ENVISIONING THE FUTURE OF DOCTORAL EDUCATION

Preparing Stewards of the Discipline

Carnegie Essays on the Doctorate

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UNMASKING UNCERTAINTIES AND EMBRACING CONTRADICTIONS

GRADUATE EDUCATION IN THE SCIENCES

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My approach to this essay is deeply imbued with thoughts and theories of the late Robert Merton, long-time, much-admired friend and colleague (what I did, by not mentioning his work in the text, was to supply a very good example of what Merton called "obliteration by incorporation"); my argument can actually be described as "social epistemology"—a happy term that I borrowed from Harriet Zuckerman.¹

Much of what follows applies, with some modification, to the continued education of postdoctoral fellows. This is, however, another topic to be discussed elsewhere.

The Argument²

1. There is far more fundamental controversy within the sciences than its practitioners are prepared to confront. Doctoral students need to understand that much of modern science still must confront basic epistemological issues of knowledge and knowing. These range from questions of knowledge organization and images of the possible to arguments about method, precision, and rigor. The contradictions and inconsistencies of
science must be cherished. Seminars should emphasize the examples of instances where the favored theories simply will not work.

2. Doctoral education in the sciences must emphasize the personality, character, habits of heart and mind, and general scholarly dispositions of the steward of the discipline. Being a steward of the discipline involves generation, conservation, and transformation (the educational and pedagogical functions of the scientist), as well as understanding the public context of scientists' work. Toward this goal, doctoral programs must encourage risk taking and intellectual adventurousness, while fostering the importance of precision and rigor.

3. The single most significant and pivotal process in science training is finding, choosing, and defining a problem and locating the problem on the larger map of one's field. Problem choice should be a major focus of the entire doctoral program—a primary responsibility for the candidate to exercise. The program should focus more work—course work, colloquia, formal and informal conversations—on the state of the field and its controversies more generally, always with problem choice at the heart of this work.

4. Doctoral programs should devote far less attention to work within the boundaries of a discipline's subfields and far more attention to the broader questions of the philosophical, sociological, and methodological contexts of work, thus combating overspecialization. This must be repeated regularly at all the important choice points in a doctoral program.

5. Doctoral education needs to "go meta" and encourage and guide the students to step back, look reflectively and critically, contemplate how it might be otherwise, and critically examine the weaknesses of the "mainstream" of the discipline, however well respected and well funded it might be.

6. Leaders in the disciplines must understand the critical roles of curricular and pedagogical work in their field and how deeply these functions are affected by the same epistemological understandings that relate to the research role. They must recognize, empirically, that most of those who earn the doctorate will spend far more time teaching and engaging with a variety of publics—in industry, policy, and community settings—than they will at the frontiers of science. Doctoral education must equip students to work in these settings.

7. Science is inexorably intertwined with the world, which today is globalized to an unprecedented degree. Doctoral students must have opportunities to explore the implications of this.

8. We must be willing to rethink the features of our doctoral program so that we are focused on doing what is necessary to produce stewards of the discipline.
Understanding and Cherishing the Contradictions in Science

The doctorate in the natural sciences is seemingly in a much better shape than are the much-debated doctorates in the social sciences and the humanities. It is much less controversial, and its objectives are less often questioned, for the scientific community indulges in a greater sense of consensus than is the case in the professional communities in other areas of academe.

Why do I say “seemingly”? The problem lies exactly in the reasons it looks so unproblematic. There is, in fact, far more fundamental controversy within the sciences than its practitioners are prepared to confront. This leads to doctoral preparation of the next generation that leaves students and new Ph.D.’s living in a dream world of putative consensus and shared premises. They need to understand that much of modern science still must confront basic epistemological issues of knowledge and knowing, ranging from questions of knowledge organization and images of the possible to arguments about method, precision, and rigor.

Elsewhere in this volume, Catharine Stimpson tells us that the humanities are messy, pointing out the double sense of their messiness: turmoil, disorder on the one hand, and healthy complexity on the other. The same is true about the natural sciences. Just as in the humanities and the social sciences, there are no complete theories of anything in the sciences: the theoretical structures are far from complete; the foundations abound in internal contradictions and rapidly changing presuppositions. But whereas the social sciences and the humanities accept incompleteness and contradiction as a given, perhaps even welcome it, most of the natural sciences ignore it, claiming that incompleteness and the accompanying contradictions do not affect the daily work of the scientist, neither in experimental work nor in theoretical deliberations, for they are considered unimportant passing phenomena—actually, mere noises—on the long and uninterrupted road toward certain and complete knowledge and Truth.

It would be presumptuous on my part (or, for that matter, on anybody’s part) to make general statements and suggest correctives or changes in the scientific contents of the doctorate in the different disciplines. This essay will, therefore, concentrate on aspects of the doctorate that are, although inseparably connected, beyond the narrowly understood scientific content of what is being taught, transferred, and internalized by doctoral students.

So now we are in the domain of epistemology, even though the working scientist wants to think this domain away from the center of the scientist’s attention. That was not always so. There is no nineteenth- or early-twentieth-century textbook that does not start with an extended
epistemological chapter, dealing exactly with the question, What constitutes knowledge in this discipline? This is especially important if we think of the multivolume compendia, or "Vorlesungen," covering whole disciplines. It served a good and very educational purpose, in that the same scientist was supposed to cover the whole of physics or the whole of biology. In the process of doing that, contradictions came up, competing paradigms became inescapable, and even if the self-chosen task of the writer was to eliminate such disturbing "noises," they had to be dealt with nevertheless.

With increasing specialization, this tradition disappeared, and the need to confront inconsistencies between and among subfields disappeared with it. Imagine that today the repeated and deep disagreement on the foundations of physics between Philip Anderson and Steven Weinberg (perhaps the two most important Nobel Prize-winning leaders of the profession) would not come out in little-read "philosophical" asides of the two. Neither would it emerge through the contradictory advice they might offer to a Senate committee deciding whether or not to fund the multi-billion-dollar superconducting supercollider. Instead, each would be expected to write a comprehensive, five- to six-volume textbook of the whole of today's physics, each writing about elementary particle and solid-state physics. These two series would then be the ideal training for the doctoral student in physics, regardless of the student's specialization in either elementary particle or solid-state physics.

Would Anderson and Weinberg disagree on the body of knowledge in physics? No, they would not. They would disagree over a question that is much more fundamental: Could it possibly be that different levels of organization of matter (subatomic, atomic, molecular, cellular, and so on) obey different sets of laws that are not necessarily reducible to each other, as Philip Anderson (1972) dares to ask? Or, on the contrary, as Weinberg (1992) has been consistently claiming for several decades, are we indeed approaching the final theory?

To understand such a debate, we must indulge in interpretation, which is part of a theory of meaning. Yet it is clear to all what enormous differences these are and how much is at stake. Nonetheless, consensus among the community of scientists has been an ideal, or even an ideology, ever since the emergence of modern science in the 1660s in London and Cambridge. Like every other intellectual movement, even if it is formulated in terms of nonpolitical, objective, value-free, context-independent scholarship, science emerged in the framework of a given political context. That political context influenced, to a great extent, the socially determined confines of the movement, even though it did not influence the very content of the ideas involved.
Be assured that I am not claiming that the content of knowledge is socially determined.\(^5\) Mine is not a facile postmodernist and rather primitive claim of the sort that invited the no less facile and primitive "science wars," culminating in the Sokal Affair (described in some detail in Chapter Twenty). What I claim is that we must not ignore what follows from serious contextual history: modern science emerged after the painful and cruel wars of religion that killed off a substantial part of the population of Northern Europe. The worried scholars—helpless observers—became more and more convinced of the need to create a new type of knowledge that would continue to serve the glory of God but not be dependent on the differences between the various religious movements. They had, if you will, a strong urge to establish value-free, objective, rationally based knowledge; they had an ideological need for what became modern science.

That urge did not influence the content of knowledge; there are no socioeconomic roots of Newton's *Principia*. But it did set the context in which such a science would emerge.\(^6\) The tradition of consensus was very strong among the first modern scientists and was verbalized repeatedly by Robert Boyle, who helped formulate the ideology of the Royal Society. This tradition is now deeply ingrained in the training of scientists and is part-and-parcel of doctoral training. Whereas in the humanities and in the nonquantitative social sciences, disagreement on basics is considered an intellectual desideratum, in the sciences, it is not.\(^7\) The quest for consensus in the sciences is at the root of the fact that very often students in the early years of their studies are not being told expressly that still, as of today, there is a fundamental theoretical and empirical contradiction between the standard model and gravitational physics. In many cases this contradiction is a revelation to the doctoral student.

Why does this embrace of consensus persist? One recent influence comes from the philosophy of science. Although philosophy of science never had any significant influence on the growth or direction of change in science, the ideas put forth by Thomas Kuhn in his *Structure of Scientific Revolutions* were most widely read and reflected upon since its publication in 1962 (Kuhn, 1996).

Kuhn's resulting influence was twofold. First, Kuhn articulated and strengthened the view that was already shared by most scientists: when a science becomes mature, it becomes mono-paradigmatic, and then it is as nearly consensual as is possible. Change comes uncontrollably in the form of a "scientific revolution." One cannot prepare for it, and thus one need not educate for it. If we agree with this view, doctoral training in the sciences is just right as it is.

Another important influence of Kuhn is on our conception of science as an integral part of culture in general (science as a cultural system) and
on science studies; a more important influence has been on science policy (including policies of granting agencies). Kuhn opened the door for serious discussion of how the social context influences the growth of knowledge. The new and rich research area of science studies (which combines history, philosophy, and sociology of science, obviating the need to distinguish between them as separate disciplines), although far beyond what Kuhn found acceptable, contributes to a much deeper understanding of the historical and social conditions under which new disciplines and new problem areas emerge. 

Most of those who read Kuhn were historians, philosophers, sociologists of science, and the educated public. Perhaps most important, politicians and those engaged in science policy also read it. It had, however, very little influence on working scientists. (Steven Weinberg puts this point very strongly: “We learn about the philosophy of science by doing science, not the other way around” [2001, p. 84].) However, it is interesting that most scientists agree with Kuhn, and having found reassurance for their initial beliefs, often claim (albeit, ceremonially, in valedictory speeches or while opening and closing public addresses) to have been influenced by him. (Weinberg, of course, disagrees with Kuhn and asserts that no science is ever in a mono-paradigmatic stage: competing paradigms always exist, and the fights among them are being conducted by the leaders of the field, sometimes at the forefront but more often in relative obscurity.)

What else contributes to this sense of consensus and avoidance of the contradictions in science? Avoidance of contradictions follows from the antiduallegetical nature of science. When the emerging corpus of modern science in the seventeenth century evolved, emphasizing strongly its consensual nature, there was no place in it for dialectical thinking. A dialectical approach suffers contradictions and allows for the fact that different formulations of a question may yield different answers. Rhetoric is dialectical; so is legal thinking; so is a basic approach like that of Philip Anderson, according to whom different levels of organization of matter may be guided by different sets of basic laws, which are not reducible to each other. Also “dialectics” reminds us of “dialectical materialism,” namely Marxism, which has no place in the body of science, and thus the wrong gut reactions come to the forefront when a “dialectical approach to science” is mentioned.

Another barrier to exploring contradictions (and even controversial new ideas) is the abhorrence of contradictions in a rationally built up corpus of arguments. Science is considered the rational field of inquiry par excellence, and a rational argument does not suffer contradictions—an idea that is very close to the Kuhnian claim that a mature science is practically mono-paradigmatic. If two contradictory theories come to be dis-
cussed in a scientific discipline, it is presupposed that at least one of them is false. Moreover, it is also presupposed that a mature science offers a complete overview of its domain. As this thinking goes, there are minor, not-yet-solved problems, but because knowledge is cumulative, these minor gaps will be filled in due course, and the newly acquired additional knowledge will find its rational and coherent place in the corpus.

We are even lullled into thinking (and telling the public, and, worse, teachers of science) that we are unified in our approach to our work, for we are at all times following the “scientific method.” In truth, as Weinberg (2001) observes, “We do not have a fixed scientific method to rally round and defend... most scientists have very little idea of what the scientific method is, just like most bicyclists have very little idea of how bicycles stay erect” (p. 84). Ever since William Whewell's History of the Inductive Sciences from the Earliest to the Present Time (1858), many students of science have made this very same point, not the least of whom was Albert Einstein.

Implications for Doctoral Education in the Sciences

Without calling it philosophy of science, most doctoral students in the sciences tend to internalize a sense of consensus in the sciences because the great majority of their supervisors, without much critical reflection, hold this notion. And this view leads supervisors to hold the notion that it is efficient, from the point of view of the economy of time, for the speed and depth of the training of the doctoral student, not to waste time on contradictions in the field. As a result, critical preparation for the possibility of new thinking at the very foundations is often absent from doctoral training. Rather, the attitude is this: if and when a real genius appears on the scene, he or she will know what to do.

However, it is important historical information that no scientific theory in any discipline has ever been complete. We never had a complete theory of life; that is, we never had a complete biology. Nor did we ever have a complete physics or a complete economic theory—to say nothing of a complete theory of the human being (a complete psychology) or a complete theory of society (a complete sociology). We have partial theories. We have competing paradigms underlying our partial theories. Yet if we do not tolerate contradictions, then we must always decide which partial theory is to be discarded, thus greatly impoverishing our ability to reflect critically and comparatively on our intellectual options.

Understanding different paradigms should be made part of doctoral education in the sciences. I find it patronizing and condescending to doctoral students not to do so, and I believe we have a moral obligation to
acquaint our students with all the nonconsensual elements of the state of the art in their chosen field. I do not accept that it is more efficient to neglect these aspects of the sciences. **Doctoral training in the sciences should include a serious study of the foundations of the disciplines, the internal contradictions, incompleteness of prevailing theories, and competing paradigms.** The approximate degree of intellectual security and the meaning of taking intellectual risks in the disciplines should be verbalized as part of doctoral education. The contradictions and inconsistencies of science must be cherished.

How best to do that? Certainly not by preaching. Nor can one make sure that every department has among its faculty one of the leaders of the field, whose very thinking would include everything suggested here. What remains is the rhetorically efficient and strong means of engaging in controversies: dialogue. To facilitate this pursuit, the relevant literature is always available, and whether the doctoral supervisors master that literature or will be exposed to it on equal footing with the doctoral students is immaterial. Perhaps there are advantages to students and their teachers being confronted together with new thoughts and approaches.

**Ongoing critical reflection in the form of a departmental seminar on the state of knowledge in the discipline must be an integral part of doctoral training.** Seminars should emphasize examples of times when the favored theories simply will not work. For example, to strengthen the students' ability to think dialectically, a course parallel to the basic course on the elementary theory of the discipline should be given; in this course, students would explore a theory that does not work. This is as possible in physics as it is in economics. A seminar could address different formulations of a partial theory, showing how they are actually the same discipline. For example, Goldstein, Poole, and Safko's classical textbook, *Classical Mechanics* (2002), can be understood as basically the same body of knowledge as Lanczos's *The Variational Principles of Mechanics* (1986). This is usually done for the Schrödinger formulation and the Heisenberg formulation of quantum mechanics, and that is extremely healthy for doctoral students. When, following the launch of Sputnik, new approaches to teaching science were developed, different versions for teaching biology emerged: a molecular, an evolutionary, and an environmental approach. No effort was made to show that these approaches each teach and study the same subject matter. Equivalents can be thought of in other disciplines.

**Guiding Principles for Developing Stewards of the Sciences**

The training of doctoral students is unquestionably meant to educate scholars who are professionally well equipped, are aware of the human
and social side of the life of their profession, can cope with rapid changes in the problem areas and in the very foundations of their discipline, and can become, in due course, stewards of their discipline. In addition, and not less important, they have to be complex and many-sided individuals who can grapple with the myriad aspects of modern life.

Actually, the need to discuss the nature of stewardship as part of our mandate in these essays is much deeper than the question of whose task it is to be a steward of the discipline. Obviously, there are many social institutions that can and do take it upon themselves to lead, shape, and protect—that is, to serve as stewards of a given discipline. These may be private and public granting agencies, university administrations, and national institutions such as the National Science Foundation, the National Institutes of Health, and the National Academy of Sciences; sometimes individuals' achievements and status make them de facto institutions. But what concerns us here is this question: What in the education of the doctoral student prepares him or her for becoming a steward of the discipline and profession?

Doctoral education in the sciences must emphasize the personality, character, habits of heart and mind, and general scholarly dispositions of the steward of the discipline. Doctoral programs must ask how they can encourage risk taking and intellectual adventurousness while fostering the importance of precision and rigor. For rigor must not be permitted to dominate the personality of a future investigator so that the speculative and conjectural courage needed to do good science is destroyed. Programs must model, practice, and reward risk taking.

Let us consider the education of the doctoral student toward the four aspects of stewardship: generation, conservation, transformation, and responsibility for the field.

*Generation*

**EMBRACING RISK AND RIGOR.** Considering the notion of stewardship brings us to the most important and yet the most imponderable part of the education of any scholar, specifically of a scientist: how to educate a daring, risk-embracing scholar, whose flight of imagination is untrammeled and who is daring yet responsible, risk-embracing without being a Don Quixote, and self-confident in rational limits without overestimating his or her intellectual powers? If the young scholar is to err, it is better to be too confident than too risk-averse.

Here we are on very sensitive ground. We wish to educate a generation of scholars who are considerate, egalitarian, democratic, modest, and so on. Yet in my opinion, the greatest deficiency a young scholar can have is
being so cautious that scientific endeavor is paralyzed. Applying relentlessly the critical mode of reflection, not taking anything for granted, not accepting that any past achievement or authority should remain unquestioned is the sine qua non of doctoral training. But all these attributes come in as secondary requirements. If I were to formulate it in a slogan, the doctoral student’s attitude should be “It is so. But it could be otherwise.”

This not only is a controversial formulation but, even if agreed upon, is extremely difficult to achieve. Moreover, by the time the undergraduate budding scientist becomes a doctoral student, it is very often too late. So much in the undergraduate training in the sciences is typified by huge floods of material to be mastered, and so much of it is accompanied by being humiliated by better students and often by faculty, that many of the bright freshmen who come with myriad questions on arrival at the university have, by their third year, only a repertoire of answers, having forgotten to ask questions or to question what is being fed to them. Yet this is, in my eyes, the most central aspect of producing a steward of the discipline. I shall not try to indulge here in penny psychology as to how to achieve this goal. Let me leave it at this: difficult as it may be, developing critical thinkers must become an integral part of the intellectual climate of a doctoral program.

Actually, much of what I expect should be the task of undergraduate education. By extending this argument we could reach the conclusion that social awareness, moral responsibility, and knowledge of the real world could have been the task of high school. Let us instead concentrate on doctoral training here. The readiness to challenge established ideas should have been achieved already in the student’s undergraduate education, but the readiness to challenge advanced theoretical foundations or emerging new paradigms on the level of cutting-edge research is definitely a new ballgame and clearly belongs to doctoral training. The difficulty lies in developing students’ ability to do independent research, to solve theoretical or experimental problems on their own, with all the confidence and positive verve this takes, and yet to retain the critical, questioning eye for their own ideas and those of peers and superiors. This can be achieved only in informal discussions, small seminars, interdisciplinary discussion groups—in short, in the dialogical mode. A precondition for such a dialogue is a nonhierarchical climate. What must count is what is said rather than who says it. I admit that this is much more of a problem in Europe than in the United States, but even here it is often an issue.

It is common wisdom to say that “one responsibility that a steward has to the disciplinary community is to conduct their own research and scholarship according to accepted standards of rigor and quality” (Carnegie
Initiative on the Doctorate, available online). These words sound good, yet they are very problematic. It is precisely in inculcating the doctoral student with demands of rigor and quality that supervisors, believing that they know what the scientific method is, exactly, and that their chosen method is identical with quality and rigor, stifle all budding attempts at risk-embracing questioning.

This is more easily demonstrated in some areas of scholarship than in others. For example, in philosophy, especially in the philosophy of science, it was once an accepted rule that only analytical methods can guarantee rigor. Now, when many leading philosophers have realized that there are no sound analytical ways to approach the most important aspects of life—love, religion, and ethics—many have embraced these major existential questions by abandoning all rigor. But one could illustrate this also from the natural sciences. For example, what would constitute rigorous research in evolutionary biology? Surely, much that is learned in molecular biology does not apply. At the same time, it is being realized more and more that training on the doctoral level should not allow a cleavage between these two branches of biology. What will count as rigor in such a “combined” department? What do we do with the criterion of predictivity, for example?

Clearly the concept of rigor is all-important. It has to be reflected upon in terms of critical thinking, which takes nothing for granted. Yet being rigorous does not mean that nothing is taken on authority or trust; there is no way to put to the test all the accumulated knowledge before applying it. So quality and rigor will have to mean that whenever an experiment based on previous knowledge fails to yield the predicted result, not only the technical details or the calculations but the previously accepted knowledge on which the experiment has been based have to be questioned. Although in principle this is self-evident and would always be acknowledged by any scientist in a ceremonial address or an introductory lecture, it is very rarely encouraged in a busy laboratory. Yet this is exactly when daring and readiness for risk taking should come into play.

Even in specific recommendations for rigor, some critical approach is mandatory. Let us touch, for example, on experimental replicability—in principle, of course. Rarely do we have to repeat de facto experiments, unless there are recurring problems with the theory or with related experiments. However, take the enormous number of data that have to be collected when deciding that, indeed, a new elementary particle has been discovered. Do we really mean that such a series of measurements, often involving hundreds of scientists, technicians, engineers, and hundreds of thousands of pictures, will be repeated simply for the sake of rigor? At the same time we must be able to know when scholarship is irresponsible.
There are no simple recipes for discovering sloppy or irresponsible work. But rigor should be critically discussed, and different possible ways of being rigorous should be integrated into lectures and laboratory settings.

CHOOSING PROBLEMS. If there is a single most significant and pivotal process in science, it is finding the problem. A scientist must define his or her problem and locate it both on the map of his or her field and in the broader landscape of science, beyond the traditional boundaries of the field. In doctoral education, too often the chance to learn this process is absent altogether; the mentor assigns the problem or a problem is engaged quite uncritically.

Instead, problem choice should be a major focus of the entire doctoral program—a primary responsibility for the candidate to exercise. The student should choose, defend, critique, and examine how it might be otherwise. The student’s peers should be invited to examine the problem with the student, as he or she will do for them. The program should focus more work—course work, colloquia, and formal and informal conversations—on the state of the field and its controversies more generally, always with problem choice at the heart of this work.

Indeed, unless we accept the anti-intellectual habit in some of the disciplines, in many of the less elite academic institutions and in the more hierarchically organized universities in Europe (including Eastern Europe), that the supervisor allocates the topic of the dissertation, problem choice is the most crucial point in a doctoral program.

Why do I believe this? Knowledge, and even more important, understanding, in the discipline is more complex than is obvious. The obvious approach is to master the theoretical and empirical body of knowledge, the usual technical know-how to operate the equipment needed in the discipline, the assessment of data and how to choose appropriate controls, techniques of calculation relying on the appropriate and sufficient knowledge in mathematics, and the tacit knowledge that is typical to the group or the team of the laboratory.

But doctoral education should go beyond the obvious. Students should be getting acquainted with the foundations that are not often referred to or not even mentioned in the course of the daily work of the research team. They should be aware of the underlying disagreement and competing paradigms, should realize to what extent the consensus on the “correct” methodology is local, and should understand that in a different lab or within a different group, even if everyone is working on the same problem (or seemingly the same problem), the method to which all adhere might be quite different. They should have encountered the basic para-
digm decisions that may have preoccupied the previous generation of scientists but are now all resolved, at least until the next difficulty occurs. And above all, the students should have arrived at the moment of problem choice after deep and exhausting deliberation of alternatives and after a thorough check and balance on all levels of the final choice: doability, chance of innovation, risks involved, costs and continuous fundability, and opportunities for the topic to broaden into other areas of research.

In many disciplines, the chosen topic involves working with models. A model is a simplified picture of, we hope, reality, in which many parameters that were too numerous to be accounted for have been eliminated. It is rarely the case in the process of the doctoral studies that prime time and attention is spent on parameters that should be retained or eliminated and on what the problem would look like if the choices had been different. Once the model is in place, it is often presumed that the limitation of computing capacity is the only criterion for deciding within which limits the parameters are to be taken into account. That means that the real scientific considerations for deciding the breadth of the spectrum of permissible values are neglected, and the limitation of the machinery is allowed to make the decision. Needless to say, this is anti-intellectual, and it is behind much conventional and not very high-level work in thousands and thousands of doctoral theses. Possibly, the supervisor has gone through such considerations. If so, the quality of the work may be secured, but certainly the training of the doctoral student isn’t. Closely related but different are the questions of when and to what extent to rely on models, and when confrontation with the real world is indispensable. These questions should be crucial considerations as the doctoral student chooses a problem.

The process of choosing a problem should be deeply complex. Although it should not be determined solely by the adviser, it should not be chosen by the doctoral candidate alone. The process of choosing a problem should be an example of interdisciplinary teamwork par excellence. It would be useful to involve with this team an expert in the comparative study of methodology (even if, heaven forbid, this would involve a philosopher of science). Further, the problem and the process leading to its delineation should be discussed at a departmental seminar, with the faculty and other doctoral students present.

EXPLORING THE BOUNDARIES BEYOND SPECIALIZATION. The superhuman pressure under which doctoral students in most elite departments labor leads to an ever-narrower, highly competent specialization. The sheer quantity of the material to be mastered is ever growing, and the graduate student is trained to be a perfect problem-solving apparatus in
a particular narrow, very well-defined area. This type of training is, from the earliest moment onward, considered as an integral part of a goal-oriented, often ritualized career structure—much like the careers of young instrumentalists in music, who are becoming ever better technically and less and less capable of a broad, enjoyable roaming in the full spectrum of diverse musical styles, genres, and abilities. There is no place in this training for doubting, rethinking, slowing down, meditating, meeting neighboring problem areas, or reflecting on the great existential questions of life. In short, it is the perfect intellectual straitjacket. It is believed that any pause in the race will endanger the career.

This attitude is inculcated during the doctoral training and is, in my opinion, a disaster both for the development of the individual scholar and for the development of the profession. In today’s world, the rapidity of changes in the body of knowledge and the globalization factor make it necessary for individuals to be well prepared for sharp and quick changes in their research program and frequent physical relocations to new and different social and academic milieus. The broader the training, the better the acquaintance with the varieties of cultures in general and academic cultures in particular. The more flexible the mental habits, the better the chances for leading a successful professional life. If the present style of training and of career structure is good for anybody at all, it can be appropriate only for the very few who will end up in an elite institution, in a high position, and stay there for their entire academic career. The others are programmed for unhappiness and disappointment. So much for individuality.

Historically, it is well known in all areas of scholarship, the sciences included, that many of the most important new ideas and new discoveries occurred at the border between neighboring disciplines and very rarely at the center of well-defined areas of knowledge. This phenomenon was called “alien wisdom” by the great humanist-scholar Arnaldo Momigliano. Yet in this race, doctoral programs devote far too much attention to work within the boundaries of a discipline’s subfields and far too little attention to the broader questions of the philosophical, sociological, and methodological contexts of work. **Addressing the broader questions and contexts takes time. It is not a task that can be completed with an introductory seminar (although the design and teaching of such courses are very important) but must be repeated regularly at all the important choice points in a doctoral program:** after qualifying exams, at the time of problem formation, and during the dissertation process.

As for the future of the profession—in Kuhnian language we could say that this training and career structure prepares exclusively for what seems to be “normal science.” I use “seems to be” because, if indeed no science
is ever mono-paradigmatic, the need to rethink the foundations is much more frequent than what we take into account in the training of graduate students. Moreover, due to the rapidity of changes occurring in the body of knowledge, it would be self-evident that every two years (and that would mean in the midst of doctoral training) the student takes time to reassess the problem in the context of the map of the field. I am not suggesting that students should change their research topic in the middle—that would be detrimental—but should rethink the exact place of the chosen problem in the discipline; this indeed could—and should—influence the final formulation of the results.

In general, doctoral education fails to “go meta” in encouraging and guiding the students to step back, look reflectively and critically, contemplate how it might be otherwise, consider the disciplined eclectic of the middle range as against the oversimplification of the single, dominant paradigm, and critically examine the weaknesses of the “mainstream” of the discipline, however well respected and well funded it might be.

Doctoral training in the sciences should also emphasize the need to reassess, periodically, the place of the problem in the discipline. Doctoral students should be encouraged to accept the idea that it is not a waste of time to stop the hectic activity and take some time off to think, meditate, recharge the batteries, and indulge in reassessment.12

Conservation

Related to exploring the boundaries of the disciplines and subdisciplines is students’ need to balance breadth and depth in the field.

The inherent tension between depth and breadth must be addressed in all the science disciplines, not just in typically interdisciplinary fields (although more and more areas of research are of this nature).

In this volume there are two important essays dealing with neuroscience. One is by Steven Hyman, a neuroscientist who was at the helm of several leading institutions that were instrumental in shaping the emergence and growth of this flourishing interdisciplinary academic pursuit. Therefore, it is very important to note that although he analyzes and emphasizes the need to go into depth and breadth simultaneously, he does not come up with practical suggestions for building a doctoral program that deals with the problem.

Indeed, there is no recipe to indicate the exact (or even approximate) amount of depth or breadth that doctoral training in any of the sciences should involve, nor is there a way to calculate how much to sacrifice the completeness of training that is the goal of existing, classical disciplines (depth) and replace it with a broad view across disciplines (breadth).
There is no way to foretell how much breadth across disciplines or historical knowledge of a discipline a student will need in order to achieve the necessary depth for solving a problem. It depends on the specific problem and, even more, on the stage of the problem-solving process.

The other essay, written by Zach Hall, gives some practical solutions. He suggests that laboratory rotation is an absolute prerequisite for training doctoral students in interdisciplinary areas, that it helps promote both breadth and depth. I would add another experience, beyond the laboratory rotation: every doctoral student should spend at least six months in a different department, either in the same or “neighboring” discipline but definitely in a different culture.

Another practical approach: when students are choosing a subdiscipline to concentrate in or the problem for the thesis, they should survey the discipline and try to locate the subdiscipline or problem in the whole picture. This will help identify the amount of breadth and historical knowledge the problem will require.

Clearly, much of what I have said so far is indispensable for the young scholar who has just finished the doctorate. He or she must have an overview of the field—its foundations and methods, objectives, and research agenda. And on top of that, as I discuss next, the student must have a moral and social understanding of the role that the given discipline is capable of and meant to play in academe and society at large—a tall order but not impossible. Once the mental habit of critically and reflectively investigating every aspect of the chosen field of study becomes second nature, it is no longer a question of how much time to dedicate to such issues but rather an almost automatic reflex of critical inquiry.

The process of critical inquiry should include reviews of the older literature. Encouraging doctoral students to refer to older literature—well beyond what is on the Internet—is not an antiquarian taste. We do know how many important discoveries were made as a result of “aimless” meandering among books or older articles, or simply by serendipity (Merton and Barber, 2004).

To foster that sense of the role and habits of critical inquiry, I would add to doctoral training a critical study of selected biographies of past caretakers of the profession. It should be made an enjoyable part of the doctoral training; it should not be an exercise in hagiography but definitely a normatively constructed series of critical case studies.

Transformation

It is the task of a steward of the profession to be able to communicate the knowledge of the field to others in an understandable and thought-
provoking, critical, reflective, and dialogical mode. We can understand this by considering the professional as teacher and the professional who contributes to the public understanding of science.

This role of the steward involves transformation (the educational and pedagogical functions of the scientist), as well as integration and review—work that is generally dismissed or ignored. Leaders in the discipline must understand the critical roles of curricular and pedagogical work in their field and how deeply these functions are affected by the same epistemological understandings that relate to the research role. They must recognize, empirically, that most of those who earn the doctorate will spend far more time teaching and engaging with a variety of publics (industry, policy, and community settings) than they will at the frontiers of science. Doctoral education must equip students to work in these settings.

Moreover, all doctoral students in the sciences should understand, with respect to communication, whether among peers or to the public, that “the standard scientific article” is, in the words of Nobel laureate Peter Medawar, actually a fraud. It gives a rational description of how a discovery should or could have been made, not how it was actually made. All the judgment, false turns, and errors are eliminated.13

CLASSROOM TEACHING. The most neglected part of the training of scientists is their training as teachers. It is usually presupposed—obviously wrongly—that knowledge is itself sufficient for its communication. Second (and this is even worse), it is presupposed that communicating or teaching involves mere didactics, simply using educational gimmicks; teaching and learning are not considered a science (one of the main areas of advanced research of The Carnegie Foundation); neither is the question of how to teach or communicate subject matter considered an epistemological question. And because the question of how to teach or communicate anything at all is, in fact, an epistemological question, the content of knowledge has to be thought of in different terms when its communication is at stake. As a result, gaining a new, insightful way of thinking about the body of knowledge of a discipline enriches the scientist’s understanding.

In most good doctoral programs, the students are expected to serve as teaching assistants; they often conduct exercises or even give advanced courses on the topic of the doctorate, in which they are expected to be unique experts. Also in good doctoral programs, the department is aware of the need to teach well, and some attention is paid to that aspect of the faculty’s work. My main point here is not the moral notion that this is, indeed, the obligation of a faculty. What I want to stress is that if the principles of rethinking for teaching purposes are not considered—that is, if
the process of rethinking the place of cognitive results in the corpus of the discipline is not a systematically studied process—the whole effort is in vain. Unlike some of my recommendations, which involve special seminars, for this purpose I do not necessarily suggest a special course. Rather, I suggest an awareness of the need and the readiness to integrate a study of teaching into all courses. It can be done by bringing historical examples, by inventing special illustrations, by including some philosophical or epistemological asides, or by telling the students what thought processes the teacher has to go through when preparing to communicate a body of knowledge.

Doctoral training must involve a realization by the faculty that teaching and learning is a science, that teaching poses epistemological problems, and that the reflection on these problems will yield a deeper understanding of the body of knowledge and thus has to be made an integral part of the program, whether in formal courses, informal seminars, or, even better, any dialogue with the students about the topic under discussion.

COMMUNICATING TO THE PUBLIC. Contributing to what is called "public understanding" of science is not tantamount to giving a simple explanation of technical details. The public's ability, for example, to evaluate the necessity, desirability, or risks of nuclear reactors will not be developed through technical explanations of the functioning of nuclear reactors. The ability to assess the dangers involved (or not involved) in consuming genetically manipulated food will not be achieved by studying an accessible explanation of the principles of genetic engineering. Scientists need to be able to convey to the public the arguments for and against the use of these devices and materials and to have some understanding of medicine and ecology, even if on a popular level. This is quite different from "transforming" the science into easily understood descriptions. A scientist needs to be able to rethink the foundations of a given science in its social context. Communicating in this way to the public should be part of the training of the doctoral student.

Who would train them? Alas, few scientists, not even the leaders, are capable of this type of thinking. Doctoral training in any science should involve social thinkers with sufficient scientific literacy who can discuss with the faculty and the students the kind of arguments needed in order to communicate the discipline to the public.

Responsibility for the Field

Hyman Bass, in his thoughtful essay in this volume, when speaking of the stewardship of the discipline distinguishes between mathematics (or any other field) as a discipline and as a profession. I fully endorse what he says
there, but I want to draw attention to another aspect of being a professional. Early on, doctoral students correctly internalize dedication and loyalty to the profession—to its institutions, its ritualized methodologies, its professional ethics, its authorities. This professional loyalty is so strong that it often overshadows or eliminates any loyalty to the academic institution the doctoral student will serve later as a faculty member. This is becoming part of the behavior pattern in the process of building up a professional career, and it results in many phenomena that can be described as character damaging. Although we preach values of collegiality, "communism" (one of the old Mertonian values), sharing of information, and openness of competition in a friendly way for the benefit of science, at the very same time we encourage a cut-throat, competitive, inconsiderate career-mindedness that only loyalty to the profession seems to justify. It is, again, an aspect of the doctoral training for which there is no recipe, and yet it is important enough to be discussed and suggested for integration during doctoral training.

Our current training of stewards of the discipline is successful in one way: doctoral students internalize very early, as part of their ambition for a successful career, the value of becoming referees of papers, editors of journals, or advisers to granting agencies; they aspire to serve on the appointment committees of future colleagues, receive grants and prizes, and, ultimately, if they are ambitious enough, win the Nobel Prize, the Wolff Prize, or the Fields Medal.

The reverse side of this success is the fact that a thin layer of academics fill all these positions. Members of that relatively small group referee the journals, receive the grants, advise governments and granting agencies, and sit on the appointment committees of elite universities. And in recent years, much more than before, the pressure for accountability is such that the gatekeepers of the profession either genuinely or hypocritically claim that they cannot afford the risk of irresponsible ideas, let alone to take the risk of charlatantry, because they are the guardians of public money. And thus the chances for a new, controversial, or counterrtrend idea to receive a hearing or a grant become miniscule.

This success is a direct invitation not to support any idea that is beyond the intellectual horizon of referees or grant advisers. Yet we know that, by definition, any new idea is beyond the intellectual horizon of the gatekeepers: it may be close enough to the received view that the seniors recognize it as feasible, doable, or reasonable, even if it had never occurred to them, or, it may be far-fetched enough to be really beyond their intellectual horizon. Is there a danger of supporting a stupid or irrational proposal, or, heaven forbid, an idea that is fake? Yes, there is. Is there a recipe to prevent such occurrences? Only if those ideas that are
within the intellectual limits of the seniors, that is, of the previous generation, are accepted exclusively.

My solution: training doctoral students should involve historical case studies pointing out innovations that were strange and unacceptable to the leaders of the older generation but later turned out to be the new truth—ideas that were rejected by the gatekeepers (the negative example) or accepted in spite of the risk (positive example).

UNDERSTANDING CONTEXTS. At the opening of this essay, I discussed the social embeddedness of science. The point I made was that the context does not determine the contents of science—ideas are not created by societies—but the context determines the necessary conditions for the possibility of ideas to emerge. A steward of the discipline understands the contours of the context.

The sociopolitical context of contemporary science and scientific research has changed in recent years, and it is critical that students be given opportunities to explore and understand how the globalization of knowledge has changed scientific discourse and changed the way scientists engage with the world.

More than half of those with a doctorate in science will not remain within academia. Of those with doctorates in science and engineering who are currently faculty members (about 46 percent), half are in research universities and half are in other academic institutions (National Center for Education Statistics, 1999). The remaining working scientists and engineers are employed in government, industry, finance, business, transnational corporations, security-related research, and legal firms dealing with the growing issue of intellectual property rights.

This fact is intimately connected with globalization. We live in a world where not only markets and information technology are globalized but knowledge, even ideas at the basis of political systems, are globalized. At the same time, in all societies those aspects of local life that do not touch on the market tend to become stronger: there is a renewed emphasis on local languages, religions, and other dimensions of culture. In all extra-academic activities (but also in academe) the fresh Ph.D.’s who enter the job market will encounter all these characteristics of real life. Doctoral programs must consider how to prepare their doctoral students for this new and changing context. Although doing so might entail supplying the students with technical know-how, it is of greater importance to influence their mode of thinking about the world. At the end of their studies, doctoral students should be encouraged—and helped to spend some time in an internship in a nonacademic milieu, preferably abroad. This experi-
ence will also strengthen the student's understanding of what is meant by the "international character" of science (Biddle, 2002).

Moreover, there is another, albeit imponderable, aspect of the thinking and attitude of scientists: their widespread and well-documented "angst" about being contaminated by politics and social problems. Regardless of whether this attitude is being artificially fostered by the intellectual climate in science departments, or, possibly, is a side effect of the very nature of preoccupation with ideas about Nature and the pursuit of objective Truth, it has to be directly confronted. As Helga Nowotny has phrased it so aptly, not only do we have to educate the doctoral student to become a steward of the discipline but also, and no less important, to become "a steward of public interest" (personal communication, 2004). What must be understood about the "local" in order to function intelligently and efficiently in scientific areas, which are seemingly independent of the cultural elements of life? Clearly, most of the answer is nongeneralizable and belongs to the vast domain of "tacit knowledge" (Polanyi, 1966). Yet as multinational companies and even security-driven Western interventions have taught us, there is much in the "local" that has an impact, or at least should have an impact, on the way we handle situations.

UNDERSTANDING THE ANTAGONISM TOWARD SCIENCE. In her essay, Stimpson enlarges on the defensive, self-deprecatory self-image of the humanities and the prevailing anxiety of being threatened by the social sciences, but even more by the natural sciences. This anxiety has many dimensions: poverty of means in numbers and in moral support by the public. Fear of and antagonism toward the sciences is rooted in the claim of the sciences for monopoly on truth, or rather Truth.15

Indeed, there is ample literature that makes this point about the sciences' claim that they have a monopoly on Truth.16 But without getting into the merit of the humanities' argument with the sciences, what is important for us is to recognize the fact that the sciences are on the defensive, too, and much of their insistence that they have the monopoly on Truth stems from their own anxiety about losing means, moral support of the public, and recognition of their achievements. Although the number of doctorates in the sciences and engineering outnumber those in the humanities and social sciences,17 and although the financial support of the sciences from both public and private sources still overshadows that of the humanities, and although technology and industry continue to rely on basic training in the sciences, popular support, if not yet declining, is certainly being repeatedly questioned. This has been going on for several decades. The sharpness of the "science wars" can be attributed, at least
in part, to the prevailing angst about irresponsible postmodern influences. The defensive literature stresses the danger of the growth of public embrace of a great variety of "irrational" preoccupations such as astrology, Lysenkoism, creationism, parascience, and UFO-ology.

Whatever the reason for the anti-science phenomenon, it is a fact to be reckoned with. Society cannot afford a serious decline of science; too much of our daily life is dependent on a steady supply of experts in science and technology and of new ideas to cope with the steady flow of new problems, even if it is true that some, or even most, new problems are created by our very progress and efforts to solve previous ones. Thus the defensiveness of the scientific community is understandable. Awareness of the problem must be an integral part of scientific education. Moreover, science is a product of the inquisitive human mind, on a par with the social sciences and the humanities. These pursuits must be protected and their support made secure together, as part of one and the same enterprise.

If the argument is about economic relevance, many of the scientific disciplines and mathematics are as vulnerable as the humanities. However, the consensus among scientists and their reliance on the lack of technical understanding among the representatives of many of the granting agencies help hide this vulnerability.

But even more specifically, "if science cannot claim preeminence for its intellectual virtues or an excellence for its methodologies and sense of design, then it will have great difficulty laying claim to a rational share of the nation's resources for its perpetuation" (Gross, Levitt, and Lewis, 1996, p. x). Indeed, every doctoral student, whether in the physical, biological, earth, or health sciences, will find these remarks on scientific methodology familiar and congenial—perhaps too familiar. Most doctoral programs in the sciences give a seminar or course on the scientific method. The course should be made critical and reflective, comparative, and deliberately controversial. This goal could best be achieved if several scientists, preferably from different disciplines and with diverging methodological convictions, would co-teach such a course.

MAKING COMMON CAUSE WITH THE HUMANITIES. There is a tacit presupposition that the "culture wars" of the 1960s and 1970s (not the same as the "science wars") had primarily to do with the humanities. As described in Stimpson's essay, the culture wars were waged around four issues: "the nature of the United States and its role in the world... race and race discriminations; gender and gender discrimination, and sexual norms." It is easy to overlook the fact that all these issues touch on the education of the natural sciences, too, both externally and in the body of knowledge.
Externally, such issues have a decisive influence on who is going to do a doctorate in the natural sciences. Creating equal chances for daring to start on a doctorate and then succeed in it for minorities, be it for women or for people of color, is of major importance. The problem of the role of the United States in the world is more relevant today than ever. Will a patriotic American who goes along with the neoconservative, or even Bushian, ambition of acting as policeman of the world, and who sees his or her role as an exporter of democracy, human rights, or anticorruption policies, go and study languages, cultures, or comparative religion rather than the natural sciences? Or, alternatively, will those who reject this role for the United States turn more than ever to the arguably "value-free," "objective," and "rational" natural sciences? In the body of knowledge, has the gender-equality revolution weakened the typical male presupposition that because a woman takes care of children, she might spend less time in a lab, and it is therefore much more risky and wasteful to employ her in the experimental sciences? Moreover, has the sexual revolution succeeded in demoting the myth that the thinking of women is inferior and less rational and, especially, that fewer women are able to think mathematically than men? Whatever the answer to these questions, it has a major influence on who goes into a doctoral program in science, and, in the end, how the doctoral training is undertaken.

The usefulness of Stimpson's points in understanding the situation for the sciences suggests that the different emphases between the natural and the social sciences and humanities be integrated during doctoral training. Good arguments should replace the habitual labeling and mutual recriminations. For example, theories of the beautiful are as much part of physics, of complex systems, and of evolution and cosmology as they are of literary studies. Rather than trying to define what the "beautiful" is, let us consider the power of the argument that a theory is beautiful: it actually obviates the need for critical reflection. Indeed, mathematicians and theoretical physicists often announce the beauty of a new theory with prophetic fervor. We encounter the same uncritical attitude to concepts like simplicity or, on the contrary, complexity. The greatest minds indulged in this. For "simplicity," one might think of Einstein; for "complexity" one could invoke Niels Bohr, Kepler, and many others since, for arguing the truth-value of a theory from its beauty. The doctoral student should acquire a critical ability to reflect on the pet beliefs of the best and brightest.

Scientists who think in terms of beauty and methodological values should not hide these thoughts in their secret minds nor relegate them to their memoirs, written after retirement, or to ritual public addresses. They should make them themes of doctoral education. Every doctoral student
should be able to conduct an argument on the meaning of the claim, be it in physics, biology, or any other science, that symmetry rules are beautiful, and, as such, have claim on truth value.

There are other areas in which one field can learn from the other. Whether in physics or in biology, the question of the possibility of reduction of different levels of organizations, one on the other, and thus of explanations, is fundamental and should be made part of the education of the doctoral student in all the sciences. A new development, stemming from Jerome Bruner's work, is the emphasis on narrative as a cognitive process. To study narrative is to study the interpretive stance, whether in law, medicine, or any other area (Bruner, 1990, 2002). Case studies in law would not immediately seem relevant for the training of scientists, but consider that Columbia's medical school, under the guidance of Rita Charon, set up a department of narrative medicine. Students deduce, through studying the structure of the story of the patient by means of literary theory, how the story might influence the diagnosis. And thus the "scientific method" of medicine becomes deeply involved with literary theory. Narrative in meaning-making is also central to problems of relativism and constructivism, which all impinge on the training of doctoral students in the sciences.

One final thought on this matter: one of the foremost intellectuals of our time, the anthropologist Clifford Geertz, suggests that culture can best be studied by breaking it down into "cultural systems." Geertz wrote several magnificent studies: "Religion as a Cultural System" (1973a), "Art as a Cultural System" (1983a), "Common Sense as a Cultural System" (1983b), and others. Unfortunately, he never wrote a "Science as a Cultural System," but as a commentator on my lectures with this title at the Boston Colloquium for the Philosophy of Science, Geertz did not distance himself from the idea that science is also a cultural system. In "Thick Description," Geertz says that "man is an animal suspended in webs of significance he himself has spun. . . culture [is] those webs, and the analysis of it [is] . . . therefore not an experimental science in search of law but an interpretative one in search of meaning" (Geertz, 1973b, p. 5).

Practical Matters: Rethinking the Features of a Ph.D. Program in the Sciences

The question posed to me was, If you could start de novo, what would the features of a Ph.D. program in science be? Next I offer some practical recommendations in addition to those I offered earlier. Among other features, I would add to the doctoral training process two intensive, eight-week-long periods. However, the vexing problem of doctoral education
in the sciences is that the doctoral studies already take too long. So, as I discuss features of the doctoral program, I offer a way to shorten the process, even while adding the activities I suggest.

Training the Newcomers

Upon their acceptance to the graduate program, before the course work normal to most U.S. doctoral programs, all new and continuing graduate students and their faculty should participate in a twice-weekly, intensive seminar, where all the issues mentioned in this essay are systematically touched upon, accompanied by the relevant literature, appropriate case studies, and frequent oral reports by the incoming graduate students. Alternatively, a well-structured course, richly accompanied by an appropriate bibliography, could be developed. The presence of the older graduate students and of the faculty is very important, even if those would speak against some, or even many, of my suggestions and observations. The very discussion of such matters would achieve the degree of awareness that I consider essential for graduate training. At the end of such an intensive course, the student will have been made aware of the "truths" to question, criticize, contextualize, reaffirm, or abandon. This could be the course that Catharine Stimpson has called "General Education for Graduate Education" (Stimpson, 2002).

Course Work

In general, the less frontal teaching the better. Although students must master some basic material, whether they are in a well-defined discipline or in typically interdisciplinary fields, as far as possible the mode of their studies should be dialogical, that is, small discussion groups, shared problem solving, and discussion of papers by the students themselves and from the printed literature. It is of major importance that such discussions take place in a strictly nonhierarchical atmosphere (what counts is what you say, not who says it), in the presence of faculty (preferably representing several disciplines) and older graduate students.

After the Comprehensive Exams

The period after the student passes the comprehensive exams and is starting the search for a research topic is a most formative one. I consider this period the most important and intellectually most taxing part of a doctoral training period. It should be used carefully to develop the student.

Let me make it very clear once more: simply distributing research topics among the incoming batch of new doctoral students is one of the most
anti-intellectual, and even morally least acceptable, aspects of otherwise very efficiently organized departments. When topics are assigned, the only point of view is the immediate need of a professor or of a research team for solving a scientific problem, which, in itself, can be very important in the body of knowledge and also relevant for society at large. To be given a topic and thus to be used as a minor technician in a huge machinery is the opposite of being trained for intellectual risk taking. Unfortunately, it is common to structure the long period of the doctoral training so that the student spends too much time as a technician (half a year or even a year of “apprenticeship,” depending on the discipline) as a minor cog in a big wheel.

The economic argument that some huge research projects can be financed only if a large number of underpaid graduate students are employed is unacceptable from the educational point of view; in the long run, it is also wrong economically, if the future productivity and efficiency of the graduate student for the profession and the country that is subsidizing the research is taken into account.

Instead, after passing the comprehensive exams, the doctoral student should spend an eight-week period searching the literature, thinking through the foundations of the field, and mapping the problem areas to be worked on in order to zero in on an interesting, important, innovative, and doable research topic. This is a process of intensive, individual, critical reflection, of absorption of great amounts of new material. It is also an important social-educational-collegial process. In such a process, other graduate students at the same stage, advanced graduate students who have embarked already on working on their chosen topic, and the faculty of the department should be actively involved. Moreover, experts in modeling, in techniques of computing, and in planning experimental set-ups should also participate.

The student should prepare his or her idea about problem choice in as much depth as possible at this stage, accompany it with an appropriate bibliography, and present it to the others in a seminar, with faculty and other graduate students present. The student and the doctoral committee should discuss the idea critically and receive from their colleagues encouragement to continue or advice to abandon the topic.

Finding Time by Putting Learning at the Heart

Finally, let me touch on an all-important issue that is also raised by Hyman Bass in his essay. He says, "[A]n obvious and fundamental dilemma. Reform agendas . . . typically know how to add but not subtract. To an already demanding model . . . we have proposed added con-
dictions of performance. Yet the traditional model has already been criticized for the excessive time required."

How true. Bass talks about mathematics, but the argument applies to all disciplines. He has no obvious or simple solution to offer. Let me stick my neck out and propose a solution, which is not necessarily obvious but is very simple. Abandon the conventional wisdom that good preparation of a scholar means that the university has actually taught all the knowledge necessary for the future scientific work of the Ph.D.

The conventional approach suffers from the usual malaise of most educational and instructional theories: it looks at the input and not the output. In other words, we measure our success in preparing the students by what we taught (the input) and not by what they learned, or, even more important, are capable of learning in the process of problem choice—the actual work on the chosen topic (the output). Programs must insist that the students attend only a minimal number of courses to acquire the tools to learn from books or observation. I presume that in general; obviously, the specific list of courses will be discipline-specific. However, by following this recommendation, programs can drop at least half of the obligatory courses, possibly even more.

Rethinking the Faculty

There is one additional important and very complicated issue: whatever is being recommended here can happen only if the relevant faculty play along. Or, in other words, it is not enough to rethink the doctorate. We have to rethink the faculty. This cannot be done by mere preaching, although it is possible to argue and demonstrate that whatever is suggested for the training of the doctoral students will be beneficial for the profession, and, ultimately, for the individual faculty member. A central feature of these recommendations is that they are not realizable in bits and pieces. It is a somewhat different approach to department life altogether, and thus a doctoral training based in the final account on a non-hierarchical atmosphere—a critical, reflective, and dialogical mode for conducting intellectual work.

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NOTES


2. Before the abstract came to be a common prelude to a paper, authors would provide a summary of the argument the readers would find. This seventeenth-century strategy seems highly advantageous, and I offer it here.

3. The biological sciences are much more open to admitting internal contradictions and incompleteness in their theories than are the physical sciences. Of course, whether it is the state of the art in the biological sciences that invites more admitted “messiness” than do the physical sciences is an arguable point. Nevertheless, the biological sciences are much more flexible, open-ended, contradiction-tolerant, and context-dependent than are the physical sciences. Because much of the future lies with biology, the strength of the argument in favor of awareness of context dependence may be a battle half won.

4. Similar deep divides are prevalent in the debates between molecular biologists and evolutionary experts, much as the state of the art is pushing them toward problem areas where the distinction becomes redundant. In the final account, whatever the explanatory framework, the experimental work will have to be conducted more on the molecular or more on the holistic level.

5. I do not do so, either in terms of the famous 1930s formulation of “The Socio-Economic Roots of Newton’s Principia” (Hessen, 1971) or in terms of the 1970s and 1980s “Strong Programme in the Sociology of Science.” See, for example, Scientific Knowledge: A Sociological Analysis (Barnes, Bloor, and Henry, 1996).

6. Nor did this happen for the first time. When Plato introduced epistemic knowledge and fought tooth-and-nail the “metric” (that is, cunning reason) kind of knowledge that was being pursued in the poleis on the Agora, he was doing so in the framework of a strong antidemocratic, aristocratic bias. The great intellectual debate between the Augustinians and the Thomists, from the fourteenth century but especially in the fifteenth and the sixteenth centuries, was conducted in the context of the Reformation and Counter-Reformation.

7. I stress the nonquantitative social sciences, because the quantitative ones are keen on aping the natural sciences, thus their reliance on methodological individualism. The commitment to the belief that because we cannot rigorously study societies, we must study the individual and then try to extrapolate from the individual to the group, is an invitation for consensus.
8. However, emphasizing the necessary conditions (never the sufficient ones) for such developments, some claims about science as a cultural system go beyond what I would call acceptable rational limits: there is an attempt to "relativize" the results of scientific research, explaining scientific activity merely in terms of power, interests, and status, forgetting about the formative role of human curiosity and the dimension of scientific knowledge—clearly absurd.


10. This was the title of the inaugural lecture that Helga Nowotny and I gave when being appointed to the Feyerabend Chair in Theory of Science at the ETH Zürich in 1993. Later it became the title of a short book by Helga Nowotny (1999).

11. I have been a Permanent Fellow at the Institute of Advanced Study in Berlin for many years and thus a participant in their selection process for fellows. We are encountering greater and greater difficulty convincing gifted young researchers, especially in the sciences and, worst of all, in the medical sciences, to benefit from the offer to take a year off under ideal conditions and lean back and think or write or just meditate.

12. If this training is successful, the fresh Ph.D. will dare to "stop the clock" from time to time during the immense pressures of an academic career.

13. The literature on this issue is ample. See, among others, Peter Medawar's "Is the Scientific Paper a Fraud?" (1963/1990), Richard Feynman's Nobel address, "The Development of the Space-Time View of Quantum Electrodynamics" (1966), and above all, see Merton's masterly "Afterword" to The Travels and Adventures of Serendipity (Merton and Barber, 2004, pp. 269–278).

14. According to data from the 2001 "Survey of Doctorate Recipients," the employment sectors of science and engineering doctorate holders were 45.8 percent education, 9.5 percent government, and 44.7 percent industry; see "Employment Sector, Salaries, Publishing, and Patenting Activities of S&E Doctorate Holders" (Hoffer, 2004).

15. Stimpson here quotes an excellent essay, "The Sokal Affair and the History of Criticism" (Guillory, 2002).

16. Actually, scientists very rarely claim absolute truth, yet it has become a widespread belief that they do claim that.

17. In 2003, 48.1 percent of doctorates were awarded to students in the broad areas of physical and life sciences and engineering combined, whereas 46.2 percent were given to students in the fields of humanities, social sciences, and education (Hoffer, 2004).
18. It is, as usual, difficult to pinpoint exactly when and how a public debate started, yet let us start with two books: Higher Superstition: The Academic Left and Its Quarrels with Science (Gross and Levitt, 1994) and The Flight from Science and Reason (Gross, Levitt, and Lewis, 1996). Then came Sokal's hoax article in 1996. I would recommend two good readers to every doctoral student: The Sokal Hoax: The Sham That Shook the Academy (The Editors of Lingua Franca, 2000) and The One Culture? A Conversation About Science (Labinger and Collins, 2001), which is reviewed in detail in Social Studies of Science (Stolzenberg, 2004).

19. Much as I would consider relevant to the training of the doctoral student to get a glimpse into the prevailing theories of mind, I admit that this would be too far-fetched for most doctoral programs in the sciences.

20. See also my “A Programmatic Attempt at an Anthropology of Knowledge” (Elkana, 1981) and the recent “Rethinking—Not Unthinking—the Enlightenment” (Elkana, 2000).

21. The need to rethink the doctorate should not come as a surprise to anybody. The literature is quite extensive. One of the revealing research reports is a survey initiated by The Pew Charitable Trusts (Golde and Dore, 2001). The survey stresses how uninformed many doctoral students are about the various stages of doctoral training, about the details of professional practice, and also of ethical issues. Above all, many students are without any accompanying help in navigating the training process. See also the report “Assessing Research-Doctorate Programs: A Methodology Study” (Ostriker and Kuh, 2003). Unlike previous reports, here the needs of the students are treated as central.

REFERENCES


