Graduate Education in the Chemical Sciences — Issues for the 21st Century

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Graduate Education in Chemistry: A Personal Perspective on Where It Has Been and Where It Might Go

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Graduate education in chemistry, and in the physical sciences generally, has changed so gradually that those of us deeply engaged in it for most of our professional careers almost do not perceive the changes. Nevertheless, during the interval from the time senior members of the community, ourselves that is, were students until now, when we approach the last years of our teaching careers, there have been significant changes that we should recognize. Some are clearly for the better; some, essentially neutral in value, are adaptations to changes in the surrounding society; and some appear to pose serious problems.

I will describe my own perceptions and experiences in terms of the evolution of how I and, to some extent, my colleagues have worked with graduate students since we began. I will even interject a bit of recollection of my own graduate-student experience. Then I will turn to the issues of what is good, what is neutral, and what is worrisome.

HOW HAS GRADUATE EDUCATION IN CHEMISTRY CHANGED?

One evolutionary change in graduate education in chemistry that people frequently cite is the increase in research that crosses disciplinary lines. It is fashionable to establish formal interdisciplinary programs; sometimes it is almost mandatory in order to receive funding. As I reflect on what people were doing when I was a graduate student, I am not altogether convinced that there has been much change in the research, or in the perspectives, of people working across disciplinary lines. Konrad Bloch, Frank Westheimer, and Paul Doty were among a large community working during the 1950s and 1960s in the borderland between chemistry and what is now molecular biology. Linus Pauling's work on structures of biopolymers was certainly of this kind. Atmospheric scientists were comfortable doing all sorts of chemistry in the context of the behavior of substances in air. Chemistry of air pollutants was already such a well-developed subject when Sherwood Rowland decided it was time to do some research in the subject that he had to look for exotic species, and settled on fluorine. The subject of atomic and molecular collisions evolved primarily in physics but, with the work of Sheldon Datz and Ellison Taylor and of Philip B. Moon and associates in the mid-1950s, physics and chemistry developed a field that merged the two disciplines. Further from chemistry, in hydrology and geography, Gilbert White was paying no attention to the traditional bounds of any academic subject as he studied how we use and might use water. In other words, I would like to challenge the dogma that science has increasingly become more interdisciplinary. What I propose is that we have merely found it fashionable to institutionalize cross-disciplinary activity that was already happening and evolving in a healthy manner.

Suppose we grant that my challenge contains at least a little truth. What do we accomplish by institutionalizing interdisciplinary research? One change I suspect has happened, but would find it very difficult to support with hard evidence, has to do with another kind of change closely related to this interdisciplinary issue. While people were working across borderlines 40 years ago and more, other people were working in some subfields of chemistry that had become "mature" in the most pejorative sense of that word. In that period, many of my contemporaries and I looked on classical analytical chemistry and electrochemistry as fields supported by an ingrown, myopic community that had lost a larger vision. Analytical chemistry evolved out of that slough and became a much livelier subject when it looked beyond its traditional boundaries and decided that those boundaries had become irrelevant. Rather, the fundamental questions of how to make reliable determinations of what was present and how it behaved became again the focus of the subject.

Another subject that collected specialists who turned from fundamental scientific problems to solving challenging puzzles was, for some years, the synthesis of natural products. It, too, became a "mature" subject with its own rules. When Gobind Khorana used bugs to synthesize intermediates for his eventual synthesis of Coenzyme A, he violated the rules of the game and was considered déclassé for doing so. Again, synthetic organic chemistry has outgrown that stage and is again a healthy science, in part because its practitioners have looked outside traditional disciplinary bounds and flirt regularly with materials scientists. In what field do they work? I consider that an irrelevant question. It is more important to ask, What interesting and important problems are they addressing, and have they the skills (or access to the skills) appropriate to those problems?

One other "mature" area comes to mind that, according to my prejudices, has not yet emerged from its slough. This opinion will raise hackles, notably among its practitioners, who have been slow to recognize the maturation. This is the area of quantum chemistry, the application of computational methods to determine the properties of atoms and molecules (and sometimes, for the more interdisciplinary sorts, of solids and even liquids and polymers) by solving quantum mechanical equations. I would certainly not deny that people in this field have been making progress with the methods for doing such calculations. What I would raise is the question of whether the problems being addressed by that rather closed community have evolved into problems "within the club" and have become problems whose answers are of little interest to the rest of the scientific world. Can that community explain persuasively why their achievements are interesting to people working in materials science, or molecular biology, or atmospheric chemistry, or fundamental quantum mechanics? But there are fundamental, important, and potentially productive questions standing open in this field. Can anyone recast the formally exact density functional theory developed by Pierre Hohenberg and Walter Kohn, starting with first principles, in a form whose lowest approximation is the ubiquitous (but still only ad hoc) local-density approximation, that would also reveal how to make further refinements of this powerful approach?

Thus, I infer that interdisciplinarity has had a healthy effect in pulling "mature" disciplines back into true scientific import. Institutionalizing the interdisciplinarity is a way of making it financially attractive to look beyond traditional bounds. Whether such institutionalizing has also increased true intellectual interactions is a question I cannot answer, but as I indicated, I am somewhat skeptical that it has. To establish whether it has would require a serious survey, and even if conducted the survey would be suspect on the basis that it might be trying to find a particular answer. Certainly we can find many examples of productive institutional interdisciplinary structures, and we cannot expect to find people eager to point out cases in which having the institutional structure has not been successful. Unfortunately, those less successful examples, the cases people are reluctant to discuss, are the cases from which we would learn the most.

If we examine how institutional structures affect the scientific enterprise, we must be prepared to go about it objectively, i.e., we must look for both the positive and negative consequences for science and education of institutionalized supporting structures with specific goals. We must look at not only those structures targeted at interdisciplinary work but also those designed for traditional disciplines. Are we better off with designated, targeted institutions, or with an informal, fluid enterprise? Do narrowly defined structures such as "centers," whether funded or unfunded, contribute more or less to science and science education than larger, more broadly oriented, fluid organizations?

Two more of the most apparent (and related) changes in the U.S. chemistry graduate programs have been the increase in the length of time from entry to doctorate and the increase in the length of doctoral theses. Typical graduate careers, B.A. to Ph.D., lasted about 4 to 4 1/2 years in the 1950s and 1960s; now 5 to 6 years is more typical. The doctoral theses on my shelves were of the order of 80 to 130 pages into the 1970s but are typically about 200 pages or more now.

These are symptoms of change. What changes have been responsible for them? An obvious and rather trivial contributor is the use of the computer as a word processor, so that the cost of doing a long thesis is negligibly greater than the cost of doing a brief thesis. This contrasts with the situation of having to pay a typist, on a per-page basis, to put a thesis into acceptable form. Furthermore, it is vastly easier to write and revise a text with a word processor, so that we might expect the writing quality of the theses now to be higher than it was in the 1950s and 1960s. I am not convinced that has happened, but I hope someone does a doctoral dissertation on that subject so I can find out. One thing we can expect to improve with computerization of the scientific literature, something just now at its threshold, is the quality of the scholarship, specifically the accuracy of attributions and historical background. As the archives of journals are converted into searchable electronic forms, we can justifiably expect students to find and cite early works directly relevant to their research. The barriers to doing such searches have been significant, so much so that we have all heard outraged criticisms of how the literature has been overlooked, especially literature in languages other than English.

Another reason often given for the growing duration of the graduate career is the need to know much more than was needed 30 or 40 years ago. Certainly the body of scientific knowledge is vastly larger than it was in 1950 or 1960. We sometimes rationalize 6 years of graduate work on the basis that a student finishing a Ph.D. must know about the same fraction of the literature that was expected of a fresh Ph.D. in 1960. Hence, we argue that students must spend more time in school to absorb all that material. If this is so, should it be adequate justification for the increase in time to the Ph.D.? This takes us past the realm of identifying changes into the subject of the next section dealing with what graduate education should achieve.

The collaborations between universities and private firms have been growing rapidly and often involve graduate students. This has been a significant change in graduate education in chemistry and in the relationships among institutions and individuals. The history of this involvement is tied closely to the evolution of the research industries in America and to the perceptions in Congress of the role of government in assisting the evolution of science and technology. As a result of the apparently transient fad of industries divesting themselves of much of their longer-term research programs and seeking outside sources to do that research, the universities appeared to offer natural, low-cost alternatives. The most obvious symptom of the mood of Congress was the passage of the Bayh-Dole Act, which allows patenting of results of research done with government grants. The result has been a willingness of university scientists and university administrators to establish collaborations with industries. This has led to a number of changes that are discussed in the final section of this paper.

WHAT SHOULD GRADUATE EDUCATION ACHIEVE?

Let us return to the questions stimulated by our asking why graduate education programs in chemistry and other sciences take significantly longer now than 40 years ago. We may begin with a traditional view of the course of a career in science. The graduate student is probably as close to being a traditional apprentice as anyone in modern life. The graduate student works under the direction of a master, who guides and trains and, we hope, educates the apprentice until the student crosses the threshold of an advanced degree, normally the Ph.D. The student-apprentice has become a journeyman.

Here we can clarify a common misunderstanding, the bifurcation that is sometimes made between education and research. In truth, all true research is education in that it is neither simple training, if it is under the direction of someone else, nor a pursuit divorced from any impact on the researcher. Education includes any kind of experience that leads us to modify our behavior. It includes more than research, e.g., pedagogical classroom processes, but certainly the process of conducting research causes us to modify our behavior and our thinking continuously, as we carry out our investigations.

The journeyman stage of a scientific career may be unclearly defined if the person goes immediately to an industrial or government job, but even there the newly hired scientist is likely to go through a sort of training period or interval under some supervision. If the fresh Ph.D. goes instead into academia, then the journeyman stage is easy to recognize as the period between receipt of the doctorate and the achievement of tenure. One change that has happened to this part of the career path between 1950 and 2000 is the transformation of the postdoctoral stage from being exceptional to being almost mandatory. That transformation occurred about the time of the great expansion of support for science, the period following Sputnik. The journeyman stage now happens in two stages—that of the postdoctoral, even of two or more postdoctoral periods in different groups, and that of the junior faculty member. The journeyman is expected, as in medieval society, to be able to produce work that may well be of the same quality as the master's. But the journeyman is under a kind of scrutiny and evaluation that the master, the tenured faculty member, does not have to endure.

Graduate education, then, turns a novice into an apprentice and eventually into a journeyman scientist. At that stage, we expect the fresh Ph.D. to be in full professional command of at least one subject, perhaps a very narrow one. More precisely put, we expect that person to know more than anyone else in the world about the specific subject of the doctoral dissertation. We also expect that the experience of doing the research for the dissertation, and then writing and defending the dissertation, have educated the person to the stage of being able to invent or recognize, and then pursue, new research problems to their conclusion. This implies that the Ph.D. education included learning how to teach oneself new subjects and to do this well enough to use those subjects in new research.

We also expect those graduates who continue in academia to have learned something about teaching science as part of their apprenticeship. Most but not all graduate students now spend some time as teaching assistants. I think every scientist would agree that there is no better way to deepen one's understanding of a subject than to teach it. The teaching experience is not only important for those who become professors; it is also a part of the apprenticeship that strengthens the foundations of insight for all graduate students.

Graduate education serves another purpose that I would like to believe fits neatly with the apprenticeship. That purpose is to work with the research director on problems that are part of that faculty member's research program, with the goal of furthering that research. We in the world of academic natural science believe that working on problems that are part of our research programs is an excellent way for graduate students to learn the process of doing research, as well as for helping us investigate problems we think are interesting and important. (Incidentally, neither mathematics nor the humanities operate this way. It is far more usual in those fields for graduate students to choose problems of their own, which may be unrelated to any specific interests of the faculty supervisor.)

This apparent symbiosis deserves critical examination. It may be that it is indeed a healthy and productive educational pattern. It is also possible, however, that it has some weaknesses and flaws. Pressure bears heavily on faculty members to raise the grant support required to carry out a research program. "Produce!" is the cry, and producing results is a necessary if not sufficient condition to obtain funding. This means that faculty members may be subtly tempted to ask for (or demand) a considerable body of research results from students before allowing them to submit their theses. This temptation, if it exists, is obviously greatest when the student has become skilled in the research. We must look hard and critically at our behavior and ask whether some of the increase in the time spent in graduate school could be caused by demanding more of our students than our mentors expected of us. That in itself, though, is not necessarily good or bad. If we, however, answer "yes" to that question, we must go to the next question, "Are students aided or impaired in their careers, their productivity, and their creativity by our having those greater expectations?" I raise these issues not because I know the answers but because we, as a community, are obliged to find answers if we are serious about finding better ways to educate our graduate students.

WHAT DO UNIVERSITIES PROVIDE TO INDUSTRIES?

We know the answers to the reverse question. Industries provide employment in which graduates can find satisfying careers, and they sometimes provide financial support for students or for research in graduate schools. These two, and the way I have phrased them, lead us directly to an important point that we must keep clearly in mind. *The primary product that universities provide to industry is skilled, educated people.* The young scientists who take their Ph.D.s to their new industrial posts are superexperts in narrow fields that may have nothing directly to do with their new responsibilities. That narrow expertise, however, is almost never what matters to the new employer. What matters is the ability of the young scientist to meet new problems, to learn new material, and to have the judgment required to decide what to keep of old methods and what to introduce that is new. These are the qualities that make our graduates valuable to the firms that employ them, and they are the qualities that make those graduates our most important product.

Yes, sometimes a secondarily valuable product is the result of the research done in the university. This has become more valuable with the Bayh-Dole Act and the opportunity to capture private benefit from the results of public funding. This attraction, and the drift of academic interests in some areas toward problems whose answers may take the form of, or lead directly to, profit-making products, have led to a significant increase in collaborations between industries and academic researchers. In some subjects, these collaborations involve consortia of private firms working with one or several universities. In other subjects, the collaborations are generally between a single firm and a single research group. The former tend to operate in about the same manner as academic groups that have no such collaborations, following the same sociology and rules of behavior as university researchers have for many years. The latter tend toward industry-like behavior, meaning that proprietary concerns play a larger role in the one-on-one collaborations than they do in relations involving consortia.

The inducements are strong on both sides to establish collaborations. From the industrial perspective, the universities provide fresh talent from the students and high skills (and even sometimes high ability) from the experienced faculty. They also provide cheap labor. Students are paid far less than industrial researchers. The collaboration with a strong university group is a low-cost way for industry to outsource research. On the other side, university researchers and administrators see industrial collaboration as an attractive way to get funding for research. Hence, both parties are eager to collaborate.

What consequences may such collaborations have beyond the immediate ones seen by the participants? In the case of the open-style collaborations with consortia, probably only the beneficial consequences of educating the students to the industrial perspective and, if the industrial side also contributes participants, some education in the other direction. On the other hand, those one-on-one collaborations that operate under the restrictive rules of proprietary secrecy bring into the university something foreign to that environment. Secrecy and refusal to communicate and discuss research work are inimical to the functioning of a university. The stories of research groups in the same department whose students cannot discuss their work with one another illustrate the way the rules of proprietorship can erode the very heart of the academic experience. Open, candid discourse is essential for the education of the students, who are our primary product. We must find a way to establish and accept a set of rules of self-governance that will keep alive the productive industry-university collaborative interactions and in a way that maintains harmony with how universities must function in order to attain their primary purpose. We must not lose sight of our greater responsibility in the zeal to achieve second-level aspirations. Yes, each of us as a scientist considers our own research the most important thing we do, but we act not only as scientists. We are also professors, meaning that we are teachers, with a primary responsibility to educate. Only in an atmosphere of candid scrutiny and criticism, of open discourse, can we maximize the fulfillment of that responsibility. That is how we shall achieve the goals of education that Peter Eisenberger, Edel Wasserman, and, in his comments, David Oxtoby have set for us.

DISCUSSION

Ronald Breslow, Columbia University: All three speakers in this session have referred to the issue of time limits for obtaining a Ph.D. I think this group has got to search within its hearts to find out how we ended up with an unlimited Ph.D. program. We say science is moving, so there is more to learn, and therefore we need more time. The field of medicine is also moving; however, it still takes only 4 years to get an M.D. Whom are we kidding? It should be possible to complete a Ph.D. degree within a well-defined timetable. At Columbia, we have a limit of 5 years on our Ph.D. degrees and that is it. That was done, in part, to protect the students and, in part, to get the students moving. A lot of students feel comfortable in graduate school. They need a push to leave. Now, they know they have got to be out in a certain time. They had better start thinking about their future, planning their postdocs, or starting to interview for jobs.

Frankly, the time limit is also there to protect the students against some faculty—faculty who are no longer with us, who kept students on for 7, 8, or 9 years. It was preposterous. It is not fair to the students. The idea that you establish a well-defined time makes sense in every respect except one, and that is the faculty's own self-interest. We have to worry about that motivation. John Kenneth Galbraith, a well-known Democrat, once defined modern Republicanism as an attempt to give a philosophical justification to selfishness. Let's make sure that we do not do that here, by arguing that there is a philosophical reason for the Ph.D. program without a time limit.

Second, we talk about how to stimulate innovation and creative thinking. There are ways to do this, and we have to make sure that we expose the students to them. Most institutions that I know, but not all, require original research proposals from the students. Those original proposals have two functions.

First, they give students a sense that there is an area that they know more about than the faculty, because they have looked into it and have come up with ideas. I had one student who went to a major university with a research proposal that she had made. She pursued that research proposal in her independent research career, got tenure, and is now one of the major figures in an Ivy League university as a result of that proposal. This is not an unusual situation. The requirement of a research proposal also stimulates students to deal with science in a whole new way. They attend seminars, not passively listening, but thinking, what can I do with what I am hearing? It stimulates them to listen; it stimulates them to read. It is an incredible educational device, and it is preposterous that everybody doesn't use it.

We also require that a student give a seminar on a topic not related to their research to the whole group, faculty and students, within, say, the organic division. They have to look into the literature to prepare for this, and that is often the background of the research proposal. Seminars that they give are not just practice in speaking. They also give the students a chance to extend their education and become an expert in something.

One of the speakers from industry said that what he had learned in graduate school turned out to be useful for about the first 3 years, and then he went on and he needed other skills. My Euclidian geometry probably didn't even last that long, in terms of my application of it. As a component of education, however, it was absolutely critical. I think what we learn in graduate school is absolutely critical as an education: you can take a problem that looks impossible, address it, and solve it. That experience is important. I would not discount the role of graduate education, even if, in industry, people go into different areas. They can go into them with full confidence that they are competent scientists, as good as anybody else in the company, and are able to relate to anyone on an equal basis because they have a solid education behind them.

Finally, I want to raise the question of fellowships. Fellowships are important, and they have been cut back significantly. Steve was talking about what life was like when we were in graduate school. Many people had fellowships. The result is that you had your own money, so you could pick your own sponsor without asking whether the sponsor could support you. A fellowship means other things, too. Fellowships are a signal to the students that they are winners, that they have been selected as having the abilities that promise future success. I don't think you should discount that. We worry about why more Americans don't go into science. If they were winning fellowships to go to graduate school, it would be a tremendous stimulus to them, compared to going in and asking a faculty member to support them. It is a different kind of mind-set.

P. Wyn Jennings, National Science Foundation: I wanted to add a few things, particularly since I am managing the Integrative Graduate Education and Research Traineeship (IGERT) program. One issue that I would like to bring forward is globalization. One of the major concerns is the lack of globalization in American graduate students and whether or not U.S. students are competitive internationally. We know that they are competitive, at least in my field of chemistry.

Another point is diversity. I want to emphasize that this is a major issue from several points of view, both practical and philosophical. If you disenfranchise any segment of the population, it has serious and national consequences. One final point I want to emphasize is that IGERT is an experiment at the National Science Foundation. We are asking you to experiment with graduate education. We have no prescriptive rules other than to be multidisciplinary. We want you to experiment.

R. Stephen Berry: Let me respond to two particular points that you raised. First, from my perspective globalization is not a serious problem, because I see a vastly more international population now not only at the postdoctoral level but also at the graduate-student level in the United States, and it is not just postdocs who are going to stay in this country but also those who are going to return. Our graduate students now are interacting closely with many people from other countries. I don't think we have to worry too heavily about that.

On the diversity issue, I don't think this meeting is an effective forum in which to field that issue. Graduate school is too far along on the education path for us to ask about reasons for women and minorities choosing to study science and engineering. The real choice occurs when they go to college, or earlier. What is it that turns people off at much younger ages? At the graduate level, we are already dealing with a small pool that is much smaller than most of us believe that it ought to be. While we can talk about it here, whatever we can contribute would be a Bandaid compared with the real problem.

Joseph Francisco, Purdue University: In all issues that we have been discussing about graduate education, we have assumed that graduate students are coming in with the right kind of basic knowledge and skills to start with. We also assume that the bottleneck in the whole process is research. This might be true for a few schools—such as Columbia, the Massachusetts Institute of Technology, the University of California, Berkeley, Harvard, and the University of Chicago. However, for a number of other schools, extending the time line is caused, in part, by students filling deficiencies in basic chemical knowledge. Clearly, it is not just an issue of research. It is an issue of getting those students up to speed by developing a basic background before they can do research. So, as we shorten the time line for the Ph.D., are we in fact allowing these deficiencies to go unaddressed? We are creating a pipeline. The problems that are not being addressed in high school and not being addressed at the undergraduate level are soon not going to be addressed at the graduate level. In the long term, I think we will diminish the quality of our graduates.

I would like to express a worry that comes from personal experience. I had a graduate student ask a question of freshmen at Purdue University who are taking general chemistry. (At Purdue, the requirement for taking general chemistry is a year of chemistry in high school.) The graduate student poured water from a tap into one glass and water from a bottle of Perrier into another glass. She asked the students which one was a chemical. A significant number could not answer the question. This is what is being propagated in the system. I think we have to be careful.

R. Stephen Berry: Dr. Francisco, I think your point is well taken. At one time I remember saying—I think it was in my first real teaching job—that there seemed to be an inverse correlation between the number of courses required of graduate students and the quality of the graduate school. The reason is exactly what you just pointed out. It is a question of how much help graduate students need before they can start the research level of education. I think that is one of the reasons why I believe we should not make a change immediately. Let's look very carefully at what function those extra years are serving before we decide to change.

John T. Yates, Jr., University of Pittsburgh: I was intrigued by the comment that chemists and physicists work on problems that are invented by their mentors, whereas people in other fields work on problems that they themselves invent. I would maintain that the criterion for the end of the Ph.D. should be that students are able to invent their own problems or subproblems within the field of the research. Unfortunately, it takes different students different lengths of time in different types of problems to achieve that level of competency.

R. Stephen Berry: Certainly, in a field like experimental chemistry or physics, it takes not only a knowledge of facts but also a certain amount of judgment about how difficult the actual experimental work is going to be and how long it will take. There are solid reasons for those differences.

Bettina Woodford, University of Washington: I am with the Pew Charitable Trust project "Re-envisioning the Ph.D." I seem to be an imposter here, because I am a linguist and not a chemist. Our project is cross-disciplinary, so I have come to this conference. I want to ask a question about how faculty buy into all the change that is being proposed. We heard earlier today about the importance of off-campus practicums for students to get them more involved in softer skills, communication, multidisciplinary teams, awarding funding directly to students rather than to the professors, and shortening the time to degree. These are all issues that our project has heard in more than 300 interviews that we have done around the country, across disciplines and sectors. Another thing that we have heard is about getting students off campus into practicums with industry and getting more faculty involved in off-campus research partnerships. This allows the faculty to be more aware of what students can count on as career options during their graduate training. This certainly varies by discipline, and I am aware that in the sciences it is more of an option than in a lot of other disciplines, such as mine, for example.

We also heard today a concern about how giving funding to students can affect faculty negatively, especially with regard to tenure. Ed Wasserman says that he finds faculty often resist sending their graduate students to some of the softer skills courses and seminars. One of the major concerns expressed by the people we have interviewed is that, for major changes in restructuring graduate education to really work and move forward, the faculty need to buy into them. As we head into the 21st century, do you have any suggestions about what key motivators might get more faculty to buy into this process.

R. Stephen Berry: That is a wonderful charge. Your way of putting it actually fits very well with many of my own prejudices. When I have had students decide—after graduate school or after a postdoc—that they want to go to law school, I think that is great. At this point, our society needs people who know and understand science to work in areas of legislation much more than it needs scientists. I don't have a good answer for how to persuade those of my colleagues who would like to generate clones that science is not the only career path that could be productive or valuable for students. I am not an institutional guy. You probably got that sense. I much prefer having things fluid and flexible and adaptable, with a minimum of rules. I would like to offer students opportunities rather than requirements. By the same token, we should also ask, Why shouldn't industries be flexible enough to allow industrial scientists to take sabbaticals and work in universities or, for example, in government labs? I had this discussion, for example, with some of the Department of Energy labs, places that would like to have more exchange. The problem is that group leaders tend to be resistant because they don't want to lose the people in the group for even 6 months or a year. But that flexibility would be very desirable.

Billy Joe Evans, University of Michigan: I would like to mention that Steve Berry was on the faculty at Chicago while I was there. Even though I never took a class from him, he had a significant impact on me. He was always a flurry of activity in his lab in Kent Hall. For me, that was one of my models of an academic scientist. I am appalled by our powerlessness to deal with the problem that has been expressed here, by leaders in industry as well as by leaders in academics. I agree fully with Professor Berry's notion on keeping things fluid. We have talked about limits, and we have talked about capabilities. Those are not really the things that we are trying to get at, and therefore I would like to move in another direction.

The only reason that research should be conducted at a university as part of a graduate education program is for the sense of power that a student gets in a field that comes from research activity and that can only come from that activity. Until the student generates his or her own equation, or understands a relationship that has not yet been explored or understood, that student has no sense of his or her own power to move a field. So, one is not concerned about capabilities such as whether I can talk or whether I can read or write. If I have a sense of power, I will do whatever it takes for me to move this field. What that suggests is that the kind of research that we do in a university setting is different in character from the kind of research that is done in industry. Our research must be chosen so that when a student has completed it, there is a sense of power that that student has acquired relative to almost any inquiry, but certainly an inquiry of that field.

Research groups that have 20 or more members pose serious difficulties to the faculty's ability to be sure that each student has a sense of power about this discipline. I do not think that such a thing is impossible, and I do not believe that it has to be done everywhere by everybody. I think that the goal of graduate education in the chemical sciences has to be to give students a transcendent sense of power when it comes to dealing with matters of a chemical nature.

R. Stephen Berry: I would agree very, very heartily, and I would phrase my own perspective of that slightly differently. A goal of graduate education in science ought to be that each student reaches a stage of realization that he or she has contributed something to the world's knowledge that is permanent. I think that is perhaps the source of that sense of power, that you have done something that, unless all the libraries are lost, has added to the world's knowledge. Whatever epsilon it may be, you have done it.