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THE NEW SCHOLARSHIP REQUIRES A NEW EPISTEMOLOGY

If we intend to pursue the "new forms of scholarship" that Ernest Boyer presents in his Scholarship Reconsidered, we cannot avoid questions of epistemology, since the new forms of scholarship he describes challenge the epistemology built into the modem research university.

In addition to basic research--Boyer's scholarship of discovery, which "has come to be viewed as the first and most essential form of scholarship, with other functions flowing from it"--Boyer envisions three new forms of scholarship.

* The scholarship of integration gives meaning to isolated facts, "putting them into perspective . . . making connections across disciplines, placing the specialties in larger context, illuminating data in a revealing way, often educating nonspecialists, too . . . . "

* In the scholarship of application, "the scholar asks 'How can knowledge be responsibly applied to consequential problems? How can it be helpful to individuals as well as to institutions?'"

* The scholarship of teaching, which "begins with what the teacher knows," means not only transmitting knowledge but transforming and extending it as well . . . . "

If integration, application, and teaching are to be taken as "forms of scholarship" in other than a Pickwickian sense, the new scholars must produce knowledge that is testably valid, according to criteria of appropriate rigor, and their claims to knowledge must lend themselves to intellectual debate within academic (among other) communities of inquiry. But what are these kinds of knowledge, claims to validity, and criteria of appropriate rigor? And how do they stand in relation to the "old" scholarship of discovery?

I argue in this article that if the new scholarship is to mean anything, it must imply a kind of action research with norms of its own, which will conflict with the norms of technical rationality--the prevailing epistemology built into the research universities. Drawing on my experience studying MIT's Project Athena, I illustrate what this kind of action research could be like in at least one instance and suggest the epistemological, institutional, and political issues it raises within the university.

INSTITUTIONAL EPISTEMOLOGY

Like other organizations, educational institutions have epistemologies. They hold conceptions of what counts as legitimate knowledge and how you know what you claim to know. These theories of knowledge need not be consciously espoused by individuals (although they may be), for they are built into institutional structures and practices. For example, the typical elementary school is organized around "school knowledge"--knowledge contained in a curriculum, held in the minds of teachers, and communicated by instruction to pupils. One important subclass of school knowledge consists of "math facts" (the "facts" of addition and multiplication, for example). Math facts can be divided into progressive modules; that is, the more advanced are constructed on the foundations of the more basic. Modules can be incorporated into lesson plans, which can be assembled to make a math curriculum. Tests, which determine whether the pupils "get" the knowledge contained in a curriculum, can be used to control their promotion from one grade to another, and promotions result in graduation. Graduation rates, among other indices of student performance, can be used to control the promotion and compensation of teachers.

The chunking and ordering of time and space are also linked to the lesson plan. Space can be broken up into rooms where teachers work for periods of 47 minutes with groups that range from 25 to 33 children, around curriculum modules that each have a lesson plan. All of these things enter into the idea of "school knowledge," which is not knowledge per se, but what we treat as knowledge within the context and the institutional arrangements of an elementary school.

Like the elementary school, the research university is an institution built around a particular view of knowledge, as the following dilemma helps to make clear.

The dilemma of rigor or relevance. In the varied topography of professional practice, there is a high, hard ground overlooking a swamp. On the high ground, manageable problems lend themselves to solution through the use of research-based theory and technique. In the swampy lowlands, problems are messy and confusing and incapable of technical solution. The irony of this situation is that the problems of the high ground tend to be relatively unimportant to individuals or to society at large, however great their technical interest may be, while in the swamp lie the problems of greatest human concern. The practitioner is confronted with a choice. Shall he remain on the high ground where he can solve relatively unimportant problems according to his standards of rigor, or shall he descend to the swamp of important problems where he cannot be rigorous in any way he knows how to describe?

Nearly all professional practitioners experience a version of the dilemma of rigor or relevance, and they respond to it in one of several ways. Some of them choose the swampy lowland, deliberately immersing themselves in confusing but critically important situations. When they are asked to describe their methods of inquiry, they speak of experience, trial and error, intuition, or muddling through. When teachers, social workers, or planners operate in this vein, they tend to be afflicted with a nagging sense of inferiority in relation to those who present themselves as models of technical rigor. When physicists or engineers do so, they tend to be troubled by the discrepancy between the technical rigor of the "hard" zones of their practice and the apparent sloppiness of the "soft" ones.

People tend to feel the dilemma of rigor or relevance with particular intensity when they reach the age of about 45. At this point, they ask themselves, "Am I going to continue to do the thing I was trained for, on which I base my claims to technical rigor and academic respectability? Or am I going to work on the problems--ill-formed, vague, and messy--that I have discovered to be real around here?" And depending on how people make this choice, their lives unfold differently.

What are the sources of the dilemma of rigor or relevance?

The dilemma depends, I believe, upon a particular epistemology built into the modem research university, and, along with this, on our discovery of the increasing salience of certain "indeterminate zones" of practice--uncertainty, complexity, uniqueness, conflict--which fall outside the categories of that epistemology.

The epistemology of the modern research university. In a fine article published in Minerva in 1978, Edward Shils describes how the idea of the research university came to the United States after the Civil War, when American scholars who had gone abroad to study in Germany brought back with them the German idea of the university as a place in which to do research that contributes to fundamental knowledge, preferably through science.

This was a very strange idea in 1870, very much at odds with the then-prevailing conception of higher education in America, which was based on the British notion of the university as a sanctuary for the liberal arts or as a finishing school for gentlemen. The new idea of what came to be the modem research university took root first at Johns Hopkins, whose president was prepared to adopt the bizarre notion that professors should be recruited, promoted, and granted tenure on the basis of their contributions to fundamental knowledge. Gradually, the idea spread out from Johns Hopkins to places like Michigan, Columbia, and the University of Chicago, most of the Ivy League coming later because its institutions were more firmly modeled on the earlier conception of the university. And gradually the idea of the research university was established in the conventional wisdom of American higher education.

The Veblenian bargain and its consequences. As this idea gained strength in the early years of the new century, it created a very special problem for professional practice. Most of the knowledge essential to professional practice is not what the research university calls fundamental knowledge, and practitioners are not, as a rule, either scientists or scholars. How, then, should professional practice stand in relation to the universities? Thorsten Veblen's The Higher Learning in America, first published in 1918, poses an answer to this question.

Veblen was angry with the board of trustees of the University of Chicago because it wanted to introduce a business school, of all things, into the university. Veblen pointed out that this would be unfortunate for everybody concerned: the poor business faculty would try to make a show of scholarship and would, of course, fall fiat on their faces, embarrassing both themselves and the real scholars in the rest of the university. Better to keep them out in the first place. Better to make a clean separation between the research universities, the "schools of higher learning" whose proper business is true scholarship that contributes to fundamental science and systematic knowledge, and the "lower schools" of the professions whose business is the preparation of individuals for professional practice. And the bargain between the two types of schools should be as follows: from the higher schools, their fundamental and systematic knowledge; from the lower schools, the practical problems to which such knowledge may be applied.

Veblen lost his battle, and in subsequent decades the business schools came into the universities, along with the schools of engineering, dentistry, social work, forestry, and police science, to name only a few. By 1964, Harold Wilensky could write an article entitled, "The Professionalization of Everyone?"

As the professions came to place their educational branches in the prestigious research universities, they bought into the Veblenian bargain, which is to say that they agreed to look at professional practice as though it consisted of the application of science or systematic knowledge to the instrumental problems of practice. The conception of professional knowledge the professions thereby accepted, which I call "technical rationality," is that practice is instrumental, consisting in adjusting technical means to ends that are clear, fixed, and internally consistent, and that instrumental practice becomes professional when it is based on the science or systematic knowledge produced by the schools of higher learning.

The acceptance of technical rationality and the Veblenian bargain had two critically important consequences for the professional schools. One is that the schools adopted what Edgar Schein calls "the normative professional curriculum": first, teach students the relevant basic science; then, teach them the relevant applied science; and finally, give them a practicum in which they can learn to apply classroom knowledge to the problems of everyday practice. But for anyone who has actually taught a professional practicum, the predicament is that classroom knowledge is only part--and by no means the most important part--of what counts in practice. So physicians, for example, are wont to say, "I really learned to practice medicine on the wards," as lawyers say, "I really learned to practice law in the courtroom, or the law office." And the practicum instructor often becomes caught in a situation in which he or she is supposed to teach one thing according to the normative curriculum of the school, but must actually teach another thing altogether in order to prepare students for the real-world demands of practice.

Secondly, the Veblenian bargain, with its basis in technical rationality, fostered a radical separation between research and practice. Research, of the kind that was viewed as proper to the "higher schools"--rigorously controlled experimentation, statistical analysis of observed correlations of variables, or disinterested theoretical speculation--finds little place to stand in the turbulent world of practice, which is notoriously uncontrolled, where problems are usually ill-formed, and where actors in the practice situation are undeniably "interest-ed." The consequence, stronger today than ever, was that the research produced by the "higher schools" seemed to have little to say that was of value to practitioners.

TURNING THE PROBLEM ON ITS HEAD

The relationship between "higher" and "lower" schools, academic and practice knowledge, needs to be turned on its head. We should think about practice as a setting not only for the application of knowledge but for its generation. We should ask not only how practitioners can better apply the results of academic research, but what kinds of knowing are already embedded in competent practice.

Perhaps there is an epistemology of practice that takes fuller account of the competence practitioners sometimes display in situations of uncertainty, complexity, uniqueness, and conflict. Perhaps there is a way of looking at problem-setting and intuitive artistry that presents these activities as describable and as susceptible to a kind of rigor that falls outside the boundaries of technical rationality.

When we go about the spontaneous, intuitive performance of the actions of everyday life, we show ourselves to be knowledgeable in a special way. Often we cannot say what we know. When we try to describe it, we find ourselves at a loss, or we produce descriptions that are obviously inappropriate. Our knowing is ordinarily tacit, implicit in our patterns of action and in our feel for the stuff with which we are dealing. It seems right to say that our knowledge is in our action. And similarly, the workaday life of the professional practitioner reveals, in its recognitions, judgments, and skills, a pattern of tacit knowing-in-action.

Common sense admits the category of know-how, and it does not stretch common sense very much to say that the know-how is in the action--that a tightrope walker's know-how, for example, lies in, and is revealed by, the way she takes her trip across the wire. Or that a big league pitcher's know-how is in his way of pitching to a batter's weakness, changing his pace, or distributing his energies over the course of a game.

Examples of intelligence-in-action include not only the exercise of physical skills but acts of recognition and judgment. Michael Polanyi, for example, has written about our ability to recognize a face in a crowd. The experience of recognition can be immediate and holistic. We simply see, all of a sudden, the face of someone we know. We are aware of no antecedent reasoning and we are often unable to list the features that distinguish this face from the hundreds of others present in the crowd. Polanyi has also described our ordinary tactile appreciation of the surface of materials. When we use a stick to probe a hidden place, we focus not on the impressions of the stick on our hand but on the qualities of the place that we apprehend through these tacit impressions. Polanyi speaks of perceiving from these impressions to the qualities of the place. To become skillful in the use of a tool is to learn to appreciate, as if it were directly, the qualities of materials that we apprehend through the tacit sensations of the tool in our hand.

Chester Barnard has written of "non-logical" processes that we cannot express in words as a process of reasoning, but evince only by a judgment, decision, or action. A child who has learned to throw a ball makes immediate judgments of distance that he coordinates, tacitly, with the feeling of bodily movement involved in the act of throwing. A high school student solOing quadratic equations has learned spontaneously to carry out a program of operations that she cannot describe. A practiced accountant of Barnard's acquaintance could take a balance sheet of considerable complexity and within minutes or even seconds get a significant set of facts from it, though he could not describe in words the recognitions and calculations that entered into his performance.

All of these are examples of what Polanyi calls "tacit knowing" and what I would like to describe as "knowing-in-action." I submit that such knowing-in-action makes up the great bulk of what we know how to do in everyday and in professional life. It is what gets us through the day.

If a skilled performer tries to teach (and therefore, in part, describe) her knowing-in-action to someone else, she must first discover what she actually does when confronted with a situation of a particular kind. So a piano teacher might say, for example, "I don't like the way you play this transitional passage. Let me play it so that I can see what I do." She must observe what she does before she can describe it. And there is no guarantee, even then, that she will be able to describe it accurately. Similarly, a calculus teacher might have to "see what he does" when he is asked to say how he sets up a problem of differentiation or integration. Often, we misstate what we know how to do. Indeed, when we ask people to describe what they know how to do, we are likely to get an answer that mainly reveals what they know about answering such a question. If we want to discover what someone knows-in-action, we must put ourselves in a position to observe her in action. If we want to teach about our "doing," then we need to observe ourselves in the doing, reflect on what we observe, describe it, and reflect on our description.

In many instances, of course, this is not what we do. And failing this, we teach in bad faith, which is to say that what we teach is not what we know-in-action. I think this is also true of teaching in the sciences, for they also involve a practice--the practice of doing scientific research--and such a practice includes its own forms of knowing-in-action. But if we ask physicists or mathematicians, "Do you teach what you do?" they may very well reply, "Of course not! How could you expect that? We teach research results." Yet there is a great deal of critically important knowing-in-action that is not captured in research results as they are usually formulated in textbooks or published papers.

Reflection-in-action. We all have, in greater or lesser degree, the capability of reflecting on what we know as revealed by what we do. And we also have the ability to reflect-in-action to generate new knowing, as when a jazz band improvises within a framework of meter, melody, and harmony: the pianist laying down "Sweet Sue" in a particular way, and the clarinetist listening to it and picking it up differently because of what the pianist is doing--and nobody using words.

The process of reflection-in-action begins when a spontaneous performance--such as riding a bicycle, playing a piece of music, interviewing a patient, or teaching a lesson--is interrupted by surprise. Surprise triggers reflection directed both to the surprising outcome and to the knowing-in-action that led to it. It is as though the performer asked himself, "What is this?" and at the same time, "What understandings and strategies of mine have led me to produce his?" The performer restructures his understanding of the situation--his framing of the problem he has been trying to solve, his picture of what is going on, or the strategy of action he has been employing. On the basis of this restructuring, he invents a new strategy of action and tries out the new action he has invented, running an on-the-spot experiment whose results he interprets, in turn, as a "solution," an outcome on the whole satisfactory, or else as a new surprise that calls for a new round of reflection and experiment. This is the sort of thing a physician may do when encountering a patient whose particular configuration of symptoms is "not in the book." It is what a good teacher does as she tries to make sense of a pupil's puzzling question, seeking to discover, in the midst of a classroom discussion, just how that pupil understands the problem at hand.

In the course of such a process, the performer "reflects" not only in the sense of thinking about the action he has undertaken and the result he has achieved, but in the more precise sense of turning thought back on the knowing-in-action implicit in action. The actor reflects "in action" in the sense that his thinking occurs in an action-present--a stretch of time within which it is still possible to make a difference to the outcomes of action.

Yet we also have the ability to reflect on such a process, reflecting on reflection-in-action. This may sound fancy, but it is illustrated by the relatively mundane experience of a basketball player who spends Sunday morning looking at a videotape of Saturday's game, asking himself, for example, "How was I trying to get past that guard? Why didn't it work? What moves should I try next time?" Reflection-in-action occurs in the medium of words. It makes explicit the action strategies, assumptions, models of the world, or problem-settings that were implicit in reflection-in-action. It subjects them to critical analysis and perhaps also to restructuring and to further on-the-spot experiment. A practitioner such as a lawyer, a teacher, or a machinist may reflect in this way on a particular episode of reflection-in-action or on a sequence of such episodes, thereby making explicit and subjecting to critique and testing the strategies, assumptions, or problem-settings implicit in a whole repertoire of situational responses.

Deweyan inquiry and action research. In the domain of practice, we see what John Dewey called inquiry: thought intertwined with action--reflection in and on action--which proceeds from doubt to the resolution of doubt, to the generation of new doubt. For Dewey, doubt lies not in the mind but in the situation. Inquiry begins with situations that are problematic--that are confusing, uncertain, or conflicted, and block the free flow of action. The inquirer is in, and in transaction with, the problematic situation. He or she must construct the meaning and frame the problem of the situation, thereby setting the stage for problem-solving, which, in combination with changes in the external context, brings a new problematic situation into being. Hence the proper test of a round of inquiry is not only "Have I solved this problem?" but "Do I like the new problems I've created?"

Deweyan inquiry is very close to the notion of designing in the broad sense of that term--not the activities of "design professions" such as architecture, landscape architecture, or industrial design, but the more inclusive process of making things (including representations of things to be built) under conditions of complexity and uncertainty. This broader sense of designing includes a lawyer's design of a case or legal argument, a physician's construction of a diagnosis and course of treatment, an information technologist's design of a management information system, and a teacher's construction of a lesson plan.

Design inquiry consists not only in creating plans but in enacting them. It is undertaken in particular situations of practice. When it is effective, it deploys knowing-in-action already accessible to the practitioner, and it may also generate and test new knowledge for action--for example, a view of what may be wrong with a patient (conceived, as the psychoanalyst Erik Erikson once wrote, as a "universe of one"), or of what potentials and constraints are inherent in a particular architectural site, or of how a particular set of mathematical concepts and procedures might be explored and communicated through a new lesson plan. Such practice knowledge--generated in, for, and through a particular situation of action--may be made explicit and put into a form that allows it to be generalized.

The kind of generalization I have in mind is not of the "covering law" variety--a general, perhaps statistical, proposition applicable to all instances in which certain combinations of variables are present. It consists, rather, in framing the problem of a problematic situation, and strategies of action appropriate to its solution, in such a way that both the problem and the action strategies can be carried over to new situations perceived as being similar to the first. In the new situations, one must still test the validity, actionability, and "interest" (the term so beloved of academicians) of the practice knowledge derived from the initial situation. Through these processes of reflection-in-action and reflection on reflection-in-action, the newly generated practice knowledge may be modified and incorporated into the practitioner's repertoire so as to be available for projection to further situations.

This is very much what Kurt Lewin, the renowned social psychologist and "practical theorist," meant by action research. During and after World War II, Lewin dealt with such mundane problems as how to get children to drink orange juice and eat liver. But through such practical inquiry, he developed the idea of the role of the "gatekeeper" in gaining acceptance for new ways of thinking and acting, and the notion of fields of social forces and of quasi-stationary equilibria within them, which have long since entered into our common stock of ideas in good currency about the operation of the social world.

In Lewin's work, we find the idea that a practitioner's reflection on knowing-and reflection-in-action can give rise to actionable theory. Such verbally explicit theory, derived from and invented in particular situations of practice, can be generalized to other situations, not as covering laws but through what I call "reflective transfer," that is, by carrying them over into new situations where they may be put to work and tested, and found to be valid and interesting, but where they may also be reinvented.

The new scholarship implies action research. The new categories of scholarly activity must take the form of action research. What else could they be? They will not consist in laboratory experimentation or statistical analysis of variance, nor will they consist only or primarily in the reflective criticism and speculation familiar to the humanities.

If teaching is to be seen as a form of scholarship, then the practice of teaching must be seen as giving rise to new forms of knowledge. If community outreach is to be seen as a form of scholarship, then it is the practice of reaching out and providing service to a community that must be seen as raising important issues whose investigation may lead to generalizations of prospective relevance and actionability. If we speak of a scholarship of integration--the synthesis of findings into larger, more comprehensive understandings--then we are inevitably concerned with designing. The scholarship of application means the generation of knowledge for, and from, action. Indeed, Boyer's proposition, "New intellectual understandings can arise out of the very act of application," is a fairly exact formulation of Lewinian action research.

The problem of changing the universities so as to incorporate the new scholarship must include, then, how to introduce action research as a legitimate and appropriately rigorous way of knowing and generating knowledge. If we are not prepared to take on this task, I don't understand what it is we are espousing when we espouse the new scholarship. If we are prepared to take it on, then we have to deal with what it means to introduce an epistemology of reflective practice into institutions of higher education dominated by technical rationality.

All of us who live in research universities are bound up in technical rationality, regardless of our personal attitudes toward it, because it is built into the institutional arrangements--the formal and informal rules and norms--that govern such processes as the screening of candidates for tenure and promotion. Even liberal arts colleges, community colleges, and other institutions of higher education appear to be subject to the influence of technical rationality by a kind of echo effect or by imitation. Hence, introducing the new scholarship into institutions of higher education means becoming involved in an epistemological battle. It is a battle of snails, proceeding so slowly that you have to look very carefully in order to see it going on. But it is happening nonetheless.

PROJECT ATHENA AT MIT

I turn now to examples drawn from my experience at MIT. Granted, the institutional issues generated by the new scholarship will be significantly different in a place like MIT than in liberal arts institutions, community colleges, "streetcar colleges," or state universities. Nevertheless, the case of Project Athena at MIT reveals processes pertinent to all institutions of higher education.

Project Athena, begun in about 1982, was MIT's attempt to introduce computers into undergraduate education, and, in the process, to re-establish itself at the leading edge of institutional computing. In support of this enterprise, the Digital Equipment Corporation (DEC) and IBM donated some $100 million worth of computers to MIT, and the then-dean of engineering raised another $12 million to support the faculty's development of innovative educational software. I became involved in Project Athena in the late 1980s when, together with Professor Sherry Turkle and several graduate students, I was asked to undertake case studies of the project in the departments of civil engineering, chemistry, physics, and architecture. We were not charged with evaluating Athena; no one in MIT's administration favored that. In fact, the then-provost expressed the fear that an evaluation might demoralize Athena's champions. But the late dean of undergraduate education, Margaret MacVicar, was strongly supportive of an inquiry into just what Athena was coming to mean for MIT's faculty and students.

Athena began with several key premises. One was that the computer held the potential for revolutionizing undergraduate education. It would do so, as the dean of engineering put it, by increasing educational effectiveness and efficiency. The computer was expected to eliminate most of the "grunge work," the laborious calculations that took up so much of MIT students' time, and to enable faculty and students to communicate electronically. And computer-based "intelligent tutors" were expected to relieve faculty of the near-impossible task of providing individual assistance to students who were having difficulties with particular subjects or problem sets.

Another of Athena's premises was that faculty members would invent educational software. Funds were made available for this purpose, and faculty were invited to ask for grants to develop educational software for subjects of their choosing--for example, French language, methods of negotiation, or one of MIT's many courses in thermodynamics. Two proposal review committees were established, one for the School of Engineering and one for the schools of Science, Humanities, and Architecture and Planning. Athena's architects assumed--erroneously, as it turned out--that because MIT faculty were smart and technically sophisticated, they could treat the development of educational software as an exercise for the left hand.

From the larger story of Project Athena I extract two stories that suggest general lessons for organizational change--and epistemological battles--in behalf of the new scholarship.

Is developing educational software a legitimate form of research? The first of these stories has to do with someone I'll call Bob Shaler. When I met him in about 1989, Shaler was in his late 30s or early 40s, an engineer working as a junior faculty member in the field of civil engineering. He had developed two programs for computer-assisted instruction. One of these, Program A, was conceived as an intelligent tutor designed to help civil engineering students learn statics. It would administer problems, register the correctness or incorrectness of the student's proposed solutions, and for incorrect answers, would diagnose the errors in question, provide the correct principles, and re-administer new problems that posed the same kinds of difficulty. Program B, on the other hand, was conceived not as an instructional program but as a "design tool." It made it possible to draw a structure on the CRT screen--a bridge or truss, for example; specify its dimensions and materials; and apply a load of a given magnitude. The program would then instantly display the structure's deflection in response to the load.

When we interviewed students who had used both programs, we found they hated the intelligent tutor. They thought it dull and boring and used their MIT smarts to subvert it so that it simply spat out the right answers (thereby deriving some educational benefit, I suppose). But Program B, which was intended to function only as a "design tool," turned out to be extraordinarily educational for about a third of the students who used it in their design projects. These students would draw and load their structures in the virtual world of the machine, expecting a certain kind of outcome, and would often be surprised to get something else entirely. They would ask, for example, "How could this structure become stiffer when I take something away from it?" And because these were confident MIT types, they did not respond by questioning their aptitude for engineering. Rather, they said, "Wow !" and tried to find out what was going on. They would revisit Statics 1.05, which they'd taken in their freshman or sophomore year, and would discover that the formula that applied to the phenomena was one they already knew. But they would say: "I knew the formula, but I never understood how the damned structure behaved until I tried this program." It happened that Bob Shaler, who was a protege of a senior professor in the department, came up for tenure. When his case came before the Academic Council, they found that his work did not look like high-quality research to them. They also asked, "Who can read this stuff and critique it? How can we judge it?" Shaler was denied tenure.

Because this process was going on in the midst of our study of Athena, we were able to look into what was happening. We found that members of the committee judged Shaler's research to be of poor quality--or even more damaging, unable to be evaluated. But we also found that Shaler, although he knew how to design software and get it to work in the design laboratories, did not know how to make "research" out of it--that is, to read into his inquiry a question that could be subjected to empirical research.

Shaler was surprised by how the students used and reacted to his two programs. In my view, this surprise could have been a springboard for research. Shaler could have gotten interested in what the students were actually making of his software, how they used it, what it meant to them. Indeed, I think all educational software designers should get interested in this question, for they would discover that the object they designed often takes on a meaning in use that differs from their design intention, and they might be led to think differently about the "knowledge" they intended to convey. In Shaler's case, such an inquiry would have raised the very interesting issue of the relationship between two very different ways of representing and "holding" the knowledge of statics: as formal, symbolic generalizations, or as what Professor Woodie Flowers of MIT's Mechanical Engineering Department calls a "visceral" feeling for the behavior of structures under load.

As it turned out, a former doctoral student of mine, Shahaf Gal, recently did a very nice piece of research based on Shaler's work. Gal studied 10 students who were participating in a bridge design contest--using Program B among other media--observing how they made use of these media and how their ways of using them interacted with their respective approaches to bridge design. But Shaler would have been unable, at least at the time of Project Athena, either to formulate or to carry out such a research program. So the problem of Shaler's tenure was reciprocal: not only was the institutional system so dedicated to technical rationality that it could not recognize the legitimacy of a particular version of action research, but the faculty member in question could not generate that research.

When I interviewed the dean of engineering about the issue of Shaler's tenure at the time, he said, "Our school has a 'second' tenure track. We have agreed that some people can get promoted and tenured on the basis of outstanding teaching." However, the second track existed in the realm of espoused theory; at the level of theory-in-use, it existed minimally if at all. The dean was not being disingenuous; he simply had never examined systematically what happened to tenure cases based on teaching excellence in the absence of what MIT calls "world-class research."

This first story of Project Athena raises several issues of importance for organizational change linked to the new scholarship.

First, it illustrates the need for a kind of organizational learning. People in a position to influence the institution's promotion and tenure processes would have to learn how the design of educational software can be seen as legitimate research--the scholarship of application, or teaching, or both. The institution also would have to learn how to critique such research, to create for it a community of inquiry capable of fostering an understanding of the kind of rigor appropriate to it--perhaps even to help younger faculty members learn how to do it.

Secondly, this story illustrates a gap that often exists between espoused educational policy and policy-in-use--the policy that is advanced in formal policy documents, verses the policy implicit in actual patterns of behavior--and the significance of that gap for the fate of faculty members who may seek to engage in the new scholarship.

And finally, this story suggests that the problem of introducing and legitimizing in the university the kinds of action research associated with the new scholarship is one not only of the institution but of the scholars themselves.

"We don't know anything about learning!" As we began to learn more from our case studies of Project Athena, we proposed to Athena's Executive Committee--then planning its next stage of activity--that MIT might get interested in carrying out studies of this kind on a continuing basis. We argued that it would be useful for Athena, in its continuing redesign, to learn more about what was actually happening in student and faculty experience--to discover, as we had with Shaler's two programs, not only what had not worked out as planned, but what unheralded benefits had occurred.

Our proposal was discussed at a meeting of the full Executive Committee. At that time, there was some readiness to think in new ways about Athena since, for a variety of reasons, many students and faculty members were up in arms about it. But in the midst of the discussion, a member of the administration who was also one of MIT's senior cognitive scientists made the following declaration: "We don't know anything about learning! Nobody does. And since we don't, it's ridiculous to talk about doing research on how the computer may help people learn science or engineering." Our proposal was dismissed, and as far as I know, MIT has never carried out further studies of student and faculty experience with Athena, although it has conducted surveys of Athena usage and initiated many technical improvements in the Athena network. The then-dean, who actually had some sympathy for our proposal, explained to me after the meeting that he could not possibly support it when a senior figure in the administration, a cognitive scientist, says we don't know anything about learning.

Where did such a statement come from? In a similar vein, an MIT philosopher once said to me, "We don't know anything about meaning," and I asked him, "Have you given up using the word?" Of course, he had not; nor had the senior administrator given up talking about learning. What he meant was that by the current standards of the discipline of cognitive science we know nothing about learning. What Turkle and I had in mind, however, was a kind of action research that enhances common sense, a form of inquiry that builds on and feeds back to modify what we already know-in-practice. Studies of this kind would proceed, as in our study of the use of Shaler's programs in civil engineering, through observations of students at work with the program, interviews with them about their experiences, and reflection on the data generated through such observation and interviewing. But the senior administrator made his judgment about our proposed studies in the light of the norms, methods, and objectives of his preferred version of cognitive science.

In other academic contexts, where similar disputes arise about faculty research on teaching and learning, the role played in the Athena story by cognitive science might be played by a normal-science version of social science. In such a context, a senior faculty member might argue that if you can't name the variables and measure their values, and if you can't create control groups or manage random assignment of subjects to treatment and control groups, then you can't possibly generate valid knowledge. In the absence of these conditions, he or she might argue, you're not doing rigorous research, it can't count as real scholarship, and it's not deserving of promotion or tenure.

But, of course, for any problem of interest to teaching and learning, insofar as it arises and is studied in the actual contexts of practice, one cannot establish true control groups, create random assignments, eliminate potentially confounding phenomena, or, in general, meet the standards of normal-science rigor. Hence, there can be no such thing as a "scholarship of teaching" unless we can change the rules that govern what counts both as legitimate knowledge and as appropriately rigorous research into teaching and learning. So it was, then, that our slow-moving battle of epistemologies attached itself to a decision about research on the educational uses of the computer.

CONCLUSION

The basic argument of this paper is simple, although its consequences are far from simple. The new forms of scholarship call for a new institutional epistemology. If the scholarship of synthesis, application, or teaching requires that the scholar contribute to knowledge according to norms shared and developed within a community of inquiry, then the new scholarship cannot achieve legitimacy within an institution exclusively dedicated to technical rationality--the epistemology around which the modern research university was originally established and which still underlies its key institutions.

The new forms of scholarship advocated by Boyer and others lie much closer to practice. They proceed through design inquiry, in the Deweyan sense. They are infused with a tacit knowing that their practitioners usually cannot describe (at least without observation and reflection devoted to that purpose), and they are inimical to the conditions of control and distance that are essential to technical rationality.

The epistemology appropriate to the new scholarship must make room for the practitioner's reflection in and on action. It must account for and legitimize not only the use of knowledge produced in the academy, but the practitioner's generation of actionable knowledge in the form of models or prototypes that can be carried over, by reflective transfer, to new practice situations. The new scholarship calls for an epistemology of reflective practice, which includes what Kurt Lewin described as action research. But in the modern research university and other institutions of higher education influenced by it, reflective practice in general, and action research in particular, are bound to be caught up in a battle with the prevailing epistemology of technical rationality.

In my admittedly special example of MIT's Project Athena, I have tried to show how the introduction of the kinds of inquiry inherent in the new scholarship are likely to encounter a double impediment: on the one hand, the power of disciplinary in-groups that have grown up around the dominant epistemology of the research universities; and on the other, the inability of those who might become new scholars to make their practice (of teaching and curriculum development, in this instance) into appropriately rigorous research.

In order to legitimize the new scholarship, higher education institutions will have to learn organizationally to open up the prevailing epistemology so as to foster new forms of reflective action research. This, in turn, requires building up communities of inquiry capable of criticizing such research and fostering its development. The story of Project Athena also suggests how the growing interest in design and its teaching, and in the educational potentials offered by computer, may be used as occasions for introducing and legitimizing the epistemology of reflective practice that underlies the new scholarship. In this way, we might begin to answer a question Everett Hughes, the distinguished sociologist of the professions, voiced in the early 1970s shortly before his death. At an MIT-sponsored meeting on professional education, Hughes pointed out that "American universities are products of the late 19th and early 20th centuries." Then he said, "The question is, how do you break them up in some way, at least get some group of young people who are free of them? How do you make them free to do something new and different?"

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