

LOOKING TOWARD THE 21ST CENTURY: CHALLENGES OF EDUCATIONAL THEORY AND PRACTICE

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INTRODUCTION

While it may be a truism it is nonetheless true that much of what we do, individually and collectively, is shaped by our personal histories. For that reason I begin this paper by describing some aspects of my background. Doing so provides a context for what follows, and for understanding my hopes and aspirations for our profession and our society.

I was born about mid-century in Brooklyn, New York. My parents didn't have much money. What they did have, along with many others at that time, was the absolutely firm commitment to insuring that their children should have better lives than they did – and the equally firm belief that education was the passport to those better lives. When I was growing up, education was assumed to be a gateway to opportunity. More importantly, there was a widespread belief that society had a moral obligation to provide a high quality education to all children.

I am a beneficiary of that belief. From kindergarten through 10th grade I attended public elementary, junior, and senior high schools in New York City. When my family moved just outside the city limits, I attended a local public high school. I attended Queens College of the City University of New York as an undergraduate, paying the "non-resident" fees of \$232 per semester; had I lived within city limits, the fee per semester would have been \$32. I was provided an astoundingly high quality education at no cost from kindergarten through high school, and at negligible cost in college.

To this day I am grateful. And to this day I believe that our society has a moral obligation to provide every single child the kinds of opportunities that I was lucky enough to have.

Another relevant part of my background is that I began my professional career as a mathematician. Let me describe two aspects of mathematical culture that also shape what you are about to read.

The first is a tradition of identifying important problems. In 1900, David Hilbert delivered a keynote lecture entitled "Mathematical Problems" at the International Congress of Mathematicians. Hilbert identified a number of problems of deep theoretical interest whose solutions he believed would advance the mathematical enterprise. Over the century that followed, mathematicians took up the challenge. Many of those problems have since been solved, and their solutions have truly advanced the field.

All mathematicians – even those who now work in education – are mindful of that tradition, which becomes increasingly salient as we near the threshold of the next century. There is a great temptation to ask, "How might one characterize the major problems that our field needs to confront, and on which we can make progress, over the century to come?" Of course, problems in education are very different from problems in mathematics. It might be better to pose the issue as follows: "How might one characterize fundamentally important educational arenas for investigation, in which theoretical and practical progress can be made over the century to come?" This paper attempts to address that issue.

The second mathematical tradition I need to discuss puts it at a great distance from education. Educationists care about the real world and its problems, and those problems tend to be messy. Problems in education resist the clean formulation of mathematical problems, and educators resist the abstraction of problems away from their contexts of meaning. It's different in mathematics. Let me offer a mathematical quotation to highlight the contrast. The quotation comes from a famous book by G. H. Hardy entitled *A Mathematician's Apology*. Hardy uses "apology" in the sense of its Greek root, *apologia*, meaning "defense." His defense of pure mathematics is neither defensive nor apologetic:

"I have never done anything 'useful.' No discovery of mine has made, or is likely to make, directly or indirectly, for good or ill, the least difference to the amenity of the world. . . Judged by all practical standards, the value of my mathematical life is nil . . .

"The case for my life, then, . . . is this: that I have added something to knowledge, and helped others to add more; and that these somethings have a value which differs in degree only, and not in kind, from that of the creations of the great mathematicians, or any of the other artists, great or small, who have left some kind of memorial behind them." (p. 92)

I note with more than a touch of irony that pure and "useless" mathematics such as Boolean algebra provided the mathematical basis for the computer technology on which I prepared this talk – and more seriously, that equally pure mathematics provided the underpinnings of the scientific technologies such as magnetic resonance imaging that have saved my daughter's life. Be that as it may: In education, the kind of purist attitude epitomized by Hardy just won't do.

Fortunately, it doesn't have to. The two main points I wish to make are as follows:

It is possible to conceptualize educational (and other) research in such a way that "pure" and "applied" work are not in conflict, but so that contributions to basic knowledge and contributions to practice can be seen as compatible and potentially synergistic dimensions of our work;

Educational research has evolved to the point where it is possible, much of the time, to conduct research in contexts that are of practical import, working on problems whose solutions help make things better *and* contribute to theoretical understanding. Finding and working on such problems is a high-leverage strategy for making a difference in the years to come.

As indicated above, the main purpose of this paper is to identify a series of arenas for investigation – arenas in which significant progress needs to be made. I shall discuss both theoretical and pragmatic matters, but in close conjunction: when considering theoretical issues I will highlight practical concerns, and vice versa. What follows will, in a way, echo the sequence of my career, beginning with the abstract and then ascending to the concrete.

This paper has three main sections:

- Arenas for theoretical development (with an eye toward practice)
- Theory and applications in synergy
- Arenas for practical development (with an eye toward theory)

In the first main section I identify a half dozen theoretical issues on which we, as a field, need to make progress. Although my main focus is theoretical, for each of those issues I suggest ways in which basic work can have profound practical import. In the second main section I outline a conceptual framework intended to substantiate my assertion that theory and practice can thrive in synergy. In that section I give one extended example of productive theory-practice interactions, as a case in point. In the final section I identify some arenas in which there are critically important practical problems – problems that can be framed and studied in ways that contribute to fundamental understandings.

ARENAS FOR THEORETICAL DEVELOPMENT (WITH AN EYE TOWARD PRACTICE)

We start with theory. In Hilbertian terms the question is, what are some basic arenas in which we need to make progress? In a sense, much of what follows is not new: we are dealing today with problems that educators and philosophers have grappled with for centuries. But there have been significant advances in recent years, and it may be possible to frame some of these issues so that substantial progress is possible over the decades to come.

The six arenas discussed are: unifying the cognitive and the social, learning, the brain, transfer, reconceptualizing the discussion of "nature versus nurture," and social systems. Because of space constraints I can only provide scant detail in my discussion of the first two arenas, and even less detail in my discussion of the rest.

- **Unifying the Cognitive and the Social: Thinking, Acting, Being**

The central question here is: Is it possible to build robust theories of how we think and act in the world – theories that provide rigorous and detailed characterizations of "how the mind works," in context? To get a sense of the level of characterization I intend, one might think by way of analogy about the study of physiology as a means of addressing questions of "how the body works." Can we describe the workings of the mind-in-context in ways that are as careful and rigorous as the ways we have begun to develop for describing the workings of the body?

Over the past few decades there have been extraordinary advances in the study of what people know and how that interacts with what they do. These came about, in large measure, because of a new interdisciplinarity. By the 1970's fields such as anthropology, artificial intelligence, education, linguistics, neurophysiology, philosophy, and psychology – to mention a few – had all shed light on important aspects of human thought and behavior. Yet, each was far too narrow, exploring only a small part of the totality of thinking-and-acting-in-context. I am reminded of the parable of the blind men and the elephant: each of a number of fields, with its disciplinary blinders, was capable of revealing at best fragmentary and partial truths regarding the ways we act in the world. In the latter part of the 20th century, the cognitive sciences drew from these disciplines, transcending many disciplinary boundaries in the process. The result was the coalescence of an interdisciplinary enterprise, which addressed with some success a range of issues for which its constituent disciplines had proven inadequate. Progress due to that synthetic enterprise has been extraordinary.

Nonetheless, we are still a long way from having an integrated theoretical perspective that provides an adequate unified view of the ways we think and act. In fact, we may today be facing a situation not unlike that of the 1970's. There is still, in large measure, a schism between "fundamentally cognitive" and "fundamentally social" studies of human thought and action. Those who work in (what might loosely be called) the cognitive tradition have made significant progress in describing some aspects of knowledge and behavior in rigorous ways. Broadly speaking, however, the cognitive community has failed to make substantial progress on issues of self and identity, of social interactions, of what it means to be a member of a community – and of how all of that relates to who we are, what we perceive, and what we do. In almost complementary fashion, those who work in what might be called the social tradition have made significant progress on some of the issues I have just mentioned – but using a set of methods and perspectives that tend to be at a very different grain size and that seem at times almost incommensurate with those of their cognitive colleagues. (At times, a grandeur of scope and vision are accompanied by a lack of fine-grained detail; see "learning" below.) Moreover, in the separation between the social and the cognitive, some fundamentally important issues such as affect and motivation have fallen between the cracks. We need to build new frameworks and perspectives that do justice to all of these. And we need new methods to inform the work done within those perspectives.

Thus far my comments have focused on theoretical issues. As noted above, I want to highlight potential applications to practice when possible. For that

reason I now point to two specific kinds of theoretical work that can serve as bridges to practice:

- Theories and models of competence in various content areas,
and
- Theories and models of acting-in-context.

The basic idea of a theory of competence is that it addresses the question, "What does it take to really be able to do X?" X might be any of a number of things, for example: "be able to read well"; "analyze current events using the perspective of an historian or sociologist"; or "be an effective mathematical problem solver" (or "a productive member of the mathematical community"). The reason for developing theories of competence should be obvious: the better you understand how something is done, the better you can help people do it. This is not an abstract point, but a very practical one. For example, evolving theories of competence in mathematics and science over the past two decades have provided the intellectual underpinnings for curricular "reform." Current reforms of instruction have been shaped by growing understandings of what it means to "think mathematically" or "think like a scientist." Instruction no longer focuses almost exclusively on the mastery of facts and procedures, but also on strategies, metacognition, beliefs, and engaging in intellectual practices central to the discipline. Similarly, some programs in reading and writing instruction have been grounded in theories of competence related to reading and writing.

Theories of acting-in-context are closely related. Such theories tend to focus on human decision-making in complex, dynamic social settings. For example, one might ask, "How does a short-order cook juggle all the varied constraints of different customers' orders, and have a wide range of meals come out of the kitchen, well-prepared, when they're supposed to?" Or, more to the educational point, "How do teachers manage to do what they do, 'on line,' in the classroom?" (see, e.g., Schoenfeld, 1998). On the one hand, work addressing such teaching issues is deeply theoretical; it calls for delineating a teacher's goals, beliefs, knowledge, and decision-making, and modeling how all these interact. On the other hand, such work will have significant practical payoffs. It will provide tools for identifying practices and knowledge that support desired kinds of teaching, as well as tools for examining various forms of professional development and their impact.

• Learning

The central question here is: Is it possible to build robust theories of learning – theories that provide rigorous and detailed characterizations of how people come to understand things, and develop increased capacities to do the things they want or need to do?

I should begin here as in the previous section, by noting that we have made great strides toward understanding the nature of learning, but that we have a long

way to go. I should also note that the very definition of learning is contested, and that assumptions that people make regarding its nature and where it takes place also vary widely. For example, the previous paragraph contains an implicit definition – coming to understand things and developing increased capacities to do what one wants or needs to do – that contrasts in a number of ways with some classical definitions. Here is the definition of "learn" from the *Oxford English Dictionary's (OED)*:

1. to acquire knowledge of (a subject) or skill in (an art, etc.) as a result of study, experience, or teaching.

Definitions are important, so let us examine this one closely. It is broad in some ways, for example in that it includes learning from experience as a possibility. That is good, for learning in school, although fundamentally important, is only one kind of learning. The classroom is, of course, a privileged locale for learning – the first place we think of with regard to intentional learning. But we cannot underestimate the role of the school yard as well – or the home, the workplace, the after-school club or the shopping mall. It is not reasonable to believe that there are different mechanisms by which people learn in these different places. A theory of learning should explain how people develop increased understanding and capacity in all of these arenas.

The *OED* definition is also narrow in some important ways. One is the limitation of the domains of learning to subjects (i.e., bodies of knowledge) or domains of performance (arts, trades, etc.). To mention just two additional and fundamentally important domains: there is learning to interact with others; there is the learning of self-awareness, reflectiveness, and other aspects of metacognition. But another far more serious limitation is the implicit epistemological stance represented by the metaphor of "acquiring knowledge." In Lakoff and Johnson's (1983) terms, this reflects the "knowledge as substance" metaphor, with the idea of learning as "adding more substance." There are many, many problems with this conceptualization, not the least of which is that it denies or at least obscures the constructive nature of the learning process. Thus, I would prefer the notion suggested above: one has learned when one has developed new understanding or capacity.

With this stipulated, we can then get to the heart of the question. How do we begin to unravel the nature of the learning process, a relationship between an individual and the environment that results in the individual having new understandings and capacities?

There are at present no good candidates for answers to this question. Moreover, a major reason for this state of affairs is the cognitive/social split discussed above. As I see it, no theory of learning is complete without a theory of mechanism – an elaboration in detail of the processes by which learning takes place. Whatever mechanism is proposed must, as noted above, apply to the broad range of ways in which learning takes place, both in and out of school. At this point, candidates for possible mechanisms from both the cognitive and social

communities are deeply flawed – and interestingly, they seem to have complementary weaknesses.

We need look no farther than recent issues of the *Educational Researcher* to see clear evidence of the schism. The lead article of the May 1996 issue of *ER*, "Situated Learning and Education," reviewed "the four central claims of situated learning with regard to education" and claimed to "cite empirical literature to show that the claims are overstated and that some of the educational implications that have been taken from these claims are misguided" (Anderson, Reder, & Simon, 1996, p. 5). This somewhat intemperate language has been matched by equally heated rhetoric from those on the social side of the fence.

In an effort to be fair, I shall try to be equally nasty to both camps. I begin with the cognitive. On the one hand, the cognitive camp's attempts to specify mechanisms for learning do have an admirable precision: see, e.g., Anderson's (1983) book, *The Architecture of Cognition*. On the other hand, they are extraordinarily narrow, and attempts to justify breadth or educational applications tend to extrapolate far beyond what is scientifically justified by the details of the work itself. To put it simply: there is focus, detail, and tunnel vision.

Work on the social side offers an interesting study in complementarity. On the one hand, there are some wonderfully general ideas about processes by which learning takes place, such as "legitimate peripheral participation" or LPP (Lave & Wenger, 1991). I think this particular notion is tremendously promising, and if properly elaborated, it has the potential to bridge the cognitive and the social. On the other hand, when one asks for details as to how LPP works, the details are not forthcoming – indeed, they are not considered to be part of the intellectual program. This avoidance of detailed explanation reminds me of the exchange in Moliere's play *Le malade imaginaire* in which a candidate for a medical degree is asked to explain how opium puts people to sleep. He responds "quia est in eo, virtus dormitiva, cujus est natura, sensus assopire" – in essence, "opium puts people to sleep because it has the dormitive property." To put it simply: no matter how nice the idea, to name it isn't to explain it.

I hope, over the decades to come, to see a synthesis that takes the best from both worlds. We need explanations of learning that have great scope, and that apply equally well to the growth of understanding and capacity not only in school and in the experimental laboratory, but on the job, at home, and anywhere else. And we need to elaborate those mechanisms with the care and precision that really explains how they work.

[N.B. I have not pursued the pragmatic implications of developing an increased understanding of learning, because they are obvious. However, I will point ahead to the discussion of "learning by apprenticeship" in the final section of this paper.]

Would that space allowed for discussions of the remaining theoretical issues at comparable (though still relatively cursory) length. At this point my discussion becomes truly telegraphic.

- **The brain**

The central question here is: How can we integrate brain research with research on human performance, over the next century? At present it is much easier to write about what we don't know than what we do know. But this will change, and we need to be prepared.

In preparing this paper I canvassed a number of colleagues about candidates for possible Hilbert problems for education and their opinions about them. Here, without attribution, is what one colleague wrote about brain research.

"Education and brain research. Educators are using what they think is brain research in all the wrong ways right now (e.g., left-brain/right brain mantras, claims that brain development is over by 3 months (or 18 months or 5 years); some of this is actually harmful. Serious scholars of brain functioning and development today mostly believe that cognitive science has more to teach brain science than vice versa. But this is going to change as the initial period of 'mapping the brain' comes to an end. Education research needs to be part of this process. We should be engaged in research that studies instructional, cognitive and brain processes in reading, in math, etc. No one can see exactly where such lines of collaborative (or at least interactive) research are going to end up, but if we're not part of the process. . . ."

I couldn't say it better. The links between brain research and applications are tenuous at this point, and the claims that are being made are usually unjustified extrapolations. Yet, there will be an emerging body of solid work in the years to come. That work will be relevant and useful if we engage in serious and thoughtful communication with brain researchers; it runs the risk of being separated from education (but applied to it nonetheless) if we do not. It is essential for us to establish a foundation for careful and thoughtful work. (And the potential is enormous. To mention just one area, think of developing a better understanding of how we process all kinds of information, such as visual and verbal, and how that can be used for purposes of developing instruction. At the fundamental level, think of issues of transfer and development, mentioned below.)

- **Transfer**

The central question here is: How do we make sense of the ways in which people use knowledge in circumstances different from the circumstances in which that knowledge was developed?

Transfer is ubiquitous. We couldn't survive if we weren't able to adapt what we know to circumstances that differ, at least in some degree, from the circumstances in which we learned it. Yet transfer is mysteriously absent from the psychological laboratory; it seems to vanish when experimenters try to pin it down. This apparent paradox vanishes when you realize that in the laboratory, researchers are typically looking for *pre-determined* transfer; the connections they hope their subjects will make have been determined in advance. That may not happen very often. But, people are making connections all the time. The issue is to figure out which ones they make, on what basis – and how and why those connections are sometimes productive. (See Lobato, forthcoming.)

I should note that members of my research group at Berkeley expressed some surprise that I listed transfer as a separate arena of study. They asked, "Isn't transfer part of learning?" It is, of course. But, the issue of transfer is so important that it deserves attention on its own.

- **Reconceptualizing the "Nature *versus* Nurture" discussion**

The central question here is: Can we reframe the discussion of "nature versus nurture" in a way that is amenable to rigorous analysis and is socially productive?

The issues in this category are complex and thorny, perhaps the most problematic I will address. Some trusted colleagues said I was best off avoiding this topic, and I thought about doing so. But it's too important to avoid.

The reason for trepidation is that there is a long history of pseudo-science and bad science being used as justifications for racism, sexism, and discriminatory social policies. In the late 1800's and early 1900s, for example, the "science" of phrenology was used to provide warrants for discrimination. According to phrenologists, the shape of one's skull provided evidence of one's likely knowledge and behavioral tendencies. Maps of the skull delineated thirty-seven different faculties, including "benevolence," "spirituality," "destructiveness" and "amativeness." Certain skull shapes indicated a tendency toward higher mental functioning, while other skull shapes indicated low intelligence and likely antisocial behavior. It should come as no surprise that the typical "Caucasian" skull was deemed of the "highest type," and that various minority and immigrant groups were discovered to have skull shapes that indicated criminal propensities (see Davies, 1955). In the latter part of the 20th century we are more sophisticated. Today we have IQ testing and complex argumentation of the type found in books like *The Bell Curve* (Herrnstein & Murray, 1996). For a discussion of many such examples, see Gould (1981).

There is, thus, reason to tread lightly. But, there is reason to address such issues, albeit carefully. At core, the issue here is development; the question is the degree to which it is malleable, under what circumstances. The honest questions we can ask, the productive questions we can ask, are:

What develops, in what ways, given what kinds of support structures? What kinds of interventions can facilitate development? And, how can we use such knowledge to good ends?

We need to make progress on this issue. Given the huge inequities in our system – inequities in the resources that students have available to them both inside and outside of our schools – we need to develop focused interventions that can facilitate students' growth and development. Please see the section "the pursuit of opportunity" for a discussion of practical contexts for investigation.

- **Social Systems**

The central question here is: Can we develop theoretical understandings and build functional models of complex social systems? Is it possible, for example, to characterize in a precise and detailed way the factors that shape what happens in a school district, in a school, in a classroom? How do we characterize "community forces"? How much, and how, do they matter? What role does school organization play? What about curriculum? What about individual agency? (See Martin, in press.) What are points of leverage on the system? When, and in what ways, are which resources likely to make a difference? And most importantly, how do all of these interact?

The complexity of this issue makes the first issue discussed above, the unification of social and cognitive perspectives, look simple in comparison. In thinking about the current set of tools we have available for dealing with this issue I am reminded of the pre-cognitive science period with regard to individual cognition, in that there exist a wide range of perspectives regarding aspects of social systems and the actors in them, none of which provides significant purchase on the relevant issues. Social and cognitive perspectives play a major role here, of course. The question of what goes on in the minds of the various actors, individually and collectively, is central to understanding such social systems. The roles of intersecting cultures and microcultures – in the community, in the school – and individual and collective decision-making, are barely understood. And there is much more.

To pick just one example, consider econometric analyses of education. These tend to be correlational: typically some "inputs" (money spent, class size or proxies for it) are seen to be related, or not related, to "outputs" such as test scores. My personal bias is that such studies explain little, if anything. In addition to the fundamental difficulty, that standardized test scores don't capture much of substance and are thus tend to be inappropriate outcome measures (see the discussion of "assessment" in a later section of this paper), there is the fact that correlations may point to relationships but don't say how they work. They can be productive pre-analytic tools, suggesting arenas for investigation. They can be useful post-analytic tools, providing confirmations of relationships. But the real analytic substance should be provided by theories and models that actually explain what's happening.

I should acknowledge that I have a long-standing bias against ungrounded correlational and factor-analytic statistical studies. Nearly twenty years ago I called for a moratorium on factor analyses of mathematical abilities until someone could actually say what perennially recurring "factors" such as "visual mathematical ability" actually meant. My bias was that theories of competence would render such statistical artifacts superfluous, and that has turned out to be the case. Likewise, though it may take a full century, I think we need to turn our attention to developing a detailed understanding of how individuals and groups interact. In the process, it will be necessary to unify a wide range of currently disjoint perspectives and methods.

Having said this, I do want to stress that, despite the complexity of this issue, it may well be possible to make significant short-term progress – in contexts that matter. See the section "making change happen" later in this paper.

This concludes my discussion of theoretical arenas for investigation. I now turn to the relationship between theory and practice.

THEORY AND APPLICATIONS IN SYNERGY

This section begins with some general comments about the relationship between pure and applied work in education. I then illustrate those comments with a case study of a productive dialectic between the two.

- **The argument in general**

In *Pasteur's Quadrant: Basic Science and Technical Innovation*, Donald Stokes discusses tensions between theory and applications in science and technology. Stokes argues that in both our folk and scientific cultures, basic and applied research are viewed as being in tension. He traces the origins of this perspective to Greek philosophers in the sixth and fifth centuries BC. After a quick historical tour, Stokes brings us to the twentieth century. A quote from C. P. Snow's essay on "the two cultures" describes how scientists at Cambridge felt about their work: "We prided ourselves that the science that we were doing could not, in any conceivable circumstances, have any practical use. The more firmly one could make the claim, the more superior one felt." This statement is amazingly reminiscent of the statement by G. H. Hardy quoted in the introduction to this paper – no surprise, perhaps, since Hardy was part of the same culture. But the statement refers to science, not mathematics. We are all aware of the many applications of science. Nonetheless, in elite circles, "pure" science was considered far superior to its applications.

This perspective was reified in the United States by Vannevar Bush, who served as head of the U. S. Office of Scientific Research and Development under President Franklin Delano Roosevelt during World War II. Roosevelt asked Bush to map out a plan for post-war scientific research and development. Bush

did so in a report entitled *Science, the Endless Frontier*, which ultimately provided the philosophical underpinnings of the U. S. National Science Foundation (NSF).

Echoing Snow, Bush wrote that "basic research is performed without thought of practical ends" and that its defining characteristic is "its contribution to 'general' knowledge and an understanding of nature and its laws." He went on to say that if one tries to mix basic and applied work, that "applied research invariably drives out pure." Federal funding should support basic work, he argued; out of that basic work would come a broad range of applications. The tension between basic and applied work is represented in Figure 1, and a hypothesized progression from basic research to use-in-practice is represented in Figure 2.

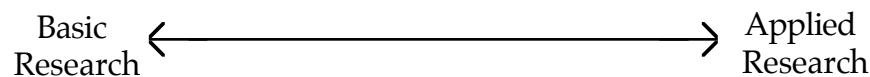


Figure 1.
Basic and applied research seen as polar opposites

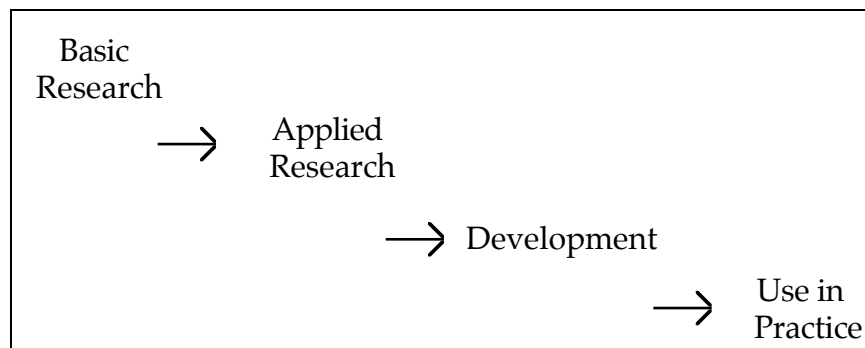


Figure 2. The progression from research to use
(after Stokes, 1997, p. 10)

One can point to researchers whose work fits cleanly at various points in the spectrum illustrated in Figure 1 – paradigmatic examples being Niels Bohr and Thomas Edison. Bohr's work on the structure of the atom was conducted without thought of applications; in contrast, Edison disdained theory while pouring his energy into "electrifying" the United States. You can imagine Bohr situated on the far left of Figure 1, and Edison on the far right.

But what about Louis Pasteur?

Pasteur's work in elaborating biological mechanisms at the microbiological level – working out the "germ theory of disease" – is as basic as you can get. But, Pasteur did not engage in this activity solely for reasons of abstract intellectual

interest. He was motivated by problems of spoilage in beer, wine, and milk, and the hope of preventing and/or curing diseases such as anthrax, cholera, rabies, and tuberculosis.

So, at what point on the spectrum in Figure 1 would you place Pasteur? Do you split him in half, putting 50% of him at each end of the spectrum? Or do you "average" his contributions, placing him in the middle? Neither does him justice.

Stokes resolves this dilemma by disentangling these two aspects of Pasteur's work, considering basic knowledge and utility as separate dimensions of research. He offers the following scheme (Figure 3):

Research is inspired by:

		Considerations of Use?	
		No	Yes
Quest for Fundamental Understanding?	Yes	Pure Basic Research (Bohr)	Use-Inspired Basic Research (Pasteur)
	No		Pure Applied Research (Edison)

Figure 3. A 2-dimensional representation of "basic" and "applied" considerations for research (Stokes, p. 73)

Pasteur has a home in this scheme – and moreover, *considerations of use* and *the quest for fundamental understanding* are seen as living in potential synergy. My point is that this perspective, elaborated by Stokes in the case of science, applies equally well to educational research.

Looking toward the next main section of this paper, I would like to start with this basic idea and make two friendly amendments. The first is to note that the perspective represented in Figure 3 can be used in generative fashion. As we consider problems for research – and as we consider how to frame them – we can make the conscious attempt to do so in such a way that the research makes as large a contribution as possible on both dimensions. When we consider

undertaking any research, we can ask: How can the issues under investigation be framed so that the contributions to fundamental understanding are as large as possible? And, how can we situate this work so that the contributions to practice are as large as possible?

The second friendly amendment is historical. It should be noted that at different points in the development of a field, it may be difficult for any one corpus of work to contribute simultaneously to both theory and practice. Sometimes the state of theory is such that it may best be nurtured, temporarily, aside from significant considerations of use (consider the origins of cognitive science, which was nurtured in laboratory studies). Sometimes the need to solve practical problems seems so urgent that theoretical considerations may be given secondary status (consider the post-Sputnik period, during which engineering efforts such as "putting a man on the moon" and its educational analogue, the development of new curricula for mathematics and the sciences, were given highest priority). Even in such circumstances, however, one can always ask the questions raised in the previous paragraph: Where can we situate our work so that it is as useful as possible? And, how can we go about doing it so that what we discover casts as much light as possible on underlying issues of importance?

Reviewing progress in educational research over the past few decades, I conclude that our field has now reached the point where it is possible to work on many problems that have both theoretical and practical import. I will describe some problems of this type in the next main section of this paper, but first I want to give a concrete example of the productive interaction of basic and applied work. My apologies for the self-referential character of the following, but it is easiest for me to discuss the work I know best.

• Cases in point

As mentioned in the introduction, I began my professional life as a mathematician. I was happily publishing papers in topology and measure theory when, in 1974, I read George Pólya's *How to Solve It*. It's a wonderful book. In it Pólya identifies a number of "tricks of the trade" used by mathematicians as they solve problems. I was enchanted. The more I read the more I smiled. On page after page Pólya described strategies that I recognized myself as having used, though most often not consciously. At first my reaction was glee: "I must be a real mathematician after all. I do all the things Pólya says they do!" But, something nagged at me. If these were such powerful strategies, why hadn't I been taught them? Why did I have to pick them up by myself? Was mathematics a secret guild, where you have to figure out the rules before being admitted to membership? Or, was there something wrong? I asked around. It turned out that any number of people had tried to teach problem solving strategies à la Pólya, without success.

This was a lovely dilemma. It was a neat theoretical issue: Although there was evidence that some people do use such strategies, even without being taught to use them, explicit attempts to teach students to use those strategies had failed.

Was it possible to figure out why? And it was a neat practical issue: What more important goal can there be for the mathematics curriculum than to help students become effective mathematical problem solvers? The combined practical and theoretical appeal of this problem was irresistible, and I changed fields.

The story of the next ten years is the story of a dialectic between theory and practice. I started by reading, becoming familiar with ideas from artificial intelligence (AI) and allied fields about problem solving strategies. There I found a major clue: in AI, strategies have to be characterized in very fine detail in order to be implementable. Was it possible that Pólya's strategies weren't adequately specified? Yes indeed – and some laboratory studies indicated that when the strategies were specified in adequate detail, students could actually use them to solve problems. But this raised a new issue. Each of Polya's strategies wasn't really one strategy; once explicated in detail, it was seen to be a family of perhaps a dozen strategies. In consequence, the twenty or so "powerful ideas" in *How to Solve It*, when fully explicated, represented more than 200 problem solving strategies! Even if someone could learn all 200 strategies, how would that person know which ones to use when confronted with a new problem? (By way of analogy: Imagine yourself in front of a locked door holding a key ring with 200 keys, one of which unlocks the door. Unless you had a way of identifying a small number of "candidate" keys, you could waste an awful lot of time trying one key after another.) I realized that what was needed was a "managerial strategy" – a way of identifying which problem-solving strategies to use, in which circumstances.

I did some experimentation related to these ideas in domains simpler than problem solving. Once I was convinced that there was some substance to the notion of a managerial strategy, I developed one. I then designed a problem solving course that would teach students a range of problem solving strategies along with a managerial strategy for deciding which ones to use at which times. Equipped with my theoretical understandings of the problem solving process I went off to teach.

Guess what? I didn't always teach the way I "should have," given my theoretical understandings. Sometimes my instincts as a teacher led me to do things differently – and, as a result, to re-examine the ideas underlying the instruction. This kind of give-and-take resulted in a number of refinements of the course, and in further experimentation to explore theoretical issues that arose in the course.

At that time, videotape became available as a research tool. I taped students before and after the problem solving course, studying their problem-solving behavior in minute detail. In the process, I discovered the devastating effects of poor metacognitive strategies – students often failed to solve problems even though they knew the right methods, because they went off on "mathematical wild goose chases." As a result my instructional focus changed from prescriptive managerial strategies to a more general approach that emphasized the development of monitoring and self-regulation. Ongoing laboratory studies

refined the approach I took in the classroom, and elaborated the nature of decision-making during problem solving.

The videotapes also raised other issues. It was puzzling that students, at times, ignored what they "knew"; they made conjectures that contradicted results that they had just shown to be true. How could this be? Pursuing this issue led to the study of students' mathematical beliefs – and as I began to understand the issue, to attempts to craft the environment in my problem solving class to support the development of more productive mathematical beliefs. Theoretical ideas of "communities of practice" became salient as the course evolved.

So it went, back and forth, for ten years or so. The end products were a well-refined problem solving course *and* a theory of competence regarding mathematical problem solving – a relatively comprehensive description of what you need to know and do to be a good problem solver. At times the ideas came from the laboratory, or had to be elaborated in that setting; at times, they came from the course and had to be worked out in practice. (See Schoenfeld, 1985, for the details of the problem solving work, and Schoenfeld, 1987, for a more extended version of this history.)

Now, the punch line. I will state unequivocally that the course could not be as effective as it is were it not for the research, which revealed important issues for instruction. But I can be equally strong in stating the converse. The theory of mathematical competence that developed over those ten years could not have emerged and been refined in the way it was were it not for the course, which both served as a source of and a test bed for basic theoretical ideas. Both theory and practice were better off for their close interaction.

Let me briefly mention a second bridge between theory and practice, one that is likely to require a much longer time span. In the section on unifying the cognitive and the social I referred to theories of competence and theories of action-in-context. The work on problem solving is an example of the former. Here is an example of the latter. For some years the Teacher Model Group at Berkeley has been working on the development of a theory of "teaching-in-context" – a rigorous characterization of how and why teachers do what they do, on the fly, in the midst of instruction. I want to stress a few points, which echo those above. First, the motivation for the work is once again both theoretical and practical. On the practical side: to improve teaching, you need to understand it. On the theoretical side, teaching is a wondrously complex, highly interactive, knowledge-dependent act. We have now reached the point where we can hope to understand it, and to build detailed models of it. If we succeed at this, we will have developed enhanced understandings of human behavior in complex social contexts. Please note that this work is situated in practice – we conduct studies of real teachers who are trying to make things work – but also that these studies are conducted with an eye toward theory, both in the selection of the cases and in the goal of *really* understanding what enables teachers to do what they do. See Schoenfeld (1998; 1999) for details; see the section on teachers' professional lives, below, for a pragmatic agenda.

SITES FOR PROGRESS (ISSUES OF PRACTICE, THROUGH A THEORETICAL LENS)

We now turn to issues of practice. My intention is to delineate some "sites for progress" – arenas where there are significant practical challenges, which can be conceptualized in ways by which theory can be advanced. I shall highlight six such arenas: curriculum; assessment; making change happen; developing a research infrastructure; teachers' professional lives; and the pursuit of opportunity.

• Curriculum

For far too long there has been at best a tenuous relationship between curriculum development and research on thinking and learning. This has to change.

Curriculum development can provide an ideal site for the melding of theory and practice. The reason I described the evolution of my problem solving course at some length in the previous section is that it shows the ways in which instructional development and theory development can co-evolve, to the advantage of both. In my opinion, such co-evolution is necessary for significant long-term curricular improvement. If you really want kids to understand or do X, you need to know what it means to understand or do X. That is: if we are to do it right, theories of competence must provide the underpinnings of instruction. That is the case with regard to helping students develop understandings of particular topics in various content areas. It is also the case for much broader goals. [Consider, for example, the goal of preparing students for the world of work. At present the "school-to-work" transition is dramatically under-conceptualized; vocational education tends to be woefully inadequate, as does preparation for the workplace in regular instruction. It is time to begin an R&D program that elaborates the kinds of competencies people need in an ever-changing workplace, and suggests how those competencies might be developed.]

We must recognize that the kinds of combined research and development efforts suggested here will require significant amounts of time and money – and that to date we have not been willing to make such investments as a nation. Early attempts, such as the science and mathematics curriculum projects initiated following the Soviet Union's successful launching of Sputnik, tended not to involve the educational research community. More recent attempts have done so, but they have not been of adequate scale. For example, I applaud the National Science Foundation for taking the initiative, earlier in this decade, to fund some major curriculum projects. Some of those projects had solid connections with the research community. But – and this is not a criticism of NSF, but of the constraints that shaped the approach they took – it doesn't take a very close look to realize that the conditions under which the curriculum developers were compelled to work were next-to-impossible. One project, for example, had a five-year grant to develop five years' worth of curriculum. Six months into the project, the project leader said she felt that she was already very

far behind schedule! That kind of compressed time scale allows time for little more than the following four-step development cycle:

- write instructional materials on the basis of what you know;
- field test the materials you've written;
- revise once on the basis of what you've seen; and
- pray.

That just won't do. We have to make a long-term investment in building instructional materials, and learning from what we do as we do it.

• **Assessment**

Everything I've said about curriculum, and more, applies to assessment. If you are going to test for students' understanding of something, then

- A. You need to have an adequate characterization of what it is you're assessing, and
- B. You need to have a good idea of how performance on the assessment corresponds to being able to do whatever it is that's supposedly being assessed.

On A: It would seem reasonable that in order to test for students' understanding of X, you should have a good idea of what it means to understand X. That is, theories of competence are every bit as essential for meaningful assessment as they are for curriculum. Virtually none of the current assessments in wide use are grounded in well developed theories of competence.

On B: Let us suppose that one has an adequate theory of competence regarding whatever it is that is being assessed. Even given that, there are a host of serious questions regarding how well a student's performance on any particular task or collection of tasks reflects that student's understanding of that content. The kinds of theories of competence that have emerged in the past few decades have a very different epistemological grounding than the kind of trait psychology that gave rise to the current psychometric notions of "reliability" and "validity." Those concepts as presently defined are of little or no use, and they need to be rethought.

This is a significant theoretical as well as pragmatic challenge. It is also a challenge of some urgency. In the United States, the widespread use of external, standardized assessments at almost every grade level, for a wide range of purposes, has had seriously deleterious effects. Since "teaching to the test" these days typically means teaching to a set of skills that have little to do with deep competence, the current incarnations of most assessments serve disruptive rather than productive functions. In addition, it should be noted that widespread and frequent testing is not necessary to achieve many of the sorting and accountability purposes for which tests are typically used; cross-national

comparisons indicate that other nations achieve the same purposes in other ways. We should question the degree to which we rely on frequent standardized tests. To the degree that we use them, we should have assessments that are actually meaningful and informative.

- **Making change happen**

Let me start with a maxim that lies at the heart of the concept of design experiments: "Sometimes you have to build something to see if it will work." Without stopping, let me add: "and then you have to study the hell out of it." We don't do nearly enough of that.

The idea is a simple one, and it is compatible with the shift from the linear "research leads to development" sequence represented in Figure 2 to the "research and development can and should live in productive dialectic" perspective discussed in the previous main section.

Sometimes an instructional experiment is motivated for theoretical reasons. For example, an epistemological concern for the role of social interactions in learning can result in the reconfiguration of instructional practices, in the hope that a different kind of environment will improve learning. Sometimes the motivation is one of urgent need: a school or a district is "in trouble," and *something* needs to be done. In either case, an intervention takes place – not a pre-determined comparison study in which one randomly selected group of students receives Treatment A and another receives Treatment B, but a good faith effort that draws upon available knowledge to make needed change in a real world context.

The challenge is, what sense can you make of all this? The issues may be focused on learning: how does a non-standard classroom structure affect the ways students interact and learn? It may be focused on the teachers: how does what the teachers know or believe shape the ways that they respond to change, grapple with the contingencies of instruction, or interpret the ideas they are asked to implement? It may be focused on systemic points of leverage – what impact do various attempts to move the system actually have on what takes place in classrooms, on what students actually learn?

It should be stressed that this kind of approach does not represent a weak alternative to conducting controlled experiments, but a different option altogether. Sometimes the only way you can understand complexity is to study complex things. Part of the job, in that case, is figuring out what to look at and how to talk about it. This is a huge challenge, but it is one we should not shy away from. In pragmatic terms, it is important for us, when we can, to work in the places that really matter. The fact is that the vast majority of educational change is unplanned (or minimally planned) and goes unexamined (see, e.g., Elmore, 1999). A school or a district does *something* because of a perceived need; time passes without any real lessons being learned from the experience; and then the school or district finds itself trying something different. We have to break this cycle, and begin to learn from what we do in the field. Doing so raises

fundamental theoretical issues, for we need be able to figure out "what counts" amidst the glorious complexity of practice, and how to characterize it in careful ways. There is a desperate need for such work.

- **Developing a research infrastructure**

Not long ago the president of Mattel Incorporated said he believed it was essential to invest 5% of corporate expenditures in R&D efforts. Pharmaceutical investments in research and development are more than four times that percentage. In contrast, according to the U. S. House Committee on Science (1998), "currently, the U.S. spends approximately \$300 billion a year on education and less than \$30 million, 0.01 percent of the overall education budget, on education research." The committee report goes on to say that "this minuscule investment suggests a feeble long-term commitment to improving our educational system" (p. 46). In percentage terms, it certainly does. But things are even worse than these figures suggest.

Two months after delivering the AERA presidential address in which I discussed the figures given in the previous paragraph, I received my June 1999 issue of *Smithsonian* magazine. Toward the front of that issue you will find a 2-page spread, an eye-catching advertisement that shows a young boy hugging his dog. The text on the side of that picture says, in part:

"We're a world leader in animal health care. We're devoted to animals their entire lives, from their first vaccination to medications for older pets. We lead the industry in research, *spending over two hundred million dollars a year*, looking for new treatments designed specifically for animals. In fact, we're the people who introduced the first arthritis medication in the U.S. specifically for dogs." (*Smithsonian*, June 1999, pp. 14-15. Emphasis added)

In absolute terms, then: last year a single pharmaceutical company spent more than six times the amount of money studying animal health than our entire federal government spent on educational research. Just think about what this says about our national priorities.

There are numerous issues to be dealt with here. There is, of course, the political issue: we need to make a public and compelling case that educational research can make a difference. There is the human resources issue: how do we lure and support large numbers of talented people, especially those who are from and/or willing to attend to the needs of underrepresented communities? (I am pleased to say that AERA has been working closely with the Office of Educational Research and Improvement on ways to address this issue.) And then, there is the issue of preparation for educational research.

I have spent a lot of time thinking about this issue, both "at home" (in terms of our own graduate program at Berkeley) and in general. Needless to say, preparing students to enter a field that is inherently interdisciplinary, that demands broad knowledge, and whose paradigms and methods are in flux, is a

challenging task! If space permitted, I would go on at length about this. I have, elsewhere; please see Schoenfeld (1999b). What I want to note here is that our own graduate programs can serve as ideal sites for study and reflection. For some years we have bandied about the notion of "learning by apprenticeship." Indeed, some graduate programs claim to work that way – but as a field we have not engaged in the systematic study of how such academic apprenticeship really works. This is a lovely opportunity for us to add to our own understanding, while improving the preparation of those who will be our colleagues and carry on our work. In short, theory and practice can once again work to each other's advantage.

- **Teachers' professional lives**

For the most part, they don't have them – that is, teachers in the United States don't have professional lives, in any sense worth speaking of.

Let me make it clear that I am not engaging in teacher-bashing; just the opposite. For the past six years I have spent my Tuesday mornings teaching mathematics in Berkeley's elementary schools. I can say flatly that grabbing the interest of a group of second graders and seducing them into thinking deeply about mathematical ideas is as challenging – and draining – as any task I've ever taken on, including graduate instruction (easy in comparison!) and my own research. Teaching is an extraordinarily difficult and demanding profession. To do it "right" demands very high levels of knowledge, skill, and dedication.

Yet, think of how we select, prepare and sustain teachers. There are few incentives, except for the intrinsic ones, to join the profession; pay is generally low and job status even lower. A typical "teacher preparation" program consists of a year of boot camp in which intending teachers learn classroom management techniques and some generic teaching methods. (That's all there's time for, if you just allow a year for getting ready to teach.) Then, once teachers are hired, their work days are structured in ways that provide negligible opportunities for interacting with colleagues in meaningful ways. Beyond that, almost no teachers have opportunities on the job, or outside it, for sustained and well-conceived professional development. That's what I mean by saying teachers don't have professional lives. One of the most important issues that we need to confront in the coming decades is how to change this state of affairs.

There are two sets of issues to be dealt with here, one social and the other intellectual. Cross-national studies show that teachers in other nations *are* treated as professionals – for example, Chinese and Japanese teachers have their work days arranged so that they can collaborate with colleagues in the study of curricular materials (Ma, 1999; Lewis & Tsuchida, 1998; Stigler & Hiebert, in press; Yoshida, April 1999). Experiences that promote growth are designed into those teachers' professional lives, and they pay off. It is no accident that despite having many fewer years of formal schooling than their American counterparts, experienced Chinese elementary mathematics teachers tend to have a much deeper knowledge of mathematics and of how to help their students learn it (Ma,

1999). And, as we know, Chinese and Japanese students outperform ours by significant margins (Stevenson & Stigler, 1992).

In short, teachers' lack of professional status is a critically important social issue. Things need not and should not be that way, as comparisons with other nations make all too clear. There is a great deal of work to be done in making it possible for teachers to be accorded the professional status they should have.

Now let me turn to the relevant intellectual issues. The study of teaching offers wonderful opportunities for both fundamental and applied research. Teaching is a knowledge-based activity; it is highly interactive and contingent on dynamically changing circumstances; and it calls for rapid decision-making in the service of multiple and changing goals. On the theoretical side of the coin, to be able to describe and provide detailed theoretical models of such activity, explaining how and why teachers do what they do amidst the complexity of the classroom, is to make significant strides in understanding human thought and action. This hardly tells the whole story – for example, a theory of teaching-in-context does not address the major theoretical issue of how teachers *learn* from their teaching – but it sets the stage for such work. On the practical side, it should be clear that the development of theoretical models of teaching has the potential to inform professional development in numerous ways. As indicated above, such work can identify necessary components of competence – what knowledge is really needed to be successful at a particular kind of teaching? It can also offer some pleasant surprises, like the fact that three accomplished teachers whose classroom behavior and style *seem* very different are actually doing very much the same thing. This suggests that others may learn to do the same thing too (see Schoenfeld, 1999a). In addition, the capacity to capture teachers' thinking and decision-making "on line" suggests that we may be able to understand teachers' capacities at different points in their careers, and be able to map out aspects of teachers' developmental trajectories. As we do so, we can begin to determine what kinds of understandings tend to develop at various points in their teaching careers, and at what points certain kinds of interventions might be particularly helpful. In sum (as with all theories of competence), efforts aimed at understanding teaching will also help us to improve it.

• The pursuit of opportunity

As I reach the end of this paper I want to come full circle, returning to my opening comments. I began by noting that I am the beneficiary of our public education system, having received a superb education from kindergarten through college at virtually no cost. My career as an educational researcher and my attempts to help make research make a difference are part of my way of paying society back for what it has given me. I believe in the importance of public education. I want all children to have access to the opportunities I was fortunate enough to have. We owe that to our children. And, there is no better investment in the future of our nation.

We have not lived up to that commitment. We have not even come close.

If you haven't read Jonathan Kozol's book *Savage Inequalities*, please do. If you have, take a moment to think about the terrible injustices that it documents. It is nothing short of a moral outrage that in a nation as rich as the United States, so many children live in poverty; that poverty correlates so strongly with race; and that the children who have the least resources tend disproportionately to attend the schools that have the least resources. In some ways, I think that our greatest educational failure is that we have allowed ourselves to become a nation that tolerates these outrages, often without giving them a second thought. We must work to undo these injustices.

As we do so, we will be presented with significant theoretical opportunities. We have an obligation not only to invest in our children, but to study and learn from that investment (cf. "making things happen," above). Engaging in that study, in the contexts where children have the most desperate needs for help and resources, is one way to address some of the fundamental questions raised in the section on reconceptualizing the nature/nurture discussion. Let us provide the necessary support. And as we do, let us ask: What develops, in what ways, given what kinds of support structures? What kinds of interventions can facilitate development? And, how can we use such knowledge to good ends?

IN CONCLUSION . . .

The main point I have tried to make in this paper is that it is possible and desirable to think of research and applications in education as synergistic enterprises rather than as points at opposite ends of a spectrum, or as discrete phases of a "research leads to applications" model. We can choose to explore theoretical issues in contexts that really matter; and, when we work on important problems we can try to frame them so that our work helps us make progress on fundamental issues. Beyond that general point, I have tried to suggest some major theoretical issues that need attention, and some sites for research that are potentially of practical and theoretical importance.

I do not deceive myself about the difficulty of the tasks I have outlined, or about the commitment of time and resources it will take. We have our job cut out for us, in both social and intellectual terms. But the task is vitally important. Let us have at it.

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