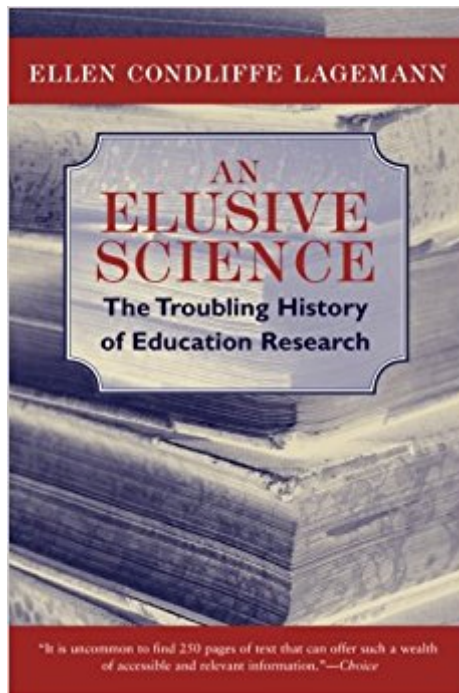




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An Elusive Science: The Troubling History Of Education Research



Synopsis

Since its beginnings at the start of the 20th century, educational scholarship has been a marginal field, criticized by public policy makers and relegated to the fringes of academe. *An Elusive Science* explains why, providing a critical history of the traditions, conflicts, and institutions that have shaped the study of education over the past century. "[C]andid and incisive. . . . A stark yet enlightening look at American education." —*Library Journal* "An account of the search, over the past hundred or so years, to try and discover how educational research might provide reliable prescriptions for the improvement of education. Through extensive use of contemporary reference material, [Lagemann] shows that the search for ways of producing high-quality research has been, in effect, a search for secure disciplinary foundations." —*Dylan William, Times Higher Education Supplement*

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Customer Reviews

Lagemann, who is chair of humanities and the social sciences at New York University and president of the National Academy of Education, has a long history in the field of higher education. In this candid and incisive study, she examines how Americans perceive the people who practice education and explains why their low-status work has undermined the possibilities for developing a strong professional community and a generative scholarly tradition. Weak financial support led to weak research, which in turn led to continued weak financial support. This catch-22 situation has also contributed to the lack of public support and respect. Taking a historical perspective, Lagemann critically examines problems associated with educational scholarship and argues that

federal requirements for program evaluation have resulted in an improved understanding of the education policymaking process. She considers not only where education research may have gone astray but also the promising directions it may be taking in the future. A stark yet enlightening look at American education, research methodologies, and federal government funding agencies and their practices, this book is recommended for academic and larger public libraries. DS Samuel T. Huang, Northern Illinois Univ., DeKalb Copyright 2000 Reed Business Information, Inc. --This text refers to an out of print or unavailable edition of this title.

"[C]andid and incisive.... A stark yet enlightening look at American education." - Library Journal;
"[A]n account of the search, over the past hundred or so years, to try and discover how educational research might provide reliable prescriptions for the improvement of education. Through extensive use of contemporary reference material, [Lagemann] shows that the search for ways of producing high-quality research has been, in effect, a search for secure disciplinary foundations." - Dylan William, Times Higher Education Supplement

Might be one of the best ed books I have ever read. She has truly done her hw, and the amazing quality and quantity of useful info is the end result of her efforts. Bravo!

Ordered this for class. Very interesting book!

This is a must read for anyone that is thinking about going into or already is involved in US education research. Book gives one a clear idea of why education is the way it is. A MUST Read!!!!

great

In crucial ways this is a disappointing book. As a history of education research it devotes a great deal of descriptive attention to substantive and philosophical issues and to the personalities of early researchers, but very little to research itself. I agree that an artificial separation of the substance and philosophy of research from research as it's actually done would give a grossly distorted view of the real nature of education research. However, focusing almost exclusively on motives, disappointments, intra-disciplinary triumphs, persistently low status in the university hierarchy, and identification of funding sources of education research also seems misleading. It's all well and good to discuss testing and school surveys as research-based activities that dominated the late 19th and

early 20th Centuries, while also acknowledging that alternatives, such as those proposed by John Dewey, received much less attention. However, I was hoping to read about some of the technical issues involved in these research-based activities. After all, psychometrics has developed in ways never dreamed of by Hall, Thorndike, or Terman. A good example is provided by the technical expertise required by Rasch modeling, something that has been with us for more than forty years. The author, however, leaves us pretty much in the dark as to how much early advocates of testing knew about even classical testing theory, such as that presented in Nunnally and Bernstein's widely used text. No, I don't expect Lagemann to offer a mini-course in measurement, but a brief discussion of the technical adequacy of early tests would be helpful. As it is, we don't know how much these perhaps misguided folks who set the stage for contemporary over-emphasis on standardized test-based research and evaluation really knew about what they were doing. Similarly, just how was school surveying done? Anyone who has studied survey research, and especially the statistical methods that make for accurate and efficient sampling, knows that properly executed school surveys require a substantial amount of technical knowledge. How much of this was available to early advocates of school surveys is not something Lagemann discusses or even acknowledges as pertinent. Yes, some of them studied and even took degrees in statistics. But what did that mean ninety years ago? And what about the survey data so laboriously collected from so many different locations. Where is it? Lost in history? In a giant archive? What might an historian such as Lagemann learn from looking at some of it? And how were truly massive amounts of survey data analyzed before the advent of high speed digital computers and user-friendly software? The fact that data analysis was an extremely laborious process in the early days of education research, before the introduction of computers, is a pertinent topic that seems not to have occurred to Lagemann. *An Elusive Science* covers more than a century of developments in education research. At the end we are left with the impression that other methods have come to challenge testing and surveys for preeminent status in education research. As one who taught education research at a mid-sized state university for twenty three years, I can confidently attest to the fact that this is not the case. Education undergraduates at the university where I worked were required to take only one course related to education research: Tests and Measurement. The scope was broadened at the graduate level, but not in a way that Lagemann would find encouraging. Yes, there has been a change in the status of testing as a tool for research and evaluation: its status has been dramatically elevated! This is especially manifest in the accountability measures introduced by George W. Bush and Barack Obama under the innocuous and hopeful labels of No Child Left Behind and Race to the Top. Today, American public schools do nothing so well as teach students to take standardized

tests, thereby de-skilling teaching and rendering the content of what is taught increasingly superficial. Writing a book that was published in 2000, before the Bush and Obama accountability-based reforms were introduced, may help to explain why Lagemann failed to see the massive increase in the frequency of testing and the exaggerated emphasis attached to test results. Nevertheless, an exploding emphasis on testing, far exceeding anything that had existed in the past, was first manifest in the late 1970's. When high school graduates no longer could be reasonably confident that they could step out of school and into a job that provided the material rudiments of a middle class life style, schools became suspect, and the standardized test-based accountability movement was well under way. Surveys, too, remain the primary source of data for the enormous amount of non-experimental quantitative research that is done in education every year. The 1980's, for example, were dominated by use of school survey data to approximate interpretable comparisons between public and private schools. Which type of school is more effective? Which type is more equitable? Simple questions, really, but after hundreds of articles, conference presentations, and research monographs purporting to answer these questions, there is no consensus, except the commonest of commonsense judgment that everyone knows that private schools are better. Due in part to computationally intensive technical improvements in the statistical analysis of survey data (see, for example, structural equation modeling and random coefficient modeling), surveys have lost none of their appeal for the quantitatively inclined. Moreover, since surveys that include aptitude and achievement tests have become commonplace, increased concern with testing has actually made surveys of schooling even more popular than in decades past. Too often, as with the obsessive concern with curriculum that characterized the 1920's and 1930's, Lagemann simply fails to make a connection between the issue at hand and research. If curricular issues were subjected to more than speculation and debate, the author does not make that clear. How one went about doing research on curriculum is a question that Lagemann does not address. She does acknowledge, however, that curriculum was an area in which the lines between research and practice, investigation and imposition of a favored curriculum were badly blurred. In fact, as best I can tell from her account, nothing that might be legitimately characterized as research occurred. It is difficult to see, thus, why this material was included in a book purporting to be concerned with the history of education research. Perhaps Lagemann wanted to make the point that with or without research public education was contested terrain. If that is her meaning, I'm sure she's right. Toward the end of her account, Lagemann gives voice to being encouraged by recent developments in cognitive science, holding, in effect, that a better understanding of how the central nervous system works will enable us to better educate our children. As a complement to progress in

cognitive science, she lauds increased interest in ethnographic methods as providing a means of situating education in its social context and learning how contextual factors impinge on what goes on inside schools and even inside children's brains. To the best of my limited knowledge, however, cognitive science has not made it into the curriculum of prospective teachers, administrators, or even school counselors. Piaget, Vygotsky, and Skinner are dutifully introduced in diluted textbook form in educational psychology classes, but that's about as close as students get to cognitive science, a very long stretch indeed. I agree that ethnographic research has been invaluable in understanding schooling and its social context. I revere skilled ethnographers such as Ray Rist, Paul Willis, and Jean Anyon, and I have learned a great deal from their work, more than I've learned from even the best quantitative studies. If you're looking for a federal grant to fund ethnographic research, however, you may be in for a long search. Even conventional, time-honored multiple regression-based proposals no longer have a good chance of getting funded. Not rigorous enough, or so say the feds. Random assignment or regression discontinuity designs have taken pride of place for claims on federal money for research and evaluation regarding educational practices and innovations. This places extremely narrow constraints on the kinds of issues that can be addressed. Moreover, it represents a triumph for the most textbookish and stilted kinds of quantification, marking a return to the mindset of the early decades of the twentieth century. I think that there is a good case to be made for the notion that, whatever was going on under the rubric of education research, U.S. schools were pretty good during the Post World War II era, roughly 1946 to 1972. Arthur Bestor may have thought that public schools' offerings were intellectually thin, but, as noted above, during this time of national prosperity a high school diploma paid off, and a college degree was a sure bet. The fact that this is no longer the case has nothing to do with schools and everything to do with the global economic context. Even the most thoroughly research-based schooling can't overcome the costs to working people of internationalization, down-sizing, out-sourcing, and labor-saving technological developments. The organization of the economy, however, is sacred, so under the guise of research, we tinker with the most simple-mindedly obvious institution, public schooling. These insights are best acquired through once-high profile historical research in education, such as that provided by Michael Katz in *The Irony of Early School Reform*, Bowles and Gintis in *Schooling in Capitalist America*, and Jean Anyon in *Ghetto Schooling*. Lagemann acknowledges that historical research in education was once an influential force in American education research, and she also acknowledges the contribution of more recent historical research done from a critical perspective. I think, however, that she fails to see its potential contribution to understanding why education and education research are persistently under the

gun. Education research that fails to focus on macro-level contextual factors will yield little of value. In the mean time, with relentless research-engendered tinkering and our obsession with accountability, we render schools less effective than they were. No wonder educational innovation has proved to be such a disappointing enterprise. No wonder researchers and evaluators can't find payoffs for educational reform. Perhaps education research has remained a low-status irrelevancy because, all along, it has been asking the wrong questions. Lagemann hints at this when she refers to studies of the social ecology of education, but our focus has to be broader. Schools are not powerful agencies of social reform, but they can do fine things given a suitable context, one in which investments in education pay off. For now, such payoffs are more insubstantial and less certain every day, and education research, as with education generally, is a convenient scapegoat for our inability to address the economic costs of globalization.

"An Elusive Science" is a book that chronicles (with minimal editorial) about 100 years of education's attempt to become a science. The book starts off in the late 1800's, when, thanks to William James, psychology was just becoming a serious discipline, and the business of education was largely seen as a trade. Soon, "normal schools" were founded with the purpose of teaching educators, which gave rise to university departments, which gave rise to the idea of education as a research discipline. The author's main theme - if there is a theme to this historical chronicle - is that psychologist Edward Thorndike won the war of "visions" of what education research would look like, against his colleague, philosopher John Dewey. Following Thorndike, education research became very quantitative, behavioristic, and attached to developing systems. Curriculum development specialists wrote various methods of teaching in an effort to "objectify" a very personalized discipline. (Education, thus, was trying to become mechanized in the same way of Frederick Taylor's "scientific management" of the late 1800's.) The author follows this history up to the 1980's, where the tide slowly, but not completely, turned in favor of more qualitative approaches to education. The biggest problem I have with this book is that the author did not make a greatly persuasive case that these results and this history was troubling, as suggested in her subtitle. Yes, education research became almost obsessive about quantization, systemization, and ranking things in hierarchy. The body of the book is only devoted to the idea THAT this happened, not WHY it was a "troubling" development. (She does this only in the introduction and epilogue.) This book is, however, a very interesting history that finds much historical overlap with *Left Back: A Century of Battles over School Reform*, and philosophical kinship with *The Trouble with Ed Schools*. As another reviewer astutely points out, this book reminds us that the current field of education as a

university research discipline was nowhere a necessary state of affairs, but rather, it evolved slowly and painfully. This book will certainly be of interest to scholars interested in learning about the history of this development.

As I'm starting a PhD in Education Research, I was interested in a book that would give me the historical background of the debates and history of ideas in education and education research. While this book certainly gave me a great historical background, it did not go into much detail in the ideas of each of the people involved. For example, while the book did give a great account of Dewey's academic career, it didn't go into much detail on his ideas and contributions in the field. While the history of these thinkers was highlighted well in this book, their ideas were not given as much attention. I would recommend this book ONLY to people who already have a good handle on the ideas each of the great thinkers in education have contributed. If you know what Dewey, Judd, Thorndike, etc have contributed, this will be a great book to learn more about their academic histories and how they related, rather than an introduction to their contributions.

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