). Echoing this attitude over 2,000 years later, John Henry Cardinal Newman argued that the advantages of advanced education are "in one word, the culture of the intellect" (*The Idea of a University*, 1852).

While this definition of education is simple, it is not entirely consistent with that of many Americans. In the minds of many, education means schools, colleges, and other institutions that, in turn, are called on to provide many functions other than intellectual development. Thus, "education" can be viewed as a means for socializing children,[1] providing day-care, vocational training, conferring status or credentials,

[1]Article 26 of the United Nations Declaration of Human Rights says, "Education shall be directed to the . . . strengthening of respect for human rights and fundamental freedoms. It shall promote understanding, tolerance and friendship among all nations, racial or religious groups, and shall further the . . . maintenance of peace" (Dec. 10, 1948).

and stimulating national development. When, in a recent survey, parents were asked to rank the relative importance of various attributes of their children, intellectual curiosity ranked tenth (after characteristics like honesty and good manners) and success in school was twelfth. Letters to the Committee from researchers and educators also showed a range of expectations for educational institutions. In these letters, 40 percent emphasized that learning should be their primary function, but 60 percent mentioned training for occupations, good citizenship, mental health, or national development. These views suggest that if intellectual development were the only outcome of schooling, parents would not expect their children to spend at least twelve years in school, and the public would not spend an average of over $15,000 per child on schooling. More to the point, it suggests that our discussion would be too narrow were we to focus on intellectual development alone.

We have decided to use, for the purposes of this report, a two-dimensional definition of education. On one hand, education is personal and intellectual development or learning, which may occur either inside or outside schools. On the other hand, education is what educational institutions do, or are expected to do. It is our belief that fundamental research is relevant to education to the extent that it leads to an understanding of these domains.

What is fundamental research? The Committee decided, arbitrarily, that there was no need to make a distinction between the traditional term "pure science," the popular "basic research," and "fundamental" research. Basic research need not be equated, as it once was by many, with laboratory work or research conducted exclusively in academic departments. We believe it has come to mean disciplined research to discover general principles, but not necessarily by a particular academic or methodological route. Thus, for example, some of the work of psychologists on learning from Sesame Street, conducted on the site where the program was developed, is basic research, truly fundamental to understanding how children learn.

Fundamental research in education is disciplined inquiry whose purpose is to understand why and how education takes place. These processes are the subject matter of the behavioral and social sciences, such as economics, sociology, political science, psychology, and anthropology, and some of the humanities, such as philosophy and history. Our ability to comprehend the basic activities of education, to recognize and articulate problems, and to suggest ways and means for solving them depends heavily on the knowledge developed by these sciences and humanities.

Objectives

As a guide to the Committee's work, the National Institute of Education (NIE) asked us to consider three questions:

1. What are the principal lines of research being pursued at the present time that are significantly strengthening the scientific foundations of education, and what are some of their possible contributions to American education? Are there some lines of research that appear particularly promising and deserving of higher priority than they are now given?
2. Are current modes of conduct and support of fundamental research relevant to education adequate to ensure its quality and ultimate usefulness to education? If not, how might they be improved?

3. In light of answers to the above, what possible additions to or changes in policy relevant to fundamental research, if any, are recommended for consideration by the National Council on Educational Research or other appropriate bodies?

We began our examination of these questions by defining, operationally, the specific issues we would address and the array of work we would evaluate. Three major considerations affected our decisions. First, we were asked to prepare a draft of the report in nine months; second, we did not wish to repeat what other groups had done recently; and third, we wanted to allocate our limited time to those issues we felt were most important and about which we were most knowledgeable.

The first question asked of the Committee by the Director of the NIE received considerable discussion and study. Implicit in the task requested--that of identifying lines of fundamental research having potential significance for education and describing research deserving of higher priority--was the more basic task of articulating how fundamental research makes a contribution to education. In short: How does education improve? How does one define a "contribution" from research? Our reading of government documents and reports on education, the testimony of government officials before Congress, and our discussions with congressional staff and program officials greatly increased our concern with these questions. We finally concluded that the usual evaluation of the impact of fundamental research knowledge on education is far too limited, and deserved our primary attention.

Education is a human service, a massive one. It does not change by leaps and bounds, and even when changes are introduced by design, as in a new curriculum, one finds upon analysis that adaptation to the novelty takes place through a slow, complex, political process. Since clearly defined improvements in education are rare, it is also rare to find a direct and simple movement from fundamental research knowledge to educational practice. And yet anyone familiar with schools, school management, teacher training, and parent-school relationships knows of the movements that have taken place from disciplinary knowledge to public discussion, curricula, and teacher beliefs, which ultimately define practice in education. We found that many reports, program guidelines, and budget documents reflect a far more limited perspective. Presumed in these written materials and the words of many government officials who spoke with us was the conviction that there must be identifiable change that clearly results from a well-defined, once-articulated set of ideas.

We therefore undertook to examine a subtler and deeper vein of transmission from knowledge to education. We believe that educational change is slower, more subtle, and more complex than that usually envisioned, and that one of the most important influences that fundamental research has on education comes through diffusion rather than dissemination. Our conception of this diffusion process is discussed in Chapter 2.

The consensus we reached as a committee on the contribution of basic research to education had considerable impact on a subsequent decision to restate the first question directed to us as: How does fundamental research contribute to education? It was our judgment that ideas from basic research flow gradually, and in complex ways, to the educational community,

citizens, parents, and students. These ideas affect not simply educational techniques but the way people think about education, the criticisms and enthusiasms they have regarding it, and the aspirations they hold for themselves and others. Some of this influence can be foreseen, roughly, in fundamental research as it progresses, but much of it cannot. We therefore felt it inappropriate to rank specific lines of research that might make a contribution to education. Instead, we attempted to delineate, by example, fundamental inquiry that has potential usefulness for education. We wished, through these examples, to illustrate the variety of methods that can be used to address topics and problems relevant to education--the process of building the scientific foundation of education--and to demonstrate the potential of contemporary basic research. These examples are found in Chapter 3.

We had also a more general task of evaluating the range of basic research and the health of work relevant to education. The earlier work of individuals and groups aided in this task. One of the most thoughtful volumes we read was *Research for Tomorrow's Schools*, edited by Cronbach and Suppes. We also read a large number of papers written by working groups of researchers who had been sponsored by the NIE. Reports by distinguished groups identified interesting and promising lines of reserach in neuropsychology, information processing, cognitive development, social development, linquistics, sociology, anthropology, and various kinds of learning difficulties. We discovered, in addition, a large number of literature reviews in the various disciplines that identified important problems on which excellent research was being conducted. Finally, we considered a series of other reports, some sponsored by the National Research Council, that evaluated and listed promising lines of research. (These sources are listed in the bibliography.) Once we were fully aware of all this previous work, we concluded that the question of what research, in particular, might be usefully supported had already been adequately answered, at least for the time being. Promising topics for fundamental research have been laid down, if not to the complete satisfaction of all the Committee members, then in abundance and with sufficient regard for quality and promise. Furthermore, we think that identification of promising research must, as a general practice, be based upon the implicit guidance that derives from the system of peer review.

The second and third questions directed to the Committee by the Director of the NIE concerned federal policy, which we were to evaluate and about which we were to formulate recommendations. In this effort we were guided by two assumptions based upon training and experience. First, research is only so "relevant" as its quality allows. If research is not of high quality, no amount of apparent pertinence to important educational issues in its content, method, or site of study will make it relevant. Therefore, research policy must enable the research environment to promote quality in the work of researchers whose support derives from the federal government. Second, today's research cannot be conducted, on the whole, without financial support from the federal government. The problems are far too complex and numerous, the facilities required are too expensive, and the training needs are too sophisticated. Thus, we examined research policy with attention to the adequacy of funding as well as to the quality it encouraged.

Our evaluation of research policy, described in Chapter 4, provoked much discussion and resulted in the recommendations ending the report. We hope these recommendations communicate our continuing belief that the federal government can and should support the growth of knowledge about education that we need to alleviate its problems, to build upon its strengths, and to shape it for the benefit of future generations.

CHAPTER 2:

FUNDAMENTAL RESEARCH AND EDUCATION

Ideas about the relationship between fundamental research and education are commonly limited by a stereotypic view of what that relationship is. The stereotype can be described as a dialogue between a research psychologist, assumed to be a university professor, and a classroom teacher: Using the results of research, the professor advises the teacher how to teach. That stereotypic view of the connection between research and education generally assumes that the knowledge of greatest value to educators specifies, at least ideally, a set of pedagogical "dos" and "don'ts," and that the prime consumer of that knowledge is the classroom teacher. These assumptions have been widely held since the development of psychology as a science; they were a force in the creation of many schools of education and guided early educational research. Their popularity is understandable, for given the subject matter of psychology, it seems reasonable to expect it to be of direct benefit to persons whose occupational concerns are interpersonal. And yet, they are unwise assumptions, for they tend to act as a set of blinders, closing off a fuller view of what education can gain from research.

What is needed is a breaking out of the stereotypic view. The results of research and the practice of teaching are related in many more ways than as a dialogue between a psychologist and a teacher. First, the research side of the dialogue includes representatives of all the social and behavioral sciences and some of the humanities. Each relates in a fundamental way to the complex process of education. Physical and natural scientists should also be represented, for they contribute much to what educators teach.

Second, the teacher's side of the dialogue includes administrators, school board members, textbook writers, and all kinds of educational specialists as well as the state and federal legislators and other policy makers whose decisions help to shape the educational system. The cast of educators grows quickly, for the concept of education involves far more than schooling, no matter how close the pairing of education and schools in everyday thought. Families educate, as do peer groups. Education goes on in churches and work places, in libraries and museums, and in front of movie and television screens. Any discussion about the relationship between fundamental research and education cannot be restricted to what goes on in schools and classrooms. It must break out of those boundaries if it is to treat the panoply of settings in which people become educated.

Having enlarged the cast of characters, however, we are still faced with a metaphorical dialogue that does not do justice to the relationship between research and educational practice.

There is no army of educational practitioners expectantly waiting to hear what the fundamental researchers have to say, nor is there a corresponding group of researchers. The truth is that most practitioners do not turn directly to researchers for advice, nor do most researchers offer it. The two groups talk more among themselves than they do to each other--and so they should if they are to do justice to their respective tasks. The metaphor does not jibe with the facts.

Introducing a third party to the dialogue, whose job it is to facilitate communication between the first two, might improve the usefulness of the metaphor. This group, the "disseminators," would include the popularizers, the translators, the journalists, and the reporters, who put the writings of the fundamental researchers into a form that is useful to practitioners. Professors of education who extract practical implications from work that appears not to have any may also act as disseminators.

The introduction of disseminators may add a touch of realism to the dialogue, but it does nothing to free us from the limitations of the belief that the ultimate contribution of fundamental research, from whatever source, is to tell the practitioner how to teach. To escape from the constraint of that belief, we must turn instead to thoughts about thought itself, particularly those of educational practitioners. The goal is to find some way of describing in general terms the possible linkages between research, on one hand, and the practitioner's world, on the other.

Conventionally, we think of practitioners as doers, people who apply skills and knowledge to the solution of practical problems. It follows from this view that to help practitioners is to influence their way of doing, to influence their actions, in the settings in which they work. Hence, we come to the conclusion that the results of research (or for that matter, any other activity purported to be of value to practitioners), leave their traces in some modification of that activity we call "practice."

This view of the practitioner is too simple. Certainly, practitioners have changed and improved what they do as a function of what scholars and researchers have said. Sometimes those changes have been dramatic and the lines of influence direct. Quite often, however, the shifts in practice are caused indirectly--their scholarly roots buried in a tangle of causal agents that include public opinion, political expediency, and practical necessity. In short, the conventional view of practitioners and how they change lacks subtlety.

The conventional view has another, more important weakness: It fails to acknowledge the special perspectives of practitioners. In so doing, it obscures what stands to be changed other than the practitioners' way of doing things. We need a framework within which to discuss the approach of practitioners to their profession--their manner of thinking about what they do. We find it helpful to discuss the perspectives of educational practitioners in terms of four groupings: (1) a view of reality, (2) a vision of the achievable, (3) know-how, and (4) a commitment to act. Each of these constitutes a region of influence--a set of ideas, beliefs, and opinions vulnerable to change. Fundamental research relevant

to education is but one set of forces--though an important one, we believe--contributing to changes in each of these groupings. Even as heuristic devices, the four groupings require a much fuller elaboration than can be given here, but we present a sketch of their meaning.

"A view of reality," as the phrase is used here, refers to the educational practitioner's way of seeing the world, together with the language used to talk about that world. It also refers to the relative importance attached to what is seen and talked about, the notion of valuing. In the most general terms, then, it contains the practitioner's answer to the question of what is real and what is important, insofar as that reality pertains to educational matters.

To a large extent, the practitioner's view of reality is commonsensical and shared by us all. All of us, if called upon to describe the contents of educational settings, would be quick to identify teachers, students, textbooks, and most of the other physical paraphernalia commonly found there. We would also claim to see that students differ from one another in their psychological makeup, that teachers carry certain responsibilities, and that some textbooks are better written than others. Yet even these shared perceptions, these common facts of life, differ in salience for those who are practitioners and those who are not.

In addition, even more specialized ways of seeing and speaking, which educational practitioners do not necessarily share with the rest of us, tell us something about how they see the world. Words like overachievement, hyperactivity, cultural deprivation, and reading readiness stand for a way of looking at things that sets their users apart from others.

The manner by which practitioners acquire their view of reality is as complicated as the view itself. Part of it doubtlessly derives from the common events of life, a portion is surely attributable to professional training, and another to professional experience. The question of how fundamental research contributes to this view is in itself worthy of serious investigation. For example, one might trace the roots of the remarkable change in views of gifted pupils that educators have undergone. Having abandoned the widely held misconception of the gifted as socially immature, physically weak, and prone to insanity (a view challenged by the research of Hollingworth and Terman, for example) practitioners began debating the merits of skipping grades, special classes for the gifted and talented, and various means of challenging their brighter pupils. Even without such an investigation, however, we can readily see that concepts of social class, intelligence, bureaucracy, ethnicity, cognition, and others used daily by practicing educators had their origin in the work of scholars and researchers or have had their meanings modified by that work.

"A vision of the achievable," as the term implies, refers not to what is, but what might be. It encompasses a view of the future, expressed in terms of purposes, goals, objectives, or aims. All purposive action implies such a vision. A vision of the achievable includes such narrowly defined goals as curriculum objectives of the sort found in lesson plans and teacher guidebooks, of course, but it also embraces vaguer hopes and grander expectations, including aspirations sufficiently broad in scope to shape policy and to inspire action. When educators speak of producing good citizens or helping to erase social inequities, they are expressing a portion of this vision.

As is true for the educator's view of reality, the sources of these visions of the achievable, large and small, are rooted in a causal network too complex to unravel completely, yet changes in that vision have occurred over time. It is now thought, for example, that far more people of all ages and stations in life stand to benefit from formal schooling than was thought to be so a generation or two ago. This belief was bolstered by fundamental research: "The quality of intelligence can be modified." "Our inner-city schools are not 'hopeless'." "The severely retarded can be taught." Fundamental research contributing to these expectations has included animal and human studies of deprivation, social psychological studies of children's attitudes and self-esteem, family interaction and prejudice, and investigations of environmental disadvantages and of childhood in other countries.

Work on the remediation of serious physical and psychological handicaps has inspired, in the last fifty years, an entirely new branch of educational endeavor and a willingness to spend time on people who in earlier generations were neglected. The kinds of research and scholarship that have revealed the conditions of the underprivileged in this country and throughout the world have served to intensify educational efforts to overcome the devastations of cultural and social impoverishment. Each new advance in understanding of how the mind works, each contribution to thought that serves to deepen the appreciation of social justice, has the potential of altering educational vision.

"Know-how" is an old-fashioned phrase that means craft, technique, procedure, plan of action, method. In addition to seeing the world in a certain way and extending that sight into the future in the form of goals and objectives, educators must be prepared to act. They must know what to do to attain the goals they envision. When people seek to understand what fundamental research and scholarship might contribute to the teacher or the school administrator, know-how tends to get exclusive attention.

Educators, however, need more than a set of procedures for carrying out their work, crucial though such procedures might be. Traditional concern with translating the outcomes of research into a plan for action is not so much wrong as excessively narrow. Moreover, the narrowness derives from more than the fact that practice per se has been the focus of the search for a linkage with the world of scholarship; it also has to do with the almost total absorption with the goal of improving practice and discovering better techniques. We seldom ask whether educators might now be doing as well as can be done in many aspects of their endeavor. We might pay more attention to the possibility that educators may deserve and benefit greatly from some external confirmation of the appropriateness of much that they are now doing.

For example, there are hundreds of children who are obviously bright but are not very good students. Sensitive teachers give these children emotional support and encouragement, raising the children's self-esteem. Basic research suggests that many of these teachers are doing as well as anyone could, given what we know and what we do not yet know. We do know that skills mature at different rates: some children will be quick to learn addition and slow to ride a bike; others will be slow to learn to add but quick to learn to ride. We know that all of these children need confidence in themselves and support from adults who expect they will

eventually succeed. We do not know why these children differ, and trying to "prevent" the differences by tampering with curricula, desks, noise levels, and so forth is simply premature. Using what we already know about children and their development and building on that knowledge is more sensible.

So long as we remain fixed on the goal of improvement, we tend to overlook the many kinds of support for the efforts of educators that knowledge from the social sciences or elsewhere might provide. We tend to forget that a firmer rationale for current practices might prove a greater boon to the vitality of educational efforts than would an entire compilation of suggestions about how to improve this or that pedagogical technique.

Finally, the educational practitioner, by definition, is not simply a person who knows how to do something--teach a class, run a school, plan a curriculum, design a test, or what have you--but is also a willing actor who practices with some degree of enthusiasm. The willingness of practitioners to continue their work, which we are calling a "commitment to act," can be strengthened or weakened by a vast number of considerations, ranging from such mundane matters as salary and working conditions to those principles that can add a sense of vocation, a calling, to work. That sense of calling makes of the practitioner, not simply a person performing a task, but also a person of principle.

It is difficult to speak of the commitment to act without leaving the impression that all educational action is inspired by noble thoughts. Such an idealized image is of course false. Yet we also know, or at least suspect, that if all such thoughts were absent, if the practice of education were motivated by nothing more than the need to make a living, the enterprise itself would falter. It is imperative, therefore, to understand how to sustain this sense of mission in practitioners.

Is it not possible that fundamental research may in some fashion contribute to practitioners' commitment to act? Certainly we can imagine educators thinking about what they read and how it relates to their work. Such an attitude of seriousness in reaching out for deepened understanding is itself an expression of the practitioner's commitment to act. A person's seriousness feeds upon the seriousness of others, and sound scholarship provides a rich resource.

Thus, one way of describing the manifold connections between fundamental research and the practice of education is to establish the potential of such research to alter practitioners' views of reality, to change their conceptions of what is educationally possible, to offer them better ways of working as well as an improved rationale for their actions, and to deepen their commitment to their work. Though admittedly incomplete, this conception of how research might have an impact on education is offered as a substitute for the conventional stereotype of omniscient scientists telling teachers how to teach.

How do we know that fundamental research does indeed influence educators in the ways we suggested it may? The usual reply to such a query, even when limited to the traditional link between research and practice, is to select dramatic examples that will overcome the critic's doubts. Typically, a search turns up the names of past greats, such as Freud, Dewey, and Thorndike, or outstanding contemporaries, such as Skinner,

Piaget, and Mead, whose ideas have obviously left their mark on both thought and practice in education. The work of these people is surely concrete evidence that fundamental research makes an important difference in educational affairs. Educators, parents, government officials, and other people throughout the world see reality differently and talk about it differently as a result of what these few people have written and said. Educational goals and practices have clearly been modified as a result of their seminal ideas. It is even possible to gather testimony that would show that the educator's commitment to act has in many instances been strengthened by the insights of these scholars.

Offering such examples as evidence of the importance of research, however, neglects the vast bulk of scholarship (and, therefore, the great majority of scholars) in favor of a few of its stars. So constrained, we limit the search for effects that, as it were, have surnames attached to them. In doing so, we ignore many ideas that have profoundly affected educational practice; because they have come form so many different sources and have been reinforced by the writing of so many different scholars, they have become, in effect, anonymous.

Consider, for example, research on reading. The sources of significant contributions to this research include major universities and research institutes on three continents--North America, Asia, and Europe. This international community of scholars has begun to understand why learning to speak is so easy but learning to read, for many, is so difficult. They have learned, for instance, that being able to hear, segment, and repeat phrases, words, and phonemes found in the flow of speech is one important precondition for learning to read; and that "segmentation" can be taught to those to whom the skill comes slowly. This work cannot be summarized by pointing to one or two great people but must be characterized as a cumulative flow of ideas from many sources that have outlined what can be done to improve a child's readiness to read.

If we focus in particular on an individual's contribution to education, it is easy to neglect the work of many people to bring that contribution into practice over the years. For instance, we are indebted to Jean Piaget for the concept of sensorimotor intelligence in infants; his work forty years ago changed the view of infant behavior from one of helpless, reflexive activity to one of intense interaction with the environment, undergoing systematic changes. Piaget's descriptions of infant development stimulated an enormous amount of research (particularly during the 1960s) on infant behavior: how well they can discriminate a wide variety of stimuli, learn complex associations, and, in a sense, control their social environment by eliciting stimulation from parents. Myths about what babies could not do collapsed as scientists, with new or improved techniques, demonstrated what they could do. This research had tremendous implications for the appreciation of both nature and nurture in the development of the human infant, for knowledge about individual differences, and for the capacity to help children who do not develop normally or who are "at risk." The realization of the infant's rich behavioral repertoire has led today to a

whole new field of endeavor, aimed at identifying the infant-environment combinations that will elicit, maintain, and maximize developmental potential.[2]

The influence of fundamental research, therefore, is far more significant than a set of biographical examples indicates. Scholarship in general enters the minds and colors the actions of educators through a series of filters that are as yet poorly understood. What is needed is some way of describing this filtering process (see inset p. 14, for example).

A beginning approach to that wider view, but one that still keeps us too closely attached to the contributions of individuals, is to examine the bibliographic sources used by educational writers. For example, a review of the references cited in Charles Silberman's *Crisis in the Classroom* (1971), surely one of the most widely read educational books of this decade, reveals not simply the names of the six scholars we have mentioned, but literally dozens of others, including economists, anthropologists, sociologists, historians, philoscphers, literary and social critics, jurists, and even a political leader or two.

Or consider another influential book of the late 1960s, Rosenthal and Jacobson's *Pygmalion in the Classroom*. Among approximately 230 references, one finds not only the psychologists, who might be expected to be referenced in a work that is largely psychological in character, but also scores of others from related disciplines. In a volume as exclusively educational as a recent Yearbook of the National Society for the study of Education, entitled *The Curriculum: Retrospect and Prospect*, the index is dominated by reference to educational writers, as one might expect, yet we also find there some interesting surprises: names like Niels Bohr, Kenneth Boulding, Sir Kenneth Clark, Edward Hall, David Hume, and C. Wright Mills.

An examination of references in the periodicals of education shows a similar diversity of sources. Our own limited review, described in Appendix A, indicates that the journals of education draw heavily on fundamental research. The educational magazines, written for the practitioner, also cite basic research. In fact, among the top twenty periodicals referenced in educational periodicals, approximately half are basic research journals representing an array of disciplines: psychology, sociology, economics, statistics, linguistics, political science, and anthropology.

Now it is a large step and a dangerous one to move from even a brief examination of bibliographic citations to the conclusion that the works cited have had a real influence in the field of education. The majority of such attempts to trace the impact of research fail to prove whether (or to what good) research influences practice and indicate only where research may have had impact.[3] For example, one finds in the writings of

[2]For more detailed analyses of the research described in the previous paragraphs, see Gibson and Levin (1975) on reading and Horowitz and Dunn (1976) on infant development. In the United States, most of this work has been supported by the National Institutes of Health, the National Science Foundation, and the National Institute of Mental Health. The Office of Education has sponsored research on reading, as has, more recently, the National Institute of Education.

[3]For a major review and discussion, see Clifford (1973).

THE FILTERING OF IDEAS FROM FUNDAMENTAL RESEARCH

CITATIONS FROM "DR. SPOCK" - 1946

"244. Cravin for sweets is often caused by taboos . . . Dr. Clara Davis in her experiments in letting children choose their own diets from a variety of natural foods found that in the long run they only wanted a reasonable amount of the sweeter foods (Spock, 1946, pp. 242)."

Spock is referring to Davis, C. M., Self-selection of diet by newly weaned infants. American Journal of Diseases in Children, 1928, 36, 651-679.

"289. Balkiness between two and three . . . The 1-year-old contradicts his mother. The 2 1/2-year-old even contradicts himself. (Gesell and Ilg bring this out clearly in Infant and Child in the Culture of Today) (Spock, 1946, pp. 285)."

Reference is to Gesell, A. & Ilg, F. L. (1943) New York: Harper and Brothers.

"336. Democracy builds discipline . . . Actual experiments have shown that children with a teacher who tells them what to do at every step of the way will do a good job while she is in the room. But when she goes out, a lot of them stop working. . . . These experiments showed that children who have helped choose and plan their own work, and have co-operated with each other in carrying it out, will accomplish almost as much when the teacher is out of the room as in . . . (Spock, 1946, pp. 330)."

Spock is referring to research conducted in Kurt Lewin's laboratory. (See Lippitt, R. (1940) An experimental study of the effect of democratic and authoritarian group atmospheres. Univ. of Iowa Studies of Child Welfare 16:43-195; Lippitt, R. and White, R. (1943) The "social climate" of children's groups. In R. G. Barker, I. Kounin, and H. Wright, Eds., Child Behavior and Development. New York: McGraw Hill, pp. 485-508).

CITATIONS FROM "DR. SPOCK" - 1968

"31. What regularity and flexibility are all about . . . During the first half of this century in this country, babies were usually kept on very strict, regular schedules. . . . It took many more years before doctors dared to begin experimenting with flexible schedules. . . . The first experiments were carried out by Dr. Preston McLendon and Mrs. Frances P. Simsarian, a psychologist and a new mother, with Mrs. Simsarian's new baby. . . . They called this an experiment in "self-demand" feeding. This term has become well known. . . . Since that experiment led the way, in 1942, there has been a general relaxation in infant feeding schedules, which has had a wholesome effect on babies and parents (Spock, 1968, pp. 60-61)."

"462. The control of aggression . . . nowadays I'd give a mother much more encouragement in her inclination to guide her son away from violence. A number of occurrences have convinced me of the importance of this. . . . Watching violence can lower a child's standard of behavior. Recent psychological experiments have shown that watching brutality stimulates at least slight cruelty in adults, too (Spock, 1968, pp. 313-314)."

The experiments to which Spock refers are described in Berkowitz, L. (1962) Aggression. New York: McGraw Hill.

"585. Identity. A central problem for the adolescent and the young adult is to find out what kind of person he is going to be, doing what work, living by what principles. It's partly a conscious but even more an unconscious process. Erik Erikson has called this the identity crisis and exemplified it in his biography of Martin Luther (Spock, 1968, pp. 421)."

See Erikson, E. (1958) Young Man Luther, New York: Norton.

ADVICE FROM "DR. SPOCK" - 1946

"341. The extra bright child . . . This brings up the question of teaching a bright child to read and figure at home before he starts first grade. It often does harm, and it never helps. It will only put him out of step with the other children, and may make it more difficult for him to catch onto the school's system of teaching these subjects . . . (Spock, 1946, p. 334)."

AND IN 1968

"570. The extra bright child . . . That brings up the question of teaching a bright child to read and figure at home before he starts first grade. A parent may say that the child is asking questions . . . . This is true to a degree with some children, and there is no harm in casually answering their questions (Spock, 1968, p. 406)."

The research which had most effect on changing conceptions of gifted children was probably the early work of Leta Hollingworth and Lewis Terman. (First publications: Hollingworth, L. S. (1926) Gifted Children, Their Nature and Nurture. New York: World Book Co.; Terman, L. M. et al. (1926) Mental and Physical Traits of a Thousand Gifted Children. Stanford, Calif.: Stanford U. Press.) In addition, sociologists such as Robert Havighurst (1961) Conditions productive of superior children. Teachers College Record 62:524-531, in mapping out the relationships between social class and achievement, stimulated ideas about how parental behavior in the home influenced children's achievement. The work of psychologists such as David McClelland and his colleagues (e.g., McClelland, D. C., Atkinson, J., Clark, R. and Lowell, E. (1953) The Achievement Motive. New York: Appleton-Century-Crofts.) indicated also the importance of early training in the home. Finally, animal and human research on curiosity (e.g., Harlow H. F. (1953) Mice, monkeys, men and motives. Psychological Review 60:23-32) and on stimulation (e.g., Levine, S. (1960) Stimulation in infancy. Scientific American 202:80-86) were influential in provoking and reinforcing ideas about the importance of stimulation and challenge in children's development.

IN 1946

"343. Poor reading because of left-right confusion . . . a certain number of children, particularly boys . . . begin to be confused between 'dog' and 'god'. . . . This difficulty occurs more commonly in the child who is neither strongly right- or left-handed, or who has been changed from left to right by training . . . (Spock, 1946, p. 406).

AND IN 1968

"572. Poor reading because of slow development of memory. To you and me the word 'dog' looks entirely different from the work 'god'. . . . But there are about 10% of children--most of them boys--who have more than average difficulty recognizing and remembering the appearance of words. . . . They need to be reassured by parents and teachers that this is a special memory problem . . . that they will learn to read and write and spell as soon as they are able (Spock, 1968, pp. 408-409)."

The notion that reading and other academic difficulties are primarily a matter of "normal" developmental lags (and spurts) was influenced heavily by research indicating the low correlation between tested IQs of very young children and of 18-year-olds--that is, the findings that intervention may not be necessary to counteract slowness in an academic skill (e.g., Hunt, J. McV. (1964) How children develop intellectually. Children 11:83-91). Another influence from research on ideas about reading was due to the shift in attention from the role of response-learning to that of perception and sensation in intellectual development. (See, for example, Dennis, W., and Dennis, M. G. (1940) The effect of crawling practice upon the onset of walking on Hopi children. Journal of Genetic Psychology 56.) Studies of cultural deprivation and of self-esteem influenced the growing conviction that a child's self-confidence is crucial for academic success (e.g., Lewis, O. (1961) The Children of Sanchez. New York: Random House).

educators throughout the twentieth century references to fundamental research used to bolster the movement to gear texts and curricula to student abilities. The beginnings of that movement, however, antedated the scientific research, and it was surely given impetus by such social phenomena as the increasing sophistication of teachers, whose average years of schooling advanced from twelve in 1900 to about seventeen in 1970. Nonetheless, large bodies of research show remarkably close ties with changes in practice. These changes--the assignment of different textbooks to pupils at different grade levels, the placement of children within classes in different reading groups, and the abandonment of useless, boring, and difficult tasks, on which a large proportion of students invariably failed--all required a new way of thinking about children. Fundamental inquiry has supported, even provoked, these intellectual revolutions.

We might ask at this point, if so many already contribute to educational inquiry, why make a special effort to encourage others? Furthermore, if the dynamics of influence are actually opaque and mysterious, does not supporting fundamental research in the hope of a salutary effect on educational practice or practitioners become a very risky business indeed? Finally, if educators truly feed on such intellectual resources, how is it that in their actions they falter so? If so much knowledge is available, why do we continue to hear of falling test scores, vandalism in classrooms, poor readers, and college graduades who can barely write grammatical sentences? These are tough questions. They require much fuller answers than can be given here, but we can point toward the directions in which those answers may lie.

The openness of the model of influence being proposed here, together with the vagueness of its operation, is troublesome. It allows ideas from almost anywhere to insinuate their way into the consciousness of educators and there to influence how they look at the world and act upon it. Such a model suggests that educators already have more than enough ideas.

Of course, there is always a shortage of good ideas, and always room for new knowledge. But this statement alone is unsatisfying, for it does not tell us what "good" means within the present context and does not contain any hint of what new knowledge should be pursued within the many intellectual domains open to exploration. Ideas that are good in the sense of the word used here are those buttressed by rational and empirical arguments, which are the kinds of arguments offered by scientific research and disciplined scholarship. Some knowledge, on the face of it, is closely related to the substantive concerns of educators, some more distantly so. Within broad limits, it is the former to which we would give preference in seeking support for new endeavors.

These considerations lie behind the contention that serious thinkers in the social and behavioral sciences and the humanities such as philosophy or history are likely to affect the collective consciousness of educators. Their task is to understand better how, where, and why people learn and mature. The history of science suggests that we should hesitate to predict the impact of new knowledge, but research on the brain will surely turn up insights that find their way by some circuitous route into the thinking patterns of educators, and research into the origins and maintenance of social class structure is likely to do so. The pursuit of both efforts entails some risk, to be sure. There is obviously no guarantee that any

research, fundamental or applied, will have beneficial consequences for educational practitioners. This does not mean, however, that it is absolutely impossible to predict which are likely to yield such results or to judge which have yielded results. In the end, we can judge by the evidence of use--whether the old idea, like the kerosene lamp, is discarded because the new idea, like the electric light, is more useful, sensible, and efficient.

The problems that continue to plague educational efforts, and schools in particular, are indeed an embarrassment, especially so in the light of all that has been written and said to aid the process of education. Why have we not yet learned how to eliminate reading problems? Why is learning how to write correct English still such a mystery for so many?

There is an easy answer to such questions, but it is not very satisfying: human beings are complex creatures, far more complex than the most complicated machine that they themselves have ever built. Small wonder, therefore, that we have only begun to probe the mysteries that contain the answers to educational strivings. Such a reply is undeniable but very frustrating, for it implies that we shall be saddled with the same problems for a long time to come. Yet it is also possible, if we look back, to gain some solace from the genuine progress that has been made.

The glacial advance of human understanding is a topic about which volumes have been written. More are clearly needed, for as yet we perceive the signs of social growth only dimly. Indeed, there are some who would claim that we commonly misperceive those signs, mistaking novelty for improvement, retrogression for advance. In education, it is especially easy, given the vexing problems that remain, to lose sight of the slow advance, easier still to mistake change of any kind for progress. Yet it is important that we remind ourselves from time to time of how far we have come.

We see first, even without a statistical gauge, that more people are attending school today than ever before in the history of mankind. Moreover, the fullness of that experience for the average person, the portion of his or her life and the amount of time and energy invested in the process, is also greater than ever before. We can also see that the quality of education as a human experience has undergone marked improvement over the years, not only between some distant historical point and now, but also within the lifetime of most adults. The curriculum of schools and colleges, for instance, has never been more varied in scope and variety. High school students are learning now what was once thought to be college-level material, and elementary students are acquiring skills that used to be taught in high school. While some might argue that it has become too ambitious and that we should not be trying to teach so much to so many, there is no doubt that the varied fare that schools offer today is an advance over the three Rs of our grandparents' day.

Consider also the instructional materials used in the service of today's enriched curriculum--textbooks, workbooks, films, tape cassettes, TV programs--all designed to enhance the attractiveness and efficiency of the learning experience. Again, it is possible to dismiss some of these new resources as mere gadgetry, but even the most nostalgic critic would have to admit that we have come a long way from the days when lessons were taught by word of mouth and recitation books.

And what of the classroom? Going forever, we would hope, are the hickory stick and the dunce's cap. Fast disappearing, too, are other forms of discipline that thousands of pupils have suffered in the past--rapped knuckles, standing in corners, sentences copied as punishment, and demerits for whispering in class. Also gone or going is excessive reliance on rote memorization, the parroting of answers to questions that were only partially understood, and the soporific boredom of the recitation method. The treatment of pupils has clearly become more humane over the years.

The gradual elimination of cruelty from classrooms is only one of several advances in pedagogical practice. There is also an increased tendency to treat each learner with greater dignity, to perceive each student as an individual, to shape an educational program in response to that perception, and to afford each person a wider range of choices and encourage active participation in the learning process.

Certainly, the Progressives, Dewey among them, had a hand in this development, but a fuller historical understanding reveals deeper roots to all of these ideas. Dewey's notions and those of his followers took hold, not because he had stumbled upon something new, but because he articulated what the human mind in a large part of the world was in the act of discovering--an evolving appreciation of human potential and its release through the application of reason under conditions of increased freedom. That discovery has taken a long time--and we are still at it. Systematic, disciplined inquiry, which is but another way of saying fundamental research, can help to push it along.

CHAPTER 3

FUNDAMENTAL RESEARCH TOPICS RELEVANT TO EDUCATION

> "Any basic science has an inner logic of its own, which for considerable periods of time, guides inquiry, defines problems, and discloses opportunities. This inner logic does not imply irrelevance to the practical world; it may however, imply patience in allowing the science to unravel its internal puzzles without demanding that relevance always be instant or direct."

That statement (National Research Council 1969) about the connection between research and the practical world was written eight years ago by another committee not unlike this one. It bears repeating, because sometimes fundamental inquiry and human affairs seem to be at odds--one preoccupied with dull facts and abstruse theories, the other caught up with today's crises.

This divergence is more apparent than real. True, it is dangerous to make open-ended promises that a line of fundamental research will solve one of those worldly crises. Too many ideas that look good in the beginning must be discarded in light of new evidence, and too many problems are more complex, or even of a different nature, than originally thought. In education, the character and priority of problems, and the goals their solutions assume, often undergo lengthy public debate. Yet it is, after all, the real world that scientists and scholars seek to understand. Recent work suggests that in the long run their curiosity has yielded important applications, even when the object of their inquiry had no resemblance to its eventual utility (Comroe and Dripps 1976).

This chapter is an attempt to illustrate the character of fundamental research that in our opinion is or will be relevant to the conduct of education. We have chosen examples of fundamental research that, in our judgment, are of good quality and speak to the needs of education. We have not sought to represent all fields, nor have we searched for the most important issues facing the educational community. Rather, we have tried to indicate where research might reveal general principles and broad understanding of basic educational processes, which hold the promise of relevance.

We begin each of the eight examples by posing an educational goal that many people consider desirable. We then list some of the public issues that describe or reflect the apparent problems in moving more effectively toward the goal. Finally, we describe some fundamental research that we believe may provide a better understanding of the problems and a means of approaching these goals.

Each of the examples except the first describes contemporary fundamental research. (When research is not available, we have made some guesses as to what it would be like.) We have reserved the first example, however, for a brief discussion of research and scholarship from the past to illustrate the historical complexity, breadth, and erratic advance of fundamental inquir

EXAMPLE 1: UNDERSTANDING COGNITIVE DEVELOPMENT

Educational Goal

An end product of education should be people who think for themselves and can learn on their own. They should be able, for example, to critically evaluate information presented to them and to seek out new information when their own is incomplete.

Public Issues

For at least a century, many people have believed that in moving toward this goal, educators should encourage and stimulate the natural human interest in learning and should build on the natural development of the human ability to learn, to think, and to create. Some people have always emerged from schooling with enthusiasm for learning and the ability to judge, to think critically, and to be creative. Nevertheless, satisfaction with these successes has been tempered by dissatisfaction with educational practices that fail to capitalize on or adapt to actual human characteristics, especially as they develop. In 1867, the author of a book for teachers (quoted in Schwebel and Raph 1973, p. 3) argued:

> For many years there has been a growing conviction in the minds of the thinking men of this country that our methods of primary instruction are very defective because they are not properly adapted either to the mental, moral or physical conditions of childhood. But little reference has hitherto been had to any natural order or development of the faculties or to the many peculiar characteristics of children.

Research Issues from the Past to the Present

If one looks back, it is clear that research and scholarship have been successful in delineating a "natural order of development of the faculties." Studies of child development, such as the remarkable body of research and theory stimulated by Jean Piaget, have considerably advanced understanding of the special ways that young children understand their experiences, the sources of error and misunderstanding that crop up in their thinking, and the stages and sequences in the growth of the mind. This work has had a pronounced effect on the conduct of education, especially in the elementary school. Its volume precludes a full description here, but we can typify

the kinds of communication that have existed--and still exist--between the worlds of fundamental research and education.

-- *Teachers are being taught broad principles of cognitive development.* In general, current textbooks of child development and educational psychology used in teacher training now present an abbreviated version of what is known about natural cognitive development. Piaget's writings are voluminous and subtle, and publications of empirical research are highly technical. However, summaries of cognitive development theory for teachers (see Furth 1970, Helmore 1970, Wadsworth 1971, Schwebel and Raph 1973) exist as well as books that draw more broadly from developmental psychology, by such theorists as Vygotsky and Bruner (see Landsdown et al. 1971).

-- *New school curricula are being designed using the findings of developmental psychology.* Over the last ten years, a variety of curricula have been based on notions of cognitive process, concept, and inquiry drawn from developmental psychology. The name of Piaget has become an accepted word in educational parlance, and the frequent invocations of his name may often be as superficial and as empty as the appeal to John Dewey's name once came to be. Nevertheless, a number of new school curricula do appear to rest on a deep and considered use of the fundamental work. These range from curricula for preschools to upper-level curricula in science and mathematics (see Lavatelli 1970, Brearly 1970, Sonquist et al. 1970, National Council of Teachers of Mathematics 1971, Karplus 1964).

-- *Tests and diagnostic systems are being developed to explore how children think and see their world.* At the heart of the new approaches to cognitive development is the assumption that children do not learn primarily by acquiring facts, skills, or behaviors (White and Siegel 1976). Children's thought passes through successive states or stages according to an orderly but complex process of maturation. This view of learning dictates a radically different approach to the assessment of children's abilities or achievements. Traditional assessment techniques are designed to sample attained skills or abilities; some researchers have tried to create newer assessment techniques intended to analyze the components and organization of a child's thinking (see Fogelman 1972, Tuddenham 1970, Pinard and Laurendeau 1964).

Roots of Contemporary Thinking. The fundamental research that has led to the modern understanding of cognitive development has historical roots of considerable complexity. Present-day scientific arguments about patterns of cognitive development rise from a research tradition existing for nearly 100 years and, if one looks at anticipations in philosophical thinking, still longer. Although modern theories address

psychological questions, their form depends on earlier work in biology, philosophy, comparative linguistics, psychiatry, and mathematics as well as psychology. These heterogeneous sources are as necessary to the full articulation of the theories today as they were in the past. The history of fundamental inquiry into cognitive development is far too large a topic for this forum; we can, however, provide an outline that suggests the depth and breadth of its intellectual ancestry.

One of the most conspicuous and well-known arguments of contemporary cognitive development theories is that children's thinking shows progressive reorganizations with age, so that thought has identifiable "stages." Piaget has argued that from birth to two years of age, infants show an early kind of understanding of the world that he calls "sensorimotor intelligence"; from two to seven years of age, they show predominantly "figurative thought"; from seven to eleven, they show an early form of symbolic reasoning called "concrete operational thought"; from eleven onward a more abstract reasoning called "formal operational thought." Stages that are roughly congruent have been proposed by such major theorists as Heinz Werner, L. S. Vygotsky, and Jerome Bruner.

These stage postulations rest in part on the findings of research with children, studies of their patterns of adaptation, their responses to questions, and their ability to solve problems. They rest also on evolutionary analyses of the brain and mind and philosophical analyses of epistemology. In fact, early theories of stages in the growth of thought arose well before there was any organized scientific movement directed toward the study of children's behavior and thinking. They appear to have relied on casual observations of children supplemented by a wide body of information on the organization of the mind and the nervous system.

One of the first extended accounts of stages in children's thinking is found in George John Romanes's *Mental Evolution in Man*, published in 1889. Romanes was an evolutionist, and in this book and others he was seeking to connect human mental life to the kinds of mental life found in the animal kingdom. At the same time that he was concerned about establishing congruences between people and animals, he attempted to establish specific differences. He argued that very young children share with animals a primitive kind of mental life that he called "receptual ideation," which is a kind of wisdom of action and is thus not unlike Piaget's "sensorimotor intelligence" or Bruner's "enactive representation." Romanes proposed that, in humans, cognitive development occurs as children become progressively able to symbolize or represent to themselves their own knowledge. Thus, in children, receptual ideation serves as a platform upon which is first erected "preconceptual ideation." Romanes's theory of the cognitive development of children did not receive much attention at the time it was offered. He placed heavy reliance upon casual and anecdotal accounts of the behavior of animals and children, casual cross-cultural material, and inferences from comparative philology. Yet his work stimulated a demand for more systematic and scientific data. In retrospect, his speculative postulations deserve some respect. He had sketched out a developmental, stagelike progression of human mental life that in form and substance was a clear anticipation of present-day theo

In the 1890s, Ivan Sechenov of Russia published a parallel theory of mental development in children that he called "The Elements of Thought." Sechenov was a physiologist and a historically important proponent of a reflexological view of brain function--the notion that the human brain and thus human thou

are determined by a nested, hierarchical system of reflexes. He sought in this brief volume to synthesize the findings of his physiological analysis with Herbert Spencer's evolutionary philosophy. He elaborated an argument of stages in children's thinking and knowledge that held that they first form "automatic sensory thinking," then "concrete object thinking," and finally "abstract thinking." There is in this volume not even the casual and anecdotal appeal to empirical data about children offered by Romanes. Nevertheless, Sechenov's speculative argument anticipates in important respects contemporary theories of stages in cognitive development.

These early theoretical fragments are noteworthy because they illustrate an important characteristic of contemporary analyses of children's cognitive development. Such analyses depend to an important extent on exploration of brain function and social and cross-cultural differences in thinking as well as on linguistic and philosophical analyses. If the significant factual details and propositions of such theories depend on systematic and careful study of children, their bases are a very broad range of inquiry.

Through the twentieth century, an important, growing science of psychology served to enlarge and articulate the brief theoretical sketches of the late nineteenth century. In addition to Romanes and Sechenov, James Mark Baldwin and Sigmund Freud put forth early speculative accounts of the stages of children's thought. Baldwin ultimately embodied his analysis in an extended philosophical analysis of human epistemology (Baldwin 1906-1915). Freud put forth his stages as part of his psychiatric theory of the origins of human mental disturbances. Both were evolutionary stage theories of mind, and both had direct influence upon Piaget (who in his early years undertook systematic training in biology, psychology, philosophy, and psychoanalysis).

The influence of theoretical writings about cognitive development burst onto education in the late 1950s and early 1960s. Their impact was so sudden and so large that some have tended to view the research developments as a breakthrough, the product of the genius of Piaget plus the brilliance of a few other exceptional people. With all due credit to the irreplaceable role of those exceptional individuals, their work may reasonably be regarded as a harvesting. As we have seen, the evolutionary, staged, developmental view of the mind was established by 1900. What happened in the decades from then until now? An enormous amount of "normal science" and theory was compiled in the contributory scientific disciplines:

-- Voluminous studies were undertaken to explore all aspects of children's development. Data were compiled on physical growth, perceptual development, learning, problem solving, language development, individual differences, influences of social class and culture, peer interaction, physical pathology, psychopathology, etc. (see Mussen 1970, Woman 1972).

-- From biology came extensive analyses of evolutionary mechanisms, including fundamental reconsiderations of evolutionary theory (see Mayr 1963, Waddington 1957).

-- The casual and anecdotal accounts of animal behavior of the late 1890s were supplanted by large literatures composed of

careful, detailed studies of animals in the laboratory and their natural habitats. The research and theory characteristic of modern learning theory, ethology, and sociobiology were elaborated (see Hilgard and Bower 1975, Wilson 1975).

-- Explorations of the brain and nervous system provided a growing picture of the information-processing and cybernetic mechanisms characteristic of the human's registration of experience. Perception, memory, attention, learning, and emotion came to be more and more clearly understood in neuroscientific terms (see Rosenzweig and Bennett 1976, Quarton et al. 1967, Ashby 1960, Arbib 1972).

-- Cross-cultural studies provided a broad picture of children's socialization as well as a heightened understanding of the differences that occur when children are reared in societies with and without schools. Sociological studies provided both data and theoretical frameworks with which to understand socialization of children through families and schools (see LeVine 1970, Whiting and Whiting 1975, Cole and Scribner 1974, Goslin and Glass 1968, Richards 1974).

Current Issues. The major difference between the developmental theories of the late 1890s and those of the present is that the latter are based on the growth of supportive knowledge bases. Today there are more data than ever before about children and the problems raised when one attempts to understand how all human beings learn. A glance at the writings of contemporary developmental psychologists will show how broadly they make use of outlying knowledge bases--not only the work indicated above, but also work in history, mathematics, philosophy, and the humanities. With the diffusion of knowledge across disciplines and the careful study of children, it has become possible to offer the complex theories of child development that now form a basis for educational use.

The theory of cognitive development now used in education is for the most part a rather early formulation of Piaget's system; his theory has been "frozen" for better public discussion just as, today, most public discussions of Freudian theory appeal to the earliest statement of his system and do not take into account the considerable revisions and reconsiderations of psychoanalytic theory that have taken place. During the more than fifty years over which Piaget has set forth his work, there have been distinct changes in the theory that bears his name. In reviewing his own work, Piaget (1970) made it clear that he counts himself as foremost among the revisionists of Piaget. Certainly, his new writings and his associates have made it clear that there is much more to be considered.

The erratic, small movements of fundamental research have already shown that there is something awry in the early picture of fixed cognitive development. It is not clear that the stage boundaries are as definite as Piaget once said they were. The child's cognitive development is not as structurally unified as the classical theory indicated it should be. Children show large leads and lags in different aspects of their cognitive performance. They show sensitivities to situational factors that may move them "up" or "down" in their stages of thinking.

One can rather confidently predict that the current patterns of movement in fundamental research bearing upon children's learning and knowing will sooner or later produce new harvestings, better pictures of the "natural order of development of the faculties." Just as Piaget (1970) regards his work as a restatement for education of what John Dewey once said, one can expect people to offer new ideas that reshape what Piaget has said. To some degree, this has happened already. A clearer, more articulated, more accurate picture is emerging of the developmental pattern of children's thought first glimpsed in the late 1800s.

EXAMPLE 2: EDUCATION OUTSIDE SCHOOLS

Educational Goals

Although public subsidy for education is overwhelmingly centered on schools, there is a public interest in and public support for education and training outside school walls. Two particular concerns are: (1) that adults have opportunities to upgrade their technical skills or acquire new ones and have opportunities for broader, more humanistic kinds of educational experiences, and (2) that people of all ages but particularly children have access through the media to accurate and stimulating information about their world.

Public Issues

In 1900, only 18 percent of all American workers were employed in white collar (i.e., non-manual) jobs; today, 47 percent of all jobs are white-collar, and it is predicted that by 1980 this proportion will exceed 50 percent. Extrapolating from several lines of similar evidence, some have argued that America is undergoing a major social and economic transition from an industrial society organized around the production of material goods to a post-industrial society organized around the development of knowledge and the provision of services (Bell 1973). Others have urged that public education be responsive to such trends by "educating the child for a world of change" and providing more extensive opportunities for training in later life.

Education through the media, especially television, is also a public issue. Many people view the freedom of the media as a mixed blessing. Some have argued that the media amplify public disturbances by giving unbalanced attention to them. Others have argued that the media teach children aggression, violence, or antisocial behavior by offering attractive models of such behavior to children.

Research Issues

Research has broadly defined norms and variations in the development of children and the effects of schooling. There have been studies of school

processes, attitudes towards education, and relationships between schooling and other institutions of society. These studies provide some basis for planning and judgment about the education of youth. However, with heightened interest in education in adult life, there has been a growing awareness that we have no similar basis for understanding human intellectual development in the adult years. While a few studies of intellectual and personality development in adulthood have been the basis for much speculation in the planning of practical programs, the question of development after adolescence is largely an open question for future research. Sustained inquiry is needed concerning lifelong educability and the character of higher-order abilities for tasks such as systematic problem solving and extended conceptual discourse that may develop after childhood.[4] An important contribution of fundamental research to these concerns is the testing of hypotheses about adult learning and motivation and the probing of their theoretical and practical implications.

How can we discover the potential of education outside schools? In this society, schools have such pervasive influence on the education of children that it is hard to "control" for their effects and examine what other aspects of American life--family, work, experience with the media or the community--contribute to an individual's education. Some useful information can be gained if we examine the skills and the thinking of children and adults in societies without schools or with educational systems conspicuously unlike our own. For example, cross-cultural research on cognitive development has developed some useful comparative data (see for example Peluffo 1962, Goodnow 1962, Piaget 1966, Bruner 1966, Scribner and Cole 1973, Cole et al. 1971, Luria 1976) and has led, most recently, to interesting efforts to single out the specific effects of schools as opposed to other settings on the cognitive functioning of individuals.

Sociological studies (Inkeles and Smith 1974) on the degree of "individual modernity," for example, indicate that schooling is a central factor in shaping individual attitudes and values. That same research, however, has shown that post-adolescent socialization experiences--at work, in contact with the mass media, through social experiences in the city--explain important parts of an individual's outlook. This suggests, first, that much of an individual's development is sustained by forces outside the school, and second, that great potential for personal growth exists after individuals have completed formal schooling. Moreover, the data indicate that for the most disadvantaged segments of society, a year in a factory may teach a person as much arithmetic, vocabulary, and geography as a year in school.

One of the most interesting areas of research on education outside schools is concerned with television. Public controversy over the influence of television on children's behavior has generated a considerable body of fundamental research. In this regard, the role of social science research both prior to and during the preparation of the Surgeon General's report on the impact of television violence provides one example of the contribution that fundamental research can make to public discussion of contemporary social problems.

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[4]The Committee is especially grateful to G. Woditsch for his comments on this subject.

During the 1960s, laboratory studies tested two competing theories concerning the effect on children of TV portrayals of violence. One theory predicted that the provision of role models was of great importance in social learning, and hence, televised examples of violence would stimulate imitative acts of aggression; the other theory maintained that the vicarious experience of violence would have a cathartic effect upon children's aggressive tendencies. Laboratory studies supported predictions derived from the former theory; they found that the incidence of violent acts increased, often dramatically, following a child's exposure to portrayals of violence (see for example Bandura et al. 1961). As with many controversial laboratory findings, questions were raised about the applicability of these results to the behavior of children in natural (i.e., non-laboratory) environments. Subsequent field studies, however, have found similar results (see for example McIntire and Teeran 1972, Dominick and Greenberg 1972); the most persuasive of these studies (Eron et al. 1972) found that there were significant correlations between exposure to television violence in childhood (age nine) and aggressiveness in late adolescence (age nineteen).

Research on the educational uses of television raises more questions. Although television viewing occupies more than 10 percent of the waking time of the majority of Americans,[5] the educational consequences and potential of this phenomenon are poorly understood; the knowledge we have is largely descriptive and often anecdotal. We know little about what goes on in viewers' minds as they process what they see and hear on television, or how television can help individuals understand their own thoughts and feelings. We do not know to what extent it can expand a person's knowledge of the world; or how special groups such as the aged, the mentally handicapped, or the emotionally disturbed use it. Indeed, as Lesser has observed (1974), when we know so little about such a pervasive institution, we cannot formulate the most productive and educationally relevant questions. Nevertheless, we do know in a rough sense that television teaches, even if what is learned is of questionable value. Preliminary research (Gerbner and Gross 1974, Dominick 1974) indicates that individuals who are frequent television viewers, particularly of crime shows, overestimate the likelihood of criminal violence against themselves, but also believe that criminals are usually apprehended by the police. Furthermore, correlational and experimental studies (Frueh and McGhee 1975, McGhee 1975, Gross and Fox forthcoming) suggest that heavy doses of American television increase children's acceptance of traditional sex role stereotypes.

There is some evidence, then, for the significant educational potential of experiences outside school. However, to take advantage of this potential, we need a basic understanding of the process of learning and cognitive development throughout the life cycle. Among other things, we need to know the ways, if any, that learning throughout life differs from learning early in life; how institutions other than schools manage to teach; the qualities that make one organizational environment more effective in learning than another; and the subjects that other settings teach best.

[5]The General Social Survey (National Opinion Research Center) of 1975 found that a representative sample of American adults spent an average of over two hours a day watching television. More recent surveys put the figure at three hours.

EXAMPLE 3: EDUCATION AND READING

Educational Goal

Individuals should be able to read well enough to inform themselves about public affairs and government decisions, to arrive at reasoned decisions and plans for their lives as parents, consumers, and workers, and to understand written materials such as employment applications, union rules, and insurance policies.

Public Issues

An alarmingly large number of individuals--including some high school graduates--do not have the abilities we think of as comprising literacy, and the results of national testing programs indicate a failure of schools to educate in this regard. Some specific popular questions (see Gibson and Levin 1975) include (1) What is dyslexia, and why do dyslexic children fail to read? (2) Do we need to learn "rapid reading," and, if so, how? (3) How can parents help children with reading? (4) What should we do about educating adults who cannot read?

Research Issues

A major question for research on this topic is an old one, and not yet answered. How do people learn to understand the printed word? Research developments in linguistics, artificial intelligence, and cognitive psychology have recently pointed to reading comprehension as a perceptual and cognitive process (not simply a matter of remembering word associations).

The new cognitive processing approach, in theory and method, is very unlike that taken in the studies of nonsense syllable learning that were common twenty years ago. Researchers employ new mathematical techniques, computer simulations, and research designs. More important, they have demonstrated that in understanding their world, people organize information in useful, hierarchical, rule-guided ways. Even the comprehension of simple stories is guided by a kind of grammar that provides a framework of rules for organizing information so that it may be more easily comprehended and remembered (see for example Bower 1976).

The reason why this organizing process is so important can be illustrated by reference to some work on the role of familiarity in perception (see Krueger 1975). We all know that the experienced eye somehow sees more than the inexperienced. The hunter sees game where others see only trees and grass. The good reader, without seeing any more letters than the poor reader, may excel at inferring the identity of the whole word or phrase. In the past, researchers explained this phenomenon as a kind of perceptual readiness, called "set," whereby the experienced person adjusted perceptual mechanisms to tune in expected material. Later, however, it was discovered that neither set nor some sort of response bias could explain the sizable familiarity effects that were found (Broadbent 1967,

Garner 1974). To read, a person must learn so many letter combinations, spelling patterns, and full words that some connection with long-term memory and an organizing structure is required after presentation of the material to be perceived; readiness is not sufficient. Research today, then, is centered on the various organizing structures that might explain how familiarity helps one read. Among the possible hypotheses being studied are that familiarity aids one in extracting visual or verbal features; that it helps in interpreting what is seen; that it is a part of the memory storing process; or that it is part of the "output" process whereby the person reports (to himself or another) what is seen. In the future, this research may help us to understand the kinds of training that are necessary to help poor readers.

Another line of research that holds promise has to do with how people store and represent in memory the information they receive. On the basis of a wide number of studies, many using computer simulations, some researchers now believe that memory processes are the most crucial components of reading comprehension. An example of this research is a set of studies by John Anderson at Yale University. He has had some success in testing a model that predicts that the more information a person has "stored" about a concept represented by a word in a sentence, the more slowly the person will be to verify the truth of the whole sentence. Eventually this research could lead to helping those students whose memory process may be interfering with their ability to read.[6]

One of the most dramatic developments influenced by the new work on memory and cognitive process has been a rethinking of what we mean by intelligence. For years, of course, it has been known that people who obtain high scores on intelligence tests tend to read and remember well. (The tests were designed to predict academic performance, which requires these skills.) Classically, intelligence has been viewed as a static structural attribute or set of attributes of a person. Recently, however, many quantitative and cognitive psychologists have begun to study intelligence as the manifestation of differences in those cognitive processes that are components of what a person must do to perform intelligently on an IQ test. One researcher (Hunt 1974), for example, has developed a computer model of a cognitive style that solves all of a set of Raven's Progressive Matrices problems, which are used in ability testing.

Many important questions remain to be answered. Theorists disagree, for example, about the way syntactic and semantic knowledge interacts in a reader's cognitive representation of a message. They have no firm understanding of the nature of cognitive deficits that reduce people's ability to understand written text. That much progress has been made in the last ten years, however, is quite clear from the success researchers have had in roughly simulating human thinking. A few years ago, for example, researchers at Carnegie-Mellon University demonstrated that a very simple computer program, using perceptual processes already employed in computer chess programs, moves its attention about the board in a way that resembles the eye movements of a human chess expert. Two simple programs, one concerned with extracting information, and the other with

[6]For discussion of work on which this is based see Anderson and Bower 1973.

retaining positions in memory, are adequate to account for the known ability of chess masters to reproduce a chess position from memory although they have seen the board for only five or ten seconds (Simon and Barenfeld 1969).

The question of how people learn to understand text is being pursued by investigators from several disciplines, including but not limited to cognitive psychology, computer science, linguistics, and psycholinguistics. Probably, there will be increasing emphasis on fundamental research conducted in school and other educational settings and tests of hypotheses using data collected in these settings. Although there are obvious difficulties, this kind of research sometimes points the way to more valid theory. Carroll's work (1974) on the relation of ability tests to time-in-learning of school subjects, for example, has provided stimulation for the development of new theories about tests of ability at one age and achievement at a later age.

Fundamental research relevant to literacy includes many topics: perception, cognitive processing, mental structure, and the measuring of children's and adults' skills in comprehension and its component parts. Research on skills that are closely related, such as speaking, listening, paying attention, and noticing, is also being pursued. Some of this research has great potential; in fact, it is already widening our conceptions of what skills are basic. There is also linguistic research on language learning and the learning of second languages in school settings. Finally, there is research on artificial intelligence that involves the development of computer models of knowledge structures, the efficient retrieval of information, and adaptable systems capable of learning, understanding, and producing natural language. Work on each of these topics is aimed at increasing the understanding of how the human mind works and how mental skills, such as reading, might be more easily acquired and refined.

EXAMPLE 4: THE BRAIN AND NEURAL PROCESSES

Educational Goals

Fundamental goals of education are learning and increasing the capacity to learn.

Public Issues

The brain and the nervous system are the physical substrate of learning and of the allied processes of stimulus detection and observation, information processing and retrieval, coding and language, and motor performance. Although this fact rarely enters public debate, there is considerable public interest in the capacities of the brain, the factors that may reduce its functioning, and the possibilities for treating neurological handicaps.

Research Issues

One aspect of research that shows much promise involves the recording of evoked potentials from the surface of the human scalp. This work is only in its infancy and there are many disagreements and controversies surrounding particular findings at the moment. Nevertheless, there are unquestionably clear and significant relationships between such psychological variables as attention, set, readiness to respond, and cognitive variables and different components of the evoked potential, such as amplitude, latency, and relative amplitude between the left and right hemispheres.

To take two specific examples of ongoing research, a study by John et al. (1967) visually presented square and diamond patterns of two different sizes to subjects and recorded the evoked potentials from the scalp, which signal neuronal activity. They found that the evoked potential wave form seemed to code stimulus quality; that is, the responses were much more similar for the same object of different sizes, than the other way around. Furthermore, sometimes the subjects reported seeing a square when in fact they were shown a diamond, and vice versa. Under those conditions, the evoked potential corresponded to what the subject reported seeing, rather than the physical stimulus--a striking example of the correspondence between a category of neuronal activity and a psychological process that is not stimulus-bound. The second example is provided by a study of Teyler et al. (1973), which demonstrated that evoked potential wave forms do appear to code the cognitive-linguistic aspects of language. That is to say, different meanings of an ambiguous word exhibit different wave forms: rock (a stone) exhibits one kind of wave form, whereas rock (to rock a boat) exhibits another kind of wave form. When the subject reports thinking or is instructed to think of one meaning or the other, the two wave forms are clearly distinguishable.

A second type of experiment that has profound implications for brain and behavior in humans, particularly in relation to early environment and subsequent education, is the work of Rosenzweig (1970) and his group at Berkeley. In brief, they raised some rats in "rich" environments in which they lived in groups in large play areas with many toys and interesting stimulus objects; others were raised individually in laboratory cages. In the animals raised in the rich environments, they found substantial and significant increases in the number of synapses formed on neurons in the cerebral cortex as well as an increased thickness of cortex, greater number of glial cells, and greater amount of AChE--all of which indicates neurological benefits from the stimulating, open environment. Other laboratories are pursuing the explanation of this phenomenon.

Perhaps the key problems in psychobiology are the brain substrates of learning and memory. At present we have a rather good idea of the neuronal mechanisms underlying habituation, which roughly means adapting to, or getting used to, a stimulus (Kandel 1976, Thompson et al. 1973). Habituation is perhaps the simplest form of learning. It has been and is being studied in a variety of laboratories whose subjects range from spinal models and intact-organism behavior to simple invertebrate animals. In many of these laboratories, there is a growing consensus that habituation is due to a process called synaptic depression. After repeated activation, certain synapses show a pronounced and prolonged decrement in

transmission, which requires substantial periods of time to recover. Of course, habituation is a simple, even trivial, aspect of behavior in humans. Far more important in the present context is associative learning. We have as yet no really accurate ideas about the role of neuronal mechanisms in such learning; however, as indicated above, certain compelling parallels seem to be developing between brain studies of learning and memory in animals and human information-processing approaches (Atkinson and Shiffrin 1968).

Another important topic is the relation of chemistry and behavior--psychopharmacology. We have only scratched its surface, since we have yet to do the fundamental studies relating to the effects of various chemicals, ranging from LSD and other psychogenics to anesthetics, on chemical synapses in the brain and their relations to the generation of behavior. It is reasonable to expect that fundamental breakthroughs will come only when we understand how these chemical reactions at synapses alter behavior and experience. A specific example in the context of education is the widespread use of drugs for "hyperactive" children. To date there have been few adequate evaluative studies of the effects of these drugs on learning and related processes, let alone behaviors more directly related to activity level. The recent review by Whalen and Henker (1976)[7] analyzed the effects of psychostimulant medication for hyperactive children. In general, they found conflicting evidence regarding possible beneficial effects of the drugs on attention, cognition and learning in these children. Furthermore, up to 40 percent of children so treated showed no behavioral improvement! Whalen and Henker strongly emphasized the dearth of knowledge and theory regarding the use and effects of such drugs on children.

The examples given above are only a small selection of the many ways in which an increased understanding of the neurobiological substrates of human information processing are relevant to the ability to improve and direct the process of learning and assist those who are handicapped in these abilities.

EXAMPLE 5: INNOVATION AND CHANGE IN EDUCATIONAL INSTITUTIONS

Educational Goal

Schools have changed their organization and methods of instruction over the last decade (e.g., open classrooms, modern math, pass-fail grading), but the results of these innovations are largely unknown and we have little basis for evaluating further proposals for change. It would be extremely useful if we could more accurately predict whether a given innovation would increase educational effectiveness in a particular setting.

[7]See also the discussion of psychopharmacology in the previous example.

Public Issues

This nation has traditionally valued innovation. Each decade has brought new social issues to public awareness--the science-technology gap, the plight of the poor, the role of women--and attached to each have been calls for new educational approaches. Today, however, debates about the value of innovation and change per se are frequent.

Many citizens consider past educational innovations as expensive, ineffective fads, adopted more in response to the hopes of educators than because a compelling body of knowledge or experience argued for them. Others object more to the way decisions are made, especially in large, consolidated school districts. There is concern about the adequacy of the information that parents and taxpayers are given about proposed change. Educators are worried about innovation and change. Some believe that they are pressured to move from one innovation to the next with such rapidity that nothing succeeds. School administrators argue that change costs money that they do not have, while others view innovation as a way to command increased resources. Still others complain that although innovations developed in one setting do not transfer well to others, the courts or state boards of education often require uniformity. On a more general level, nearly everyone would like program decisions to be more "rational."

Research Issues

Fundamental research on change and innovation in organizations, as compared with applied or evaluation research, is designed to discover how people in organizations receive, communicate, accept, and reject information or ideas; how they make decisions for the organization; and (less frequently) how they are affected by organizations. The field may be characterized by two general approaches: the application of principles of human behavior derived from laboratory studies (for example, information processing [see Slovic et al. 1977] or small group decision making [see Staw and Salancik 1977]) and the development of general theories of organizational communication, motivation, decision making, and productivity. For brevity, we confine our remarks here primarily to the latter category.

A new development in fundamental research on organizations is the study of communication and its implications for decisions made in organizations. Imagine, for example, the following decision: to reallocate expenditures in a large school district so as to equalize access to special programs among pupils. For several reasons, among them the lack of a single decision maker and the geographical diffuseness of affected citizens, a traditional decision model is inadequate (Connelly 1977).[8] One must consider how all the participants in the decision process communicate with each other, where they are located, and the impact of official and unofficial components of their communication (informal meetings among teachers, for example).

Some of the research on this problem draws on both systems theory and information theory (see for example Richards 1974); other work is designed

[8]This review includes an interesting discussion of problems in organizational communication research.

to refine and test the adequacy of such concepts as power and uncertainty (see for example Pfeffer and Salancik 1974). This work has stimulated a rethinking about communication in organizations. Traditionally, researchers have assumed that organizations (as represented by an individual, coalition, etc.) pursue goals, and that the function of communication is to obtain information for reaching these goals. Communication, however, may be serving different interests, such as providing incentives to individuals to continue participating, justifying the outcomes of decisions, or even discovering what the goals are (Georgiou 1973, Weick 1969).

Research on communication in organizations shows promise for better understanding of why organizations innovate and how to devise effective innovations. For example, some theorists suggest that communication functioning to reduce uncertainty (about how others will react to a program, for example) will cause some individuals to seek out more information than they need to make an accurate decision. This hypothesis has been derived from Bayesian studies of conservation, empirical studies of organizations, and studies in very different contexts (e.g., mental health diagnosis) (see for example Edwards 1968, Ackoff 1967, Oskamp 1965). One implication is that the design and implementation of innovations might be improved if there were some way of reducing overloads of irrelevant information, rather than by increasing the quantity of information, as is sometimes prescribed.

Other fundamental research on organizations aims at understanding decision making; this work is probably the largest activity relevant to innovation and change now under way (see Allison et al. 1975). Some of this research concentrates on understanding the class of institutions we call public or nonprofit (including educational institutions). Decision making in public institutions is quite different in some important ways from that in private enterprise. In educational organizations, for example, objectives and goals are often ambiguous and difficult to measure. The objective "good citizenship" in a school is far more open to alternative interpretations and is much harder to measure than is "good productivity" in an automobile factory. The result of this ambiguity and uncertainty may be decision making designed to define a problem (rather than solve it), to create stable operating procedures (rather than effective ones), or to distribute rewards according to a subunit's contribution to the organization's resources (rather than its contributions to outcomes). Recent studies, for example, indicate that budgetary decisions in educational institutions may be more attuned to satisfying the demands of departments or units whose power differs (because, for example, they have attracted more or fewer students) than to maximizing the benefits of various budgetary allocations (see for example Pfeffer and Salancik 1974, Shumway et al. 1975).

Some research on organizations is designed to create models of optimal or usual decision making. This research suggests that prescriptions for educational institutions in the future will differ from those for organizations whose objectives and technologies are relatively clear. One example of this research is the theoretical work now being done on alternatives to the traditional analytic model of organizational decision making. The analytic paradigm assumes that individuals (and organizations) maintain well-defined preferences for different ends and evaluate options by using this preference ordering. Theorists including Herbert Simon, Charles

Lindblom, and others have argued that this model fails to recognize the special characteristics of organizations like educational institutions. One alternative approach being developed is known as incremental decision making (Lindblom 1975). This model assumes that decisions in public organizations proceed in small, marginal steps, designed to cope with uncertainty and conflict of values or ends and to enable changes in plans when the effects of decisions are unacceptable. One theorist (Steinbruner 1974) has likened the process of decision making to a simple cybernetic paradigm and shown that information-processing principles can account for non-incremental changes in policy.

An important aspect of decision making according to these untraditional views is that decision makers, for both political and cognitive reasons, tend to adjust their thinking about problems to normative beliefs and values as well as to decisions they have made that are related to those beliefs and values. Decisions, then, may be more closely related to salient beliefs than to empirical information. Some research on this topic has been heavily influenced by laboratory studies of consistency, post-decision changes in attitude, and commitment, many of which were carried out by the intellectual descendants of Kurt Lewin. Case studies of organizations have stimulated ideas about how beliefs come to be shared and the circumstances under which they become salient in organizations. Cohen and March's study (1974) of university presidents, for example, has suggested that ambiguity in organizations causes minor problems to spill over into conflicts about values. A discussion of some research laboratory space, for example, can easily become an argument over the relative importance of teaching versus research in a university. A process of that sort could have important implications for understanding the problems an organization faces when it has to take new, important actions.

The research we have described cannot be claimed by any single discipline. Researchers from psychology, political science, economics, sociology, anthropology, and mathematics are involved. Many associate themselves with the overlapping multidisciplinary fields of management science, industrial psychology, organizational behavior, and communication. While much of the work in these latter fields is properly viewed as applied research, another portion of it is fundamental.

To understand innovation and change in educational institutions, there exists a clear need for fundamental inquiry. Public discussion and research on educational policies often presume that innovation is a single attribute of organizations that can be evaluated independently of specific programs and contexts. Conclusions about innovation have erred in their generality ("innovation does not work") and in their ambiguity ("all innovations in school practices are not automatically good").[9]

[9]The Washington Post (Dec. 23, 1976) greeted the results of a three-year, nationwide evaluation of educational innovation (Project Longstep) with a first-page story headlined, "Innovative Education Held to Make Little Difference"; such articles led the report's authors to issue a revised press release maintaining that their findings showed only that "innovation per se does not guarantee dramatic improvement in student achievement." (Quotations are from the revised press release: Does Educational Innovation Pass or Fail? American Institutes for Research, January 7, 1977).

Fundamental research may help us to understand much more precisely why specific programs are ineffective or effective and how they come to be adopted by some institutions but not others.

EXAMPLE 6: OPPORTUNITIES FOR HIGHER EDUCATION

Educational Goal

Post-secondary education has a central role in determining an individual's socioeconomic attainment. For this reason and because higher education provides an unusual opportunity for intellectual development, it has been generally accepted by most Americans that the chance to pursue a higher education should be offered to all persons who would benefit from it.

Public Issues

Ultimately, policy making for higher education prompts a reexamination of the goals of higher education. As far as economic considerations are concerned, these goals have frequently been summarized under the slogan "access, efficiency, and equity." In particular, this implies (1) that higher education should ideally be available to everyone for whom the "benefit" exceeds the "cost"; (2) that the costs of these benefits should be minimized; and (3) that the system should ensure equity in the distribution of benefits and costs.

Research Issues[10]

The U.S. higher education system emphasizes "consumer sovereignty," at least in the choices of institutions by students. Public and private institutions compete for students, through both "price" (tuition and financial aid) and "product" (program selectivity, location, size, etc.). Surprisingly little is known about the factors that influence individual student choices or the demand for higher education.

It is known, of course, that numbers are important. Individuals, institutions, and the federal government during the last decade invested at ever-increasing rates in postgraduate education (Cartter 1976). Yet current demographic trends are causing a levelling off of enrollments in higher education (or at least a decline in growth rates). This, in turn, has caused a dramatic decline in the demand for new faculty, which has

[10]The Brookings Institution recently organized a conference on Public Policy and Private Higher Education. Although the focus of the conference was on public policy issues in a circumscribed sector of U.S. education, the conference papers and discussion touched on most topics in current fundamental economic research relevant to education at all levels. This section is based upon the issues and research discussed at the conference (see Breneman and Finch, in press).

resulted in unemployment and underemployment for persons with advanced degrees in most fields. This result has potentially threatening consequences for the intellectual vigor and age structure of universities and research institutes. What is particularly curious about the latter state is that, to paraphrase one demographer's analysis, a few paper-and-pencil calculations could have been made over a decade ago to predict the virtual inevitability of current distress.

In the current quest for intelligent policies concerning the future of graduate education, academic tenure and retirement, and the support of scientific research, both legislators and administrators will require more adequate understanding of the functioning of the academic labor market and, more generally, the demand for and supply of educated persons. The collection of longitudinal data on the career plans of individuals and the plans of institutions will be important in understanding these processes, as is more general research on the relationships among education, occupational attainment, and income. The association of income and education has, during the last decade, begun to attract the attention of social scientists doing empirical and theoretical work not only in economics but also in sociology and social psychology (see for example Taubman and Wales 1974, Mincer 1974, Sewell and Hauser 1975, Jencks et al. 1974).

Important benefits have flowed from cross-disciplinary interest in this topic. For example, well-known psychological variables such as IQ have begun to be regularly employed in economic analyses (see for example Taubman and Wales 1974, Griliches and Mason 1972, Bowles and Nelson 1974), while standard econometric methods (e.g., structural equation models) have been widely adopted as important tools in sociological research.[11] Substantative work in these and other areas frequently requires longitudinal information on individuals whose careers are followed for many years.[12]

Fundamentally important questions are also raised by the recent suggestions that federal and state governments aid private universities more directly in their struggle to survive. First, there are questions familiar to both experts in public finance and to public citizens: Who really pays for such support from tax dollars and who benefits? (Carnegie Commission on Higher Education 1973). Because of the enormous complexity of the U.S. tax structure and the fiscal relationships among federal and state agencies, any attempt to answer these questions brings us to the frontier of basic research in economics.

A second set of questions arises when we consider the need to maintain such institutions. Typically, their existence has been justified by the

[11]See discussions in *Sociological Methodology: 1969* and following years and Goldberger and Duncan 1973, Blalock, 1971.

[12]Longitudinal data sources include the Wisconsin longitudinal survey of socioeconomic achievement; the National Survey of Health and Development in England; the National Longitudinal Survey of Youth, and the Comprehensive Roster of Scientists and Other Persons with Higher Degrees. The collection of such data is expensive, and longitudinal studies are too often abandoned prematurely because of the vagaries of funding. Although more costly than much traditional social science research, these costs are small by comparison to those incurred in the hard sciences. Sustained support for the careful collection of longitudinal data would greatly facilitate basic research on these topics.

claim that private institutions can provide excellence and diversity more easily than their public counterparts. To put this claim in perspective, however, we need a deeper understanding of student demand for alternative modes of higher education as well as the consequences of such experiences (Radner and Miller 1975).

Finally, the prospect of increased government support of private higher education has been accompanied by a more intense demand for accountability to government agencies. One can predict that government agencies will not provide funds from the public treasury without strings attached. One can also predict that uncritical application of traditional accounting and operations research paradigms of business to education will produce many silly (and some potentially disastrous) outcomes. But the fact is that we presently know little about the principles of rational administration of institutions that deal in processes and outputs that have thus far defied comprehensive quantification. Obviously, any adequate accounting of costs and benefits should include social as well as private benefits and costs and qualitative aspects. Here again, the questions bring us to the frontiers of social science research, although, in this case, it is a frontier that has been relatively neglected by an economic science that is predominantly market-oriented.

EXAMPLE 7: EDUCATING CHILDREN FOR A CULTURALLY PLURALIST AND MULTILINGUAL SOCIETY

Educational Goals

The children who enter American schools have mixed cultural and ethnic origins; some do not speak fluent English. While there is no consensus about the responsibility of schools toward children from different cultural and language backgrounds, the following general principles are widely accepted:

1. Schools should educate all children in the dominant language and culture sufficiently to permit participation in national life.
2. Schools should introduce children to other cultures and languages and educate children in analytic rather than moralistic approaches to social and cultural differences (e.g., understanding how the same behavior or symbol might come to have different meanings in two cultures).
3. Schools should be responsive to the families and communities from which children originate. They should teach children to understand and respect their own cultural heritage.

Public Issues

The question of what educational accommodations should be made for diverse ethnic groups is very much alive in current political debates. An earlier national policy of "Americanization" through uniform schooling has been supplanted by a more pluralistic policy. Recent legislation favors the

provision of extra resources and special arrangements for minority children, enhanced parental and community control over schooling, and programs to make schools more hospitable and effective for children of diverse backgrounds. Generally, controversy no longer centers on the desirability of the trend toward "cultural democracy," but rather on the kinds of accomodations needed and the effectiveness of specific programs. Among the most frequently mentioned questions are: How can we enhance the general educational attainment of children who enter school with a language and a cultural background different from the majority? How does one modify the curriculum and the management of schools to give children a sense of their own special background and cultural identity? What is the importance of the curriculum tradeoff between the time spent on "basic skills" and the time spent on "cultural heritage?"

Research Issues

Multilingualism is one aspect of this topic that raises important social science questions. One of these questions--the cognitive advantages or disadvantages of children's learning two or more languages--is being explored on a fundamental level through the study of psycholinguistics. While knowledge about first language acquisition has grown (see for example Brown 1973, Moore 1973), we need to forge theoretical links between this knowledge and the understanding of the learning of a second language. We still do not know to what extent learning a second language involves "beginning again." The problem derives, in part, from inadequate descriptive knowledge of the process of successive language acquisition. There are also theoretically important questions about the existence and character of transfer processes in language learning (see Carrol and Freedle 1972). In what manner, if any, does mastering the grammar and lexicon of one language aid--or impede--the learning of subsequent languages? How are the grammatical rules governing different languages psychologically differentiated by multilingual persons? Does the mastery of a second set of symbols for representing experience affect the fluency of an individual's thought? Does such mastery alter the rate or character of children's intellectual development? Some theoretical work and empirical evidence pertinent to these questions exists. For example, longitudinal studies of English children who attend bilingual (French-English) primary schools in Quebec (Lambert et al. 1973, Barik and Swain 1976), have begun to dispel the widely held notion that early bilingualism retards cognitive development.

There is still considerable controversy about the relationship between language learning and cognitive development. In recent years, however, theorists have begun to consider new alternatives. Thus, those who have long maintained that cognitive structures (or understanding) precede the acquisition of linguistic structures (language to express what is understood) are now examining this simple notion of "one-way action."[13] The

[13]See review and discussion by Beilin (1976).

currently increasing emphasis on cross-cultural studies of development[14] may in the future force substantial revision of ideas about the role of linguistic factors in general and bilingualism in particular in the mental development of children.

Cultural pluralism raises other issues. In analytic approaches to cultural differences and social problems, the education of children presumes the existence of sound knowledge and theory about the way in which children think about such matters; but in fact we know little about how children at different ages conceptualize larger social problems and solve the social dilemmas with which they must deal. Over the past fifty years, the most influential theories of children's cognitive development have grown from studies of the nature of children's thought about physical reality, dreams, causality, and moral behavior (see, for example Piaget 1928, 1946, Kohlberg and Turiel 1971, Selman and Lieberman 1975). Curiously, there exists little parallel work describing the nature and development of thinking about important social processes, such as peer pressure, conflict, or economic exchange. The reason, possibly, is due to the assumption that because children are not skilled at verbally expressing adult-like concepts of social situations, they do not have any such concepts. Some researchers think, however, that children are able to perceive many things they cannot express well in words (Ginsburg and Koslowski 1977). Although we cannot foretell the theoretical questions that will emerge from this fundamental research, we believe that research can at least serve to chart the correspondences between the development of logic in children and the evolution of their understanding of social processes.

If the understanding obtained from this research is to be applied in educating children about social and cultural matters, many additional questions remain to be studied. For example, Do textbook descriptions of cultural differences adequately allow for children's developing ability to view behavior from other people's perspectives? To what extent do children in this society experience different cultural settings? What effect does this have on them? How do they adapt their interpersonal style to changing contexts? How do young children understand history? For example, when they see a depiction of slavery on television, can they put themselves back in time to see how it might have happened? While short-term results cannot be expected, fundamental research on these and related issues can serve to lay a foundation on which to construct better informed educational policies.

EXAMPLE 8: SCHOOL ENVIRONMENTS

Educational Goals

Schools should offer safe, productive environments for teaching children, and children in the classroom should behave in a manner that is conducive

[14]This interest is evidenced by the founding in 1966 by the International Union of Psychological Sciences (under subvention from UNESCO) of the *International Journal of Psychology* whose major task is providing a forum for cross-cultural studies of psychological processes, and the more recent creation of a *Journal of Cross-Cultural Psychology*.

to learning, that is respectful of others, and that promotes development toward responsible adulthood.

Public Issues

Decisions about the organization of school environments so as to facilitate learning are usually understood to be the responsibility of teachers and school administrators. Some aspects of school environments, however, have become a matter of broader concern. Prominent problems for some schools, especially big-city schools, have been disorder, disruptiveness, and lack of safety. A less urgent but more sustained kind of debate exists about classroom environments that are perceived as unsupportive or harmful to children. Parents do not agree--as, indeed, school professionals do not agree--on definitions of an optimal classroom environment. Some disagreements derive from the fact that different children need different environments, and some reflect different images of what a proper school should be like. In the end, there are those who argue for environments that are structured and orderly; those who argue for environments that are free and open; and those who argue for classrooms that will be hospitable to children from special backgrounds or who have special needs.

Research Issues

Perhaps the most urgent needs of schools today are to reduce crime, vandalism, and disorder. It would be unwise to assume that these are technical problems to be solved through research or, for that matter, to assume that the sources of those problems are within the school walls. Schools must take in children of different races, classes, and ethnic affiliations, children who are prone to crime, children afflicted with emotional disorders. They take in community conflict and community problems. From time to time, public initiatives such as desegregation or mainstreaming may create disturbances in schools--disturbances it is hoped that will be short-term and that will lead to counterbalancing long-term benefits.

Research will not adjudicate the political conflicts and choices impinging on schools, any more than it will do so for the larger society, but it can be of distinct value in defining the problems attributable to school environments. For example, some famous studies have played a prominent role in defining the terms of public debate about schools because they indicated where schools and school environments might make a difference in exacerbating or alleviating social problems. These include works on such questions as the economic and social impact of schooling--the Coleman Report (Coleman 1966) and the Jencks et al. (1972) volume on inequality. In addition, historical studies (Cremin 1961, Tyack 1974) are offering new conceptions of the functions that schools have served and may now be serving for American society. Sociological studies (Flanagan et al, 1962, Coleman et al. 1966, Lash and Sigal 1976) can try to provide a clearer picture of what children and their lives are like. Studies by sociologists, psychologists, political scientists, and organizational theorists (Becker 1963, Tannenbaum 1937, Lemert 1972, Keniston 1967,

Coleman 1961) are designed to find the reasons why people deviate from the rules or standards of society or are perceived as deviating from them. In short, a major benefit of research is that it points toward the sources and nature of social problems, including those school problems that people subsume under the category of discipline.

The debates over what school environments do and do not do--what impact they have--has stimulated interest in the particular effects of different classroom environments. There is research, not only on the kinds of settings that alleviate "discipline" problems, but on classroom environments that create or reinforce prosocial behavior and positive motivation. For example, an important question in education generally is how one teaches cooperativeness, responsibility, and persistence. Some recent research on the development of expectations and causal attributions of children indicates that these cognitive processes are important components of motivation and behavior (see for example Bandura and Walters 1959, Seligman 1975, Jones et al. 1971). Some of this work has led already to the testing of new teaching techniques for handling problems such as disorderliness and fear of mathematics (see Dweck 1975). Yet fundamental research relevant to problems of classroom environments has broader, more long-term benefits, too; we list a few:

1. It can contribute methods for the more careful description and analysis of classrooms and of their effects on children. At present, methods drawn from anthropology, ethnology, and psychological ecology are being brought into increasing use in the careful observation of classroom processes and interchanges.
2. It can provide ideas about important individual differences in children and suggestions about their educational implications. One of the most critical problems in the management of any classroom is the range and variety of human individuality. Children differ in the way they approach problems and in their reactions to frustration, distraction, and stress. Current research on cognitive styles has revealed some of the idiosyncratic ways in which children look at problems and think about them (see Kagan and Kogan 1970). There are systematic social class differences in the way children approach school, and these, too, need careful description.
3. It can identify major growth patterns in a child's development of moral judgment, ego strength, and social judgment (see Kohlberg 1964, Jones 1968, Loevinger 1966, Livesley and Bromley 1973, Flapan 1968). Children understand themselves and others differently as they mature. They manage themselves and their learning in systematically different ways.
4. It can assist in the handling of special problems. Some idiosyncratic problems of children have an unusually large effect in creating disturbances of the classroom. Hyperactive children in the early grades and aggressive or emotionally disturbed children in the later grades play an important role in disrupting classrooms. To the extent

that research can assist in locating sources of these problems and finding techniques of management, it can contribute to the maintenance of a more optimal learning environment for the majority of children (see Henker et al. 1976).

CHAPTER 4

FEDERAL POLICY

This nation has always been committed to the goal of providing and improving education for its citizens. But in the era of our grandparents, education was much simpler and its character more identifiable with local or regional problems. Traveling the country seventy-five years ago, our grandparents might have seen a half-empty high school in the Midwest, whose absentee students had been pulled out to help with the harvest. In the South, they might have happened on a school for black children, whose "library" was a shelf of secondhand books. In the cities of the East, they could have visited schools in which teachers struggled to teach English, hygiene, and "American" beliefs to thousands of poor immigrant children and their parents. On the same trip today, we would find some of the same basic problems--students lagging far behind their peers, lack of facilities, and children who do not understand the language of their teachers and vice versa. These problems are no longer geographically or conceptually isolated--they belong on the list of national issues of a complexity and difficulty unforeseen at the opening of the century.

Because educational problems are national in scope and enormously complex in nature, they require sustained resources and a national pool of talent for understanding their bases and building a strong foundation of knowledge. We on the Committee think it necessary that the financial resources for fundamental research related to education be primarily the responsibility of the federal government. The states and private sources can and do help, but for many reasons they are not able to provide sufficient support for fundamental research. One of these reasons derives precisely from the diffuse effects of fundamental research that we have discussed. In comparison with the federal government, smaller systems, whether public or private, are less able to see the benefits of fundamental inquiry within their own spheres of responsibility. If, for example, research on adults in Florida enlightens the classrooms of Alaska, one cannot expect state legislatures to support research at nationally optimal levels. State involvement in research and its use is important and is to be encouraged, but a larger system, which is national (or even international) in scope, must help represent the interests of society as a whole.

Throughout the history of the United States, the federal government has supported activities of national benefit that smaller bodies could not undertake on a sufficient scale. Fundamental research relevant to education falls in this category and has received recognition as such from the Congress. The Committee believes, therefore, that the conduct of federal policies for the support of fundamental research deserves serious attention.

THE COMMITMENT TO FUNDAMENTAL RESEARCH

Federal policy for research in education is not new. In 1867, Congress established a national Department of Education and directed it to collect " . . . such statistics and facts as shall show the condition and progress of education . . . as shall aid the people of the United States in the establishment and maintenance of efficient school systems." The belief that the federal government should gather information and "facts" for schools has not been seriously challenged since 1867, and subsequent legislation has greatly enlarged the federal commitment to research in education, including fundamental scientific research. Today, it is the nation's policy to build the sciertific foundations of education. This policy supporting fundamental research relevant to education evolved slowly over 200 years, culminating in the legislation of 1972, which established the National Institute of Education.

One hundred and seventy years ago, the Coast Survey was created; the Department of Agriculture (1862), the Department of Education (1867), the Geological Survey (1879), the National Bureau or Standards (1901), the Hygienic Laboratory (1901), and the Bureau of Mines (1910) followed. These agencies and bureaus did what private industry and the states could not do: they provided centralized resources and information for exploring the continent, developing trade and shipping, improving agricultural productivity and school efficiency, improving the health of immigrants, and standardizing weights and measures. These activities marked the beginning of federal responsibility for building knowledge in the service of national needs. Four major changes in policy have expanded that responsibility since, and have provided the basis of the present policy for fundamental research relevant to education.

Extramural Research and Development

One significant change affecting research in education has been the development of flexible policies for supporting research and development outside government through contracts, grants, and cooperative agreements (Danhof 1968). Until the outbreak of World War II, most grants gave special aid to the states, such as funds for experimental stations in the state colleges of agriculture, and most government contracts procured supplies and equipment for the military. There were strict requirements for open advertisement and competitive bidding during peacetime; in time of war, they were suspended so that industry could respond quickly. But even during war, contracts specified a product, and, on the whole, basic scientific research, applied research, and experimental development of technology were left to industry, foundations, and universities.

Prewar American policy stated that research and development was a private matter, and contract regulations reinforced that policy. As late as the 1920s, there was considerable opposition to an experiment with negotiated contracts whose purpose was to give the troubled aeronautics industry a stronger scientific and technological base. By 1930, the government supported less than 15 percent of the nation's total scientific research (Bush 1945).

The successes of World War II changed that attitude toward research and development. Negotiated contracts for private aeronautical research and development had resulted in the design of aircraft models B-17, 24, 25, 26, 29, the A-20 and 24, and the P-38, 39, 40 and 47, most of which were used extensively in the war. The contribution of scientists and engineers from private industry and the universities was spectacular. By the end of World War II, the United States had reached world preeminence in defense technology, atomic energy, medicine, and military selection and training. That experience caused a permanent shift in policy toward flexible contracting for research and development in industry, research centers, and universities--toward what we know today as "extramural research and development

Basic Scientific Research

The experience of World War II significantly altered policy towards basic science as well. The development of rocketry, atomic energy, and penicillin had depended upon basic research in the physical, mathematical, and life sciences, much of it European in origin. After the war, federal officials recognized that Europe could no longer be counted on as a major source of America's scientific base. The United States would have to build its own scientific resources for the future. In the spirit of this conviction, President Roosevelt asked a committee chaired by Vannevar Bush to plan the nation's future commitment to science, "the endless frontier." In 1950, after long negotiations with many people, Congress authorized the National Science Foundation. This legislation gave recognition to science as a national resource and attempted to ensure its independence from the shifting priorities of federal mission agencies and the requirements of government research bureaus.

These changes in federal policy have great significance for fundamental research relevant to education. In the behavioral and social sciences, most basic research on learning, intelligence, child development, and social institutions prior to World War II was privately supported. During the 1930s, for example, the pioneering research of Kurt Lewin and his students at the Iowa Child Welfare Station was funded by the General Education Board of the Rockefeller Foundation. Federal funds gave some impetus to research on testing during World War I, but the federal government did not support the work of behavioral and social scientists on a large scale until World War II.

During World War II, scientists were mobilized to help win the war--not to carry out basic research. For behavioral and social scientists interested in the basic processes and problems of education, this was no less true. Nevertheless, the problems of war were often problems of education, and the talents and experience of these scientists proved of great use.

Behavioral and social scientists developed tests for selecting officers and assigning soldiers and sailors to duty; they devised efficient training programs and altered old ones; they helped design machines to fit and capitalize on human capabilities; they created successful programs to reduce illiteracy in the ranks and to make possible the drafting of previously incarcerated persons; they developed techniques for increasing civilian cooperation with wartime programs; and everywhere they used scientific methods to evaluate the success or failure of the new applications. Among these scientists were anthropologist Margaret Mead, psychologists Neal Miller, Arthur Melton, and Donald Hebb, social psychologists Kurt Lewin and Carl Hovland, sociologist Samuel Stouffer, and educational testing and training specialists, John Flanagan, Lloyd Humphreys, and J. P. Guilford.

After the war ended, many of these scientists received new federal support for fundamental research. Programs supporting basic research through contracts were begun in the Army and the Office of Naval Research; grant programs were instituted in the National Institute of Mental Health and the National Institutes of Health. Each of these programs supported some fundamental research relevant to education. Problems of learning, training, social and cognitive development, physical development, perception, neurological processes, adjustment, individual differences, retardation, thinking, and social interaction were covered. Although relatively small in size, these basic research activities were among the first supported by mission agencies. This research was given additional stimulus after 1950 by the National Science Foundation, which in its early years gave small support to psychology, anthropology, and economics.

Application of Research and Development to National Problems

The third major change in federal policy having implications for fundamental research in education is traced most clearly to 1957, when the Soviet Union launched Sputnik I. This achievement threatened America's claim to preeminence in science and technology, and in response, the United States began its huge space program. Because human resources in science were considered an important component of the nation's technological superiority, the federal government in ten years multiplied expenditures for improving science education curricula by thirty.

The success of the technological endeavors in space probably reinforced expectations that age-old social problems such as poverty, inequality, and crime could be solved in the same way. In its extreme form, this "way" was to (1) identify the objective and plan the required sequence of activities (carry out research first, then phase into development, then disseminate the results and demonstrate the solution); (2) set definite time limits (e.g., ten years); (3) and procure the research and development (R&D) required. However unrealistic the parallel in practice, the concept caught on (see Nelson 1974). "Social R&D" now has a major role in federal research and development. Agencies, subagencies and special programs apply R&D to the solution of social, economic, and

health problems.[15] Among these is the improvement of education and the reform of schools.

By tradition, research in education has served school reform efforts (Cronbach and Suppes 1969). The new federal policy, however, added an important dimension to this association: the methods and assumptions of federally sponsored science and technology. Thus, vocational research centers, educational policy research centers, research and demonstration centers for the handicapped, regional laboratories, R&D centers, instructional materials centers, research coordinating units, and information clearinghouses were established. Many of these copied the model of research and development in engineering and the military that had proved so effective during World War II; some, like the R&D centers, followed the model of the agricultural experiment stations. By 1969, at least fifteen major task forces or committees had conducted needs assessment studies and had made recommendations for the new R&D tasks in education.[16] Identified for attack by R&D were such problems as equality of educational opportunity, urban education, teacher militancy, the relevance of education, and local control of education. Many groups urged more problem-focused research, more curriculum development, better efficiency of R&D, and the kind of management by objectives and timetables employed in the National Aeronautics and Space Administration (see for example Gideonse 1967). Legislation (the Elementary and Secondary Education Act of 1965) was passed to encourage application and dissemination of the results of R&D.

The National Institute of Education

The joining of federal research and development programs in education with programs to improve education had great importance. But until the late 1960s, commitments to fundamental research generally remained separate from commitments to improve education through R&D. Unlike the National Institute of Mental Health, the National Institutes of Health, and the Department of Defense, the Office of Education sponsored very little basic research in the behavioral and social sciences relevant to its mission; no legislation existed to direct such a course. Pressure increased to change that policy, however. In 1967, the director of the Office of Education stated his intention to begin support for "basic studies," and began doing so on a small scale by asking a National Research Council/National Academy of Education group to select recipients of some $2 million in grants.

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[15]The number of new programs added in the last decade is very large. The National Science Foundation document, *An Analysis of Federal R&D by Function* (NSF 75-330) shows a net increase of 121 federal civilian R&D programs during the period 1969-1976, from a base of 192.

[16]Among the first was the report to President Johnson in 1964 of the Task Force on Education, chaired by John Gardner, and the report in 1967 of the Research and Technical Programs Subcommittee of the House Committee on Government Operations, chaired by Henry S. Reuss (1967).

Others urged that fundamental research be supported to improve the overall quality of R&D and to build basic knowledge about educational processes (see for example Levien 1971).

When Congress established the National Institute of Education (NIE) in 1972, it made explicit a policy for fundamental research relevant to education. The Institute was given four responsibilities (Levien, 1971):

1. to help to solve or to alleviate the problems of, and promote the reform and renewal of, American education;
2. to advance the practice of education, as an art, science, and profession;
3. to strengthen the scientific and technological foundations of education; and
4. to build an effective educational research and development system.

The 1972 act thus gave the NIE primary responsibility for research in education and authorized it to support fundamental scientific research relevant to education. This action placed fundamental scientific research squarely among the educational functions of government. The wording gave fundamental scientific research relevant to education a status that is, in theory, independent of applied research and development. The act also made explicit a role for fundamental research that had evolved implicitly since the end of World War II, as basic science gained federal recognition and support.

FEDERAL POLICY IN PRACTICE

The way a federal agency allocates its resources and attentions is one measure of the agency's commitments--a measure far from perfect. Different activities are not equal in cost, they do not demand equal attention, and they do not capture the interest of congressional committees and constituents equally. Moreover, the data needed to make comparisons are sometimes unreliable or unavailable. The Committee has nonetheless examined the distribution of resources and attentions of the NIE and other agencies with interest because they are probably the best way to evaluate operating policy for fundamental research relevant to education.

Relative Levels of Effort

The federal government takes several routes to solving or alleviating the problems of American society. Among them are direct and indirect subsidies to individuals, public services, and incentives to local, state, and private action. Another route is social research and development, broadly defined. Applied research and experimental development can help to produce new practices and technology; social experiments and demonstrations can test or refine new programs; evaluations can provide an assessment of new activities; basic research can improve understanding of society and its problems. It is inaccurate and unrealistic to discuss these activities as though they were different ways of doing the same thing. Basic research,

for example, is not designed to solve a practical problem but to build knowledge, whereas many demonstrations are not designed to produce either new knowledge or new applications of what is known. Nevertheless, each has an important part to play in moving toward the enrichment and improvement of society. The questions we address here are whether each element in social R&D for education has the support it deserves, and whether the distribution of effort reflects a balanced, realistic perspective on improving and enriching education in this country.

Based on its detailed survey of federal programs, the Study Project on Social Research and Development of the National Research Council (NRC) has estimated that in 1975 the government obligated $1.65 billion for what they call "knowledge production and application"--endeavors to acquire knowledge and new solutions for social, economic, and other non-medical and non-technological public problems. This estimate is considerably higher than the estimate of civilian research and development from the National Science Foundation (NSF), because it includes policy research, demonstrations, statistical and information services, and evaluations not counted in the annual survey by the NSF. We prefer to begin with the NRC data, because they show a fuller range of elements in modern social R&D. We refer later to data collected by the National Science Foundation and others.

Table 1 presents the NRC estimate for social R&D and the results of the Study Project's survey of R&D related to education. The data in Table 1 show that twenty-four agencies or subagencies support research, development, and other activities directly related to the future improvement of education. (We have summed for this table all programs the NRC classified as functioning to improve general public education, science, health and cultural education, education and training for employment, and development of human resources.) Other programs in agencies such as the National Institute of Mental Health and the basic research divisions of the NSF support work relevant to education, although their primary function is different. Even given the number of agencies involved, the total support for education R&D is quite large--33 percent of all social R&D, and 3.6 percent of all federal services to education--or just over $.5 billion.

Table 2 presents two independent estimates of the way education R&D is distributed among types of activity and the principal areas of education to which these activities apply. The first estimate is based on the NRC Study Project's survey of R&D programs within agencies. The second estimate is based upon a classification of individual projects in a limited sample of agencies. The two estimates differ understandably in size, but they are in surprising agreement about the distribution of R&D activities in education.

Of the activities described in Table 2, the oldest are probably the programs for R&D in education of the Department of Defense. We have included these because our review of projects showed a substantial number that are clearly pertinent to education in general. There are projects, for example, on literacy, the development of quantitative skills, and the measurement of achievement. (Purely military training and employment projects are not included in these data.) Most of the work that applies to education and training for employment and to the development of human resources is supported by the Department of Defense and the Department of

Table 1 Total and Education-Related Federal Obligations for Social Research and Other Related Activities, Fiscal 1975 (dollars in thousands)

Agency	Total Social Research and Other Related Activities	Education-Related Research and Other Activities		
		Total	Research	Other Activities
Department of Agriculture	263,639	56,825	40	56,785
Department of Commerce	93,356	220	100	120
Department of Defense	59,174	43,105	25,292	17,813
Department of Health, Education and Welfare:				
Alcoholism, Drug Abuse, and Mental Health Administration	85,635			
Ass't. Sec. for Education	15,797	15,797	7,513	8,284
Center Disease Control	5,220	1,956	108	1,848
Health Resources Administration	75,805	14,775	4,885	9,890
Health Services Administration[a]	40,497			
National Institute of Education	73,820	73,820	11,700	62,120
National Institutes of Health	66,566	4,765		4,765
Office of Education	191,445	191,445	12,192	179,253
Office of Human Development	62,829	1,120		1,120
Ass't. Sec. Planning & Evaluation	30,004	2,714	1,168	1,546
Other	39,643			
Department of Interior	12,967	1,727	10	1,717
Department of Labor	85,276	64,909	14,007	50,902
Department of State	19,409	1,447	610	837
Department of Treasury	37,236	110	110	
Appalachian Regional Commission	9,154	1,300		1,300
Civil Service Commission	5,255	5,255	1,531	3,724
Common Service Administration	8,292	2,500		2,500
Equal Employment Opportunity Commission	2,504	2,504	301	2,203
Federal Mediation & Conciliation Service	105	105	59	46
National Foundation Arts and Humanities	17,511	17,511	190	17,321
National Science Foundation[a]	109,744	39,375		39,875
Smithsonian Institution	9,117	3,187	1,900	1,287
Veterans Administration	3,877	3		3
All other agencies	230,903			
TOTAL AGENCIES	1,650,780	546,975[b]	81,716	465,259
Distribution by function	100%	33%		
Distribution by activity[c]		100%	15%	85%

NOTE: Social research and other related activities includes basic and applied research, evaluation, statistical activities, development of materials, demonstrations, and dissemination whose function is to understand or improve society in areas such as health, education, and the economy. Biomedical, technological, and space problems are excluded. Education-related research and other activities includes all the activities listed above whose primary function is understanding or improving general education, specialized education, and employment and training.

[a]Agency supports research related to education but its primary function was identified otherwise (see Table 3).

[b]General and specialized education is 26 percent of total social research and related activities; employment and training is 7 percent.

[c]The distribution of activities for total social research and related activities is 65 percent for non-research activities and 35 percent for research.

Source: Study Project on Social Research and Development, National Academy of Sciences-National Research Council, Washington, D.C., 1976.

Table 2 Two Estimates of the Distribution of Research and Other Activities Relevant to Education, Fiscal 1975 (dollars in thousands)

Estimate 1. Programs for Research and Other Activities in Education (Study Project on Social R&D)

Type of Activity	Area of Interest: General Schooling	Science, health, and Cultural Education	Employment Manpower and Training
Research	32,539 (9%)	7,348 (9%)	41,829 (35%)
Evaluation	18,071 (5%)	813 (1%)	1,030 (1%)
Statistical Activities	2,967 (1%)	--	46,900 (39%)
Development of Materials	47,695 (14%)	40,132 (48%)	15,547 (13%)
Policy Demonstrations and Social Experiments	90,267 (26%)	3,612 (4%)	8,934 (8%)
Other Demonstrations	73,648 (22%)	12,438 (15%)	2,698 (2%)
Dissemination	79,689 (23%)	18,834 (23%)	1,956 (2%)
TOTAL	344,376 (100%)	83,177 (100%)	118,922 (100%)
Agencies engaged in support at $15 million or more:	Office of Education, National Institute of Education, Assistant Secretary of Education, Department of Agriculture	NIH, CDC, HRA, NSF (Science Education), National Foundation for Arts and Humanities	Department of Labor, Department of Defense

Estimate 2. Research Projects and Other Activities in Education (Social Research Group)

Type of Activity	Area of Interest: General Schooling	Science and Health Education	Employment Manpower and Training
Research	21,509 (8%)	4,208 (8%)	1,590 (44%)
Evaluation	22,407 (8%)	111 (21%)	479 (13%)
Development of Materials	49,933 (18%)	22,492 (42%)	1,050 (29%)
Demonstrations and Dissemination	179,294 (64%)	6,694 (13%)	405 (11%)
Other Dissemination	7,210 (3%)	19,511 (37%)	56 (2%)
TOTAL	280,413 (100%)	53,016 (100%)	3,580 (100%)
Agencies Included:	Office of Education, National Institute of Education, NSF (Basic),[a] Department of Agriculture, NIH: NINCDS, NICHD, OHD, NIMH	NSF (Science Education),[a] NIDA, NIAAA, HSA, BCHS, SRS	Department of Labor, Department of Defense[a]

NOTE: The data for the left portion of the table was supplied by the NRC Study Project on Social R&D. The right portion of the table was prepared by the Committee on Fundamental Research Relevant to Education, using data supplied and coded by the Social Research Group (George Washington University) of the Project on Interagency Coordination. The Study Project on Social R & D data are for all programs engaged in education R&D. The Interagency Coordination Project data are for individual projects in education through age 24 in selected agencies only (see Table 3 for list).

[a]The Interagency Coordinating Project did not code projects of the Department of Defense (DOD) or the National Science Foundation (NSF). The data presented in this table, however, include research or related projects relevant to education supported by these two agencies. The Committee obtained and coded project abstract lists of unclassified research in the DOD and lists of projects in Science Education, and the basic behavioral and social science research programs of the NSF. Coding for education relevance was based upon the inclusion of key words or phrases in the abstract, such as human learning, sentence comprehension, attention, and children's development. This method and those used by other groups probably underestimate education-relevant research.

BCHS Bureau of Community Health Services
CDC Center for Disease Control
HRA Health Resources Administration
HSA Health Services Administration
NIAAA National Institute for Alcohol Abuse and Alcoholism
NICHD National Institute of Child Health and Human Development
NIDA National Institute of Drug Abuse
NIH National Institutes of Health
NIMH National Institute of Mental Health
NINCDS National Institute of Neurological and Communicative Disorders and Stroke
NSF National Science Foundation
OHD Office of Human Development
SRS Statistical Research Service

Commerce. Their programs allocate more than one-third of this R&D funding to research. Only about one-sixth goes to demonstration projects or dissemination.

The next oldest programs are those that pertain most directly to specialized education in science and health. In these programs, especially in the Science Education Directorate of NSF, the development of materials (such as new curricula) claims the largest proportion of support--about 40 percent. Research in science, health, and cultural education is allocated 8 or 9 percent.

The most recent programs, including the programs of the Office of Education and the National Institute of Education, are those that support work on public school education, and to a far lesser extent, preschool and adult education. In these programs, less than 10 percent of the work supported is research. The largest activity by far is demonstrations and dissemination. We estimate that demonstrations are allocated nearly 50 percent of the funds, and dissemination, about one-quarter of the funds.

Table 3 provides alternative figures from independent sources for estimating how research in education is distributed between its basic and applied components. Both sets of data show that, overall, basic research receives about one-third of the total research support. This proportion is even smaller (22-29% of all research) in the agencies identified by the NRC Study Project on Social R&D as directly concerned with research in education. Basic research is smallest (15-20% of all research) in the Office of Education and the National Institute of Education.

Taking as a rough guide these proportions of basic research and the known figures for total research and total R&D, one can estimate the proportions of basic research in the total R&D budget. As shown in Table 4, we estimate that the Office of Education and the National Institute of Education apportion about 2 percent of the R&D budget to basic research. (These agencies, of course, have the major responsibility for work in general education and schooling.) This estimate contrasts with the 4-percent proportion for basic research allocated by the whole array of agencies concerned with education (as categorized by the NRC Study Project on Social R&D) and with the 12 percent of R&D for basic research in all agencies engaged in social research. We derived these estimates indirectly, but they are remarkably similar in character to the National Science Foundation's own estimates. The NSF data, shown in Table 5, indicate that four agencies having a primary interest in education allocate 3 percent or less of their R&D support to basic research, while federal agencies as a whole allocate about 11 percent of R&D to basic research.

These data are evidence of the low priority that fundamental research relevant to education receives in most of the federal agencies whose responsibility is to support or improve education. The level of effort in comparison to other activities is particularly low in the agencies whose primary concern is public education in this country: the Office of Education, the National Institute of Education, and the National Science Foundation, in its Science Education Directorate.

The current proportion of support for fundamental research in education is even lower than it was prior to the enactment of explicit policy in 1972, according to these estimates. As Table 6 shows, whether one considers individual projects or agency programs, research in 1968 was

Table 3 Two Estimates of Federal Obligations for Basic and Applied Research Relevant to Education, fiscal 1975 (dollars in thousands)

Agencies Engaged in Some Education Research (Study Project on Social R&D)	Estimate 1. Programs for Research in the Behavioral and Social Sciences (National Science Foundation)		Estimate 2. Research Projects Relevant to Schooling and Other Formal Education, Selected Agencies Only (Interagency Coordinating Project)	
	Basic Research in Behavioral/ Social Sciences	Applied Research in Behavioral/ Social Sciences	Basic Research in Education	Applied and Policy Research in Education
Department of HEW:				
National Institute of Education	1,894	2,174	810	6,453
Office of Education		5,266	1,019	3,813
Subtotal	1,894 (20%)	7,440 (80%)	1,829 (15%)	10,266 (85%)
Office of Ass't Secretary of Education		550		
Health Division	9,190 (NIH)	15,067 (NIH)	1,964 (NICHD, NINCDS)	472 (NICHD, NINCDS)
		14,368 (CDC, HRA, OHD)	667 (OHD)	1,344 (OHD)
Department of Agriculture	12,484	38,216	225	83
Department of Interior	611	5,459		
Department of State	0	5,688		
Department of Commerce	91	7,810		
Smithsonian	7,643	0		
Department of Labor	877	14,401	0	12
Department of Defense	11,176	45,206	571[a]	1,007[a]
Veterans Administration	260	5,510		
Subtotal	44,226 (22%)	159,715 (78%)	5,256 (29%)	13,184 (71%)
Other Agencies Whose Research is Relevant to Education				
National Science Foundation	35,601	13,830	3,084[a]	1,322[a]
Alcohol, Drug Abuse, and Mental Health Administration	26,791	32,419	950	3,366
Health Services Administration	0	0	205 (BCHS)	0
Social and Rehabilitation Service	0	9,648	0	0
TOTAL	106,618	215,632	9,495	17,872
Distribution of Total Basic and Applied Research	33%	67%	35%	65%

NOTE: The National Science Foundation defines basic research as research in which the investigator is concerned primarily with gaining a fuller knowledge or understanding of the subject under study. In applied research, the investigator is primarily interested in a practical use of the knowledge or understanding for the purpose of meeting a recognized need. The Interagency Panel employed similar definitions.

[a]Data from Committee on Fundamental Research Relevant to Education. Coding of basic and applied research was based upon the definitions developed by the National Science Foundation.

ADAMHA Alcohol, Drug Abuse, and Mental Health Administration
BCHS Bureau of Community Health Services
CDC Center for Disease Control
HRA Health Resources Administration
HSA Health Services Administration
NICHD National Institute of Child Health and Human Development
NIH National Institutes of Health
NINCDS National Institute of Neurological and Communicative Disorders and Stroke
OHD Office of Human Development

Table 4 Estimates of Basic Research Relevant to Education as a Percentage of all R&D Activities in Education, 1975

R&D Activities in Education	Agencies		
	NIE and OE[b]	NRC-Categorized Agencies Engaged in Education Research[c]	All Agencies Engaged in Social Research[d]
Basic research as percentage of R&D (highest estimate)[a]	2%	4%	12%
Applied research as percentage of R&D	7	11	23
Total research as percentage of R&D (highest estimate)	9	15	35
Total R&D	100% ($265,265,000)	100% ($546,975,000)	100% ($1,650,780,000)

NOTE: Data from the NRC Study Project on Social R&D and the Social Research Group.

[a]Derived from percentage distribution data summarized in Table 3 (estimates 1 and 2) and base data in Table 1. For example, the highest estimate of basic research as a percentage of all research in the National Institute of Education and the Office of Education (Table 3) is 20 percent. Twenty percent of education research in the National Institute of Education and the Office of Education ($23,892,000 in Table 1) is $4,778,400, which is 1.8 percent of total social R&D in NIE and OE.

[b]See first subtotal in Table 3, where for the National Institute of Education and the Office of Education, basic research is 15-20 percent of total research.

[c]See second subtotal in Table 3, where for agencies categorized as engaged in education research, basic research is 22-29 percent of total research.

[d]See total in Table 3, where for all agencies studied (including basic behavioral science in NSF, for example), basic research is 33-35 percent of total research. Research is 35 percent of all social R&D (Footnote 4, Table 1), or about $577.8 million.

OE Office of Education
NIE National Institute of Education

Table 5 Estimates of Basic Research as a Percentage of all R&D Activities in Four Agencies Having Major Concern With Education: National Science Foundation Data, 1975 (dollars in thousands)

Agency	Total		Activities					
			Basic Research		Applied Research		Development	
	Amount	%	Amount	%	Amount	%	Amount	%
National Institute of Education	$69,868	100	$1,894	3	$2,174	3	$65,800	94
Office of Education	45,859	100	---	0	5,266	11	40,593	89
Office of Human Development	64,340	100	---	0	7,077	11	57,263	89
Assistant Secretary of Education	12,647	100	---	0	550	4	12,097	96
All agencies total R&D	19,044,260	100	2,145,834	11	4,783,376	25	12,115,050	64

Source: *Federal Funds for Research, Development and Other Scientific Activities, Fiscal Years 1975, 1976, and 1977.* NSF 76-315.

Table 6 Estimates of the Distribution of Support for Research and Related Activities, 1968 and 1975 (dollars in thousands)

Type of Activity	1968: Project Support (data from National Center for Educational Research and Development, Office of Education)			
	Office of Education		All Federal Agencies	
Research	38%	(34,650)	44%	(63,794)
Evaluation	5	(4,531)	4	(6,087)
Development	48	(44,404)	45	(66,087)
Demonstrations	2	(1,476)	2	(2,966)
Dissemination	8	(6,978)	5	(7,649)
TOTAL	100%	(92,039)	100%	(146,583)

Type of Activity	1968: Program Support (data from NSF)			
	Office of Education		All Federal Agencies	
Research	37%	(33,562)	34%	(5,364,860)
Basic	*7*	*(6,473)*	*13*	*(2,103,837)*
Applied	*30*	*(27,089)*	*21*	*(3,261,023)*
Development	63	(57,437)	66	(10,556,565)
TOTAL	100%	(90,998)	100%	(15,921,424)

Type of Activity	1975: Project Support (data from Social Research Group)	
	Office of Education and National Institute of Education	
Research	5%	(12,295)
Basic	*1*	*(1,829)*
Applied	*4*	*(10,266)*
Development	18	(46,493)
Evaluation	7	(17,769)
Demonstration	67	(169,791)
Dissemination		
Other Dissemination	3	(7,114)
TOTAL	100%	(253,262)

Table 6 (continued)

Type of Activity 1975: Program Support (data from National Science Foundation)

	Office of Education and National Institute of Education		All Federal Agencies	
Research	8%	(9,334)	36%	(6,929,210)
Basic	*2*	*(1,894)*	*11*	*(2,145,834)*
Applied	*6*	*(7,440)*	*25*	*(4,783,376)*
Development	92	(106,393)	64	(12,115,050)
TOTAL	100%	(115,727)	100%	(19,044,260)

NOTE: The National Center for Educational Research and Development and Social Research Group data are both based on expenditures for projects, but categories other than "research" and "development" may not be comparable. These project data may be used for estimating the relative proportion of research support in two years, not for comparing levels of support.

The National Science Foundation data across years are for program obligations. They are approximately comparable and indicate trends in levels of support as well as the trends in relative distribution of activities. Note also that proportions attributed to research are roughly the same whether program- or project-level data are used.

Sources: National Center for Educational Research and Development (1969) *Educational Research and Development in the United States*; Office of Education. No. HE5.212:12049 December. Washington, D.C.: U.S. Government Printing Office.

Federal Funds for Research, Development, and Other Scientific Activities, Fiscal Years 1975, 1976, and 1977. NSF 76-315.

Federal Funds for Research, Development, and Other Scientific Activities, Fiscal Years 1968, 1969, and 1970. Vol. XVIII. NSF 69-31.

more than one-third of total R&D. Basic research in the Office of Education, according to the NSF estimate, was allocated about 7 percent of the Office of Education's R&D budget, compared to 13 percent for basic research supported by all federal agency programs. In 1975, research remained above the one-third level for all federal agencies. Yet in the Office of Education and the National Institute of Education combined, research fell to less than 10 percent of R&D, and basic research, as we noted earlier, to 2 percent or less. Furthermore, the program support data from NSF reveal that dollar amounts for basic research in education (in the OE only in 1968 and the OE and the NIE combined in 1975) fell considerably in the 1968-1975 period. Support for fundamental research relevant to education has therefore declined in an absolute sense as well as in proportion to other R&D activities.

Balance and Quality of Effort

The data we have reviewed show that the government's overall investment in improving education through R&D has increased considerably in recent years (for example, see totals in Table 6). This new investment is an encouraging sign of commitment to the future of education and of concern about those who are not receiving the best that education can offer. Certain elements of R&D have received far more attention than others, however. Since 1968, support for demonstrations, such as trials of the Follow Through and Right to Read projects, dissemination, such as the ERIC program, development of curricula, and evaluation of new programs and innovations has multiplied; on the other hand, support for fundamental research has not. Some of the new R&D efforts, while expensive, have great merit--others do not.

Some Commendable Efforts

It is important that practitioners have access to reliable, current information about the practice of education and that they have an awareness of scientific work related to their field. It is also important that new and innovative programs for education be carefully evaluated and tried on a small scale before they are carried to full application. Development of up-to-date curricula and new techniques for education is another worthwhile activity. Certain of the new dissemination, demonstration, evaluation, and development activities to accomplish these ends have been of high quality. Some recent evaluations of Head Start, Sesame Street, and other projects, for example, carefully attach different kinds of assessment to different objectives of the programming. A longitudinal study of children over many years may be employed to assess academic effects; an experimental study with random assignment of children to groups may be used to evaluate immediate cognitive gains; a cross-sectional survey of involved and uninvolved adults may be conducted to assess community reaction. Alternative study designs and statistical analyses of performance have been developed to evaluate overall gains from the program, "catching up" of particular subgroups, and gains due to maturation or to extra-program factors. The best of these evaluations have greatly improved the assessment of the

cost and benefits of proposed educational applications and their alternatives. They have also moved forward the overall capability to assess the costs and benefits of government policies. In this respect, calls during the sixties for a new R&D operating policy turn out to have been wise.

Some of the most productive new programs show considerable application of knowledge and advanced methodology from basic research. For instance, contemporary designs for computer-assisted and individualized instructional programs have made, from their beginnings, considerable use of fundamental research on memory and learning by such scientists as Pavlov, Skinner, Angell, and Thorndike. Researchers who have worked on these applied programs have drawn on their own and others' fundamental work and, in developing the projects, have stimulated fundamental inquiry (see for example Suppes 1966, Suppes and Morningstar 1972, Atkinson and Hansen 1966, Atkinson 1974, 1975).

This last is not an isolated example. Another is the national television program, *Sesame Street*, whose development was supported by the Office of Education and private funds. *Sesame Street* is not the final answer to early childhood education, but children who watch it experience significant cognitive gains.

Sesame Street was developed by a team of educators, researchers, and creative television producers who also worked with outside advisory groups of teachers, children's book writers, educational planners, and researchers (see Lesser 1974). In formulating the goals, curricula, format, and evaluation techniques for *Sesame Street*, these groups drew on knowledge from fundamental research to answer questions such as: What do children aged 3-5 already know? What kinds of skills do children need to learn to read? What causes children to attend to the printed word and to understand its function? How do you know when children have learned how to think over alternative solutions to a problem? How useful is repetition? The answers to these questions were not always known, and usually it was not clear how they could be applied to television (especially in view of the paucity of research on television's impact on children). Yet it is clearly the case that basic research (most of which had origins years ago) played a useful role in the making of *Sesame Street*.

Sesame Street received the American Psychological Association award for applied psychological research, and it is properly categorized as an applied undertaking. However, the program provides some excellent examples of the many ways in which federally supported fundamental research can inform and improve practice, and the ways in which work on an applied problem can, in turn, advance fundamental inquiry as well:[17]

1. In a series of workshops held to help producers and writers better define their audience, basic researchers provided a picture of the average four-year-old child's mental development, worldly knowledge, family interactions, fears, and interests. This image was influential in determining the topics of skits, the degree of repetition, and even the use of special effects. For example, research since Freud has determined several ways of

[17]Some of this information we base on minutes of meetings, working papers, and curriculum guidelines supplied by participants in the Children's Television Workshop, Sesame Street and Electric Company workshops.

A SESAME STREET SCRIPT ON SIBLING RIVALRY

Buffy Sings to Big Bird

Scenic: Nest area

Talent: Big Bird, Buffy

Music: Lullabies as per bit

Props: Blanket for Big Bird

Big Bird Is sitting in his nest. Buffy sits next to him on the side.

BB: Hey Buffy, would you sing me a song? You use to sing to me all the time but seems like you haven't sung me a song for ages.

Buffy: Gee, I'm sorry, Big Bird. I love to sing to you. Let's see . . . what shall I sing . . .

She thinks for a while, then just sort of slides into "Rock-a-Bye-Baby."

Note: Buffy, substitute any lullabies you wish for the ones listed in this bit. I'm sure you have some wonderful ones. I only want them to be recognizable as baby-songs, lullabies, by the kids in the audience. Don't feel you must use the ones I've called for.

BB: (After a moment) Hey, that's a song you sing to babies to make them go to sleep.

Buffy: (Wasn't even aware that's what she had chosen) Well, I guess it is, Big Bird. It's called a lullaby. Don't you like that song?

BB: No I don't. Sing me something else.

Buffy: All right.

Thinks again. Then starts singing "All the Pretty Little Horses."

Buffy: Hush-a-bye, don't you cry. Go to sleepy, little baby...

BB: That's another one of those baby lullabies.

Buffy: I suppose it is, Big Bird. You don't like that song either?

BB: No.

Buffy: O.K. No sweat. I'll pick something else.

She thinks, then starts a third lullaby. Big Bird is getting mad.

BB: There you go again. You keep singing baby lullabies.

Buffy: I am sorry, Big Bird. I guess I miss my little baby. I miss not being able to sing lullabies to him . . . so I guess I automatically started singing them to you.

BB: Well I'm not a little baby. I'm a great big bird. And I don't need any baby songs you were gonna sing to somebody else. To make them go to sleep. Sing me a song you sing to a great big bird who doesn't need to go to sleep.

Buffy: O.K. O.K. I beg your pardon. *(Pause)* How's this?

She launches into a loud rousing rendition of some extremely spirited song. Like, for example, MacNamara's Band. After one line she interrupts herself.

Buffy: How's that? That O.K.? No baby's going to sleep to this song. This all right?

BB: That's fine. That's just fine. Sing that one.

Buffy sings the entire song at top volume. She practically exhausts herself, singing loud and with great emphasis. Finally at the end she turns to Big Bird.

Buffy: Well O.K.? How was that? Better?

Camera pulls back. Big Bird is fast asleep, peacefully smiling in his sleep. He snores.

EXCERPT FROM A PRELIMINARY DRAFT OF GOALS FOR SESAME STREET

Relational Concepts

An understanding of relational concepts is sought on two levels: (1) recognition of an instance of that concept and (2) demonstration of understanding through the performance of appropriate manipulations.
Recognition: Given two objects (large and small box) varying on a predetermined dimension (size) the child can indicate which object is an instance of the concept in question (big).
Demonstration: Given two objects (plane and bridge) the child can manipulate the objects (fly the plane under the bridge) to demonstrate his understanding of the concept (under).

1. Relations based upon visual cues.
   a. Size
      ex. large, larger, largest; short, tall; skinny, little, etc.
   b. Position
      ex. under, over, on top of, below, above, beneath, etc.
   c. Distance
      ex. near, far away, close to, next to, etc.
   d. Amount of number
      ex. all, none, some, more, less, etc.
2. Temporal Relationships
   ex. early, late, fast, slow, first, last, etc.
3. Auditory Relationships
   ex. loud, louder, loudest; soft, softer, softest; noisy, quiet; high, low, etc.

Classification

1. Given at least two objects that define the basis of grouping, the child can select an additional object that "goes with them" on the basis of:
   - Size: Height, length or thickness
   - Form: Round, square, triangular
   - Function: to ride in, to eat, etc.
   - Class: Animals, vehicles, etc.
2. Given 4 objects, 3 of which have an attribute in common, the child can sort out the inappropriate object on the basis of:
   - Size: Height, length, thickness
   - Function: to ride in, to eat
   - Class: Vehicles, animals
3. The child can verbalize the basis for grouping and sorting.

helping children cope with sibling rivalry, and these were made the basis for several scripts (see first inset).

2. The producers of *Sesame Street* were very concerned about specifying a set of objectives. They collaborated with researchers to design a list of behavioral goals for the program (see second inset). This work, heavily influenced by methodological advances in behavioral science that have occurred during the last fifty years, was an important factor permitting evaluation and assessment of the effects of the program on children.

3. Research on children's responses to *Sesame Street* programs was performed throughout the show's development. This research, at first intended as an evaluative and sorting mechanism, turned up some findings of fundamental importance. It was discovered, for example, that previous laboratory research on children had somewhat underestimated the abilities of children, especially their attention span and memory. The discovery echoes an experience researchers had during World War II, when properly designed applied research produced fundamental advances.[18]

And Some Not So Commendable

Unfortunately, many of the new R&D programs for education have not built on what is known, have contributed little to what is known, and have had unknown or little usefulness for the practice of education. Demonstrations and development projects, for example, have been conducted with inadequate or no planning for their assessment, and attempts to evaluate them retrospectively have proved to be of limited value. Overevaluation--or more precisely, unfocused, poor quality evaluation--is another problem.

Even more serious are those projects that contradict what is known scientifically, build on an inadequate base of knowledge, are ill-designed to fill gaps in understanding, or require quick, predictable answers from science that are inherently impossible to achieve. We have examined many recent requests for proposals (RFPs), program announcements, actual projects, and project reports and have concluded that the problem spreads throughout many government agencies, touches all elements of education R&D, including fundamental research, and is frequent enough to overshadow the work of good quality.

The following cases taken from different agencies illustrate this point. With no desire to embarass, we have deleted identifying information from the following four examples.

(1) RFP for a special education teacher certification project.

> This applied research was to be [and was] funded at about $1 million. The purpose was to identify those teaching behaviors that significantly influence learning in students

[18]The Behavioral and Social Sciences Survey Committee (National Research Council 1969) noted, for instance, that the study of short-term memory was in part advanced by the need during World War II to improve human performance of vigilance tasks, such as watching air-defense radar scopes.

with unusual problems. This information would be used to develop a new statewide licensing system for special education teachers. [It was not.] The RFP called for three phases: The Design Development, the Pilot Year, and Large-Scale Testing. During the first year, a skeleton design was to be developed. During the second year: (1) the procedures for measuring teacher performance and student growth would be developed and (2) hypotheses on their relationship were to be generated. During the third year, data would be gathered to establish "solid empirical relationships between teacher variables and pupil achievement."

This RFP imitates the techniques of NASA, identifying a product goal and setting timetables for research. The rational problem-solving approach to goal attainment, however, is inappropriately applied in this case. Measures of teacher performance are still primitive and unreliable; the special needs of different kinds of children are not yet well understood; much more work is required to understand the financing, professional impact, and social effects of teacher certification. A more adequate base of scientific knowledge is required before this applied research can pose answerable questions with reasonable tools. For the time being, an informed judgment about licensing requirements would probably serve better than this formal study. The proposal is out of touch with what is known, uses unsound methods, and fails to acknowledge the kinds of basic work needed to accomplish its goals.

(2) RFP for basic research on barriers to the entry of minorities and women in medical careers.

This RFP announced a competition for a 12-month project ("36 man-months") that would identify "all" of those factors that prevent minorities and women from becoming doctors, medical technicians, dentists, epidemiologists, and other medical professionals. The project was to include a "nationwide survey of students, teachers at all levels of education, and parents."

This attempt was simply unrealistic in its objectives or scope.

(3) Operation of an information network center.

This center has responsibility for acquiring, cataloging, indexing, and abstracting selected reports of research and development in education. The center provides "information analysis products and various user services based on the data base."

This center distributes three kinds of materials to researchers and educators: collections of abstracted research reports, staff papers that

review research in selected areas, and guides to curricula. The Committee learned during its investigation that despite concerted attempts to upgrade the quality of the information in the system, nearly 70 percent of the unpublished reports received by this center were kept for dissemination.

Given that only 5-15 percent of submissions to behavioral and social science journals are published (after review by scientific colleagues and revision), the retention rate of the center is far too high. We are sympathetic with the need to communicate unpublished data, but our impression is that much of what is disseminated through these large networks is premature, unreplicated, and superficial in content. A major problem is that the system has no feedback mechanism for discarding information of poor quality.

(4) Pilot demonstration and development of a learning disabilities curriculum.

This recent 18-month project was designed to develop and test a set of workbooks and teacher aids for use with reading-disabled students. The largest part of the project provided directed practice in tracing designs and letters.

The initiative for this project derived from correlations that have occasionally been found between reading problems and difficulty in tracing objects accurately. No causal relationship has been established or is probable, however, and no provision was made to verify whether tracing skills have an impact on learning to read. The development project was premature and unlikely to benefit students. Evidence from many earlier projects like it indicate no long-term gains and minimal short-term impact, other than what would be expected from a student's receiving individual attention.

These illustrative projects are not atypical of education R&D, nor are projects like them exclusive to education R&D.[19] In our judgment, they represent an ill-advised tradeoff of scientific quality and future understanding for promises of immediate products and superficial benefits. To be fair, we must note that the promises have not always been made by administrators. Researchers themselves have sometimes approached their work with inappropriate optimism about the speed with which science might yield results that would inform practice.

Conclusion. The application of science and technology to improve education is of great importance. On the whole, however, we believe that the federal government has adopted policies that encourage superficial and wasteful research that has the appearance of relevance but lacks the substance of general principles. We recommend a significant redistribution of emphasis toward more fundamental research in education and toward a more measured approach to education R&D of all kinds. The current resources for doing so are clearly sufficient.

[19]See, for example, the report on research conducted by the National Institute of Law Enforcement and Criminal Justice (National Research Council 1977).

MANAGING FUNDAMENTAL RESEARCH

This Committee has considerable interest in the way fundamental research is managed, and for most of us this concern is not neutral. We have committed our professional lives to fundamental research and have strong opinions about the kind of environment that stimulates our best work and that of our colleagues. We have tried in this section to outline those views by summarizing the characteristics of the research environment we think most important and offering alternative management policies for achieving them. It is our assumption that when the support of research is part of a federal agency's mission, a major goal will be to locate and fund research of the highest available quality in order to obtain results of the greatest possible use. These comments represent our best judgment concerning the conditions under which valuable research results are most likely to be obtained.

The Nature of Research Resources

The significant aim of fundamental research is new knowledge. This objective can be realized if there exists a pool of many excellent projects and interaction among creative, well-trained, and dedicated investigators who criticize and test one another's ideas. Maintaining and building the intellectual community of investigators who study cognitive, social, and other fundamental processes related to education is crucial to the development of significant new concepts for understanding education. Priorities for research funding, the procedures used in evaluating proposed projects, and the mechanisms used for managing projects strongly influence the interests and capabilities of this research community in ways that develop over relatively long periods of time. They affect the level and quality of interaction and the enthusiasm and care with which promising new ideas are pursued. Therefore, the selection and management of research projects cannot be viewed simply as a process of procuring specific items of research work for specific purposes. The administration of research is part of the research environment and must be designed in concert with its essential elements.

The first requirement of an excellent research environment is that it permits criticism to flourish. Criticism is the main feature of good fundamental research, especially in the behavioral and social sciences, because controversy surrounds the questions that researchers investigate and observations are often nonmechanical and are open to divergent interpretations. Progress, therefore, depends upon a system of checks and balances for discarding the less defensible theories, encouraging better explanatory concepts, and replicating observations. Publication in the open literature and peer review of past and proposed work strengthens this system by exposing ideas to expert criticism and competition.

Expertise is essential. To conduct or select excellent research on any problem requires extensive knowledge of the literature in the subject to be studied. It requires, not just familiarity with what has been done (which can be acquired in a few weeks or months of reading), but knowledge

of a kind that permits critical judgments of the reliability and interpretation of previous findings. Furthermore, the technical capabilities needed to evaluate and carry out significant innovative research on a problem develop only as a result of experience obtained in some years of interaction with colleagues, especially in the same discipline, who criticize and contribute to one's understanding of substantive and methodological issues.

The Committee's emphasis on peer review and disciplinary expertise does not preclude multidisciplinary, "problem-oriented" programs of research. It does, however, call for holding these programs to the scientific standards of the separate disciplines that contribute to them. These programs can be no better than the scientific rigor and significance of the work as judged against the standards of the disciplines. Each research program that is oriented to practical or public issues, then, should have at least two perspectives: one facing the relevant disciplines in the scientific community and one facing the relevant problems identified by the agency.

The evaluation of research proposals requires detailed familiarity with current knowledge about the specific issues to be investigated as well as the probable capabilities of the research methods to be used. The variety of substantive issues on which knowledge is needed and the detail in which evaluators must be familiar with those issues far exceed the capacity of any agency staff. Only by consultation with panels of currently active research investigators can an agency hope to make valid judgments about the likelihood that proposed research projects will develop usable new concepts and knowledge that will contribute to the improvement of education and to the general understanding of processes involved in education.

The second requirement of an excellent research environment is time. Discovery needs a base of careful investigation, and even if chance plays a part, new ideas need testing. Pavlov (1936) gave this advice to young scientists (p. 83): " . . . *Firstly, gradualness.* About this most important condition of fruitful scientific work I can never speak without emotion. Gradualness, gradualness, gradualness . . . never begin the subsequent without mastering the preceding. . . . But do not become the archivist of facts. Try to penetrate the secret of their occurrence, persistently searching for the laws which govern them. . . ."

Federal agencies and the public are understandably concerned about the time required to solve problems through science or to get "answers" from research. In part, as we have observed, the outcomes of research have been misunderstood. But also, the time required to formulate and to carry out productive research is usually underestimated. We emphasize that this time cannot be reduced significantly by programming sequential activities, tightly supervising laboratories, dividing labor according to function, or "buying" clusters of research. The individual is at the heart of fundamental research and he or she needs time to think, worry, and proceed with "gradualness."

The trend, unfortunately, is moving in the reverse direction. Agencies are demanding "short-term" research and "quick, usable results." In practice, this has meant an emphasis on specific contracting and a movement away from continuity of funding. These practices are appropriate for certain activities, such as archiving or the delivery of specific products,

but when the goal of a program is to produce new knowledge and understanding, rigid time schedules are likely to interfere with the need to explore the implications and the qualifications of results. Exploration of the validity, reliability, and generalizability of research will often conflict with desires for quick, usable results. However, the latter are likely to be obtained at the cost of long-term benefits, and support given to many short-term projects is likely to be wasteful of resources.

Any policy that attempts to procure basic research simply in order to solve relatively specific, immediate, short-run goals will almost surely be wasteful and unsuccessful. It will be wasteful because the capability of scientists to work productively on problems involves complex skills and substantive knowledge that is far too expensive to develop for any short-run purpose. And it will be unsuccessful because findings obtained on isolated problems without substantial basis in a general conceptual framework will almost surely be of limited validity and usefulness.

The third characteristic of a productive research environment is openness and flexibility. Research is by its nature an exploratory enterprise, and each step taken is contingent upon previous findings. As research proceeds, an investigator will often need to pursue unanticipated questions or spend more time than was planned to ensure that some results are reliable and valid. A research investigator who is not sensitive to unexpected findings is at fault for overlooking potentially important outcomes.

On the other hand, the Committee believes strongly that research investigators should be responsible to the agencies that support research for diligent effort and careful work. For example, research results should be thoroughly tested for their reliability and evaluated for their general significance. Proposals for research support should present plans in reasonable detail, so that peer review panels can evaluate the probable productivity and significance of the work. Furthermore, investigators should submit timely reports of progress to the funding agency. It must be recognized that the activities actually carried out in a research program may differ substantially from the plans that were initially made. Investigators must be free to test new ideas, follow opportunities not anticipated when the research was proposed, and recheck previous results that new findings call into question. Such departures from research agendas are not arbitrary: when research investigators substantially change a plan, they can justify the change. Recognition that good science requires flexibility and openness to unexpected findings is quite consistent with strong requirements that (1) investigators state clearly what they plan to do with public funds for support of their research, (2) that they pursue their work on the problems they undertake to study with diligence, and (3) that they report their activity.

It is the Committee's opinion that the most productive tool yet devised for managing research without destroying freedom of inquiry is the research grant awarded after peer review of unsolicited proposals. Unsolicited proposals give the responsibility for ideas to the persons who will perfrom the work; peer review of proposals provides the method for selecting persons who are most likely (not guaranteed) to be productive; and grants ordinarily provide some direction yet considerable freedom to follow lines of inquiry that show promise as the work progresses. Again, this does not preclude "problem-oriented" programs. Program officials can ask advice of

citizens, professionals, and others who are concerned with relevance to help plan the program so that proposals with both relevance and scientific merit can be selected; neither does it preclude multidisciplinary peer review designed to stimulate new directions in research.

Management Alternatives

There are several models in the federal government of how excellent fundamental research relevant to education can be managed within the framework of unsolicited research grant programs. Among the best known are the programs for basic research in the behavioral and social sciences at the National Science Foundation, the National Institute of Mental Health, and the National Institute of Child Health and Development. These are described adequately elsewhere, so we do not discuss them in detail. One special point we wish to make, however, is that in many of these programs, support is given to basic research relevant to significant social problems. In the National Science Foundation, for example, there is in the Social Science (basic research) Division a program called "Social Indicators" (National Research Council 1976, p. 43, 79):

> Social indicators . . . cuts across the established social science disciplines, involving especially sociology, social psychology, and economics. The program is more substantively focused than the disciplinary programs. . . . [It] is an excellent example of how basic research in the social and behavioral sciences can be brought to bear upon topics of great social significance, such as environmental quality, family stability, and education. There is good communication between the investigators and those who are concerned with the application of social indicators to public policy matters. . . . The social indicators program . . . can be viewed as both an effort to define and measure basic social magnitudes--hence a basic research effort--and an effort to provide measuring instruments for examining the quality of life and its relation to government policies and programs. Research targets are defined both in terms of needs for application and in terms of the level of knowledge and techniques available for reaching them. Initiatives for developing specific research have come largely from the social science disciplines, and some of the long-term planning functions that could be performed by an advisory panel are performed by a committee of the Social Science Research Council. There is a reasonable expectation of continuity of support for the projects.

We conclude that this management style of "relevant" basic research programs in established agencies is an appropriate and useful alternative for managing fundamental research relevant to education.

Another constructive alternative has been developed by the National Institute of Education, which supports programs of research on topics defined in program announcements, but not so restrictively as to discourage excellent proposals. A scientific panel of competent researchers has been

assembled for each to provide advice on the program announcement as well as review of proposals, and other groups of researchers have worked together to produce ideas and long-range plans. These programs are inadequate in size, and the research they support is too recent to evaluate, but their overall form appears promising.

A third alternative might be considered, especially when agency staff are inexperienced or inexpert in the research to be supported: the use of dual panels for giving the program direction and quality. Since the objective is to support excellent research of interest to the agency, one basic requirement is reaching the pertinent research constituency and convincing its members that only good, well-considered proposals will be seriously considered, that excellent proposals will have a moderate chance of funding, and that excellent research will receive continuity of support. Another requirement is that the opinions of expert peer-review panels that recommend proposals for funding be given substantial weight in final selection. Finally, the research funded should have as close a relationship with agency concerns and priorities as is consistent with current technological and theoretical capabilities in the field.

The mechanism for satisfying these requirements can be two panels: the outside advisory committee, made up of researchers, citizens, professionals, and policy makers, and the peer review panel, made up of researchers. The former advises on the research program and monitors its quality; the latter reviews and rates proposals for funding. There would be some advantage in the two groups' meeting or having members in common so that each is aware of the other's concerns. (Review panels, for example, should be more familiar than they usually are with issues considered important by policy makers.) There would also be value in announcing the names of panel members when the program is advertised so as to inform investigators of the nature and the quality of the audience they face.[20]

Staff

In the long run, an outside advisory committee is likely to be ineffective in improving a program of research without competent leadership and guidance from the federal manager responsible for the program. The program manager should be very familiar with research across a broad spectrum and should have a general knowledge of the agency and programs in other agencies, so that excellent research does not fail to receive funding because it does not suit current programs. There are many ways for program managers to keep well acquainted with events in the scientific community and in the agencies that support its work. These include intra-agency reviews of pertinent federal research support, travel to professional meetings, and research sabbaticals for permanent program managers. Unfortunately, these methods are used less often than is desirable.

[20]Part of this third alternative could be incorporated into the present NIE form of management by overlapping the membership of the planning and review panels with the advisory committee of the larger group. The advantages of doing so are to expose review panels to overall program objectives, to prevent in them overly narrow views of appropriate research, and to better acquaint the advisory committee with basic research programs.

Intramural research is another possibility for facilitating staff contact with research. In our experience, intramural research conducted as a permanent in-house program using agency facilities is usually not very productive. Intramural programs of this sort are unlikely to be fruitful without considerable resources and contact with colleagues active in research elsewhere--and federal agencies have less of both than the laboratories of ongoing, active research institutions. Furthermore, entrenched programs can lead to empire-building and inflexibility. We are aware that some federal agencies have had excellent intramural programs; these have not usually served the purpose, however, of supporting research by management officials who are responsible for the administration of extramural research programs.

One other type of intramural activity is individual research by active researchers from the field who join an agency for a few years and who conduct their research using nearby facilities. Permitting these "rotators" to continue their professional commitment may be viewed as an incentive for attracting competent scientists to work temporarily as program planners and managers. This research should be subject to peer review.

Relation of Fundamental Research Programs to the Agency

While a program for fundamental research primarily serves the purpose of reaching new understanding about important questions, it can have other functions in federal agencies as well. The by-product that we think most important is enhancement of the quality of development, evaluation, dissemination, and demonstration activities as well as that of ongoing services by bringing together persons involved in these programs and persons who are experts in the knowledge base. There is now, for example, a considerable fund of basic knowledge about literacy in this society. With this knowledge, researchers can estimate how readable a document is, how many people will err in following different kinds of instructions, and how to make printed materials more understandable. We can identify many agencies that could and would make use of this expertise if those who study literacy were asked to contribute their knowledge.

We have discussed previously some of the problems facing programs for improving education, such as premature dissemination and innovation. It would not be unreasonable to expect some guidance on these matters from fundamental research managers, agency-supported investigators, and members of review and advisory panels. These persons could provide informed, up-to-date information about research and its implications for practice, point to gaps in knowledge, and provide alternatives for evaluating the effects of new applications. This kind of relationship would also be stimulating for research on practical issues that arises as a natural consequence of fundamental research.

THE ROLE OF THE NATIONAL INSTITUTE OF EDUCATION

The legislation that created the NIE gave it the major responsibility for research in education. In this section, we examine how NIE has assumed

that responsibility and what might be done to promote a more effective role for NIE in research.

Current Programs

The National Institute of Education has six program groups for structuring its activities in research, development, dissemination, demonstrations, and school services. They are Basic Skills, Educational Equity, Education and Work, Finance and Productivity, School Capacity for Problem Solving, and Dissemination and Resources.[21]

The *Basic Skills Group* focuses its research on how children learn reading and mathematical skills and how teachers can help them learn. Plans in progress identify writing as another basic skill to receive attention. A large portion of the Basic Skills budget is committed to five regional education laboratories and six R&D centers. Fundamental research is supported in one of the centers and in two new centers for reading and teaching. The Basic Skills Group funds some work on tests and has supported development of curriculum packages to aid pre- and in-service teachers and administrators to assess children's educational needs in the classroom. The group has supported work in individually guided instruction, teacher competence, and court decisions that affect education.

The *Educational Equity Group* has supported the development of teaching techniques for disadvantaged children and has produced two catalogs of bilingual curriculum materials. One catalog inventories 750 Spanish curriculum materials, and the other contains a compilation of materials in four Asian languages. This program has also sponsored some policy research and evaluations of compensatory education and conducted a symposium on school desegregation. The staff plan to expand the small research components in school desegregation, female career opportunities, and school discipline and social relations.

The *Education and Work Group* is developing and testing an experience-based career education program, which combines work experience and academic training. The program has supported an alternative high school program for eleventh- and twelfth-grade dropouts and potential dropouts, career counseling, occupational preparation, placement for multi-problem families in rural areas, and the development of curricula to assist students in learning about careers. The research component emphasizes career decision making.

The *Finance and Productivity Group* has collected and distributed information and held conferences to help various state legislatures improve their education finance laws and implement a system for competency-based education. The group funds a dozen experimental school projects and alternative education programs at the University of Mid-America. It supports educational satellite programs in Alaska, Appalachia, and the Rocky Mountains, and applied research on the cost-effectiveness of new curricula and technology.

[21] See Appendix B for a list of programs in the NIE and the Office of Education at the time of this writing.

The *School Capacity for Problem Solving Group* provides direct support for local school projects in administration and management. Evaluations of management and organization in nine urban schools are being conducted to learn why certain approaches work better than others. The group has also established a Teacher's Center Exchange to help teachers share information about methods of staff development. A new research panel has been formed to advise on a program for fundamental research related to the organization of schools.

The *Dissemination and Resources Group* supports the Educational Resources Information Center (ERIC) system, whose network of sixteen specialized clearinghouses collects and makes available research reports and articles on education. This group has also produced catalogs of educational products developed under NIE sponsorship. It gives grants to some states for the development of comprehensive dissemination programs; other states recieve funds to carry out specific improvement in their dissemination programs or to plan for future programs.

Evaluation of the Programs

Considerable planning and reorganizing have gone into these programs. Each program reflects an effort to improve communication with the educational community and to respond to the desires of Congress, schools, educational associations, and state agencies. Unfortunately, service has gradually pushed out research, and applied work has driven out fundamental work. During fiscal 1976:

1. Less than one-third of the NIE budget was allocated to research.
2. Approximately 11 percent of the NIE budget (or $10 million, including the 1976 transition quarter) was claimed by NIE to be allocated to basic research.
3. According to our estimates, fundamental research obligations actually incurred during the period totaled a little more than $5 million, or 5.7 percent of the budget.
4. The Basic Skills Group was the only entity with a significant program of fundamental research. (One other program group, School Capacity for Problem Solving, was just beginning a program for fundamental research, and the Education and Work Group supported a few problem-oriented projects with high significance for fundamental issues.)
5. Approximately 95 percent of the research supported was related to primary and secondary school problems.
6. Research investigators in universities, labs and centers, and elsewhere had no clear idea of the Institute's overall intentions for research (see Consultants to the National Institute of Education on R&D Funding Policies 1975). Programs for research were abruptly terminated; some were announced but not funded; and deadlines for proposals were set, in some instances, two weeks or less after the program announcements were received by researchers.
7. The staff of NIE had diverse and contradictory perceptions of its policy for research, especially fundamental research, but nearly all agreed that fundamental research was of the lowest priority and was the first item subject to budgetary cuts.

The NIE staff at all levels list many barriers, both within and outside the agency, to increasing support for fundamental research. These barriers surely do exist, given the recent proliferation of government regulations and constituent groups having a stake in the Institute's budget. Congressionally mandated programs alone take considerable time and resources. The staff have enormous responsibility to create and direct projects that produce significant benefits; they must show progress on new initiatives and mandates from Congress without abandoning old commitments. A large part of these difficulties would be overcome by adopting and implementing a clear, strong policy for the NIE's research responsibilities.

Lack of direction is a prime cause of fractionated effort and buffeting by external forces. A clear policy, and its implementation, is needed to establish research obligations, to define sensible objectives for fundamental and applied research, to set a balance among the various programs, to protect staff from cross-pressures, and to encourage the kind of staff efforts (for example, in shepherding program announcements through the several steps required before approval) that are required for sponsoring research of high quality. Furthermore, policy is needed to reassure potential investigators that creative, high-quality proposals for fundamental research relevant to education will receive serious consideration and that excellent work will have a good chance of receiving continued support.

Many different agencies support some research in education related to their own priorities. As long as these agencies have educational missions, it is proper that they do so. The new program for research in the Science Education Directorate of NSF, for example, should stimulate advances in the understanding of such topics as problem solving and analytic thinking, long-term goals of science education (for example, public understanding of technology impact), and classroom environments that promote science education; we support this new program.

The National Institute of Education, however, can and should differentiate its role from those of other agencies--taking advantage of its responsibility to all kinds of education. NIE's programs should establish a position of leadership in research relevant to education. The Institute has a good opportunity to support high quality fundamental research related to education across the entire human life span and in its diverse settings. It can concentrate on problems that require more basic understanding and involve the interests of more than one agency. The Institute can take the lead in anticipating issues and in stimulating pioneering research in education.

Promoting better coordination of the government's research efforts in education is another task that NIE should undertake. There is currently some interagency communication, but NIE's role should be more active. The lack of coordination is more a problem of wide gaps and lack of leadership than of undue overlap or an absence of communication.

We have already identified the relative dearth of fundamental research, but one other gap needs to be emphasized--research on education outside primary and secondary schools. A significant portion of every person's education derives from experiences at home, at work, in military, industrial, and private training programs, in colleges and universities, and in the many groups with an interest in learning about specific topics, such as art, environmental problems, or the stock market. People learn (we do not

argue that what they learn is always good) from their families, teachers, peers, colleagues, and bosses and from television, newspapers, books, magazines, museums, art galleries, and concerts. A few agencies cover education in a few of these areas and in limited age-ranges, but no agency takes a general approach to research in all aspects of education. It is our belief that the function, working, and impact of schools will be understood better if education in its broad context is studied. Moreover, a general orientation could result in more knowledge about society's diverse sources of education. The NIE should stimulate this work by keeping track of what is being done and taking the lead through its own research support.

The NIE programs for research should also ensure that the work sponsored is of the highest possible quality. Quality must be measured not simply in terms of research design but in terms of the scientific significance of the research and its potential for shaping and illuminating important questions. We have provided some examples of fundamental research that the NIE might support, and NIE itself has sponsored several task forces to identify topics of significance, but the Institute should have some permanent means for obtaining the advice of the scientific research community on its overall research directions and quality. The Institute seems now to have good relationships with the public education community and consults with some well-qualified scientists. These lines of communication should be formalized and expanded by creating one or more research advisory groups who report to the Director and the program managers and who are in communication with peer review panels. Creation of research advisory groups would go a long way toward removing the excessive pressure on NIE staff to devise and control the direction of research programs. Distinguished basic scientists and scholars working with citizens and educators would help to formulate the research directions, appraise the general quality of work, and identify important educational problems amenable to scientific inquiry. Such a mixed character would encourage a better balance of relevance and scientific quality and would broaden NIE's horizons well beyond the immediate crises of public schools. The advisory group would have another function, too: to provide high-level exchanges of information among policy makers, educators, and scientists.

If the National Institute of Education were to take the steps we have recommended here--reallocating support to fundamental research, implementing formal procedures for support of self-initiated projects subject to peer review, developing means for continuing support of high quality and pioneering research, aiming research programs at understanding education in its broadest sense, and creating active research advisory groups--the Institute would move significantly, we think, toward the fulfillment of its mandate to improve the scientific foundation of education.

CONCLUSIONS AND RECOMMENDATIONS OF THE COMMITTEE

In this report, the Committee on Fundamental Research Relevant to Education has set forth its views of the contribution that fundamental research can and has made to education. That is, fundamental research has had its major and most useful impact on education through the gradual, public diffusion of new ideas and concepts that have been assimilated into the expectations, practices, and resources of education. These have influenced practitioners' views of reality, their vision of the achievable, their know-how, and their commitment to act (Chapter 2). We have described briefly by example the kinds and variety of fundamental inquiry that we believe may make such a contribution in the future (Chapter 3). We have noted that federal policy in practice does not emphasize fundamental research (Chapter 4). Our recommendations are made with the hope that the federal government will reorient operating policy in education toward fundamental research on how people learn and mature, their diverse sources and settings for learning, and the function and value of what they learn, as well as toward improving the quality of all efforts to improve or alleviate problems in education.

A Reemphasis on Fundamental Research

1. Federal policy to build the scientific foundation of education through fundamental research is established in law, precedent, and concept. Nevertheless, basic research on the processes of education is today assigned very low priority in federal agencies charged with the management of educational research and development. In federal agencies, generally, basic research receives about 11 or 12 percent of all funds for R&D; in education, basic research is allocated only 4 percent. In the two agencies primarily concerned with public education in this country--the Office of Education and the National Institute of Education--basic research receives less than 2 percent of the research and development monies. *We recommend an increase in the proportion of the federal investment in education research and development designated for fundamental research* (p. 66).

Improving Scientific Quality of Research and Development

2. Government agencies have swung toward premature attempts to provide quick solutions to educational problems, many of which are not well understood. It is our conclusion that without the guidance of understanding, these practices regularly lead to projects that are of neither practical nor scientific value. *We recommend a change in policy toward more careful assessment of what is known and what must be learned when solution-oriented programs are undertaken* (p. 66).

3. Agencies concerned with educational research are properly concerned with setting research priorities and objectives. But too often the felt significance of an educational problem has been the overwhelming factor in allocating research effort, with insufficient regard for the scientific feasibility of the proposed research. *We recommend that more active investigators be included in the planning and program review of all basic and applied research efforts in education* (pp. 67, 68).

Better Management of Fundamental Research

4. Management practices that have proved appropriate for developing new curricula and moving technical advances into the educational system have not been particularly appropriate for strengthening basic scientific research. *We recommend more extensive use of field-initiated and peer-reviewed systems of research funding* (pp. 69, 70).

5. For some of their programs the National Institute of Education and other agencies use a single review panel designed to serve different objectives, such as to improve scientific understanding, to encourage materials development, and to devise applications. This practice leads to overload, watered-down concentrations of competence, and a tendency for the more applied and immediate problems to preempt totally the resources available. *We recommend that within each major program (such as Basic Skills in the National Institute of Education or the Office for Handicapped in the Office of Education), separate budgets and review panels be established for field-initiated research. Review panels should be staffed predominantly by currently active basic researchers, with appropriate representation of those more oriented to development and application* (pp. 70, 71, 76).

A More Active Role for the National Institute of Education

6. The National Institute of Education has not made significant progress toward fulfilling its mandate to strengthen the scientific and technological foundations of education. *We recommend that the National Institute of Education take immediate steps to implement a policy of strong support for fundamental research relevant to education* (p. 75).

7. The National Institute of Education should offer leadership in fundamental and applied research relevant to education. *We recommend that*

the Institute redefine its role and implement policies to attract and maintain research of high quality in the field of education, to provide long-term support for work on important problems of education that affect broad sectors of society, and to encourage pioneering applied and fundamental research (p. 75).

8. **The National Institute of Education now limits itself almost exclusively to education in public schools.** *We recommend that its mission be broadened to include sponsorship of fundamental research on learning throughout life and in the many settings in which education occurs* (pp. 75, 76).

9. **The staff of the National Institute of Education must be well informed about research.** *We recommend that the National Institute of Education adopt personnel policies that will facilitate the staff's knowledge of research and of programs for research* (p. 71, 72).

National Science Foundation Participation

10. **The Science Education Directorate of the National Science Foundation is now planning its first deliberate program of support for research on science education.** *The National Science Foundation should establish a strong program of support for fundamental research related to science education* (p. 75).

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