

# A Ph.D. Is Not Enough

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*A Guide to Survival  
in Science*

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Peter J. Feibelman



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# *Contents*

**Preface: What This Book Is About** ix

**Acknowledgements** xv

vignette – a brief evocative description, account, or episode

**I Do You See Yourself in This Picture? 1**

A set of **nonfiction vignettes** illustrating some of the ways that young scientists make their lives more unpleasant than necessary or fail entirely to establish themselves in a research career.

**2 Important Choices:  
A Thesis Adviser, a Postdoctoral Job 17**

A discussion of what to consider: young adviser versus an older one, a superstar versus a journeyman, a small group versus a “factory.” Understanding and attending to *your* interests as a postdoc.

**3 Giving Talks 27**

Preparing talks that will make people want to hire and keep you, and that will make the information you present easy to assimilate.

**4 Writing Papers:  
Publishing Without Perishing 39**

Why it is important to write good papers. When to write up your work, how to draw the reader in, how to draw attention to your results.

**5 From Here to Tenure:  
Choosing a Career Path 53**

An unsentimental comparison of the merits of jobs in academia, industry, and in government laboratories.

**6 Job Interviews 71**

What will happen on your interview trip, the questions you had better be prepared to answer.

**7 Getting Funded 83**

What goes into an effective grant proposal, how and when to start writing one.

**8 Establishing a Research Program 95**

Tuning your research efforts to your own capabilities and your situation in life, e.g., why not to start a five-year project when you have a two-year postdoctoral appointment.

**Afterthoughts 107**

A behaviorist approach to professional success.

## *Preface:*

### *What This Book Is About*

My scientific career almost never happened. I emerged from graduate school with a Ph.D. and excellent technical skills, but with little understanding of how to survive in science. In this I was not unusual. Survival skills are rarely part of the graduate curriculum. Many professional scientists believe that “good” students find their way on their own, while the remainder cannot be helped. This justifies neglect, and perhaps not incidentally, reduces work load. There may be some sense to the Darwinian selection process implicit in “benign neglect,” but on the whole, failing to teach science survival results in wasting a great deal of student talent and time, and not infrequently makes a mess of students’ lives.

Since science survival skills are rarely taught in a direct way, most young scientists need a mentor. Some will find one in graduate school, or as a postdoctoral researcher, or perhaps as an assistant professor. Those who do not, to paraphrase Mencken, have an excellent chance of moving from graduate study to scientific retirement without passing through a career. The unmentored can only succeed

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A Ph.D. IS NOT ENOUGH

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by being considerably more astute than the naive, idealistic, and very bright young persons who generally choose a science major.

These thoughts have been on my mind ever since I almost had to tell mom and dad that their golden boy was not good enough to find a permanent (or any!) job in physics, a job for which his qualifications included eight years of higher education and four more of postdoctoral work. The agony of those days is not easily forgotten—the boy with the high I.Q., who had skipped a grade, graduated from the Bronx High School of Science at 16 and from Columbia, *Summa Cum Laude* at 20, found himself in a muddle at 28. How do you choose a research problem? How do you give a talk? What do you do to persuade a university or a national or an industrial lab to hire and keep you? I hadn't a clue, until, midway through my second postdoctoral job, I had the good fortune to spend some months collaborating with a young professor who cared whether I survived as a scientist. Although this mentoring relationship was brief, it helped me acquire a set of skills that graduate education did not, skills without which my lengthy training in physics would have been wasted.

This book is meant for those who will not be lucky enough to find a mentor early, for those who naively suppose that getting through graduate school, doing a post-doc, etc., are enough to guarantee a scientific career. I want you to see what stands between you and a career, to help you prepare for the inevitable obstacles before they overwhelm you. In short, I hope to enable you to use your

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Peter J. Feibelman

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exceptional brainpower in the way that you and those who put you through school have dreamed about.

I begin with some brief “case histories.” This may help to put your own early career in better perspective. At least I hope it will give you a feeling for how important mentoring can be. Succeeding chapters are arranged in parallel with a career trajectory. Please skip ahead to whichever may be relevant to your situation. Chapter 2 deals with choosing a thesis or a postdoctoral adviser. My choice of thesis adviser was based on two criteria: Who is the most eminent professor in the department? And whose students finish soonest? Was this intelligent, or did it represent a first mistake? Chapter 3 concerns oral presentation of your work. However brilliant your insights, they will be of little use if you cannot make them appear interesting to others. If no one pays attention, what difference does it make if your results are clever! There are of course Nobel prize winners whose orations are Delphic, whose overhead foils look as though they were scribbled during a particularly turbulent flight, and so on. But you are not one of them yet, and if that is how your talks are prepared, you never will be either. There is more to Chapter 3 than “neatness counts,” however. It contains several important ideas on making your oral presentations effective.

In Chapter 4 you will find a discussion of paper-writing. Through your scholarly articles, you can make yourself known nationally and internationally. This means that your reputation in science does not just depend on what your boss says about you, but on documentation that is

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A Ph.D. IS NOT ENOUGH

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available in any science library. You should therefore view publishing as a means to attaining job security, and take the task of writing compelling journal articles very seriously.

Chapter 5 is devoted to career choices, mainly the merits and defects of positions in academia, and in government or industrial labs. The focus is on being reflective and rational rather than naive or romantic about key decisions in your scientific life. In Chapter 6 I discuss job interviews. There is more to an interview than wearing your Sunday best, and having a firm handshake. Doing your homework and persuading your potential employers that you have a sense of direction are the most important issues. Incidentally, this is not a matter of deception—knowing who your colleagues will be, and developing an idea of what you want to know, scientifically, are keys to having a productive career. There are also a few choice words in this chapter about negotiations, once you do get an offer. Negotiating for what you will need, when your leverage is maximal, can make a large difference to your happiness and to your success.

In Chapter 7, I discuss what to many is the bane of scientific life, namely getting money. This used to be the exclusive headache of academic scientists, but nowadays it is also a significant part of the lives of government and industrial scientists. I suggest that you view the preparation of a proposal as an important scientific exercise. Coming to see, and being able to articulate how your work fits into “the big picture” is not only essential to winning financial support, but also to being a first-class researcher. Learning

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Peter J. Feibelman

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to distinguish extravagant “pie-in-the-sky” from promises that you have a chance of fulfilling is also very valuable.

The most difficult problem in being a scientist is selecting what to work on, and it is even more difficult when you are just launching your career. Therefore, in Chapter 8, I venture a few comments on establishing a research program. Jumping into the hottest research area may not be a very good idea, nor is taking on a project that you have no realistic hope of completing before your short-term employment comes to an end. The main idea is to establish a program that simultaneously maximizes your chances of continuing employment *and* of scientific achievement. The focus is on strategic thinking.

As this book is written, economic times are tough worldwide and funding for scientific research is contracting. I hardly need to emphasize that when resources become scarce, competition intensifies for what remains available. To win a permanent position in scientific research, and the funds to carry on serious work, you will have to be exceptionally thoughtful about your career choices. My hope is that this “pocket mentor” will help you to become more introspective about what it will take to succeed.

—Albuquerque, NM  
August, 1993

## *Acknowledgements*

In order to make this handbook accessible to people whose backgrounds, experiences, and scientific interests differ from my own, I have prevailed on several friends and colleagues for advice. I am very grateful to Professors Michael J. Weber and Alison P. Weber of the University of Virginia, for numerous constructive criticisms of the first draft. I would also like to thank Dr. Ellen Stechel, my colleague at Sandia National Laboratories, and Professor George Luger of the University of New Mexico for their critical readings of the manuscript. Finally, I thank my wife, Lori, for many editorial improvements.

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# Do You See Yourself in This Picture?

The brief stories in this chapter have a common theme: that understanding and dealing rationally with the realities of a life in science is as important to science survival as being bright. Once you leave graduate school, the clock is ticking. Unlike a fine wine, you do not have many years to mature. As a young professional, you must be able to select appropriate research problems, you have to finish projects in a timely manner, and you ought to be giving compelling talks and publishing noteworthy papers. When job opportunities present themselves, you should be able to assess their value realistically. Romanticizing your prospects is a major mistake, and is likely to have serious consequences, not excluding dropping out of scientific life prematurely. The first story is an excerpt from my own scientific beginnings. The others are also nonfiction, though I have altered locations and personal characteristics to avoid invading the privacy of the protagonists. I have deliberately identified the various characters with initials, rather than names, to avoid any ethnic implications.

*What Do Scientists Do?  
Technique versus Problem Orientation*

Virtually all classroom work and much of what happens in a typical thesis project is aimed at developing a student's technical skills. But although the success of your research efforts may depend heavily on designing a piece of apparatus or a computer code, and on making it work properly, *no technical skill is worth more than knowing how to select exciting research projects*. Regrettably, this vital ability is almost never taught. When I signed on with a research adviser in my first year of graduate school, I was thrilled to be given a problem to work in the physics of the upper atmosphere. The fact that I had no idea what motivated the problem did not prevent me from carrying out an analysis, on a supercomputer of the day, and publishing my first paper at the age of 22. For my thesis, I consciously switched to a project that would require learning the tools of modern quantum physics, but again I found myself assimilating technical skills without ever grasping the significance of the problem, without understanding how or whether it was at the "cutting edge" of science. This way of working became a habit, one that seriously threatened my career. My first seven publications were in seven different areas of physics. In each case I relied on a senior scientist to tell me what would be an interesting problem to work on; then I would carry out the task. I assume that it was my ability to complete projects that impressed my superiors sufficiently to keep me employed. It certainly wasn't my depth in any field.

Four years and two postdoctoral positions after earning a Ph.D.—still having little sense of what I wanted to learn as a scientist—I was in the job market. More than anything else, I needed good recommendations from faculty at the university where I was employed. I was asked to give the weekly solid-state physics seminar, and realized at best dimly that my performance in this venue was either going to make or break me as a scientist.

The talks I was giving at this point in my career reflected my approach to science. There was little in the way of introductory material. Much of the presentation was technical. I would describe a few “interesting” problems I had worked on and explain the methods I had used, but would give little idea of context because I really didn’t know what it was. For the seminar at hand, I prepared my usual hodgepodge of this project and that, with no introduction, no theme, and ultimately, no meaning to anyone but an “expert.” Fortunately, the professor supervising my research, C., understood what was about to happen to me, and asked for a preview of my seminar in his office. Thank goodness I accepted this invitation. C. expressed surprise at how poorly I had prepared my talk (though I don’t think he was surprised at all), how little grasp I seemed to have of the reasons that the problems we had worked out were meaningful, and consequently, how uninterestingly I was going to present them to my audience. But, he told me, he thought I was too good technically to be allowed to fail in the way I was about to, and he gave me the lesson I needed.

His most important advice was:

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A Ph.D. IS NOT ENOUGH

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1. There has to be a theme to your work—some objective—something you want to know. There has to be a story line. (Do not start with: "I have been trying to explain the interesting wavelength dependence of light scattering from small particles," but rather "There is a widespread need to explain to one's kids why the sky is blue.")
2. If you know why you have chosen to work on a particular problem, it is easy to present an absorbing seminar. Start out by telling your story, why the field you are working in is an important one, and what the main problems are. Give some "historical" material showing where the field is, the relative advantages of different methods, etc. Then outline what you did, and describe your results. Conclude with a statement of how your results have advanced our understanding of nature, and perhaps give an inkling of the new directions that your work opens up.

Do not assume that your audience is comprised of "experts." There may be a couple of them, but even experts like to hear things that they understand, and particularly to have their colleagues hear (from someone else) why their field is an important one.

3. Lastly, rehearse your talk in front of one or two of your peers and/or professional supporters. Choose listeners who will not be shy about asking questions and giving constructive suggestions. Giving a seminar is serious business. Your future depends on the strong recommendations of your senior colleagues. If your talk is a hodgepodge of techniques or experiments or equations, if you seem to

have no idea of where you are headed, if you reek of deference to the experts in the audience, you will not be perceived as a “rising star,” a budding scientific leader. You will fail.

The wonderful result of C.’s mentoring was that I finally learned what it means to be a scientist. By making my work meaningful to others, it became interesting to me as well. No longer was I just working on somebody else’s problems. I was part of an intellectual enterprise with relatively well-defined goals, that might actually make a difference to humanity. I scrapped most of the equations I had planned to show, and refocused my talk using thematic material I had garnered from C. I gave an excellent seminar—people I scarcely knew complimented me afterward on the interesting field I had chosen to work in, and remarked on the clarity of my presentation. In science, the reinforcement doesn’t get much more positive than that. I had learned a key lesson and was on my way.

### *Timing Is Everything*

Having completed a respectable thesis problem and having acquired a reputation in graduate school as an excellent sounding board and scientific consultant, T. accepted a postdoctoral position with a leading scientist at a first-rate government laboratory. There, he was offered and began to work on a computational research project that first involved arriving at a numerically practical mathematical formulation of a problem, and then a considerable

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computer programming effort. As the months passed, and with the necessity on the horizon of finding a permanent job, T. absorbed himself totally in his very challenging work. Whereas, in graduate school, under little time pressure, he would have spent a few hours each week visiting labs and contributing to projects other than his own, as a postdoc, T. became utterly single-minded.

Working 12 hours a day and more, he managed to complete his computer program soon enough to be able to run test calculations. The results were promising, but not especially interesting, which meant that neither T. nor his audiences found his job seminar particularly exciting. Besides, since he had not spared time to meet and consult with scientists at his lab, his only strong recommendation was from his postdoctoral adviser. The lab itself was unwilling to promote T. to a permanent position, which it sometimes did, because he had not made himself useful, or even known, to a spectrum of its staff members.

On the outside, his job offers were a cut below what his thesis adviser had expected for him. In the competition for the best positions, T. did not persuade potential employers that he would ever derive useful results from his postdoctoral project, even while T. believed that he would be able to produce them within six months to a year. Other job candidates, whose postdoctoral work had been far less ambitious, but had resulted in two or three finished projects, appeared much more impressive. Moreover they had obtained excellent recommendations from the experimental colleagues whose data they had analyzed.

On the whole, it is hard to blame potential employers for their view of T. To them he was “a pig in a poke,” an unknown quantity. His thesis work might just have been done by his thesis adviser, and his postdoctoral project—though in principle a worthy one—was incomplete. Would T. be able to complete projects on his own? Is he a self-starter? The information simply was not there in the eyes of the interviewers.

To some extent T.’s fate was the “fault” of his adviser. Assigning a long-term project to a postdoctoral researcher who has to go on the job market in 18 months is a clear risk to the postdoc’s future. But, had T. been as reflective about his career as he was in carrying out his research, he himself would have realized the dangerous path he was taking. As exciting as his assigned project seemed, he would have recognized that his postdoctoral years were the wrong time for such a large effort. At the very least he would have reserved time each day or week to establish contact with other researchers at the lab, and involved himself in one or two short-term projects with a clear chance for success. Many a graduate student or postdoc spends time trying to understand what his adviser wants and getting it done. In fact, it is the young scientists who define and carry out what *they* want, who learn to be scientific leaders, who find the best jobs and have the most productive and satisfying careers. Making your thesis or postdoctoral adviser happy is sensible, and worth doing, but not more so than acting in your own best interests!

*Know Thyself:  
A Sweet Job Turns Sour*

B. obtained a Ph.D. from a top-flight university in the Midwest. He had two different thesis advisers during the course of his four years as a graduate student. The first was a Nobel Prize winner, a theoretician whose name is a household word to chemists. The second was an experimentalist, also a very widely respected scientist. Having completed his degree, and cognizant of the scarcity of "real jobs," he accepted a "permanent" position at a major laboratory instead of a postdoctoral, temporary slot. It did not take him long to realize that this apparently wonderful opportunity was a trap. On arrival at his new location, B. was presented with two opportunities. A senior staff member, who was involved in a major experiment, suggested that B. begin his tenure by working in his lab. That way, B.'s knowledge of the experimental aspects of his field would deepen, and after a couple of years he would be much better prepared to work on his own. Objectively, one would say that this was a wonderful opportunity, effectively a postdoctoral job, but at a regular staff salary and with a reasonable approximation to regular staff job security. B.'s alternative option was to begin independent work immediately. Talking to his younger colleagues, he heard that, in the eyes of management, a full staff member was supposed to run his own research program, and that at the annual performance review, if he was perceived to be

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Peter J. Feibelman

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working as someone else's "assistant," his rating, salary, and job security would suffer, perhaps irretrievably.

One does not have to be a rocket scientist, as they say, to appreciate that B.'s two-year stint as a graduate student in experimental physics was inadequate preparation for him to perform at the level of his supposed "peers." Nevertheless, unmentored, B. was not willing to risk his all-too-sweet regular staff position by choosing the training that he badly needed. This was a mistake. After three years of buying equipment and setting up a lab, B. had still not established a research program, and indeed had little idea of what he wanted to accomplish as a scientist. Thus, despite its investment in his laboratory equipment, and despite his formally very impressive pedigree, B.'s employer moved him out of basic research. In an environment where goals were clearly defined from above, he eventually matured into a real contributor, and is reasonably happy. On the other hand, he is not doing basic research any more, and he went through several very stressful years as a result of his bad start. Like many whose scientific careers flounder, his failure at work coincided with the breakup of his marriage.

### *The Ph.D. Technician*

L. spent two postdoctoral years at a prestigious lab, switching into a new field. He had been hired as a postdoc there because of the technical know-how he had acquired via his thesis work. As a postdoctoral scientist, his task was to

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A Ph.D. IS NOT ENOUGH

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build a piece of equipment combining technology in his new area with that of his thesis work. The lab where he did his stint as a postdoc was well-enough satisfied with him. At the end of his two years, the desired instrument was in place, and L. had his name on a couple of publications with his postdoctoral adviser. Of course it was recognized that L. had not really learned the basics of his new field, and so his postdoctoral employer did not offer him a permanent position.

A more aggressive or aware young man might have spent a significant fraction of his two years not simply building the desired instrument, but rather asking questions about the direction of his new field, reading as widely as possible in its literature, and formulating a research direction of his own. L. did not, however, and even at the end of his postdoc, no one had told him, nor did he realize that becoming an expert in a field and having an exciting research program is an essential aspect of being a scientist. L. did manage to land a "permanent" job after his postdoc. But as in B.'s case, permanency was an illusion.

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In his new job, L. again built an instrument. But he never participated as an equal member in the group that hired him. At seminars or in planning research proposals he had little to contribute. When he went before his manager to explain what his research plans were, he could say no more than that he planned to look around for "interesting" problems. L.'s employer was happy to possess the new instrument that he had built and got running. But it was not long before L. was moved from the research division of his company.

Some will argue that L. just wasn't suited for research, that his fate was predetermined by his personality. This may be the truth. On the other hand, I have the lingering feeling that if L. had been appropriately mentored at some point during his decade of higher education and as a post-doctoral researcher, he would have succeeded in the career for which he had trained, or perhaps would have switched earlier to a more appropriate field of specialization. It remains to be seen how well he will perform in his new job.

### *Institutionalized Conflict*

Managers make many mistakes. More often than not these hurt the people they manage, rather than themselves. Consider F.'s experience as a postdoc in R.'s lab. R. had been hired after a two-year postdoctoral position, but had the wit to appreciate that his "permanent" position would only *really* be permanent if he proved himself a capable scientist in his first two or three years. He invested his first year building a lab around a major piece of equipment, and was ready to begin to do science when F. appeared at his threshold. F. had hired on to work on a project that seemed rather exciting to its managerial proponents, but had failed to get the hoped-for, and necessary, external funding. The result was that management had to find something else for F. to do, and had decided that since his training was similar to R.'s, F. would be a postdoc in R.'s lab. The results were inevitable. Being a clever young man, F. realized that his future depended on gaining rec-

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A Ph.D. IS NOT ENOUGH

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ognition for a significant piece of work, work that would have to be done in short order. R., no less clever, understood that his probationary position required him to complete several projects and get credit for them. The result was not a happy collaboration, but months of bickering over who would turn knobs on the machine and who would get credit for the scientific progress. Despite its responsibility for a bad situation, management did not like to hear the resultant whining, from either side. F. ultimately won credit for most of the work done in R.'s lab, with the result that R., whose competence was felt to be more technical than scientific, was moved out of research. But management's distaste for F.'s complaining far exceeded its pleasure in his scientific achievements. F. was not considered as a candidate to replace the hapless R. He did eventually find another position in science, though, and I hope he will succeed.

Post-mortem: Successful collaboration is possible when one or both contributors have established reputations, or when each researcher brings a different, identifiable skill to the collaborative project, for example when a theorist and an experimentalist work together. Collaboration does not work, as a rule, for two young competitors. Neither F. nor R. was mature enough to realize that F.'s postdoc was a predictable nightmare, an arrangement that should have been refused by both of them.

If F. and R. had found or had been assigned appropriate mentors, early on, they might have been able to deal with the competitive relationship that had been imposed on them. If management had explained to F. at the outset that

R. was to be "the boss," and had discussed with both how credit for results was to be allocated, then F. could have made an informed decision on whether to work in R.'s lab, and he would have had little reason to complain later on. However, on their own, F. and R. spent a miserable year and a half together, and R.'s scientific career is just a memory.

*Impressing Mom and Dad:  
Whose Life Is It Anyway?*

A common theme in the minds of young scientists is impressing Mom and Dad. This strong motivation is to be cherished, of course, but only if it does not overwhelm one's ability to make rational decisions. H. is the eldest daughter of a successful professor of microbiology. Having obtained a Ph.D. in an area of limited interest to employers, she decided to switch fields, hoping that her technical expertise would enable her to establish a niche. However, she decided to carry out this (wise) move as an assistant professor at a prestigious university (a questionable choice, at best).

A major factor in this decision was that she wanted to show her father that she could succeed in the academic world, just as he had. Had she thought her choice through, H. would have realized that when her dad was starting out, research funding was expanding dramatically, making the odds of success much better, and also that her next five years were going to be a major struggle, a period when any desires for a personal life would have to be suppressed. Between coming up to speed in her new field,

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A Ph.D. IS NOT ENOUGH

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fulfilling her teaching assignments, writing proposals and building equipment—all essential before any research results could be produced—H. found herself spending 16-hour days in her office, the classroom, and her lab. Yes, she did receive tenure after five years. So in that sense she did succeed. But in that time, she had no life outside of her work, and by the time she was done, her marriage had disintegrated. Did this impress Dad?

In a national or industrial lab, H.'s plan would have been much easier to realize. With no teaching assignments, no committee meetings, no insistent students at the door, wanting their grades explained, she could have made her name working eight or maybe ten hours per day. After five years of building a lab and producing science, she would have had little difficulty landing a tenured job at an excellent university. In the meantime she would have had time for her family—maybe even time to have the child she wanted. She would have been earning 30 to 60 percent more, she would have had better job security, and time for reading a novel or taking a vacation. Things are working out for H. now, but she paid what I see as a high price for the romantic notion that she needed to move directly into academia to win her dad's approval.

### *Get a Mentor*

I certainly hope that reading this book will help you recognize what is in your own interest. But no author can be expected to foresee your own special pitfalls. The best preparation you can make toward the goal of having a sci-

entific career is to find yourself a “research aunt or uncle,” someone with little or no authority over you, who has enough experience to act as a sounding board and giver of accurate advice. Do not be shy about getting to know people outside your advisor’s realm. The scientists at your lab will very likely cherish the human contact. They spend lots of time behind the closed doors of lab and office, and everybody likes to give advice.

# Important Choices: A Thesis Adviser, a Postdoctoral Job

As a young graduate student, I selected a thesis adviser on the bases of his prominence in the world of physics, and his reputation as one who would not require me to spend too much time in graduate school. As with other aspects of my early career, I now see these criteria as reasonable but insufficient.

### *A Prominent Scientist as a Thesis Adviser*

Choosing a prominent thesis adviser makes a lot of sense, but not because brilliance is transferable. It is not, as I have witnessed more than once. Trying to be “another” Linus Pauling, Roald Hoffmann, James Watson, or P. W. Anderson is a common road to failure. What a prominent adviser *can* offer is: 1) being part of the “old-boy network” (he can help you survive if times are tough, sometimes even if you don’t deserve to), and 2) not competing with you.

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Point 1 is self-evident upon a moment's thought. Point 2 is not so obvious to the naive. A young adviser, trying to make his way in the world, does not want to be shown up by his student or postdoc. He has a lot to prove, and is therefore unlikely to be generous with credit for ideas or progress. An adviser who has made his mark views the accomplishments of his students with pride, even joy. They are his research "children." Thus, other things being equal, an established (tenured) professor is a superior choice for an adviser. This recommendation is a simple corollary of the way universities are organized. It is not an indictment of young professors to recognize that they are likely to view their own scientific survival as more important than that of their students.

A more senior adviser also offers you better prospects of finishing the thesis project that you start and of spending your entire graduate career at one university. Many assistant professors fail to win promotion to tenure. If this happens to your adviser, he or she will either have to move to another university or may drop out of academic science entirely. In either case you will face unwanted, difficult choices: whether or not to move with your adviser, or whom to choose as a new one, whether to select a new dissertation topic or to try to find another professor who is willing and able to help you proceed in your initial direction.

Although a senior professor may also move to another job while you are a student, the probability is lower, not only because he or she holds a tenured position, but also because the bother involved in moving an established, large group is substantial. If your senior professorial ad-

viser does decide to move, the consequences for your thesis project are unlikely to be dire. A senior scientist relocates by choice, usually because the funding situation in the new location, or perhaps other aspects of scientific life, are better. Moving with your adviser is thus likely to be both financially possible and scientifically desirable. If you do decide to move with him or her, the delay in your progress toward a Ph.D. should be minimal.

Obviously, an older professor has a better chance of becoming seriously ill or dying while you are a student. Otherwise, the chances of a senior scientist's dropping out of research entirely are rather remote.

**TENURE AND PROMINENCE ARE NOT ENOUGH:** Although signing on as the student of an established scientist has many clear advantages, choosing a good adviser is not as easy as finding out who has won the most important prizes, gives the most invited talks, or brings in the largest research grants. Does the established person you are considering make himself available to his students? Does he give real guidance? Is he comfortable talking to people who are not his scientific peers, i.e., beginners like yourself? Does his group have a sense of purpose? Do they interact with each other? And does Professor Eminent teach "survival skills"? These are important questions. Making a mistake in choosing your adviser can mean years of frustration. If you can learn the answers to the important questions in advance, by talking to his current or former students, you may save yourself a lot of grief.

**DO HIS OR HER STUDENTS SEE THE BIG PICTURE?** Prof. E. was obsessive. He was obnoxious. I have heard it said that he didn't know quantum mechanics. But his contributions

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A Ph.D. IS NOT ENOUGH

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to materials science were manifold—and his students have done wonderfully well. They knew what they wanted to learn, and they learned from each other. Thus, even if E. was often away consulting at industrial labs, his students thrived.

How do you find out in advance whether the group you are thinking of joining will be like E.'s? Visit the members. Ask them what they are doing. See if they can explain "the big picture." If they cannot, find a different adviser.

Often a prominent scientist will lead a big group. He may have 15 or 20 experimental systems that permit him to employ an equal number of graduate students, to study "trends." These students are guaranteed to finish their degrees in a reasonable period of time. In total contrast to my own graduate student experience, they are assigned very specific problems. They take their data, report their results, and get their degrees. It all seems so easy. Should you be part of this kind of group? Again the issue is whether the students have an inkling of "the big picture." Is it only the adviser who knows what trend is being studied, while student A is looking at rhodium, B has a sample of ruthenium, and C has some palladium? If the students cannot tell a good story, move on!

### *Choosing a Postdoctoral Position*

How should you be rational about the choice of a postdoctoral position? It is essential to understand what your interests are, and how they differ from your employer's. To begin, you should realize that what you actually achieved

in your thesis is not especially important to your postdoctoral adviser. If you are one of the few whose thesis represents a major breakthrough, you will probably be much in demand, and you will likely have few problems finding a permanent job. Probably you won't ever have a postdoctoral position. Your problem may be that you will spend the next several years trying to show that your initial triumph was not a fluke. This kind of thinking has paralyzed more than a few young "geniuses," but is not an important consideration for the majority, for whom this chapter is written.

If your thesis, as is more likely, has not attracted much interest, despite your worries you will probably find a postdoctoral slot. Employers generally feel that a postdoctoral employee is not a big risk. A postdoc will be gone in two to four years. If he is helpful and productive, so much the better. If he launches a successful career as a result of his postdoctoral research, that would be wonderful. But if not, it would be viewed as unfortunate, but not unusual, and not disastrous. For the typical employer, a postdoc is cheap labor. Unlike a graduate student, who has to be shown the ropes and whose education may absorb so much time that his net contribution to the progress of a project may be slight, or negative, a postdoc is a trained researcher. He can be expected to be reasonably competent, and not to be terribly demanding of his supervisor's time. Simultaneously he is "cheap."

At the laboratory where I work, and this is not unusual, a postdoctoral employee receives minimal benefits. The lab pays his medical insurance, but makes no

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contributions to a pension plan. His paid vacation is only two weeks per year and his salary is not "loaded" with substantial overhead or "indirect costs." Acquiring a post-doc is therefore much like buying a piece of laboratory equipment. One assumes that it will work for a while, helping to produce results. Then it will be replaced with a newer model.

Thus, from the postdoctoral employer's point of view, the most important qualifications of the postdoc candidate are 1) that he should present his thesis research well—this implies that he will be a good spokesman for his supervisor's research program, 2) that he should not have taken overly long to finish his Ph.D.—after all, he will be employed as a postdoc for only a few years, after which time one hopes he will have produced a paper or two per year, and 3) that he should be interactive—he should make the research group livelier.

If the postdoc candidate wants to change fields, that is not a problem. Indeed this is a very common practice. If he did not produce a major piece of new knowledge in his thesis, that is not either, because, in general, he is hired fundamentally to further his supervisor's research program. If he breaks new ground, does something important during his postdoctoral period, he may be offered a permanent job. If not, he will go away, and not much will have been lost. This is the employer's perspective. What should yours be?

You have three important tasks in your postdoctoral years: You must decide in what area of science to make your name. You must *finish* at least one significant project.

And you must establish your identity in the research community sufficiently to land an assistant professorship or a junior position in an industrial or government laboratory. You have little time to waste, because it will not be long after you begin your postdoctoral work that you will be back on the job market.

These considerations imply that: 1) you do not want a position where your field of research is undefined. You want to get to work on a significant research project on arrival or shortly thereafter. 2) You do not want a position in which a complex technique is being perfected (which means that your chance of producing results in time for your job-hunt is minimal). You want to be involved in one or several short-term projects.

If you are changing fields, you want to start your reading and learning *before* you arrive at your postdoc site. The clock starts ticking when you get to your new location. Whatever you do before you leave the nest of graduate school doesn't count, for all practical purposes.

Generally it would be wise to find a mature scientist for a postdoctoral supervisor rather than a relative novice. The main reason is the same as for a thesis professor. You do not want to be in competition with your postdoctoral adviser. If there is only one apparatus in his laboratory, if his computer budget is relatively thin, do you think he will let you use it as much as you need to? If he has only six years to prove himself as an assistant professor, will he be capable of recognizing the importance of your achieving recognition after only a year or so? Very likely not, logic dictates. Unless you can find an assistant professor or

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junior industrial researcher who is a “superstar,” or at the very least, unless you can satisfy yourself that the young scientist you want to work with understands, and agrees to accommodate your needs, you would probably be better off working with someone established.

KEYS TO SUCCESS AS A POSTDOC: Once you do take a postdoctoral position, the keys to success are 1) *finish something*, and 2) make yourself known and useful. Your first priority as a postdoc is to have something to talk about when you go job hunting. No employer wants to hire a person who starts but cannot finish projects. Even if you have put a year and a half into developing a *very* promising method, you will lose out in the job market to your competitor whose methods may be less adventurous, but who has produced a kernel of new knowledge, who has written it up and published it.

I do not recommend that you be careless in your research endeavors. Nevertheless, you should be aware that it is possible and may be desirable to publish an interesting result before the last i's are dotted and t's are crossed. It is possible, and relatively risk-free, if you are honest in your manuscript about the work that remains to be done. It may be desirable because someone who has an interesting story to tell, even if it is only supported by admittedly plausible evidence, will win out in the job market over someone whose very thorough effort is not far enough along to allow conclusions to be drawn. Although attention to detail is important, and publishing results that later turn out to be incorrect is anything but desirable, *finishing* projects and having a story to tell are essential. As a post-

doc, under time pressure, you may have to sacrifice your desire for perfection, you may have to live with the fear that you haven't got everything just right, in order to develop a story that you can use to sell yourself. This is not cynicism but realism, and is worth remembering for your entire career. The famous physicist Wolfgang Pauli is remembered for complaining ironically that the work of a young colleague "isn't even wrong." Think about *that!*

**DO NOT BE A SLAVE TO YOUR POSTDOCTORAL ADVISER:** If you just sit in your office working while you are a post-doc, your supervisor will know you, but no one else will. You will get one good recommendation letter, assuming you have performed well, and that is all. If you chose a thesis adviser with good connections, he or she may still be able to help you find a "real job" after your postdoc. But what you accomplished as a graduate student does not count for much in later life, unless it is very exceptional. If your thesis adviser helps you find a job via his or her connections, it may be looked on as being *despite* your performance as a postdoc, and the burden on you to prove yourself in a junior, "continuing" position may be greater than otherwise.

What you really want to achieve as a postdoctoral researcher is that three or four staff members where you work will think highly of you and will write you good recommendations. If you are a theorist, plan on spending two or three hours weekly talking to experimentalists, and vice versa. Barge into peoples' labs, politely, and find out what kind of work is going on. Discover whether there are other research programs to which you can contribute. Get

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copies of your lab's preprints. Read them, and if you have criticisms, questions, or contributions, make them known. Every lab is eager to employ and to recommend interactive people. If you are congenitally shy, you have a real problem, one that it would be helpful to overcome. In any case, try to focus on the idea that positive feedback from the people you help will help you psychologically, and the recognition that their positive comments to others will advance your career.

Above all, during your postdoc years, work hard! You have only a short time to prove yourself. Do not slough off now. There is no time to waste. Your postdoctoral years represent the most intensely important period in determining whether you will have a career.

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## Giving Talks

*Tourist to New York passerby: "How do you get to Carnegie Hall?"*

*Passerby to tourist: "Practice, practice, practice!"*

On a job interview trip, your task is to persuade a significant fraction of the professionals who see you that they would be excited to have you as a colleague. The seminar that you present is your best opportunity to convey the message that you are the person to hire. The same applies when you report on your progress after a year or two in a new position. The colleagues who know you best may already think very highly of you. But they have only a few "votes." By giving a good seminar you can add to the base of support that you will need to be kept on or promoted. Finally, remember that few professional scientists have much time for reading. The way that they learn of new and interesting work, nowadays, is by going to meetings and listening to seminars. If you present your work well in these venues you will be much better able to attract a following. Having a following is an excellent form of job security.

Because oral presentations will play a vital role in your career advancement, you must take their preparation very

seriously. Learning from scientists who present their talks well is a good idea. In this chapter I hope to impart some of the basic concepts.

### *The Scientist as Showman*

Although a seminar is not a theater piece, there are common elements. As the speaker you are putting on a one-man show. Your listeners are investing an hour of their valuable time. Of course they want to learn something from you, but like theater-goers, they expect to hear a good story, with a beginning, a middle and an end. They don't want to squirm when you explain something poorly or wrongly, when you put up an overhead containing an egregious misspelling or an inkblot, or when the end of the hour is approaching and you obviously have a lot left to tell. Disappoint your listeners at your peril. They might not throw tomatoes or rotten eggs, but they might dismiss you, might be unwilling to find out how good a researcher you really are—just because you put on a bad show.

### *The Introduction*

A fundamental principle, in preparing a talk, is *never overestimate your audience*. No matter how grey their beards, no matter how many papers a few might have published in your field, those frightening-looking people in the audience *want* a complete performance. They want you to say what is important in the area of interest, particularly if what is important happens to be their own work! They don't mind hearing things they already understand—it makes folks feel good to understand something.

The opening lines of a talk set the tone, make a “first impression.” The main impressions that you want to make are 1) that you know your field, 2) that you are possessed of the scientific curiosity that will make you a valuable colleague, 3) that you enjoy doing research, and 4) that you plan to convey some useful and interesting information. Tell the audience what the “theme” of your presentation is; or tell them that your work was undertaken to resolve a particular controversy, and why it is an important one; or tell them that you have demonstrated a novel technique that permits access to new and useful information.

Do not simply launch into a discussion of the experiment or calculations that you did. Establish the context of your research to the degree that time will permit, give an overview of the novel technique, ideas and/or shortcuts you have employed, and possibly, intimate what the most important conclusions are. (“These measurements, as you will see, confirm the long-standing, but until now unproven predictions in Feibelman’s early, brilliant paper.”)

This done, you can go on to discuss the specifics. If you are giving an hour’s talk you will want to expand on your introductory remarks before launching into the details of your own work. In a ten-minute paper at a large meeting, a one- or two-slide introduction may be enough.

### *Stagecraft*

Be aware of the importance of your demeanor, particularly your air of self-confidence. If you speak almost inaudibly, it will be assumed that you lack confidence in, or do not understand what you are saying. If your presentation is too

low-key, you may convey the idea that you are not enthusiastic about your work, or perhaps about research in general. Scientists are like terriers, trained to chase down and pick apart reasoning that is not rigorous. If you appear confident, your presentation is more likely to be accepted at face value. If not, you can expect to be fielding insistent questions early on, and may never get to finish your talk. Alternately, you may see people walking out of the seminar room. If you are interviewing for a job, that could be rather disconcerting.

Time is of the essence when you are giving a talk. You must plan your presentations and rehearse them, to ensure that you will be able to finish before your time is up, or at least to be sure that you will have conveyed the main ideas by the time the bell rings. You can easily determine in practice sessions how long it takes you to present an average slide. This will make it easy to fix an upper limit on the number of slides to prepare for a given time slot. Personally, I can discuss six or at most seven slides in ten minutes. If I prepare more than that, I know that my talk will be breathless and that my audience will absorb little. They may well respond to a talk too crammed with information as a "snow job," an attempt to disguise the flaws in your work by overwhelming your listeners with words and figures. Designing a "modular" talk is a good idea. After your introductory "module," you present several complete information packages in sequence. That way, if you see that your time is running low, you can excuse yourself for leaving out the last module and skip ahead to your summary.

*Don't Try Their Patience*

One of the first lessons students learn about giving a talk is to “prepare an outline.” Many of them are also apparently taught to begin with a slide that gives “an outline of my talk.” I often find these slides a waste of time, if not downright silly, and would like to dwell here on the structure of a talk, not just to help you, but in the hope that I will have to sit through fewer outline slides in the future.

Have you read a novel recently, or seen a play that started with an “outline of the plot”? When a political candidate gives a speech, does he put his outline on a chart? Of course not, and in general, neither should you. You certainly should outline your presentation in the privacy of your office. But in giving your talk, you should just tell a story. Its structure should be “organic,” invisible. Your listeners should be propelled from idea to idea with the same sense of inevitability they feel on hearing a Bach fugue.

At meetings of the American Physical Society (large meetings), contributed papers are allotted ten minutes plus two for questions and discussion. Thus, I can present six or at most seven slides in such a talk. What message do I convey if slide 1 is “The title of my talk,” and “these are the names of my collaborators, and I want to thank the Department of Energy for my funding,” and then slide 2 continues with: “I will begin my talk with a brief introduction. Then I’ll discuss our experimental apparatus. Following that, I’ll present my results for system X, and finally, I’ll end with some conclusions.” All right, this is something of

an exaggeration, but it is not an enormous one. What it conveys is that "I don't have much to say, so I'll throw away most of my time telling you how I planned my talk and who my friends are, leaving little time for any discussion of what I have learned." If you have nothing to say, you would be better off not giving a talk. If you do opt to speak, you do yourself an injustice not using virtually all your time to present your ideas and results.

One of the wonderful abilities people have is to take in different information with their eyes and ears, simultaneously. If you have collaborators not announced as coauthors, and a funding agency, do acknowledge them on your title slide (Fig. 1), but do not waste time reading their names. Someday, when you are a professor, and are trying to place your students, then you can mention their names and good qualities (usually at the end of your seminar). Now, however, you are the person you are trying to sell. Acknowledging your co-workers is important, but should not be overdone.

What you want to convey in your introduction, while your title slide is on the screen, is what got you interested in the material you are about to present, or perhaps why researchers in your field are interested, or why the community as a whole should pay attention. What you actually say should be geared not just to the subject of your work, but also to the nature of your audience. Clearly, if you are giving a ten-minute presentation to experts in your field, you should dispense with remarks of too general and introductory a nature. On the other hand, if you are giving a colloquium, whose audience includes professionals who

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*Stimulative Properties of Elixir X*

I. M. Balding

SUPERVISOR:

Prof. A. Barber

ADDITIONAL HELP FROM FELLOW POSTDOCS:

Sam Son (dendrite growth)

D. Lila (cutting tools)

FUNDING:

Nat'l Hair Council

*Figure 1*

are expert in areas other than yours, and students, then a long introduction is essential.

Attention to the technical aspects of talk preparation can make the difference between a good seminar and an excellent one.

### *Technical Matters*

Experimental solid state physicists always seem to show a slide featuring a schematic, or God help us, a photograph of their apparatus. Occasionally there is good reason for such a slide. More often than not, it is a waste of time. “Get to the ideas!” I think in these cases. In putting together the body of your talk, try to recognize digressions for what they are. If there is a good reason for showing an equipment slide, if it explains a novel technique, then do it. If the measurement method is standard, if the slide only proves that your lab isn’t empty, that you didn’t make up your “results,” forget it. Nobody minds a short, informative talk. Don’t pad your presentation by design, or by inattention to preparation.

Theoretical physicists, particularly inexperienced ones, often show slides covered with equations. (Molecular biologists show DNA sequences.) Except in very special cases, such as meetings of specialists devoted to technical advances, this is a bad idea. The audience cannot assimilate more than a small amount of information in an hour—to say nothing of ten minutes. A talk comprised of detailed technical slides is likely to be received as a deliberate attempt to persuade the listeners that since the material being presented is so complex as to be incomprehensible, it should be looked on as important. Save this for after your Nobel prize. Then, most of your audience will be afraid to reveal that they have no clue as to what you have done, and/or that they despise your snow job. For now, you need to please your audience, not to beat them into sub-

mission. Put yourself in the place of an experimentalist among your listeners. Why would he want to hire you? There is an outside chance he would act in your favor because a colleague who actually understood your equations told him that they are important. More likely he would prefer someone he thought he could talk to. To communicate with him, you need to convey not the details of your math but the basic concepts, the approximations, the results, and the predictions. Think about that. Then throw away that slide full of superscripts and subscripts.

SLIDES: A few ideas on the preparation of slides are certainly in order. When I see a beautifully computer-prepared, full-color slide, what first goes through my mind is "this guy obviously doesn't have enough to do." Granted, modern technology makes the preparation of "professional-looking" overhead foils and slides relatively easy. Nevertheless, you do not want to give the impression that thinking about how your slides look is more important to you than what they say. If you are preparing a presentation for a group of laymen—e.g., upper management or a general, or some such—by all means make your visual material spiffy. If you are talking to your fellow scientists, go easy on the "professional" look. Remember that many of them drive 25-year-old VW bugs, and the same reverse snobbism that keeps them in their clunkers probably also affects their impression of your foils.

This, I hasten to add, does not mean that your overhead foils should be illegible, smeared, or overcrowded. For the most part, they should contain a figure or two, a "cartoon" and simple text. Write big! This has two advantages. One is

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that people in the back of the room, close enough to the door that they can escape inconspicuously, can read what you've written, and might be persuaded to stay. The other is that it limits the amount of material that you can fit on a page. You don't want a lot—so writing big helps.

Using permanent ink in preparing foils is a good idea. Water-soluble ink will ball up if you sneeze or spill your coffee on it before your talk. "Permanent" ink can easily be erased from an overhead foil using vodka, or any similar solvent.

### *Summary*

By now I hope you have realized that this chapter is organized as a seminar on seminars, and I would like to reiterate the main ideas:

1. Your seminar is a performance. It needs to be carefully planned and thoroughly rehearsed.
2. Present yourself confidently. Act as though you have enjoyed doing your research and that your results are exciting to you.
3. Respect your audience. They are spending an hour to hear you. They want to understand what you have to say, even if your specialty is not theirs. They do not want to be "snowed," nor do they want to be treated as "experts" in a field where they really are not.
4. Do not waste your time with filler. Make sure each slide pushes your story forward. If your talk is a bit too short, no one will object.

5. Make your visual aids pleasing to the eye without too much of a Madison Avenue look.

Thanks for your attention!

#### ADDITIONAL READING

J. C. Garland, "Advice to Beginning Physics Speakers," *Physics Today* **44**, 42(1991).

Vernon Booth, *Communicating in Science: Writing and Speaking*, Cambridge University Press, New York, 1985.

# Writing Papers: Publishing Without Perishing

The negative connotation of the cliché, *publish or perish*, is seriously misplaced. Publication is a key component of your research efforts. It is widely accepted that a scientific endeavor is not complete until it has been written up. The exercise of putting your reasoning down on paper will frequently lead you to refine your thoughts, to detect flaws in your arguments, and perhaps to realize that your work has wider significance than you had originally imagined. Publication also has strategic significance. As a beginning scientist, not only are your hours long and your pay low, but your job security is anything but assured. To succeed, you must make your talents well known and widely appreciated. Publishing provides you with an important way to accomplish that. Your papers, available in libraries around the world, represent not only your product but also your resumé. Compelling, thoughtful, well-written articles are timeless advertisements for yourself. You can imagine that a sloppy resumé is not worth preparing. A premature or

slapdash publication is far worse. It will remain in the library indefinitely. These thoughts raise the two basic questions that are addressed in the present chapter: *when* should one write a paper, and *how* should one write it?

### *Timing*

Generally, papers that are written too soon are written in response to the fear that one's competitors will publish first, or as a result of intellectual laziness, i.e., inattention to important details. When they are written too late it is because of the fear of publishing a blunder, or because of "writer's block." Overcoming these fears and frailties is necessary for *everyone* in science. At the very least, the knowledge that they are not yours alone may help you deal with them. (Read Carl Djerassi's novel *Cantor's Dilemma*, Penguin Books, New York, 1991, for a poignant exposition of the problem of when and what to publish.)

Planning your research as a series of relatively short, complete projects (cf. Chapter 8) is the best way to achieve a disciplined publication schedule, one that serves your interests in scientific priority, self-advertisement, and job security. Even though you are working toward an important long-term goal, you report each project as an independent piece of work that has produced a new kernel of knowledge (only half-jokingly a "publon," a quantum of publication<sup>1</sup>). In the introduction to each paper of a series, you

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1. The concept of the "publon" emerged from the graduate student minds of M. J. Weber, now at the University of Virginia, and W. Eckhart, now at the Salk Institute.

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Peter J. Feibelman

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place the work reported in the context of the long-term goal, to which you thereby lay claim, and you explain how the present results take you a step closer. If your project turns out to be as significant as you had hoped, after you have published several papers in the series, no doubt you will be asked to write a review. *This* will provide you with an appropriate forum for a long, definitive article, one that will be widely referred to and will help to make your name in science.

There are many advantages to writing up your work as a series of short papers. Managers and funding agencies need concrete evidence that they have hired personnel and spent money wisely. Nothing is more helpful in this regard than the list of publications that their wisdom has fostered. Of course they will be pleased if you eventually realize a long-term research goal. However "funding cycles" are typically two or three years (cf. Chapter 7) and renewal of junior scientific positions occurs on a similar time scale. Therefore deans, research directors, and contract managers cannot wait for your long-term dreams to come true. They need published evidence of your progress on an ongoing basis.

By writing numerous, relatively short articles, you can keep your name in the spotlight. The title and authorship of each of your papers is printed in *Current Contents*. The abstract appears in abstract journals and computer data bases. The number of citations of a long publication list increases more rapidly than that of a short list. You mustn't be overly cynical about these facts of scientific life, of course. If you attempt to achieve name-recognition by padding your publication list with repetitive papers, your

efforts will soon reap scorn rather than admiration. On the other hand, the little admiration you gain for publishing an awesome *magnum opus* in one paper is surely not worth the risk that this publication strategy poses to your job security.

If you publish frequently, you are less likely to be "scooped." The longer you hold back reporting your results, particularly if they are important, the greater the chance that some other group will beat you into print. You do need to develop an appreciation for when a piece of work is complete enough to be written up. If the logic of a manuscript is clearly missing an important piece of confirmatory evidence, submitting it to a journal is likely to cause you endless, painful interactions with referees. This is the time to hold back. (Among other problems, the referees may very well be your competitors. Their own publication strategy is likely to be affected by their appreciation of where your incomplete work stands.) On the other hand, if you have completed a project, the sooner you get it into the hands of a journal, the better the chances are that you will get credit for your accomplishment.

Writing a paper that presents one new idea or result is much easier than writing a long, complex article. This is a reasonable way to address the problem of writer's block. Much of the introduction to a shorter paper can be prepared, at least mentally, when the long-term research project is originally proposed. The organization of a paper is simpler if there is not so much material to present, and it is also relatively easy to explain the conclusions in that case.

Referees are generally busy people, and prefer to review

short papers. You are likely to receive a more thoughtful and positive report on a short manuscript than on a long one. Shorter papers are of course not only easier on referees. They can be read and assimilated more easily by the scientific community at large.

Writing up individual kernels of new research also should have some appeal for the perfectionist. It is easier to get everything right when one is dealing with a small project than in publishing the results of a major, complex effort.

Eventually, of course, all the significant details of a research project need to be reported in an archival journal, so that others may repeat and confirm the validity of the new science. Writing such technical papers is an important exercise, and one that will win you credit from your peers if you do it well. On the other hand, in most cases the writing of such papers can be carried out at leisure.

### *Writing Compelling Papers*

A journal article should present a careful and relatively complete account of your research. However, it is all too easy to write an accurate description of your work that attracts no attention and that adds little to your scientific reputation, *even when your results are significant*. Learning to write articles that people will read and remember will make you a more effective scientist. It will also enhance your chances for survival as a researcher.

The structure of a newspaper article is a good model to follow in preparing a scientific publication. Your

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colleagues, like newspaper readers, rarely have much time for acquiring new information from the literature. Many do no more than skim some journals of interest and hope to come back to the interesting papers later. This is just the reason that newspaper articles present a story several times, in increasing levels of detail. The headlines, equivalent to the titles of your articles, are what sell the news. They are designed to attract attention by providing a succinct description of what is noteworthy. Scientists attempting to deal with "information overload" often turn for help to *Current Contents*, a weekly report of the major journals' tables of contents. The titles of your papers, and nothing else of them, will appear there. This in itself is a good reason to spend time writing concise, accurate, and compelling titles. ("Cute" should be avoided, as a rule.)

The abstract of a paper corresponds to the first paragraph of a news article. It summarizes the main information, what the important results are and what methods you used to obtain them. Numerous journals place a word limit (e.g., 75 words) on the abstract. It is a good idea to impose such a limit on yourself whether or not the journal does. An abstract that is brief and to the point has a better chance of being read. A wordy one, that reads like the introduction to or the body of a paper will lose readers. As in the case of titles, it is worth remembering that abstracts are circulated more widely, via abstract journals and in computerized data bases, than the articles they summarize. A well-written abstract may thus attract additional readers to your article—readers who do not ordinarily skim through the journal where it is published.

The introduction to a paper is where you tell your story, possibly illustrating the text with an important figure or some key results, but without going into great detail. Here is where you want to explain why your project was an important one to undertake and how your results make a difference to the way we understand the world. Many busy scientists read only the introduction and conclusion sections of papers, leaving the technical details for another time. Therefore it is a good idea to highlight your results, for example by placing your most important figure in the introduction. Even if your readers never take the time to plow through the complete description of your work in the body of your paper, they may think enough of the information in your introduction to make sure that they catch your talk at the next scientific meeting.

Virtually everyone finds that writing the introduction to a paper is the most difficult task. It is easy to report the procedures you followed and to describe the data you obtained. The hard part of paper-writing is drawing the reader in. My solution to this problem is to start thinking about the first paragraph of an article *when I begin a project rather than when I complete it*. I would not embark on a scientific effort if I didn't think that it was important and that my work would answer a question of rather wide interest. The reasons that I found the project in question interesting enough to work on provide half the material I need for my introduction. The remainder is a summary of my key results. The decision to start writing a paper is generally based on recognizing that a kernel of knowledge has been produced. In my introduction I want to let my

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reader know what this new information is, in a nutshell, and why it is worth reading about. Sitting at the word processor, I imagine that I am on the phone with a scientist friend whom I haven't spoken to in some time. He asks me what I have been doing recently. I write down my imagined response. If, when you try this, you feel an attack of writer's block coming on, turn on a tape recorder and actually call a friend. It works.

Incidentally, if you know why you have carried out a scientific project and what makes your results interesting, there is no reason that your paper should start with an inane cliché like: "Recently there has been a resurgence of interest in . . . (whatever the topic)," which bothers me every time I see it. If you have been working on a project for several months or a year solely because *other* people are interested in it, you have a lot to learn about problem selection. (In this case, see Chapter 8 for some help. Do not pass go. Do not collect your next paycheck.) Before you start on a research effort you must understand why it is important, and in the introduction to your publication on the subject this is just what you need to explain.

In writing your introduction, as well as the body of your paper, it is extremely important that you place your work in context, not only by explaining what you did and why, but also by citing the relevant literature. This is important, not only to provide your readers with a way of understanding your area of research, but also because your scientific colleagues are very eager to get credit for their achievements. (This is not just vanity. Scientists' careers are built on the perceived importance or usefulness of their re-

search results.) You have much to gain and little to lose by scrupulously citing your competitors' work. I said above that many busy scientists read only the introduction and conclusion sections of papers. Even more turn directly from the title and abstract to the references, to see if their work is cited. If someone's work is not but should be, you risk losing potential friends or at least some respect.

In revising and editing your article before submitting it, you should constantly be asking yourself if you have dealt with all the "loose ends" in your logic. Are there arguments that you have thought and used but not written into your text? Are you wishy-washy about inferences that you draw instead of forceful because there are missing links in the logic? If so, you either need to work a little longer before writing your paper or you need to be forthright about what are assumptions and what has actually been proven. Even if the referee does not catch the weak points of your manuscript, you must not forget that your paper will be on the library shelves for a long time. Intellectual honesty is accordingly a very good policy. This is not to say that you should be such a perfectionist that you never feel comfortable saying that a project is done and ready to be published, but rather that you should own up, in print, to what you think might be weak links in your reasoning. This is a service to the community, in that it points to further research directions. It shows the world that you are a thoughtful and forthright individual. Finally, and importantly, it provides you an "out" if your reasoning is later shown to be incorrect.

The format of the body of a paper is often dictated by

the journal where it will be submitted. Within the journal's constraints, however, the key to organizing your work is to make your text read like a story. Often it is a good idea to relegate detailed discussion of a technical aspect of the work to an appendix. That way experts or interested parties can try to understand your arguments in full detail while others do not have to guess how much of the text to skip to get back to the flow of the story.

Keep in mind that the function of a paper is to communicate, not simply to indicate how wonderful your results are. In principle a paper should provide enough information that an interested reader would be able to reproduce your work. It is your responsibility to ensure that the necessary information is made available, at the same time as you try to make your paper as "snappy" and readable as you can.

### *Snappy Papers*

In archaic times, say 20 years ago, you generally had to write your papers as though the work had actually been done by someone else. You were discouraged from using the personal pronoun "I" in favor of "we" or, even worse, "one." Journals seemed to require writing papers in the passive mood, as in "the data were obtained using the following novel method" rather than "I developed the following novel method to obtain the data." More recently, it has become possible to drop the phoniness of this style and to reveal in your papers that *you* actually did the work that you are reporting. I greatly prefer the more straightforward style and recommend that you use it.

People of a mathematical bent often connect the sentences in their papers with words like: now, then, thus, however, therefore, whence, hence, and so forth. If you want your text to be readable to the nonpedantic, you should be very sparing in using them. Go over your first draft, and challenge yourself to see how many of these connectives you can remove without undermining the logic of your argument.

In this era of fast desktop computers and laser printers there are few excuses for omitting evocative figures from a paper. A picture may be worth more than a thousand words in a scientific article, if the thousand words are not read, while the thoughtfully prepared figure is examined and the information it reports absorbed. This does mean that it is important not to prepare figures that are too cluttered. If they offend the eye, they may be ignored along with the thousands of words.

Some journals restrict the length of articles. This typically forces one to go back through the first draft of a manuscript to rewrite more economically. In preparing a first draft of a paper, it is a good idea to be as generous as possible with words. You should write down everything that comes to mind as relevant. This may not be easy, but helps get all the logic on paper. (Again, get out the tape recorder if you tend to be stingy with words.) If you have written a copious text, the exercise of cutting back may be more difficult, but is less likely to lead to a paper whose flow is compromised by something important's being missing. I recommend the approach of writing generously and then editing severely in all cases, i.e., whether or not the journal in question imposes restrictions on the length of

manuscripts. The exercise of rewriting as concisely as possible leads to more readable text and thus, to text that is read more widely.

As in the preparation of a seminar, the last section of a paper should provide not just a summary of the results reported but also some idea of how they might affect the direction of future research. The goal of the conclusions section is to leave your reader thinking about how your work affects his own research plans. Good science opens new doors.

### *Referees*

Finally, since arguments with journal referees can take many months to settle, and can be very frustrating, it is a good idea to forestall such arguments by having your manuscripts reviewed locally, by one or two of your colleagues, before submission. If you have chosen your local reviewer well, you may discover the weak points in your manuscript in a matter of days rather than months. If English is not your mother tongue (and if you are writing for an English-language journal) it is even more important to have your paper reviewed and edited by a colleague, one whose English is perfect. Your readers, including your journal's referees, are human, and thus impatient to some degree. The easier you can make their task, the better will be their response to your efforts.

Incidentally, as one who referees many papers, I much prefer receiving a cogent, well-written manuscript that I can learn from than the other kind. A paper that I enjoy

reading disposes me favorably toward the author. Your referee may be your paper's most careful reader ever. Making a good impression on this anonymous potential employer is not a bad idea!

If your referee does have serious complaints about your article, getting angry is not a productive response. A better idea is to consider why this thoughtful expert did not follow your argument and agree with it. If on reflection you believe that your results are correct, and that the referee has simply misunderstood them, it is likely that spending some time revising your text will not only persuade the referee to recommend that your paper be published but will also ultimately make your ideas less confusing to your journal's general readership.

#### ADDITIONAL READING

Sylvester P. Carter, *Writing for Your Peers—The Primary Journal Paper*, Praeger, New York, 1987.

Michael Alley, *The Craft of Scientific Writing*, Prentice-Hall, Englewood Cliffs, NJ, 1987.

Vernon Booth, *Communicating in Science: Writing and Speaking*, Cambridge University Press, New York, 1985.

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## From Here to Tenure: Choosing a Career Path

As a scientist, your goals are to make exciting discoveries, to change the way your colleagues and maybe even the public-at-large view the world, and generally to improve people's lives. However, need I remind you, you will remain a human being, with human needs, even while you are pushing back the frontiers of ignorance. No matter how romantically you view your role in research, you will not be happy without a secure, well-paid job. You will want help in accomplishing your research goals and recognition for your achievements. You will probably want to see your family on a regular basis, and more generally, to have enough free time to engage in activities outside your professional life.

It is all too easy to lock yourself into a situation where one or more of such basic desires will not be satisfied. This may adversely affect your productivity, your family life, and your ability to enjoy yourself. Thus it is important to consider rationally, and in advance, not only the benefits and disadvantages of the various kinds of scientific positions—

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academic, industrial, and governmental—but also the merits of the different roads to permanent employment.

Economic conditions may limit your choices, but if you are fortunate enough to have more than one job possibility, this exercise will save you considerable stress. It may have a significant effect on your financial well-being. It may save your marriage. I harbor a secret hope: If enough of you start to act rationally, the system may eventually be rationalized.

It is only natural to adopt as role models the people one encounters in one's formative years. For this reason many, perhaps most of us, finish graduate school dreaming of an academic career. For some the academic life may be ideal. For many it is not. Even if being a professor is the right goal, however, it is far from clear that rising up the academic ladder is the most desirable way to get there. My recommendations, and the reasons for them, are the subject of what follows.

### *The Pluses and Minuses of a Job in Academia*

The idea that a university is an “ivory tower” is commonplace. The academic freedom embodied in the granting of tenure was originally supposed to protect the professoriat from political repercussions against expressions of minority views of the world. However, tenure is in itself a uniquely desirable and economically significant benefit.

Who wouldn't want the ultimate in job security? As a professor, if you fulfill minimal performance requirements (e.g., teaching a class every semester) and maintain at least

minimal moral standards (love affairs with your students are sometimes frowned upon), and if your university doesn't shut down your department entirely in response to severe economic stress, you have a guaranteed paycheck. In fact, universities have long since recognized the economic significance of tenure. University salaries would certainly have to be higher if professors were subject to being laid off.

Tenure is a form of financial independence, and thus conveys corollary benefits. A university professor is free to choose whatever research projects may interest him or her. There is no "boss" at a university to define research projects or to decide who will work with whom. In principle, a professor can do research at his or her own pace. If energetic and ambitious, an established professor, together with a group of students and postdocs, may produce a dozen publications a year, or more. If "scholarly," he or she may publish many fewer and may not have much of a group. The department chairman or the dean may complain about a tenured "scholar's" lack of productivity, but the scholarly professor will still receive a paycheck.

Although tenure and its corollaries are the unique benefits of a professorship, they are far from the only attractive features of the job. A professor can anticipate the respect not only of class after class of students, who pay a great deal of money to be exposed to what he or she has to say, but also of the community at large.

Typically professors are free to sell their services as a consultant, perhaps one day per week, to supplement

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their salary. Many science professors found private companies to develop the fruits of their research and sell them for their own profit. Others write textbooks on university time and pay, and then are allowed to reap the royalties for themselves.

Since classes are held only nine months of the year, the remaining three are in principle a very long annual vacation or at worst unprogrammed time. Sabbaticals are typically part of a university contract. Every several years, professors can look forward to six months or a year at a distant and often an exciting location where they can recharge their intellectual batteries, learn a new field, write a book, or basically do what they please—and get paid for it!

Since the job has all these wonderful benefits, you might be surprised that many professors complain about the demands of their work, and that many scientists are happy not to be members of the professoriat. What then are the disadvantages of living in the “ivory tower”?

Probably the most widespread complaint is that a professor rarely has time to set foot in the lab and to do the scientific research that used to be so much fun. Professors have so many responsibilities and have to work so hard to fulfill them that their scientific work is mostly vicarious—it’s the students and postdocs who do the hands-on research. To say the least, professors end up with little time for themselves. There are thankfully few tenured individuals who cynically view their permanent slot as an opportunity to do nothing (although there is generally more than enough “dead wood” in a department to embitter the assis-

tant professor who is not promoted to tenure). The professors I know work many more than eight hours a day and rarely take more than a week or two of vacation each year even though in principle they could take much more.

A professorship is effectively several jobs rolled into one. A professor is of course a teacher. Although there are many stories of professors whose lecture notes are yellowed with age, taking the job of teaching seriously means devoting considerable effort to making lectures coherent, informative, and up-to-date. One needs to prepare homework sets and exams, and to develop meaningful lab exercises. One must also spend time with students during “office hours.” A professor is expected to be a good departmental citizen. This means attending a significant number of meetings to decide policies, and to discuss hiring and promotions. The ambitious professor spends a great deal of time as a manager. This means writing grant proposals, traveling to Washington to meet with grant administrators, fighting for lab space, hiring and firing students and postdocs, and so forth. He or she also spends time being an active scientific citizen, refereeing manuscripts and grant proposals, attending meetings and giving lectures. Consulting and textbook writing come on top of this. It does not take a genius to see that professors have little time for reading a novel or playing with the kids.

A job with many demands provides many opportunities for frustration. When economic times are tough, the chances of getting a proposal funded or renewed are reduced. If you have no grant money, you cannot afford to pay students and postdocs. If you cannot spare much time

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to do research yourself, this means that your research program will grind to a halt. Your ensuing lack of productivity will then make it harder for you to acquire funding in the future, a most unpleasant feedback mechanism. Apart from keeping yourself alive as a researcher, if your funding dries up you will find yourself without "summer salary." Typically a university salary is paid for nine months, and if you are not bringing in substantial outside funding your nine months' pay will not be particularly generous. (The university reasons that tenure is worth a lot, and that you are not likely to give up your sinecure for the extra pay.) When you apply for a grant you generally ask for money to pay your summer salary (most universities allow you to receive two months' pay from this source). This makes the issue of whether or not you get funded intensely important. If you win a grant, your salary increases by better than 20 percent. If not you may wonder why you are working so hard.

Interacting with students can be a great pleasure, but is often very stressful. As a teacher you will have to deal with insistent people who want to know why their exam grades were so poor, and who want private help to understand the material you have been presenting. You will have to deal with students who cheat on exams and with pre-meds who have no interest in anything but grades. Only some of your graduate students will really contribute to your research. Others will break your equipment, contaminate your samples, and install bugs in your computer programs. Some postdoctorals (particularly those who haven't read this book!) will flounder for a year or two, will be bitter

about their inability to find a job, and will complain publicly about your guidance.

*Your academic freedom is certainly a great benefit, but what about that of your colleagues? In some departments, the various groups talk to each other. However, this situation is far from guaranteed. Since there is effectively no management in a university, every professor and his group of students and postdocs tends to work independently. There is no particular reward for collaboration. This is very different from a national or industrial lab, where the job description includes working to promote the efforts of one's professional colleagues.*

**ASSISTANT PROFESSORHOOD:** If after this litany of disadvantages you still want to be a tenured professor, there remains the question of how to attain such a position. The most direct route is to work your way up from the bottom, i.e., to start as an assistant professor and to be promoted. I heartily recommend that you avoid this path if at all possible.

As an assistant professor, you suffer most of the disadvantages and have few of the benefits of a tenured academic position. Not only do you have to teach, but unlike your senior colleagues, you haven't got sheaves of lecture notes from yesteryear. You start from scratch—which means devoting many, many hours of preparation for each hour you spend in the classroom. The same is true when it comes to preparing homework assignments and exam questions.

Although being responsible about your teaching duties is necessary for you to win promotion to tenure, at a

research-oriented university it is far from sufficient. You will certainly be judged on your ability to bring in grant money. Although you will have to publish to avoid perishing, you will also have to get funded to survive. This means that you will be learning the ropes of grant writing at the same time as you are trying to establish a research effort and desperately need to produce some results.

Your salary as an assistant professor, as for all professors, will not only reflect your seniority, or in this case your lack of it, but also your success at bringing in outside money. Since you are just starting out, you will have had no such success. Therefore your salary will be miserly to poor. If you are such an exciting prospect that you have managed to land an assistant professorship at a major private university with a fancy reputation, then your salary will be even worse. Such universities expect you to accept lower pay in return for the snob appeal of their name on your resumé. They also offer significantly reduced, if any, opportunity for promotion to tenure, on the perhaps correct assumption that their name is worth more to you than job security.

Unhappily, whereas full professors might accept lower pay in return for the grant of tenure, assistant professors are expected to take the low pay without the compensation of a secure position. Quite the opposite. Responding to the American Association of University Professors' (AAUP's) efforts to protect you against exploitation, most schools adhere to the policy that an assistant professor who hasn't been granted tenure after seven years must be fired. Thus, ironically, thanks to a labor organization that

purports to represent your interests, you will lose your job if you are not promoted!

There *are* pleasures to working as an assistant professor. Teaching and interacting with students can be exciting. The university environment is in itself very stimulating. There are certainly more kinds of people with more diverse interests than in any industrial lab. You do get respect from the community. On the other hand, the price of being an assistant professor is much too high. The hours are long, the pay is terrible, and the job security is bad. After your years of study for a Ph.D. and further years as a postdoctoral apprentice, you will probably be about thirty years old. You'll probably be starting a family. Your former colleagues who went to engineering or business school will be making their way in the world, earning good salaries, and having time to participate in activities outside their jobs. Do you want to be working 16 hours a day for half what they are earning, on the chance that after five or six years your department may give you tenure? If enough of you answer no maybe the job conditions will improve. Until then, I recommend that you find a position in an industrial or government research lab. There you can establish a reputation with much less pain, as discussed below, and, reputation in hand, can start at the top in a university job, if that is still what you want.

### *Industrial and Government Research Positions*

Research jobs in industry or at government labs have some serious disadvantages but many benefits relative to

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university professorships. At some of the national labs there are tenured research positions, but for the most part tenure is not offered outside the framework of the university. You can be laid off, for a variety of reasons if you work for private industry, of course, but even if you are employed at a government lab. There is no doubt that tenure is a valuable benefit. However, you should remember that your real job security as a scientist is the recognition and approval of your peers around the world. If your published research is admired and used by fellow scientists everywhere, you have little to fear. One day you may have to change job locations, but unemployment should not be an important worry. Since industrial and government labs provide an environment where it is relatively easy to establish a scientific resumé, if you are competent the issue of tenure ends up being relatively insignificant. (Incidentally, the reluctance of the managers who hired you to admit that they made a mistake provides an additional, if melancholy, form of job security at a research lab. Firing you after six or seven years if you are not promoted is not built into the system as at a university.)

The most important advantage of working in a research lab, whether industrial or governmental, is that your job description is relatively simple. You are expected to be a scientific leader, to produce interesting results in one or more areas of importance to your employer, and you are expected to make yourself useful to your fellow employees. The modern world being what it is, you may also be expected to attract some outside funding. Since your main task is to produce knowledge that will sooner or later

benefit stockholders or the taxpayer, your lab will *want* to provide you with the necessary hardware (within budgetary constraints, of course), and if your work has a high priority, this hardware will be in the form of the latest and highest power models. For example, while your university colleagues are writing lengthy proposals to buy a work station, at a research lab you will be struggling to keep up with the latest upgrade to the multi-GigaFlop massively parallel processor. You get the idea.

Since your job description at a research lab is simple, you can perform up to expectations without working unusually long hours. As a professional, you will certainly find yourself working long days occasionally, when you are on the threshold of an exciting result, or when you have to submit an article by a certain deadline. However, you will not be spending half your time doing work that is necessary but not sufficient for your survival (i.e., teaching, explaining to students why they got a D on your last exam, etc.). You will therefore have time to help your spouse with dinner, to read a novel, to see your kids' school play or to be a soccer coach. You won't have historians, specialists in Russian literature, or bassoon professors for colleagues. Thus you will have to make more effort to enhance your cultural life than at a university. On the other hand you will have more time to spend with your friends from outside the workplace.

A research lab is a *managed* environment. We'll consider the downside of living with managers momentarily. The advantages are that management monitors the functioning of the lab and has the power to make it work

better, also that management is paid to do bureaucratic dirty work that would otherwise find its way to your "in box." At a government or industrial lab, significant portions of annual pay raises are awarded for "merit" rather than for having been employed one more year. There is unavoidably some arbitrariness and subjectivity in the annual "performance reviews" by which merit pay is determined. Nevertheless, the fact that a group seriously considers whether your work is achieving recognition and deserves a special reward, whether you and your colleagues are interactive, and whether support personnel are doing their jobs makes the atmosphere at an industrial or government lab enormously different from a university's. Employees who know that their attitudes and performance will make a difference to their paychecks take collaboration more seriously. At a research lab, you will find librarians who offer to photocopy articles for you and who will do computerized literature searches, you will find computer support personnel who want to advance their own careers by helping you make your computer programs more efficient and who will hold your hand while you are learning a new system. You will find groups of professional scientists addressing the same complex problem from several different perspectives, groups who meet to share new results and think up succeeding experiments. At a university, such collegiality is rarer.

There are many ways that management can make your life less rather than more pleasant. Abrupt changes in corporate or congressional priorities may be imposed on you, if you work at a commercial or government lab. You may

have to redirect your research plans, or even terminate a project before it is completed, because of your company's poor earnings or because of political changes in Washington. Heavy-handed scientific managers may insist that it is more important for you to work on their latest (hare-brained?) idea than your own. They may reinforce this by refusing to buy the equipment you want for your own purposes. They may insist that you put their name on your papers or patent applications. Managers may try to avoid their responsibility to learn what is going on in their domain by requiring you to write reports on a too-frequent basis. They may badger you with the latest buzzwords or theories to emerge from business schools<sup>1</sup> instead of inspiring you with rewards in the form of new instruments for your lab and more money in your bank account. Finally, personality conflicts with someone who has the power to fire you, to determine whether you can give an invited paper in a faraway place, and to control the size of your paycheck, can cause you plenty of grief.

Obviously, if you work in a managed lab, you need to have some feeling that you will not be subject to a too-heavy hand. A bigger lab, for example, will provide you

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1. "Empowerment" and "total quality management" are recent ones. Not a few scientists have concluded that "empowerment" means gaining the ability to be blamed rather than heard. Formal "quality training" and "quality management" are widely viewed as superfluous by professional researchers. The likelihood that one's work will be repeated already provides ample motivation to avoid publishing mistakes. Peer review of manuscripts and grant proposals also provides ongoing, external quality assessment of scientific achievement.

more freedom to correct a bad situation than a smaller one. At a large lab, if you just can't get along with your supervisor, there may be several other groups who would be happy to benefit from your wisdom and whose supervisors would be easier to deal with. As your reputation grows, of course, your management will look to you for new ideas and will be less likely to suggest that you change directions. In a sense, this is another aspect of the reward system in a managed environment. The more credibly you play the role of a scientific leader, the more freedom you will have to follow your own research ideas. This is a real incentive, I can assure you.

Management suggestions of an important research project or area, incidentally, need not always be bad. Michelangelo was asked by the pope to paint the Sistine Chapel. He didn't write his own proposal to an "Arts Council of Rome." Although research driven by applications is often viewed with some disdain, the desire to fulfill a real need can and has led to extremely important basic science—e.g., the Nobel-prize-winning invention of the transistor—and has changed the world. You can and should judge your superiors' suggested research ideas thoughtfully and on a case-by-case basis.

If you are considering a job in a commercial or government lab with the idea in mind that you will make a name for yourself and then return in style to academic life, you must be careful to determine whether your projected position and laboratory policies are consistent with your plan. If the research group you are considering works in an area that is important to the company in question, but is of little

basic scientific significance, you will very likely not be a viable competitor for an academic position several years down the track. You will have attended the wrong meetings and your papers will not have been read in the academic world. If your scientific results are going to be treated as proprietary information, i.e., are not going to be published, to protect commercial advantage, or if they are going to be hidden from the outside world as "classified data," you will not be able to achieve recognition comparable to that of many of your contemporaries. Thus even though their scientific competence may be no greater than yours, many of your peers will have a significant advantage over you in the competition for tenured academic positions.

Apart from the problems of dealing with management, one of the worst features of scientific life in many industrial and government labs is a lack of helpers. Whereas a university professor can enlist an army of students and postdoctorals to further his research efforts, if he can procure the necessary funding, a staff member at a research lab is lucky to have his own technician and an occasional postdoc. (This is much less of a problem in the biotech industry than in companies that perform physical research, according to my sources.) There are some opportunities to alleviate this shortage, for example, by collaborating with a professor and his students. However, these kinds of opportunities must be aggressively pursued and may only be possible in favorable geographic situations. The scientist who has dreams of attacking a problem from many sides at once will not be able to fulfill them at a government or

industrial lab unless he can persuade his colleagues that they should help.

### *Money*

In deciding what kind of scientific position to aim for, you will certainly want to consider relative pay scales. There are dramatic differences between universities and research labs in this regard. While the salary distribution for government or commercial labs is a relatively narrow bell curve, whose peak is in the realm of the upper middle class, the histogram for the professoriat has two peaks. The university pay scale starts lower than in industry, and the median university salary is also lower. On the other hand, the incentives for senior scientists at a university are substantially greater than at a national or commercial lab. If, as a professor, you bring in substantial grant money, you are very valuable to your university and, not surprisingly, you reap big rewards. The ratio of highest to lowest salaries in a physics department might be 3 or 4 to 1. In an industrial lab it is likely to be less than 2 to 1. In addition, at a university you can supplement your income by consulting and by writing textbooks on university time.

Financial priorities thus dictate the same career path as the scientific ones. Entry level salaries are better in the research labs, and the merit pay increases they provide can keep you earning more than your university colleagues until you reach the somewhat poorly defined level of "senior scientist." After that, if you want to maximize your salary in industry or in a government lab, there is no alter-

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Peter J. Feibelman

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native but to move into a management position. (One thing managers seem to do very well is reward themselves.) If you want a high salary and at the same time want to keep a hand in research, the best alternative is to seek a full professorship. Having established an outstanding scientific reputation working eight hours a day at a commercial or government lab, you will know what a good contract proposal looks like, you will be relatively successful at bringing in money, and so you will have a good salary, many students and postdocs, and all the good things a university has to offer.

Circumstances, economic, family, or other, may prevent you from following the optimal career trajectory. But at least I hope you will now go into the job market with a clear idea of how you would like to arrange your career and why.

#### ADDITIONAL READING

"Careers '92: Alternate Paths," *Science* **257**, 1707(1992).

## Job Interviews

Succeeding in a job interview is much easier if you have an idea of what is expected of you. It is amazing how many job candidates fail because they are totally unaware of what their interviewers are looking for, and what makes their interviewers nervous. Although the criteria are considerably less stringent if you are seeking a postdoctoral rather than a “permanent” position, the basic themes are the same: Are you a “self-starter” or a drone that always needs to be told what to do next? Are you a leader or a follower? Will you take an interest in your colleagues’ work or will you shut the door to your lab or office and never come out? Do you possess scientific curiosity or do you view research as just another job? The drones, the followers, and the noninteracters, in general, need not apply.

The best preparation for a job interview, just as in the case of exams in school, is to work out in advance what questions are likely to be asked and to have answers for them. In the case of a job interview, the most important question is some variation of “What will you do here if we hire you?” A good time to prepare your answer to this question is when you are putting together your resumé. In addition to giving you a head start on your interview

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preparation, if your resumé includes a persuasive paragraph or two on the subject of the research efforts you plan, it may help you land an interview in the first place.

*No Dilettantes Need Apply*

As is true in general, being bright, even very bright, is not enough to succeed as an employment candidate. I was recently part of a group that interviewed a young man with high grades and extremely good recommendations, from one of our country's best graduate schools. Recommendations are not always trustworthy, of course. Over time there tends to be an inflation of the praise level from any one institution, since if a previous student was hired, a professor does not want to say that a subsequent candidate is any less worthy. Nevertheless, in this case we had high expectations since the recommendation came from a professor well known to members of our staff. As it turned out the candidate, V., did appear to possess excellent analytical abilities. In his job seminar he explained how he had developed mathematical tools that made it possible to extract useful information, in a nonprejudicial way, from an experimental technique that is widely used but was previously hard to interpret convincingly. V., a theorist, had gone into an experimental lab, had perceived a difficulty in making sense of the data that was being obtained, and by eliminating that difficulty, had made an important contribution. This is how he had won, and why he deserved, high recommendations.

The down side appeared after the formal talk. A mem-

ber of the audience said he thought that V.'s new technique could be applied to a considerably wider class of experiments, and gave some specific examples. V. appeared to be unaware of the opportunities to exploit his success, and thereby not only to make himself useful to many others but also to achieve much wider recognition for his work. What is worse, he didn't seem to like the idea. In our private interview, V. explained that he did not want to be "pigeon-holed" as an expert in one particular area. He thought that if he exploited his success, he would lose the freedom to work in other areas later. V. appeared fixed on the idea that he had the potential to contribute in so many areas of research that it would be dangerous to focus on any one of them for very long.

To his interviewers, the message was that V. is and wants to remain a dilettante. V. said that if he was hired as a post-doctoral researcher, he wouldn't want it to be to work on a specific project or even in a specific group. He would want to spend a month or two on arrival looking around the lab for something "interesting" to work on. He said he was a "generalist." I wanted to know if V. thought he could find enough experimentalists at our lab who needed help understanding their data that he could make a career of work similar to that of his thesis. He said he preferred analyzing the errors of others to making his own mistakes in the attempt to create new knowledge at the forefront.

For all his brainpower and wonderful academic pedigree, and despite his real contributions, V.'s interview trip was a failure. It would certainly have been too risky to hire him in a permanent slot. He seemed much too immature.

It was even worrisome to imagine him as a postdoc. After two years, would V. have found something interesting enough to work on? Would he be salable for a permanent position at that point, or would we have to worry about his struggle to avoid unemployment?

### *The Employer's Viewpoint*

It is important to understand the job interview from the perspective of the employer. He probably does not fill research positions very often. His research staff is generally not very large, and if the staff is broken down by subfield, the number of staffers with whom you might collaborate is even smaller. Therefore, offering to hire you is a big risk. Start-up funds are limited. Lab and office space is hard to come by. If you turn out to be directionless, if you are noninteractive, if you are unproductive, you will represent a huge waste of time and resources, percentagewise. If you are one of ten staffers in related areas, then if you fail, the department is only 90 percent productive at best. If it takes "only" three years before you are let go because you are not "working out," realize that three years may be almost 10 percent of your colleagues' careers, a substantial fraction of their work years during which they might have been more productive had they had another colleague who stimulated them.

Given the perceived high stakes, it is not surprising that the scientists who interview you will want considerable assurance that you will make their department a more interesting place, and will not just occupy space and absorb

funds. Given this concern, it is absolutely fatal not to have thought about your scientific direction, not to be able to articulate what you plan to do in the next two or three years and why. Under no circumstance should you indicate that you are willing to do “whatever the department wants,” or, as V. said, that you will arrive without a clear direction and then will look for something “interesting” at the lab. Being collaborative is important, but having no inner compass is fatal. Your fellow scientists hope to learn from you. If you are simply going to be another pair of hands, a technician is a lot cheaper, and much less of a risk. If you imply that you will sit in your office or lab waiting for inspiration to strike, there are enough others applying for the job who will “hit the ground running” that you will simply not get an offer.

Even if you are applying for a postdoctoral job, and expect to be working under the close supervision of a professional, it is still important that you express personal interests—a burning desire to know something. The lab where you work will continue to hire postdocs after you are gone. If the word gets out that postdocs do well at a particular lab, that they end up with permanent research positions at prestigious institutions, then the best Ph.D.’s will want to apply to the lab for postdoctoral slots. If, on the other hand, it seems that after two years the lab’s postdocs have not accomplished much and have difficulty finding good positions, then university advisers will likely assume that postdoctorals at the lab in question are not getting appropriate guidance, and will steer their best students elsewhere. Thus a laboratory has a very real stake in

your success. Its future is at issue. If you publish an important paper or two during your two years, that will be perceived as a real contribution. If you interact constructively with the local staff, you will have a particularly good chance of landing a permanent position locally. Nevertheless, from the lab's perspective, your main task as a post-doc is to do whatever it takes to be able to land a good job in a timely fashion when your brief tenure is up. Your task at your postdoctoral job interview is to provide confidence that this will be the case.

Although you should come to an interview prepared to describe your own scientific goals, you should realize that if your "inner compass" appears to point in a direction that is totally orthogonal to your hosts', you are unlikely to look like an ideal colleague. Thus, you can enhance your chances for success, by spending some time in a library, boning up on the research interests, and accomplishments of the members of the group to which you are applying for a job. Just as *your* publications represent your resumé, the same is true of the scientists you will be visiting. If you understand your interviewers' perceptions of what is important, you will be able to tailor your description of your own goals accordingly. In "doing your homework," you should aim to develop a description of how your research interests mesh with those of the group in which you would like to work. (If you cannot think of a reasonable formulation, you are probably applying to the wrong group.)

Incidentally, if you are interviewing for a professorial position, it is likely that you will be asked what courses you would like to or would be able to teach. If you are un-

prepared to answer this question, your commitment to being a good departmental citizen may come into question. This, then, is another area in which doing your homework might make a difference.

A few days after your personal interviews are done and you have gone home, staffers that you visited will be trying to remember what you said in order to write up impressions of your performance. If you were able to ask intelligent and pointed questions about various staff members' work, and to explain how your research will complement their own, their memories will be excellent, and it will be easy for them to write glowing reviews. If you hadn't a clue what is going on in their labs, and expressed no understanding of how your work might help them achieve their goals, their memories will need refreshing, or perhaps they will be wondering whether you have the desire and/or the ability to make a serious contribution.

### *Remember How You Get to Carnegie Hall*

Practicing your thesis presentation, your seminar, before your interview trip is absolutely vital. If you feel comfortable giving your talk, then your audience will feel more at ease and more willing to accept what you have to say. If you have dealt with tough questions before, then being subjected to aggressive questioning will not be as likely to make you be defensive, to make you want to find a hole to crawl into. To this end it is a good idea to practice at your home institution by giving your talk not just to your thesis adviser's group or to a collection of your friends but

to a wider representation of your department. You want to learn to deal with unfriendly questions. Apart from the fact that such questions might help you refine your understanding of your own accomplishments, they will certainly make it easier for you to be quick on your feet when you are out job hunting. Every lab values staff members whose sharp questions at seminars expose the important qualifications of the science being presented. Thus you can be almost certain that there will be an inquisitor or two in the room trying his best to make you squirm—often it will be the last young scientist to be hired, who is trying consciously or not, to impress the older staffers with how valuable an asset he is. You will feel and look a lot better if you are prepared to deal with this aggression. If someone raises an issue that you had not thought of, you will look much better responding by saying that the point in question seems very interesting and is one that you will certainly be investigating in the coming months, than by cringing and spluttering.

In succeeding chapters concerned with grant applications and developing a research program, you will read words very similar to those you have read here. The preparation you make for your job interviews should in no sense be thought of as just an exercise that is necessary to land a position after your Ph.D. Thinking about what you want to accomplish as a scientist, trying to grasp “the big picture” that makes your accomplishments meaningful, and learning what excites your colleagues, and why, are all vital for your success after you have won a junior position. The thinking, resumé writing, and library homework

that you do in order to succeed in your job hunt will make it much easier for you to prepare successful grant applications and to decide what research projects you will want to do. When you arrive at a new job, it is very likely that your life will switch to "fast forward." The time between your arrival and when you have to be renewed, be considered for tenure, or go back on the job market will seem very short and very precious. Whatever thinking you have done in advance and written preparation you have made will lighten your burdens and may keep you out of the panic mode.

### *Responding to a Job Offer*

In the happy event that you receive one or more job offers, in addition to selecting the one you want to accept, there may be some negotiating to do. If you are a "hot property," for example if you received some special recognition for your thesis or postdoctoral work, or if you have several offers from prestigious institutions, you may be able to negotiate a higher salary from the one where you would like to work. Generally, however, at the junior scientist level, there is little flexibility regarding salaries. On the other hand, there is considerable latitude concerning start-up funds, lab space, the assistance of technicians and other working conditions.

Since your research productivity on a short-time scale is going to determine your job security and the likelihood of your remaining in research, you should try to arrange to have as few distractions from research as possible and to

have whatever equipment and space you will need available on your arrival. There is no harm in asking the chairman of a university department that wants to hire you for a relatively light teaching load for the first year or two while you are writing proposals and setting up a lab. You should also be able to specify what equipment you will need to purchase and how much it will cost, and to justify these expenses in terms of the scientific output they will bring. Do not be afraid to ask for a lot, within reason. You want the department's respect not its love. If you examine the science world around you, you will see that *he who spends the most money has the most influence*. I do not suggest that you spend money frivolously. I know more than one young scientist who failed after setting up a lab that looked like the cockpit of a modern jetliner but had lost track of the idea that it was also necessary to generate some interesting results. Nevertheless, if the problems you want to solve require certain expensive equipment, you should ask for it. You certainly do not want to arrive at your new institution and then have to sit around for months unable to begin useful scientific work.

In getting the working conditions you want, the key concept is leverage. Generally this takes the form of job offers from competing institutions. Once you have turned down your other job opportunities and are committed to the institution whose offer you have accepted, your leverage is greatly reduced. Of course your new boss has an interest in seeing you succeed. But he has only limited funds available, and when you arrive as a new hire you are at the bottom of the heap, your credibility as a scientist is mar-

ginal and therefore you are not in a good position to win battles for money, space, working conditions, or whatever. The time to negotiate is before you have eliminated your other options.

If you can manage to get the results of your negotiations in writing it would not hurt to do so. It is not that your superiors will be intentionally dishonest. However, having your offer, in all its glory, in black and white, can be useful for refreshing people's memories if the going gets rough. This raises the question of how to get a written offer without appearing to call your new employer's honesty into question. One clever strategy is to write the offer out yourself, in the following way:

Dear Dr. Honcho:

I very much appreciate the time you spent discussing my professional opportunities at LAB-X. As I understand it, the position you are offering will include the following:

(Specify the important terms here, lab space, equipment, summer salary, freedom from teaching for some time, whatever.)

Please let me know whether this list accurately reflects our conversation, so that we may proceed accordingly.

Sincerely yours,  
Dr. Ima Mover

It is not infrequent that an institution offering you a position will want an acceptance or rejection within some time limit, for example so that it can make a timely offer, or send a rejection letter to a runner-up for the job. This may put you under considerable pressure, if other places

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where you have interviewed are moving too slowly. If you are not prepared to answer yes or no as a deadline approaches, you should ask for more time. If the extra time is not accorded, in deciding how to respond you should keep in mind that it is *your* life and *your* happiness that are paramount. If you are unwilling to let go of offer number one while waiting to hear from institution number two, it might be reasonable to accept the first offer. If the later offer is better, you can take it and apologize to the first offerers for changing your decision to accept. You will not make friends by withdrawing your acceptance, and breaking a promise is certainly not something you should do lightly or often. Nevertheless, your life comes first. If an institution "plays rough" by pressuring you for a decision, it should be prepared to accept the fruits of its tactics. It has probably experienced such consequences before.

Keep in mind that as a junior scientist you are the weaker party in all your negotiations. It is not for you to make life easier for the stronger parties. In general, you will not be offered a written contract or particularly good job security. Although you should consider how your handling of a job offer will affect your long-term standing in the scientific community, you should not dismiss your own needs out-of-hand for the sake of a potential employer's priorities.

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## Getting Funded

*“While you’re up get me a grant.”*

You have probably already heard that if you want to succeed as a professor, you will have to bring in money. You may not have heard that in the difficult environment of the 1990s, you may even have to get a grant if you want to work in a national laboratory.

In the “good old days,” prior to World War II, scientists did not apply for, nor did they receive, research grants from funding agencies. Unsurprisingly, there weren’t many scientists in that era. If you were independently wealthy, or perhaps if you could persuade some investors to support your work, you could build up a laboratory. Otherwise you had to make do with what your university salary and personal resources would allow. In the latter part of this century, the realization that the products of the hard sciences can protect us from our enemies, can cure our illnesses, and can yield products that lighten our daily burdens revolutionized the funding of science. Government and industry learned that investing in scientific leadership is necessary for prosperity (although nowadays it is no longer clear how well that lesson is remembered). At the

same time, universities discovered the blessing of receiving government and other outside funds. Although you may think that current tuition costs are astronomical, money taken in from students does not cover a university's costs. Charitable donations take up some of the slack. But major universities would have to shrink their programs considerably if it were not for millions of dollars brought in via research grants. As a science professor, whose salary is considerably higher than those of your colleagues in the art history department, it is your responsibility to help support yourself and your department by winning funding from the outside. If you do not, you will find yourself *persona non grata*. If you are untenured, you will be asked to find employment elsewhere. If you are tenured, you will be unable to employ graduate students and postdocs, your salary will diminish relative to inflation and your influence on departmental directions will be slight to nil.

This set of realities means that if you arrive at a university as an assistant professor, it is essential for you to win a research grant as soon as possible. Like many of your responsibilities as an assistant professor, getting a grant is necessary but not sufficient for your job security. Because getting funded is so important, and because the demands on your time and thought processes will be very heavy when you begin your university career, I strongly recommend that you plan and perhaps even draft your proposal before day one of your university job. The best time to think about the contents of your initial proposal is when you are preparing for your job interviews. As I explained in the last chapter, your interviewers will be very eager to

know what your research plans are. Thus at the same time as you are formulating the ideas necessary to win yourself a job, and writing the "research directions" portion of your resumé, you can be writing the basic elements of your proposal. Having done this, you will be able to begin your assistant professorship with a somewhat lighter burden. If you do not know the format of a grant proposal to one or another funding agency, ask around among the professors at your current university. My guess is that they will be pleasantly surprised at your thoughtfulness about your future, and will be glad to help out.

In writing your proposal, it is important not only to address important research issues, but to present research plans that have a realistic chance of being completed. Major initiatives that will require numerous years of labor are inappropriate for a first proposal. If you are a full professor, with several graduate students and postdoctoral associates, and if you have a record of accomplishment that proves your ability to bring a large project to fruition, then you have a chance of acquiring funds to embark on a major effort. As a beginning assistant professor, however, you have none of the above. If your stated ambitions are too unrealistic, the referees of your grant application will certainly notice and will inform the agency that solicited their opinions that competing proposals to do "incremental research" have a better chance of success. If you have an important idea for a major project, you can include it in your proposal as an exploratory effort along with several short-term efforts that have a good chance of being completed. Alternately, as I discuss in the next chapter (Choosing a

Research Program), you can begin your major project without seeking to have it funded, spending a few hours a week on it; a couple of years down the road, you can make it the focus of another grant proposal, when it is closer to bearing some fruit.

Research grants for beginning scientists are typically awarded for two or three years (at most five). In a grant renewal application you will be expected to report on the progress that the funding agency's money has bought. Evidently you want to be able to demonstrate some significant results. For this reason, and in view of the fact that as an assistant professor you will be spending at least half your work hours not doing research, it is an excellent idea to include in your first grant proposal some projects that are quite far along. Knowing that you will have some real successes to trumpet in your renewal is excellent for your mental health. This does not mean that you should hold back completed or nearly-completed research for very long. Particularly if your work is in a hot area of research, you run the risk that some competitor will publish your results before you do. That would be bad for your mental health, to say nothing of your chances for promotion.

As in the cases of writing papers and giving talks, your grant application should be generous with references to the literature. You have very little to gain by glossing over the sources of your ideas and the accomplishments of your competitors. These very competitors are going to be asked to judge your proposal. If your application appears to ignore their efforts, they will not be shy about telling the funding agency that either you do not know the literature,

and are therefore likely to waste your time and the agency's money repeating the work of others, or they will say that you are so unoriginal in your thinking that you have to try to steal ideas from your fellow scientists. Neither of these comments is likely to win you support. In preparing your proposal you should take pains to search the literature for work of a similar nature or that is related to what you are proposing. You should discuss the significance of this work in the body of your application, and carefully explain how your own research will be different or will build on it, or whatever. Flattering your competitors and referees, within reason, by taking their work seriously cannot hurt your chances and may help them considerably.

A current trend in research funding is to award grants to research groups rather than to individuals. If you are asked to participate in a group grant application, you certainly ought to do so. Being a good citizen of your department is another of the necessary but not sufficient conditions for success. In addition, if the scientists in your department are collaborative enough to want to work together toward a common goal, you should take advantage of this unusual situation if you can. Nevertheless, you should realize that if the group grant is awarded, credit for bringing in the money will not be divided equally. Unless you bring something very special to the proposal, most of the credit will go to the senior members of the group. It will be assumed, not without some justification, that the success of the application was the result of their track records in research. The fact that they have found a youngster, i.e., you,

to help them succeed again is a credit to them, not to you. Thus, even if you participate in the writing of a group grant proposal, you should not fail to write one of your own. When it comes time to renew your assistant professorship or to consider you for promotion, your ability to bring in money is going to be important—and it will be the results of your individual initiatives that will bring the needed recognition.

The more idealistic among you may be reluctant to apply for grant money from Department of Defense agencies or other applications-oriented institutions like pharmaceutical companies, preferring that of agencies like the National Science Foundation (NSF) or the National Institutes of Health (NIH), which you presume to be “untainted.” My own opinion is that if the money you receive is for research that you want to do, research that you think is important, you are unwise to question the motives of the agency that grants you the funds to do it. Its motives are *its* problem, not yours. I would add that money granted to NSF or NIH by taxpayers is available for essentially the same reasons as that which is filtered through the Department of Defense. People are largely motivated to spend money by fear, greed, and lust. Leaving the last of these out of consideration, where science is concerned, the reason that taxpayers and their representatives are willing to spend a lot of money on “pure” physics research is certainly not that taxpayers are interested in arcane theories or the results of subtle experiments. It is that they believe that supporting first-rate physics research will provide their armed forces with the best weapons to defend their interests and will provide their industry with products that will

keep their country competitive in the world economy. In the realm of biology, it is largely the fear of disease that keeps the money flowing into research—hardly taxpayers' fascination with the workings of the cell.

If you have a good idea for a research project, you should submit it in the form of a grant application to as many agencies as you think might be interested in funding it, tailoring the introductory remarks to the goals of the various agencies. Your chances of winning funding from any one agency are poor enough that if you allow inappropriate scruples to stand in the way of submitting applications you may find yourself unfunded and out of scientific research entirely.

### *What Your Proposal Should Say*

A new grant application should persuade its judges of two main ideas: 1) that the work you propose to do is important and timely, and 2) that it is realistic to suppose that you can muster the resources to fulfill your promises. The first section of your proposal should provide the background for your ideas. You should point out what it is that you hope to learn and how the accomplishments you hope for will fit in with or revolutionize current scientific thought or our ability to acquire important information.

In areas of research that have been popular for some time, the "boilerplate" quotient of the introductions to proposals is often quite high. Scientists have been promising to deliver solutions to the same important problems year in and year out. With this in mind, it is a good idea to be modest in making promises, thereby showing your

awareness of the distinction between “pie-in-the-sky” and what you can realistically expect to achieve. You can point out the long-term dreams that have motivated spending in your area of research without pretending that your two- or three-year contribution is going to change history. Without being unnecessarily modest, understatement is likely to win you more respect than overstatement of your possibilities.

Here is an example of what I mean, an introductory paragraph for a hypothetical proposal in my own field, the science of solid surfaces:

One of several reasons that research in surface science has been actively pursued for the past two decades is that vastly important chemical reactions, from the elimination of noxious gases in automobile exhaust to the production of petrochemicals, are catalyzed on the surfaces of appropriate powdered metals and oxides. Learning to make commercial catalysts cheaper and more efficient is thus a goal worth hundreds of millions of dollars to the world economy. Surface scientists often point to this fact, despite the common knowledge that twenty-some years of surface science have not led directly to the development of a single new catalyst material. The reason for this “failure” is that chemical catalysis on surfaces is a very complex affair, and even the elementary processes that together comprise a catalytic reaction, such as the dissociation and sticking of a molecule to a surface, are not very well understood. One area where surface scientists *have* made significant progress is in developing tools to determine the arrangement of atoms at a surface. As a result of this progress, the atomic arrangements of quite a variety of crystal surfaces are now known. Surface science has therefore turned to the study of elementary molecule-surface interactions.

By pursuing this kind of work, for example, by studying both theoretically and experimentally how a simple molecule like H<sub>2</sub> interacts with a relatively simple metal crystal surface, we believe that we are taking important first steps toward understanding the elements of molecular chemistry on catalyst surfaces.

Notice that surface science pie-in-the-sky has not been ignored in this paragraph. The underlying reason for the work to be performed is that it will ultimately lead to inventions worth billions. However, the writer makes clear that he does not expect the contract managers to believe that his work is going to have a direct, and enormous, economic impact. The author wants funding to address an important science problem whose solution will bring us one step closer to realizing a long-term dream.

It is important in explaining the background for your proposal to provide credible evidence that your objectives are realistic. Thus you should describe your own recent progress and explain how it motivates the work you will do, or if you are starting in a new direction, you should describe the publications of others and point out how they suggest new efforts. If you have developed a new technique and plan to use it in the proposed research, you should explain the technique carefully enough that your referees can understand it. Your fears that your competitors may try to steal your methods for their own use may be realistic. Nevertheless, if you do not explain what you plan to do in enough detail, your referees may find your plans hard to take seriously. Life is full of risks. This is one you will just have to take.

Funding agencies specifically ask the referees of grant proposals to evaluate the impact of the proposed work if it should be successful. It is a good idea to be helpful along these lines. You should provide an overview of the field you plan to work in and make clear how the research you will do will be important if it succeeds. It is essential that you show that you understand "the big picture." This means that your proposal writing is actually an important scientific exercise, not merely a pedestrian attempt to extract money from the government. If you can persuade yourself and others that your work represents an important piece of a jigsaw puzzle you will find it much more exciting and rewarding, and your colleagues will take you more seriously. Say what kind of information your work will make accessible that previously was not. Explain what mystery has been impeding intellectual progress in your area. Describe why the isolation of a certain particle or reagent is likely to be important, or why the interpretation of a previous experiment was misleading and how it confused later work. Generally, show that you appreciate the intellectual history of your field and that your work is intended to provide new and important ideas.

It is important to remember, in writing your proposal, that both the referees and the contract monitors who will be judging it are professional scientists, and therefore have a good understanding of how research works. They know, in particular, that research projects often lead in different directions from those that were planned, that ideas that seemed wonderful at the outset lead to dead ends, and that new results that appear "out of the blue" can make it

reasonable to abandon a planned project in favor of another. Your proposal should be coherent and make sense at the time it is written. If a year later you think it is reasonable to adopt an alternate approach or start on a new project, you needn't fear for your proposal's renewal, provided that you have or are very close to having obtained significant results when renewal time arrives. A grant does not bind you to following a path that is shown to be a false one in the course of your work. The success of your application depends on your demonstrating that you have picked a good problem at time zero and your renewal on the salability of your product after two or three years. This means that in writing your grant application you should not try to cover all bases by writing down every possible approach to your problem that you can think of. Make a good case for one or two projects and mentally reserve the right to do something different if those do not work out.

The preparation necessary to win research funding, in sum, is very similar to that required to succeed in a job interview and to establish an effective research program. At some stage in your life, when you are managing several research grants and graduate students and postdoctorals, it may be reasonable to view the writing of a grant application as a time-consuming chore. However, as a beginning scientist, the exercise of preparing a proposal is an integral part of what you must do to make the transition from someone who is technically able and somewhat knowledgeable to a real member of the scientific community.

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## Establishing a Research Program

I wish I could tell you how to go about winning a Nobel Prize. (I wish I could tell myself!) However, my goal in this chapter is considerably more modest. I want to help you see how the research program you establish will affect your chances not only of producing important science but of staying in science at all.

To succeed, you will have to make a rather cold-blooded analysis of your capabilities. This means planning not just scientifically interesting projects but ones that you can complete in good time. You need to consider how your present activities will affect your long-term interests. This may lead you to broaden your efforts well beyond the field you were hired to work in. On the other hand, you should recognize when your experience gives you an advantage relative to your competitors—a special perspective based on your work in another field, or an unusual technical capability—and choose projects that exploit your advantage.

Although it is a good idea to build on your experience, whether by using novel techniques that you have developed, complex ones that you have mastered, special reagents that you have purified, or organisms that you have isolated, you will greatly improve your chances for long-term productivity and survival in research if you can teach yourself to be problem- rather than technique-oriented. Problem-orientation means keeping clearly in mind the scientific problems you want to solve, and working toward their solution even if it means learning or developing a new technique from time to time. You want to be more than simply the master of a particular technique, uninterested in any scientific issue to which it is not applicable. If you operate in the technique-oriented mode, you are unlikely to be a scientific leader for long, and your freedom to pursue personal research interests will probably not last. Being problem-oriented does not mean that you need to master *every* technique necessary to solve a problem of interest—often it will make more sense to take on a collaborator than to learn yet another method. What it does mean is that you will be primarily a *scientific* leader and only secondarily a *technical* one.

Some fields of research are riskier than others. For example, if you work in an area that is sufficiently developed that there is just one “big problem” to solve, the chances that you will be the one to solve it may be rather slim. Starting your career off in an area where your contributions have a better chance of gaining recognition would seem more sensible, if somewhat less exciting.

### *Timing Is Everything*

Timing is one of the most important issues in establishing your research direction. A problem that will take two years to finish must not be the main focus of your activities if you are a postdoc and will be looking for a permanent position in a year and a half. If your postdoctoral adviser suggests that you work on a major, long-term project, you should at the very least ask what he thinks you will have to show for your efforts by the time your job hunt is to begin, and whether he will continue to support you financially if your results are still several months off when your postdoctoral term is due to end. If you hold a two-year position and he cannot persuade you that your project has a reasonable chance of yielding publishable, interesting output within 18 months, tell him, respectfully, that you need to start on some short-term research efforts first, or perhaps simultaneously. If he insists that you devote yourself to the long-term project he has selected, remember that ultimately *you* are responsible for your success or failure as a scientist. If your adviser places *his* interests above your own, do not be too surprised, especially if he is young. Seek a different group to work in, one that offers you a more realistic opportunity to produce short-term publishable output.

In looking for an alternate research group, do not “whine” about adviser number one to prospective adviser number two. Your goal in interviewing for a new opportunity is to persuade the new group leader that you are

mature enough to understand what is necessary to launch your career. Without complaining, you can explain that although your initial adviser's project seems to be an interesting one, you fear that you are not going to be around when the important results are obtained and published, that you will get little credit for your contributions, and that you want to avoid living on an unemployment check two years hence.

### *Technique versus Problem Orientation*

Most young scientists emerge from graduate school having learned a set of technical skills. Many are tempted to try to build a research program around them. This frequently leads to an unfortunate mode of thinking about what to do next, which I call, with apologies to Pirandello, *Six Techniques in Search of a Problem*. The institutions that hire young scientists often reinforce the "technique-oriented" approach to research planning by looking for new Ph.D.'s or postdoctorals who have worked with a particular instrument, for example at a Synchrotron Radiation facility or who have experience with a "hot" new technique, like Scanning Tunneling Microscopy or transgenic organisms. If a new hire swallows the idea that he is to be "the man at the Synchrotron," and particularly if he feels that he must reject any project that does not involve synchrotron radiation, he is likely to have little impact on the world of science, with corresponding consequences to his career.

When a remarkable new instrument such as the laser, or a technique like Nuclear Magnetic Resonance Spectrome-

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try, becomes available it is often profitable to ask how its capabilities can be applied to solving outstanding problems. Few scientists, however, are able to make a long-term success of applying their favorite technique to one problem after another. Eventually the well runs dry. It is the researchers who focus on a significant problem, and are willing to bring to it whatever resources are necessary, who give the most interesting talks, write the most significant papers, and win grant support most easily. I strongly recommend that you try to teach yourself to be "problem-oriented," to plan your research projects so that they address important scientific issues regardless of what techniques you and your coworkers will need to use.

The people who hired you because of a certain technical expertise may be somewhat to very disappointed when you first announce that you will not be spending *all* your time working with the Synchrotron, Scanning Tunneling Microscope, or whatever. On the other hand they will not be pleased, some years later, if you have become obsolete along with your particular technique. If and when you decide that you need to branch out or move away from your initial technical role, you must make certain to fulfill your commitments to ongoing projects. Assuming that you do this gracefully, your group's disappointment at your broadened effort leads you to the solution of an important science problem, enables you to win new research funding, and maintains or enhances your standing in the research community.

### *Strategic Thinking*

There are several strategies for establishing a record of accomplishment that will help make you more salable or will enhance your chances of winning promotion to a "continuing" scientific job. The most obvious is to aim at an important long-term goal by planning your work as a sequence of short-term projects. Each of the latter should yield an identifiable and publishable milestone (a "publon"). Your papers and oral presentations can then begin by identifying you and your work with an exciting research area, while the new kernel of knowledge that you describe will give confidence that you are a person who completes projects and who will be a credit to the department that hires or keeps you.

Planning and publishing the results of short-term projects minimizes your chances of being "scooped." No matter how clever you are, and particularly if you choose to work in a fashionable research area, you will have some very clever competitors. Packaging your ideas in publishable bundles, and getting them out into the literature is important if you are to get credit (to "establish priority") for your work. Apart from enhancing your personal scientific reputation, this is important to the people who pay for your research and want recognition for that.

Each time you lengthen your publication list by publishing the results of a short-term project you lower your risk factor in a potential employer's eyes. A proven producer is always preferred to a "pig in a poke," and a substantial publication list is the best evidence that you have been

and will be productive. Although professionals rightly scorn colleagues whose publication list is padded by repeated articles on the same work, you win no brownie points for writing long, multifaceted papers (cf. Chapter 4). Each time you publish the results of one of your short-term efforts you advertise your productivity, and that of the institution you work in, to your fellow scientists, your contract managers and your potential future employers. You also perform an estimable service to the research community, because the timely introduction of new ideas speeds up the development of a field and prevents duplication of effort. There is always an opportunity to write a comprehensive review when several small projects add up to a major accomplishment or discovery.

Incidentally, publishing more papers rather than fewer will help you in several ways with the "bean-counters" among those who judge you. They will not only look at the number of papers you have published, but will also consult the *Science Citation Index* to see how many inches of citations your papers have attracted. If you have published twice as many articles, this "objective measure" of their impact will be roughly twice as great. You may find this idea crass. I do. But it is safe to assume that there will be bean-counters among those who determine your future, and it certainly does you no harm to please them.

Another important strategy for establishing a successful scientific career is to work on more than one project at a time. This has several advantages: It means that when you temporarily run out of ideas related to project A, you need not waste the rest of the day, week or month, but can

simply turn to project B. When a project has been completed, you do not have to spend entire days wondering what to do next, but can budget some time to push ahead on another, one hopes publishable, piece of science.

Working on more than one project is the only way a young (or any!) scientist should undertake an inherently long-term project. I spent 10 years (!!) writing a computer program to model the energetics of atoms and molecules on metal crystal surfaces. Although I was able to publish several pieces of technical progress along the way—e.g., mathematical tricks that made portions of the computation more efficient—the really significant science output could only be produced when the computer code was substantially complete. I survived this project scientifically by establishing several collaborations in which the tools required to generate results were either completely or almost completely developed. By devoting about 50 percent of my time to short-term projects using these tools, I maintained a publication record, four or five papers a year, that persuaded my peers and my employer that I was not brain dead.

I do not, by the way, recommend ten-year projects as a good idea for young scientists. I waited till I had established a strong scientific reputation before risking it. But even if you want to carry out a three-year project, having something else going on is strongly recommended.

Working on two or three projects simultaneously has at least two other advantages. One is that it forces you to be broader than otherwise. There is a strong tendency to become narrower and deeper as you progress scientifically,

particularly if you work in an industrial or government laboratory. At a university, teaching requirements counteract this tendency. Without at all wanting to argue that you should strive to be broad and shallow, or that you should spread yourself so thin that you are unable to make progress in any area, I suggest that by having your fingers in several pies you are more likely to prosper scientifically. As one area loses its scientific appeal, another, with which you are already familiar, may increase in importance. The clever ideas you learn, or develop, in one area may be applicable in another. This can be an extraordinarily efficient way to make progress.

The second advantage of having more than one project underway is that it will lessen the impact on your career if you should be scooped. This is something to worry about if you have chosen to work in a “hot” area.

### *Establishing a Name for Yourself*

It is particularly important that a young researcher establish his or her identity in the community. Collaborating with other scientists is certainly an effective way to build up a publication record. However, except under special circumstances—e.g., if you bring a unique and identifiable skill to the collaboration—most of the credit for the papers you write will go to the senior partner. Instead of your work’s being referred to as “Young Postdoc, et al.” it will be the paper published by “Honcho’s group.” This is independent of the fact that your name came first on the paper.

For this reason it is important for you to start thinking

up, working on, and publishing the results of projects where you are the sole author, or perhaps the only theorist in a collaboration with an experimental group. In the latter case, it is not enough just to act as the "house theorist," the data analyst who performed regressions on demand. You must perceptibly contribute new ideas—ones that your experimental colleagues would be unlikely to have produced on their own.

### *Risky Business*

Although working in a hot area is exciting—major meetings are mob scenes, the scent of a "prize" is in the air—it is a risky business. Before moving into a fashionable field, you must ask yourself whether you have a realistic chance of emerging from the mob as someone who has made an important advance. If the problem is solved and this hot area is the only one you know well, how long will it take you to establish yourself in another one? Are your ideas sufficiently different from others' that you can hope to beat the competition to the answer?

A less risky course is to try to lead rather than to follow fashion. This mode is not for everyone—but my style is to select a problem of obvious importance, one that is recognized as difficult because its solution will involve a great deal of work. By investing my labor in such a problem, I stake it out as mine. My peers have no trouble distinguishing my contributions from those of others working in the same area. I have few if any competitors. No one wants to work so hard.

On the whole, one way to lessen the risk inherent in undertaking a major project is to make sure that you spend enough money on it. After a research department or funding agency has invested enough in your goals, it has a real stake in your success and becomes very reluctant to admit that your project is not working out. No one ever got ahead in science by saving money. In my own area of research, for example, there have recently been great algorithmic advances that make it possible to compute the properties of solids in a fraction of the time that was previously required. Does this mean that people are requesting smaller computer budgets? Not on your life! They are scaling up the size of the problems they propose to solve. They are asking for bigger computers than are currently available and for more computer time.

Ambition is rewarded in scientific life. Lack of it leads to the exit. Let your management worry about pinching pennies. That is not your job. Let the people who pay the bills know that you are scientifically alive not only by publishing exciting results, but also by keeping up your requests for support.

## *Afterthoughts*

*Experience is the best teacher  
(but only when the experience isn't fatal).*

The tacit premise of this book is that behaviors appropriate to launching a scientific career can be learned. Many of my colleagues doubt this, throw up their hands and propound the Darwinian approach. They say that scientific maturity comes with experience and cannot be taught. The fittest students will survive. The rest will not, according to the law of the science jungle. As I mentioned at the outset, adopting this fatalistic, *laissez-faire* viewpoint does have the advantage that busy professors need not spend time trying to teach their students science survival strategies. On the other hand, if they are wrong, then they are guilty of avoiding an important responsibility.

I take a behaviorist viewpoint. Although the inner feelings and thoughts that go along with "scientific maturity" may be real, and may only come with experience, what is needed to make the transition from graduate student to professional researcher is to learn certain "behaviors." It is not important whether a student prepares an adequate introduction to his seminar because reading my book con-

vinced him it would be a good idea, rather than because of a deep inner conviction based on experience. What *is* important is whether his seminar ends up communicating interesting ideas to his audience. Arguments over the possibility of teaching students to be mature should not stand in the way of teaching the skills involved in giving good talks, writing excellent papers, succeeding in job interviews, and so forth. They are not all that hard to learn, and the underlying ideas do not tax one's intellectual powers greatly. It should be obvious that the problem with waiting for experience to dictate appropriate behaviors is that one is very likely to fail as a result of the bad experiences that are supposed to produce the appropriate feelings. *It is far better to learn from the bad experiences of others than from your own.*

The result I have hoped for in writing this book is that you will become more reflective about your career and will act in a way that is appropriate to being successful and productive. If you stop to think about whether that talk you have been working on is well organized, whether the paper you are writing is one you will be proud of in five years, or whether the research program you have developed is appropriate to your station in scientific life, I will have succeeded. No matter how well you do in these regards, you will certainly still experience difficult times, have regrets about some of your choices, and possibly fail anyway. Nevertheless, your chances for having a scientific career will be greatly improved.

I wish you every success!

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Peter J. Feibelman

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*Readers' Suggestions Are Welcome*

My view of the world of science is inevitably framed by my own experiences and those of my colleagues. You can help make subsequent editions of this guide reflect a broader view of what it takes to establish a scientific career. Send anecdotes, suggestions, criticisms, and comments to me, care of:

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