THE CHRONICLE OF HIGHER EDUCATION
Advice

November 19, 1999

Choosing a Research Topic

By Richard M. Reis

"It is really important to do the right research as well as to do the research right. You need to do 'wow' research, research that is compelling, not just interesting."

George Springer, chairman of the aeronautics and astronautics department at Stanford University

Of all the decisions you'll make as an emerging scientist, none is more important than identifying the right research area, and in particular, the right research topic. Your career success will be determined by those two choices.

The research you do as a graduate student will set the stage for your research as a postdoc and as a professor. While it is unlikely that your later research will be a straightforward extension of your dissertation, it is also unlikely that it will be completely outside your field. Stories to the contrary are the exception, not the rule. The knowledge, expertise, and skills that you gain early on will form the foundation for your later investigations. Choosing the right topic as a graduate student will help you insure that your research will be viable in the future. The right topic will be interesting to you, complex, and compelling.

According to Cliff Davidson and Susan Ambrose of Carnegie Mellon University, "The most successful research topics are narrowly focused and carefully defined, but are important parts of a broad-ranging, complex problem."

Finding the ideal research problem does not mean simply selecting a topic from possibilities presented by your adviser or having such a topic assigned to you, attractive as this may first appear. It means going through the process of discovering and then developing a topic with all the initial anxiety and uncertainty such a choice entails. This is how you develop your capacity for independent thought.

There are a number of factors to consider when selecting a research area. Some of them have to do with your particular interests, capabilities, and motivations. Others center on areas that will be of greatest interest to both the academic and private sectors.

The chemistry professor and author, Robert Smith, in his book Graduate Research: A Guide for Students in the Sciences (ISI Press, 1984), lists 11 points to consider in finding and developing a research topic:

1. Can it be enthusiastically pursued?
2. Can interest be sustained by it?
3. Is the problem solvable?
4. Is it worth doing?
5. Will it lead to other research problems?
6. Is it manageable in size?
7. What is the potential for making an original contribution to the literature in the field?
8. If the problem is solved, will the results be reviewed well by scholars in your field?
9. Are you, or will you become, competent to solve it?
10. By solving it, will you have demonstrated independent skills in your discipline?
11. Will the necessary research prepare you in an area of demand or promise for the future?

Let's take a closer look at Mr. Smith's list. Clearly it is important to pick a problem you are enthusiastic about (1), and one that will interest you over the long haul (2). Much research is just that, re-search. At times it will be mundane, and it will surely be frustrating. Experiments won't go right; equipment will fail; data from other sources won't arrive on time (or at all); researchers who pledged their assistance won't come through as expected, while others will do work that competes with your research. During these times you'll need courage and fortitude.

Picking a problem that you can solve in a reasonable period of time (3), that will lead to further research (5), and that is manageable in size (6) is a particular challenge for most graduate students and postdocs. Doctoral students tend to take on more than is necessary to achieve what ought to be their goal: completing a dissertation or obtaining another publication or two. That's why it is essential to have the right supervisor. It's his or her job to help you determine how to make your dissertation original and publishable, yet also manageable. (More on choosing the right adviser in my next column.) There will be plenty of time for further work after you complete the Ph.D.

Whether or not a problem is worth solving (4), will make an original contribution to the literature in your field (7), and if solved, will have results that will garner the attention of scholars in your discipline (8), is at the heart of what is meant by choosing compelling topics leading to a meaningful "stream of ideas."

One way to tell if a subject is compelling is to note how many people attend seminars or symposia on different research topics. In some cases, attendance may be up for big-name speakers, but often it is because the work presented is of broad interest. These seminars can give you clues to possible research directions and topics. Of course, going into an area where there are too many other researchers has its drawbacks, but beware of going to the opposite extreme. You don't want to be the only researcher in an area that has little chance of drawing interest or support.

Your capacity to tackle the problem (9) will depend somewhat on your innate abilities. However, to solve the problem you'll also need to develop basic knowledge and technical understanding, computer skills, and experimental expertise. To acquire such skills you'll need direct access or Web access to courses and seminars, library materials, independent-study opportunities, and most importantly, other students, postdocs, faculty members, and even industrial scientists and engineers.

To develop independent skills in your discipline (10), start by defining and developing a problem that is sufficiently robust. You'll then need to acquire a fundamental understanding of certain phenomena or behaviors and experimental techniques in order to solve the problem. However, as Peter Feibelman, author of the popular book A Ph.D. is Not Enough (Addison-Wesley, 1993), says: "It is important that your focus be on problems and not on techniques or specialized tools. The latter come and go and as a researcher you want to be able to shift your approaches as needed to solve the more fundamental problems."

Choosing a research area that will be in future demand (11) can be tricky. Some fields, such as semiconductor physics and fiber optics, may have been compelling for some time, but are now approaching maturity and shifting focus and are likely to be less promising in the future.

Other areas, such as telecommunications and biotechnology, are quite popular. However, their very popularity may have oversaturated the fields. In such cases, large numbers of investigators often compete for limited financial and experimental resources.

Some fields drive the technology for other fields, and therefore may be in a better position to thrive as specific applications shift. You need to look at emerging fields and see if your work can affect these areas in some specific way. For example, work on amorphous silicon may apply to the emerging field of flat-panel displays, which, in turn, is part of an even broader field of low-power portable communications systems.

Finally, you need to pay attention to the broader implications of your work and to the possible appeal such work has to both academia and industry. As Smith notes:

"Interdisciplinary research is no substitute for good disciplinary training during the greater part of a graduate career. It is advisable, however, to seek exposure to interdisciplinary activities in graduate as well as postdoctoral training since most researchers engage in interdisciplinary research during their professional careers."

Once you've found the ideal research topic, your next challenge will be choosing the right research adviser. I will discuss a strategy for doing just that in my next column.

Richard M. Reis is director for academic partnerships at the Stanford University Learning Laboratory, and author of Tomorrow's Professor: Preparing for Academic Careers in Science and Engineering, available from IEEE Press or the booksellers below. He is also the moderator of the biweekly Tomorrow's Professor Listserve, which anyone can subscribe to by sending the message [subscribe tomorrows-professor] to Majordomo@lists.stanford.edu

Have a question or a suggestion for Richard Reis? Please send comments to catalyst@chronicle.com