THE NEED FOR CLASSROOM RESEARCH

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Note: The purpose of this paper is to set forth the rationale for Classroom Research in the context of the research literature on college teaching. The paper serves as a springboard to discussion with various groups of interested professionals and also as a conceptual paper guiding the development of strategies and tools for Classroom Research.

The reports that constitute the educational reform movement of the 1980s have taken as their major mission the improvement of undergraduate education. Most of the recommendations have to do with what is taught, i.e. the curriculum. Some seem to think that is where the problem lies--that students don't learn what they should learn in college. I am inclined to think, however, that how students are taught is even more critical. What is taught is important, but how it is taught makes the difference between a lifelong learner and a grade grubber, between enthusiasm for learning and indifference to it, between an educated society and a credentialed one.

Our educational system is based on the belief that something important happens when teachers meet students in the classroom. Most of any institution's budget is allocated to costs of instruction, and yet instruction receives very little attention

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from college administrators--not because we don't think it is important, but because we don't quite know what to do about a number of things. College teachers, for instance, are authorities in their specialties. No one else at the institution knows quite as much about their particular specialties as they do, so there is an understandable reluctance to tell faculty what or how to teach. Moreoever, we in higher education equate academic freedom with the sanctity of the classroom, and there is a tradition of restraint in probing too deeply what goes on there. And finally, there are a set of age-old questions that have not been answered to the satisfaction of many--What constitutes effective teaching? Who should evaluate college teachers and how? Can good teaching be recognized and rewarded?

Riding on the back of a strong educational reform movement of public schooling comes public and legislative demand for a closer look at student learning in college. Today's high interest in institutional assessment is one way of demanding attention to what students are learning without actually entering the classroom. Yet, what the assessors really want to know is--What are students learning in college classrooms?

Most of the reform reports are not happy with the learning that goes on in college classrooms, but the blame is generally placed, not on instructors, but on societal and institutional values that result in what might be called the Rodney Dangerfield syndrome, "Teaching don't get no respect." Faculty don't give it much respect themselves; both personal and institutional prestige are associated more with the scholarship of faculty than with the scholarship of students.

The reform reports are harsh in their criticism of these attitudes. They blame the lack of attention given to teaching on everything from "a failure of nerve and faith" and "a vacuum in educational leadership" (Bennett, 1984) to "a misguided over-emphasis on research and a corresponding neglect of teaching" (Project on Redefining...1985). Graduate schools are charged with the production of "too many narrow specialists" (Bennett, 1984), and with "awarding the Ph.D. degree to generation after generation of potential professors professionally unprepared to teach" (Project on Redefining...1985). While there is plenty of dissatisfaction with the quality of instruction, there are few constructive suggestions for what to do about it.

Part of the problem, I think, is that there are two unexamined assumptions that underlie most of the educational reform movement. The first is that educational reform consists of making large highly visible policy decisions--the installation of statewide testing or incentive systems, for example, or the appointment of blue-ribbon commissions to study curricular reform. There is very little attention given to the potential impact of thousands of small classroom reforms that might add up to real and substantial change. Somehow we have convinced ourselves that there is more power in <u>making</u> decisions than in implementing them. So we worry about involving professors in policy decisions, but we fail to consider what each teacher acting in his or her own classroom might do to achieve reform.

The second assumption of the current educational reform movement is related to the first. We assume that if we really want action, it must come from the top--that state legislators,

campus administrators or some other authority should assume responsibility for creating a system of rewards and punishments that will shape the behavior of those we want to change --in this case college teachers.

Most of the reform reports assume, for example, that if the rewards for teaching were greater, more time and attention would be given to instruction, and it would improve. Without denying that greater rewards, more attention, and more value placed on teaching would undoubtedly help, it is by no means certain that we really know what to do to improve teaching. The notion that greater rewards will improve teaching assumes that professors know how to teach well, but are not doing so because there are other things more rewarding.

I am going to assume, for purposes of discussion that this is an ideal world, that the big dramatic policy decision has been made, and that good teaching will be appropriately rewarded. Now what?

First we have to decide what good teaching is. We are not exactly novices in discussing and researching that question. There is an extensive literature on what makes a good teacher. Basically, there are three ways to describe effective teachers: 1) We can describe their characteristics--training, experience, knowledge of subject matter, 2) We can describe their behaviors in the classroom--whether they ask provocative questions, call students by name, encourage discussion, etc. and 3) We can describe what students are able to do as a result of the teacher's efforts--how much they know, how well they think, their attitudes toward learning. In short, the literature consists of

descriptions of input, process, and output variables. The criterion problem is present, of course, in all of these approaches. In order to describe what a good teacher is, or does, or accomplishes, we need to know how we are defining a "good" teacher. A little reading of the literature on this issue quickly drives one to paraphrase the Supreme Court justice commenting on pornography, "I can't define good teaching, but I know it when I see it."

Well, maybe that is not such a bad position to take. All of us can identify good and poor teachers on our own campuses, and when researchers go onto a campus and start asking students, faculty, and administrators to identify the best teachers on campus, it doesn't take long to come up with a list that shows rather high agreement.

Much of the agreement, of course, comes from the heresay of student comments on teachers and courses. In recent years, the "heresay" has been formalized into systematic student ratings, and student evaluations of teaching have been widely adopted nationwide. In a recent survey, Peter Seldin (1984) found that 98 percent of four-year liberal arts colleges claimed that classroom teaching is a "major factor" in evaluating faculty, and more than two-thirds said that student evaluation is "always used" in evaluating teaching. That represents substantial growth in recent years in confidence in student evaluations; just over half of the colleges were as dependent on student ratings in 1978.

To the credit of higher education, the use of student ratings increased in proportion to the positive findings from research on the reliability and validity of student evaluations.

Although researchers can still manage to raise new questions and new reservations, I think the judgment is fairly well in by now. Students are reliable observers; they have ample opportunity to see teachers in action on good days and bad, and they are in a good position to evaluate the impact of the teaching on themselves as learners.

Moreover, the evidence suggests that student ratings have good validity, i.e. that students tend to rate most highly those courses in which they learn the most. Centra (1977) found correlations in the .60s and .70s between scores on final exams and student ratings of "overall teaching effectiveness" and "value of the course." While one can still find reported correlations ranging from negative to high positive, the tilt of comprehensive, well-designed studies--and the more recent meta-analyses (Cohen, 1982)--is clearly toward significant positive correlations between student achievement and positive course ratings.

Another test of the validity of student ratings is to relate student ratings to teacher behaviors in the classroom. Murray (1985) found that teachers who received high student ratings did, according to neutral classroom observers, teach differently from teachers receiving low ratings.

Students also tend to be reliable and relatively unbiased raters. There is no evidence to support the myths that popular teachers are mere showmen, that the mature perspective of alumni will find virtues in the professors that were not respected ten years earlier, that teachers who are tough graders will receive low ratings, or that agreement on the identity of

good teachers is difficult to achieve (Gaff and Wilson, 1971).

One answer to those who are ready to reward good teaching if it can be identified is that students know good teaching when they see it. But students can go further; they can describe the characteristics and behaviors of good teachers. The research, by this time, is fairly consistent on what students consider important factors in effective teaching. Feldman (1976) reviewed a group of studies in which students were asked to describe "good" or "ideal" or "best" teachers. He found eight characteristics that were usually ranked high in all studies: concern for students, knowledge of subject matter, stimulation of interest, availability, encouragement of discussion, ability to explain clearly, enthusiasm, and preparation. There is nothing at all surprising about his findings. These characteristics turn up over and over again in one form or another when students are asked to describe good teachers.

Factor analytic studies of student rating forms show rather similar clusters of characteristics. Feldman (1976) reviewed nearly 60 factor analytic studies and concluded that there were three major clusters in effective teaching--the instructor's ability to present the material, to encourage students to learn, and to regulate and deal fairly with students. Kulik and McKeachie (1975) reviewed eleven factor analytic studies of teacher rating scales and found similar factors which they labeled as follows: "Skill," which represents the ability to communicate in an interesting way, to stimulate intellectual curiosity, and to explain clearly, "Rapport" which involves empathy, interaction with and concern for students, "Structure"

which concerns organization and presentation of course material, and "Overload" which refers to the workload and instructor demands (Abrami, 1985).

I find all of this quite credible, and I have no difficulty believing that teachers who have these characteristics, not only rate high with students, but probably are good teachers. Since student evaluations are far and away the most common form of teacher evaluation at the college level, teacher effectiveness is currently being defined and determined by a combination of researchers, who decide which items should go into the rating scales, and by students who decide which items will be important.

The next five years will probably see the rapid growth of another set of judges and definers of good teaching. The assessment of student learning movement--for surely it can be called "a movement" now--purports to use student outcomes as the measure of educational effectiveness. At worst, this will put the definition and reward of good teaching in the hands of external agencies who will decide what students should know and how it is to be measured. At best, it will call attention to the goals of instruction and how well they are being accomplished. Statewide testing of student achievement is certainly on the increase, and while it is quite unlikely to determine an individual teacher's future, it is possible that teachers will be encouraged to "teach to the test." If the test really measures what students should learn in college, that may not be all bad, but few have that much confidence in our current measures of learning.

The assessment movement underway now has ambitious goals but quite modest accomplishments. Almost everyone would like to

measure a wide range of student outcomes, affective as well as cognitive, to develop in teachers the insight and motivation for instructional self-improvement, and to integrate assessment into the instructional process. However, what we have to date in most places is the measurement of a narrow band of fairly low-level cognitive skills. While current practices seem a long way from the ideal, the search must be undertaken. Assessment is the first step to improvement.

Yet a third set of judges of the criteria for teaching effectiveness are educational researchers. Educational researchers go about determining teacher effectiveness in a variety of ways. They count; they observe; they conduct experiments; they write ethnographic or naturalistic descriptions; they survey other people. While some would claim that researchers don't determine the criteria for teaching effectiveness and that their task is to describe what exists without imposing their own values on the data, that is not what really happens.

The major value that researchers impose on the search for criteria of teaching effectiveness is that the findings must be generalizable, that is to say, not specific to any particular classroom. The methods of traditional social science research--sampling, tests for significance, control of variables, and the like--are devised largely to prune out situation-specific influences, leaving those characteristics common to all or most effective teachers. Yet, some of the most effective teachers any of us can remember were effective because their unique characteristics worked in very specific situations. The search for criteria for teaching effectiveness that has been conducted with consider-

able energy and earnestness by researchers over the past decade is helpful in showing us what effective teachers have in common, but it masks some of the most useful information, specifically any insight into how individuals with their infinite variety and unique values and interests develop into effective teachers in a situation-specific classroom.

I have taken some time to review the current status of research on the criteria for evaluating the quality of instruction because I want now to look at what's missing and how we can find it and apply it to the improvement of undergraduate instruction.

The first thing that is missing from the literature is some good discussion of what teachers are trying to accomplish. Students, legislators, and researchers all have entrees to defining the criteria for good teaching. But, in my survey of some 200 articles and books on college teaching, I came across only two studies that asked teachers what they wanted students to learn from them. True, teachers serve on committees to develop achievement tests; they serve on curriculum committees; they are frequently polled regarding hours spent in class preparation, perceived rewards for teaching, and attitudes about the policies of their institutions. But they are rarely asked what they are trying to do in their own classrooms.

The first step then, I should think, in improving undergraduate instruction is to find out what teachers are trying to do. If that is not what they should be doing or if their aspirations are not high enough, that's one thing. But if they do not or cannot accomplish the goals they set for themselves, then that

is a different problem.

The second element that is missing from the literature on effective teaching is a constructive approach to applying research to improve practice. I specify "constructive approach" because, over the years, there has been a great deal of analysis and criticism of the failure of practitioners to use research, on the one hand, and the failure of researchers to work on useful questions, on the other. In my early career in educational research, I headed a fledgling effort at the University of California Center for Research and Development in Higher Education to "develop" the research findings of the Center so that they were useful to practitioners. It's taken twenty years to convince me that bridging the gap between researchers and practitioners is, for the most part, not a productive enterprise. By and large, the purpose of most research in the social sciences is to push back the frontiers of knowledge--to help build the foundations for understanding. It is not to improve practice, and forcing academic research into a practical mode is rarely successful. Most of us have read with some embarrassment the "implications for practice" sections of many a research report which strains mightily to convince readers that the research has some immediate practical payoff.

My point is not that traditional research in the social sciences is not useful or helpful or that it should not be pursued by those who have questions building upon the work of other researchers and arising out of theory that may open windows of insight into human behavior. Elliott Eisner (1984, p.261) points out, however, that theory is both an asset and a liability. It

is an asset when it provides the windows through which we obtain focus, but it is a liability when it creates walls that hamper our perception of processes that are not addressed by the concepts we have chosen for our theories. Current educational theory has opened many windows, but practicing teachers all too often find themselves staring at the walls that block insight into real-life classroom operations.

If our purpose is to improve the quality of teaching in the average classroom, then cognitive psychology has little to offer. The questions for cognitive psychologists and their methods of investigation and analysis are not designed to address the questions that teachers have. As many a college student will eagerly testify, those with the greatest research knowledge about cognitive psychology do not--and I would contend probably cannot --apply their knowledge about "schematic knowledge structures" and "limited processing capacity" to their own classroom teaching.

Eisner (1984, p.256) observes that in his experience as a Stanford faculty member serving on committees and making decisions of various kinds, "those who are best informed about educational research seldom use the fruits of their labors either to make practical decisions or to shape institutional policy within the institutions where they work. If educational researchers do not use research findings to guide their own professional decisions, why should we expect those less well informed to use research findings to guide theirs?" Why indeed? It appears that there is a need to find or devise an approach to research that is deliberately designed to improve classroom practice.

The third thing that is missing is a literature on how to conduct research in and on the classroom, with its inevitable variations in teachers, students, and subject matter. An articulate group of critics of traditional educational research is beginning to be heard promoting various alternatives, such as ethnographic research, naturalistic inquiry, action research, qualitative methods, and reflective practice (See, for examples, Guba, 1979; Guba and Lincoln, 1981; Eisner, 1980, 1984; Argyris, et al. 1985; Schön, 1983; Stiggins, 1985). This is a scattered but promising development, one that should add valuable perspectives to the search for knowledge about teaching and learning.

But naturalistic inquiry, ethnographic research, and the other new alternatives to quantitative, experimental research, for all their value--and it is considerable--are not the answer to a research approach to the improvement of teaching either. Many of their rules and conventions are no more applicable to the improvement of classroom practices than those of quantitative and experimental research. Naturalistic evaluation, for example, requires "lengthy and prolonged" engagement by a highly trained researcher (Williams, 1986), and part of the value of ethnography comes from the notion that findings should "evolve" from the study rather than be interpreted as "answers" to questions formulated by researchers (Smith, 1982). Perhaps Donald Schön's conception of the "reflective practitioner" comes closest to addressing the needs of practitioners for knowledge about their own behavior, but a classroom teacher needs actual data on what students are learning as well as the ability to reflect on teaching practices.

Perhaps we could simply work harder and write more (perish the thought) to fill in the missing pieces that I have identified from my review of the literature on effective teaching, but I suggest that it is time to develop a different approach, specifically designed for what we want to accomplish, namely, the improvement of undergraduate instruction. I am not suggesting that we give up traditional research of that we cease trying to devise reward policies that will shape the behavior of college teachers. What I am urging is that we make a major commitment to place primary responsibility for investigation and implementation activities with individual teachers in their own classrooms. I call this approach Classroom Research.

The purpose of classroom research is to improve the quality of classroom teaching and to foster intellectual stimulation and professional renewal for college teachers. The concept of Classroom Research springs from six basic assumptions:

1) That the quality of student learning is directly related to the quality of instruction.

2) That teachers need to know what their students are learning in their classrooms.

3) That inquiry and intellectual challenge are sources of professional renewal for teachers.

4) That the research most likely to improve instruction is that conducted by classroom teachers formulating and investigating questions that they want answered.

5) That self-improvement is most likely to result from specific feedback relevant to one's own goals and behaviors.

6) That there is nothing so mysterious or esoteric about research on college teaching that it cannot be done by anyone capable of teaching at the college level.

If we are to accomplish the purposes of Classroom Research, I believe that we must put teachers in charge of the research. While I have no objection to the optimism of many that collaborative research--in which researchers and teachers work closely together--is the answer, I confess that I do not share their optimism.

In the first place, collaborative research is too likely to place the researcher in charge and the teacher in the role of cooperating assistant or consultant. The teacher is frequently intimidated by the technical expertise of the researcher into shaping questions that can be investigated by traditional educational research methods. Traditional researchers have all too often preferred a trivial question with a sophisticated design over an important question involving a messy methodology. Since the role of the researcher in collaborative research is to shape a question that can be investigated by traditional methods, the collaboration may be counterproductive to what classroom research should accomplish.

Secondly, classroom research needs to develop its own distinctive methodology. Training teachers in the methods of traditional educational research is not what I mean by classroom research. The purpose of classroom research is to study the effectiveness of a particular teacher, working with a known group of students, on a given subject matter; it is not to search for

general "truths." While this may, at first blush, appear to result in knowledge with extremely limited usefulness to the profession of teaching, my guess is that the accumulation of knowledge from many classrooms will give more actual insight into the teaching/learning process than the search for "average" composites of learning across a variety of students, teachers, and subject matters. In short, I think we are more likely to understand student learning by studying one classroom or one student in depth than by averaging the problems and styles of many students across many classrooms. Moreover, I think it highly likely that the knowledge gained from doing research is more likely to be used than that gained from reading about research.

I suggest that the implementation of classroom research should begin with experienced teachers in the field, but I also think that every graduate student planning to teach any subject in any college should demonstrate competency in conducting investigations into the effectiveness of his or her own teaching. New graduate courses need to be designed, new methods devised, new perspectives developed.

Let me give a few examples of what some possible classroom research projects might look like.

First, I would like to give an example of the contrast between how a classroom researcher and a traditionally trained researcher might approach a similar problem. Let us assume that the problem is the familiar one of dropouts. In a traditional study of the dropout problem, the researcher typically selects representative samples of dropouts and persisters, and after collecting data from student records, determines the differences

between persisters and dropouts, inevitably concluding that dropouts come from lower socioeconomic backgrounds, made lower grades in high school, work more hours off campus, are commuters, and have lower educational expectations.

While these findings are verified so frequently by researchers that we have to conclude that they are indeed factors in dropping out of college, all of the factors identified are what Ben Bloom (1980) would call "unalterable variables." There is nothing that educators can do to change them.

Now let us see how a classroom researcher might study this problem. Let us assume that our classroom researcher is curious about the dropout problem, decides to interview some students who stopped coming to class, and finds out that a certain amount of discouragement sets in as the semester's work begins to build. As she reflects on this observation, it occurs to her that she usually hits her stride as a teacher about the fifth week of the semester and feels ready to tackle some of the more difficult units about that time. She notes that the high dropout in her own classes occurs about five weeks into the school year, and she concludes that she might try a number of things in her own classroom to reduce needless dropouts--perhaps give an especially satisfying assignment, maybe rework or reschedule the difficult unit, maybe call in a few students and talk with them about the unit or about the class, perhaps offer special encouragement, make a referral, drop a note, make a call.

The procedure of the classroom researcher is to take the most direct approach to the problem--in this case to interview students, listen sensitively, and reflect on classroom practices.

After gaining some insights, the classroom teacher might try some tentative solutions, refine the questions for investigation, then try more solutions in a thoroughly integrated approach to understanding and addressing the problem.

Faculty meetings might well be designed around classroom research projects to share data, perceptions, and possible solutions. The emphasis in faculty meetings should be based on data and systematic observation; discussion might appropriately range from sharing useful and creative approaches to gathering data, to data analysis, to recommendations for possible changes in policies and practices within the department. One of the important side effects of classroom research is that it has the potential for adding professional stimulation to faculty meetings.

I cite this example of a department-wide project to show that classroom research need not be a solitary activity. The questions raised in classroom research may be shared by a variety of others--departmental colleagues, the faculty of a college, disciplinary peers nationwide, or any other group of educators with a common question.

At the same time, classroom research is appropriate to the study of a unique problem occurring in an individual classroom--or perhaps it is not a "problem" at all, but rather a matter of curiosity to the teacher. Examples of this sort of project are numerous. Perhaps the teacher wants to know whether a "review session" prior to the mid-term helps in long-term retention or is only useful for immediate test score gains. Or perhaps the teacher is interested in knowing whether a field trip is worth the effort in changing attitudes about a particular

social problem--or would reading about it or discussing it or seeing a dramatization on videotape do as well or better? Maybe a math teacher, interested in teaching a particular math concept, reads about another teacher's method and decides to test it in his own classroom.

The projects for classroom research are limited only by teachers' imaginations. While the examples I have presented do not generally call for complicated methodologies or analyses, there is nothing to prevent interested teachers from studying very complex learning problems.

In conclusion, I think it is time to get classroom teachers involved in the study of teaching and learning. They should be intellectually curious about it as well as professionally involved in the improvement of their own teaching practices. While classroom research can be done now by any teacher with the appropriate curiosity and motivation, I believe that if classroom research is to help all of us, there should be standards for the quality of the research. We have come a long way over the past fifty years, and there is much that we have learned about the study of human behavior. My plea is not to throw out the old, but to build a new science of classroom research on the foundation of what we now know about research methods and application--with the important caution that we keep classroom research true to its own purposes, namely improving the practice of classroom teaching.

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