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TO THE

COMMITTEE ON EDUCATION AND LABOR

SUBCOMMITTEE ON SELECT EDUCATION

U.S. HOUSE OF REPRESENTATIVES

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Mr. Chairman and members of the Subcommittee on Select Education:

My name is Albert Shanker, and I am president of the 665,000 members of the American Federation of Teachers. I greatly appreciate the opportunity to testify before this subcommittee on the vital issue of the federal role in sponsoring educational research and development.

Over the years, the AFT has appeared before Congress and in other forums both as a supporter and a critic of the federal role in education research. We have, however, consistently argued for much greater federal support of education research.

No other level of government has done and can as effectively do the job of collecting and publishing statistics and other longitudinal data, and stimulating and supporting basic and applied research, development, and dissemination on issues and problems of national concern in education. No other level of government has the resources, capacity, overview -- in fact, the obligation -- to concern itself with the national interest in education.

It is I hope incontrovertible that an educated citizenry is crucial to our national well-being -- not only from an economic perspective but from a political one: democracy, particularly in a diverse and pluralistic society, rests on a well-educated citizenry who are capable of participation in our government and our society, who are able to pursue and protect the blessings of liberty. If education, then, is critical for our national well-being, and if this nation is at risk because of the neglect and shortcomings of our education system, then it is vital to affirm and expand federal support for education research. From a strictly practical point of view, surely there is no other way to monitor, assess and make more effective this nation's investment in education. But currently we spend over $300 billion annually on education and only $80 million on education research. Is there any other enterprise, public or private, that spends so little on understanding the nature, needs, strengths and weaknesses of its own investments? With such a paltry sum devoted to research, is it any wonder that it took so long to "discover" that the nation was at risk?

The AFT has a number of criticisms of the federal role in education research. But first and foremost we believe that research is one of the best hopes we have of understanding the nature and process of learning and teaching and the policy and organizational structures that support or impede these activities. We believe that in an enterprise such as education, which is often fraught with conflicting values, opinions and politics, research is the best hope we have of distinguishing between fads and facts, prejudices and informed judgments, habits and insights. Without systematic inquiry, development, and testing, we will continue to have the same babble of arguments and practices concerning what works or ought to work. Without good research, we will continue on an endless cycle of mistakes and the loss of successful insights and discoveries. Without good research, there will continue to be an endless reinvention of mousetraps, the same rehashing of controversies, and, in the end, the same faltering school system. No enterprise has changed as little as education or has as endlessly reinvented past solutions and mistakes. And centrally implicated in this rut is the conduct of educational research. Research cannot and should not displace value and moral decisions; it will certainly not replace the political process in education. It can, however, help ensure that moral and political choices are informed and that our children's education is not the playpen of our idiosyncrasies.

Unfortunately, and far more than any other sector I am familiar with in which there is federal support of research, the federal role in education research has often been idiosyncratic; it has at the very least been unstable.

Take the question of whether or not the federal education research agenda reflects America's key educational priorities. The answer is, sometimes it does and sometimes it doesn't because that agenda changes with every Administration and, sometimes, even within the course of an Administration. This would not be a problem if our educational priorities and problems were equally mercurial. However, many of our educational problems, and most of our priorities, are enduring ones: providing equal access to knowledge, equal educational opportunities, to the nation's young, and ensuring a well-educated citizenry capable of participating in this nation's political, economic, and social life. Out of this enduring set of priorities, reasonable people can agree on an enduring research agenda, flexible enough to detect and attend to new problems but stable enough to ensure ongoing attention to core sets of issues. That is the case from agriculture to public health.

In education, however, every few years we have a new set of educational priorities -- usually announced by some catchy slogan -- and a new redirection of research funds. Years of research on public school finance may suddenly grind to a halt because suddenly private schools are in. Years of work on research and development in curriculum areas may be shelved because suddenly it seems inappropriate for the Federal Government to be involved in inquiry into curriculum content and curriculum development. Just as suddenly, after years of neglect of curriculum content, subject matter becomes hot again and there is a scramble to set up research centers to rebuild this field. And after years of neglect of basic education statistics, such as the supply of and demand for qualified teachers or the courses our students were taking or a uniform definition and reporting of the dropout rate, we are frantically designing data collection efforts to understand problems and issues whose consequences we are already living with. The renewed support for basic data gathering is very welcome, and we supported it. But the neglect of basic research in education, long a problem, is now virtually complete. One would be hard-pressed to find a thick portfolio of federally-sponsored research, built up over time, on how children learn and develop or on how schools are organized and how that may promote or impede learning -- and yet these are about as basic a set of questions as you can get.

This peripatetic and politicized dance of priorities in educational research is not exclusive to the present Administration of the Department of Education and OERI, though it may be more extreme. It has been a problem at least since the creation of NIE, the forerunner of OERI, and has persisted despite various reorganizations. A number of reasons are apparent for this condition. First, there has been little or no systematic solicitation of the advice of education groups and, particularly, practitioners about what issues, priorities and problems they see in schools and students, let alone their notions about what the research agenda should be. This omission is tantamount to a public health agency failing to keep in touch with medical practitioners (and vice versa) about the pattern of cases they are seeing. Although there has never been a good structure for field reporting, as it were, in the past few years the gulf between the main federal education research agency and practitioners has become enormous. One reason for this is the cutback of research dollars, which has prevented federal education research managers from going into the field, as they once did, and developing an agenda from the ground up. Another reason seems to be the special contempt and hostility of the present Administration toward public education, its constituency groups and practitioners. And another and longstanding reason is the artificial barriers that have been created and maintained between research and practice.

Another explanation for the unstable and frequently politicized education research agenda is that the money for research has been so modest and so much of it has been earmarked for labs and centers, that there's very little left to support other people, very few opportunities or incentives to stick to an issue (even in labs and centers), and even fewer opportunities or incentives for non-lab and -center researchers and new scholars to enter the field. I am not inherently opposed to labs and centers. I am more familiar with the work of the centers than the labs and have found much of their research enormously valuable. But the result -- again, not of the existence of labs and centers, but of the scarce overall research dollars -- is that research is concentrated in just a few institutions, among a relatively few people and their relatively few graduate students, and on a relatively fixed set of issues that may or may not be viewed as important at the next funding cycle or to the next Administration. It is very hard to focus attention on deep and difficult questions and to encourage the best minds to enter and persist with education research under this set of circumstances.

A third reason for this shifting nature of educational research priorities is that education research has been oversold and underchanged -- and in this instance I don't mean money. For whatever set of good or bad reasons, from the inception of NIE to the present moment of OERI the promises made and expectations raised about the power of education research to quickly improve practice and cure the ills of American education have been wildly inflated. No other field has promised as much (and received so little to pursue those promises). Every field instead says, "We have a set of difficult problems, and it will take a long time to solve them. We have some hunches, but there will be many false starts and blind alleys before we find a promising avenue. It will take the work of many researchers and practitioners, all learning from one another's mistakes and leads, and much testing -- a sustained effort. We do not ask that you leave us entirely alone -- after all, these are public funds. But much that we do will not seem immediately relevant or useful, and it will take a long time, if ever, before a 'magic bullet' is found, especially in a complex human endeavor. We will do our best." If the federal education research agencies have ever said anything like this to Congress, I would be surprised. I certainly know that when education researchers say this to their funding agencies, they are denigrated as ivory tower impracticals, pretentious excuse mongerers for their own irrelevant agendas. (I have my own frustrations with many education researchers, but this is not one of them.)

Not surprisingly, Congress has consistently held education research to a higher standard than it does research in any other field. And, equally unsurprising, it has always found it deficient. There is, after all, no "penicillin" in education, no manual of universal cures, no accounting of how many IQ points or SAT scores have been raised as a result of which piece of research. There has been no "quick fix" -- and the few that have been advertised that way by the government and imposed on schools have been very destructive. There has, however, been an impressive accumulation of relevant, useful and, sometimes, powerful research. And all too frequently, the pursuit, refinement, and testing of this research has been ground to a halt because of the intolerance for anything less than a magic bullet. In any other field, for example, conflicting results or ambiguity signals a new point of departure, a redoubling of efforts. In education, it frequently spells the end of support for a line of research, a budget cut -- or, at the very least, an occasion for ridicule.

The federal agencies that support medical research did not create the expectation that a cure for cancer or heart disease would be quickly found, despite the far more generous funds invested in these endeavors. Nor were researchers' feet held to the fire of immediate relevance, utility, and payoff by these funding agencies. Congress has certainly not punished these agencies for not yet succeeding in entirely solving these problems, and the minute and painstaking pieces of research sponsored by these agencies have not been held up to ridicule and branded as useless. A long-term view has not only been tolerated, it has been encouraged. A finding of "it depends," or "it only works for 25% of the population and under these circumstances" is regarded as a breakthrough. Isn't it time to apply this paradigm to education research?

Isn't it also time to encourage research and development on new paradigms of education? I am constantly struck by how the priorities of federal education agencies, and, therefore, the research they have sponsored, has taken the status quo of education -- its organization, structure, assumptions about learning and teaching and the like -- as givens, albeit to be improved, but givens. I know of no other fields save education whose structure, technology and basic ways of operating (and problems) have remained unchanged for over 150 years. There are many reasons for this, but a few stand out. One is the almost total absence of support for research by practitioners on what they practice. Another, and related reason, is the virtual halt in experimentation with and development of new paradigms and practices. I realize the difficulties of this in education, but other fields face the same ethical and other considerations in working with human subjects but nonetheless manage successfully. Certainly in education there seems to be no reluctance to impose untested "innovations" on teachers and students -- and often with disastrous results. There is therefore very little excuse for the failure to engage in development, demonstration, testing of new models in education, especially when participation is voluntary. Finally, and once again, the structure of the federal role in education research, at least as it has been discharged in its chief education research agency, NIE and now OERI, is such that it is exceedingly difficult to gain support for that which is not immediately "relevant" or seemingly useful or outside the realm of some short-term priority or traditional paradigm.

So what to do? The history of our federal education research effort as incarnated by NIE and OERI has been a short, troubled, and turbulent one. It has been marked by a surfeit of politics, short-term thinking, a declining budget and declining confidence, and much demoralization. There is tragedy in that, not only because the promise was so great but because so much good work has indeed been produced. Nor has there been a lack of dissemination, though sometimes poor work is disseminated while good work is withheld. Research has sometimes even found its way into practice, though here again I wish some of it had not and other findings had. And if much of practice has not been influenced by the best of research, the fault is neither the research nor the dissemination efforts of the researchers or of the sponsoring agencies; the reason is the fact that our school system is depressingly anti-intellectual and is not organized to consider, debate, and use research (save frequently of the worst sort that tends to get imposed top-down). The federal education research establishment did not create that situation, but neither has it done much to help understand or change it. Indeed, it has sometimes exhibited the same anti-intellectualism.

Given this short, tumultuous and under-funded history of NIE/OERI, and given my belief that the expectations and assumptions about education research that have been ingrained into Congress by NIE/OERI -- and which are part of the problem of the federal role in education research -- would be hard to change, I think that a new beginning may be warranted. I am not talking about yet another reorganization of NIE or OERI, for despite a number of reorganizations, the basic problems have persisted and some have been exacerbated (though good progress has been made in the statistics area under the capable and experienced leadership of one of education's finest civil servants). What I am instead proposing is a commission to study the persistent problems of the federal role in education research, and the reasons for them; the range of education-related research in other agencies, such as the Defense Department and the NSF, and what can be learned from how these enterprises are organized and work; the structure and conduct of other federal research agencies that are involved with large-scale issues, such as agriculture and public health; and what all of these tell us about how to organize the federal role in education research and the agency or agencies that discharge that responsibility, and how to ensure an adequate supply of top-flight people, including practitioners, working in a sustained way on enduring issues in education.

It may be that the current wheels will be reinvented, but I suspect not. For we have been bumping along now for quite some time, leveling the same charges and counter-charges, veering off in new directions sometimes even before there is a turn in the road and abandoning old directions sometimes just as the scenery gets greener. I strongly believe that the structure of our schools, and the assumptions and traditional paradigms that undergird it, is strongly implicated in the persistent problems our public education system, its students and teachers, face. I am far less expert on the structure of research or of the federal role in education research, but my strong hunch is that the way we have structured that role is implicated in the disappointments we have had, both warranted and not, in the conduct and results of education research.

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