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Advice for Graduate Students: The 10 Suggestions for a Basic-Research Career

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Originally, I titled this piece "The 10 Commandments…." However, that has been done before, and they are not particularly popular.

1. Establish an independent line of research as early in your career as possible. If you can, do so even as a graduate student. Avoid the graduate student's trap of thinking up experiments in other researchers' programs that the other researcher has missed. Of course these are useful studies, but do not form the basis of one's own independent line of research.

2. Be problem-oriented, not technique-oriented. Use a variety of techniques, methods, and orientations -- whichever are necessary to solve the problems at hand. Philip Teitelbaum used to recommend, back in the days of relay racks and electromechanical programming equipment that would take months to assemble for a single experiment, that whenever a study was completed, the equipment for the study should be dismantled, lest the experimenter be seduced into running another study with that equipment just because it was there. It is also painful to hear a major professor introduce one of his or her graduate students as "Jenny Green…she does c-*fos*" or "Tom Smith…he does meta-analysis". This may interest potential postdoctoral sponsors who are looking to hire new Ph.D.s because of the skills they can bring to the postdoctoral sponsor's lab, but this puts the new Ph.D. squarely into the role, perhaps forever, of technician rather than scientist. Remember, technology comes and goes, but the underlying questions are the meat of research. It is depressing to go to poster sessions at the big conferences year after year and see the same questions being asked over and over with different, more "cutting edge" techniques, presented by people enamored of the techniques rather than the research problems. If technology is so costly, in terms of equipment, learning time, and other resources, how does one avoid the trap of becoming technique oriented? The answer: collaborate.

3. Think beyond the next publication, or even the next grant proposal. Take the long view; look at the big picture. In other words, bite off a piece of question that may take a decade, or even a career to answer. There is a major difference between the scientist that wonders how to break the question into appropriate sized grant proposals, and one who wonders how to expand the question into a grant proposal. Furthermore, commit yourself to your question; given the time and energy it takes to answer an appropriate sized research question, pursuing a series of unrelated research questions in parallel rather than in series is often a sign of dilettantism.

4. If you do basic research, keep your eyes open for applications of your findings. On the other hand, if you find yourself doing applied research, keep your eyes on underlying theoretical implications. Often, the distinction between basic and applied research is arbitrary or fluid.

5. When conducting experiments, don't accept answers or results simply because they are publishable. Keep plugging away at the problem until the answers or results make sense or satisfy you in terms of an overall schema. Most importantly, don't accept other scientists' answers; reputation is not a guarantee against being wrong.

6. Expect unexpected results. A great deal of research data is discarded because an experiment "didn't work". However, a well designed experiment should provide positive information regardless of how it comes out. Design experiments so that all outcomes yield something: a "no difference" finding is not the same as a "negative results" finding.

7. Don't expect answers; expect more questions. Daniel Lehrman used to tell us that a good experiment will raise more questions than it answers. Perhaps non-scientists find this aspect of science strangely frustrating. However, the lack of a final solution distinguishes the scientist's quest from the engineer's.

8. Never stop asking questions. Questions are the stock-in-trade of the scientist. The corollary of this suggestion is "never make assumptions." Of course, assumptions are a necessary part of hypothesis construction, but on an everyday practical level, and in terms of research design, assumptions can be disastrous. Many times I've located hiding escaped rats that my students couldn't find because unlike my students, I did not assume that a rat could not "go there" or "do that".

9. Choose a problem that excites you. It should excite you so much that you can't sleep. It should excite you so much that when someone asks you the time, you blurt out your research topic.

10. Strive for elegance in research. The elegance of an experiment is in the quality of the thinking and the cleverness of the approach to answering the research question, not in the complexity of the design or the sophistication of the methods. Often, the most elegant experiments are simple, low-tech attacks at the heart of the problem. Study classic research in your field and appreciate the logic and thought that went into it. All too often students nowadays ignore older research because it isn't available online, or dismiss it for using old-fashioned techniques. There is much wisdom and cleverness in some of those old papers. Reading them, learning from them, and citing them, is real scholarship.