

## An 6-step program to choosing your PhD project

Perhaps the single most difficult thing to do during one's graduate career is to pick an appropriate PhD project. Moreover, it is arguably the most crucial decision in one's scientific (or in general academic) career, since it is going to affect all the major first steps after graduation (be that finding a post-doc, an initial position as a faculty or researcher, or securing funding for one's own research).

It is therefore astounding to see how many students make such a decision on fairly shaky grounds, and even more disconcerting how so many "advisors" are of very little use in advising their pupils on such a crucial matter. This little flyer does not, obviously, pretend to provide a simple solution to the problem (there isn't one). However, distilled below are some suggestions that should be valuable to most people embarking in an academic career.

Here it goes:



**Step I -- Determine your general area of interest**, such as ecology, evolution,

molecular biology, or philosophy of science. This should be done *before* applying to graduate school, most effectively through a combination of broad readings and apprenticeship in one or more labs as an undergraduate student.

**Step II -- Narrow down your field and sub-field of interest.** One cannot possibly work on "ecology," but at most on a particular area in the (still fairly wide) field of evolutionary ecology, and in particular on the sub-field of the evolution of tradeoffs (or something like that). This choice should ideally be made between the last year as an undergraduate and the summer before enrolling as a PhD student. As with step I, the keys are going to be wide and extensive readings, and some experience as an apprentice (which may help as much to determine what one does *not* want to work on

as what one is interested in).

**Step III -- Pick a good graduate program, but especially a good advisor.**

Contrary to what most students seem to think, a good advisor (better if well-known in the field, or young and enthusiastic -- or better yet, all of the above) is much more important than a well known school or department. Of course, the latter qualities are also not to be underestimated, but you'll have to work primarily with your advisor -- and s/he will have to write your most important letter of recommendation.

Depending on your characteristic, you may work better with some kinds of people rather than others. For example, if you need quite a bit of supervision you'll be better off with an advisor who abitually looks over her students' shoulders, at least in the beginning; but if you tend to be more independent, you'll need somebody with a looser advising style. Talk to former and current students of your potential advisor(s) to find out as much as you can ahead of time. If an advisor doesn't seem to work well for you within the first year, don't hesitate to change, possibly while maintaining a civil relationship with your former one.

**Step IV -- Come up with some good questions.** Of course, this is much easier said than done, but there are some characteristics that make some questions better than others, and there are some good strategies to follow in order to identify the most promising questions within your sub-field of interest. First off (and once again), you can't think on empty mind: read as much as you can, but this time focusing on the primary literature from the top journals in your field (which your advisor should have no difficulty in pointing out to you; examples may include *Evolution*, *Ecology*, *The American Naturalist*, etc.). Make also use of the now widespread publications that regularly update and summarize specific fields of inquiry (e.g., *Trends in Ecology & Evolution*, *Trends in Genetics*, *BioEssays*, and so forth).

What makes a question "good"? Several things: (a) It needs to be specific enough that one can work on it for 3-4 years and get publishable results, so things like "how did life on earth originate?" are out (though smaller components of the same inquiry might work). (b) On the other hand, you do wish to pursue

something that is of rather general interest in your broader field, which means that “what is the phylogenetic position of species X within genus Y?” may be a bit too narrow a choice (unless species X happens to be something like, say, *Homo sapiens*...). (c) A good PhD question (or, better, set of questions) also needs to be one which is going to generate good-level publications *regardless* of the particular answer you will arrive at by the end of your project; that’s what makes a dissertation on the search for extraterrestrial intelligence a rather poor choice: if you succeed you’ll probably get the Nobel, but the chances are much, much higher that you’ll obtain a bunch of negative results, which are notoriously difficult to get published. (d) It also helps, though it isn’t crucial, if the question(s) you are going to pursue can suitably be approached from more than one angle, or subdivided into smaller components; the goal should be to publish a minimum of three or four good-to-high level papers (possibly, some of them *before* you finish your PhD, to prepare you for the next step in the job market). While the often-heard “publish or perish” dictum in academia is a bit of an exaggeration, you will not get a job with few or low-level publications.

**Step V -- Choose a suitable experimental system.** This is almost as crucial a decision as the previous one, and it is well worth to take your time and “shop around” for a suitable system (animal, plant, or whatever) to work on -- *given your chosen questions and sub-field of interest*. Under no circumstances pick a system because it is “cute” or “cool,” and even established (“model”) systems may not represent the best choice for your particular needs.

Talk to people in your department, chat with your advisor, read around, and don’t hesitate to email people at other universities to seek more information about potential candidate systems. I cannot emphasize this enough: make the wrong choice at this stage and your entire academic career may well be headed for the drain before it even commences.

**Step VI -- Don’t be afraid to make adjustments.** Contrary to the impression one may receive from a naive reading of published technical papers, science doesn’t proceed in a

straight line from question to experiment to results. It is a tortuous, and in some way much more fascinating, path -- more similar to the investigation of a crime than to the solution of a logical puzzle by way of deductive reasoning.



What this means is that you should not be afraid of playing with your questions and experimental system until the two shape each other in a satisfactory fashion. You are likely to end up with slightly (or sometimes dramatically) different questions than you started with, but one of the crucial characteristics of a good scientific investigator is the ability to follow her nose and seize the opportunities that serendipity lays out.

Of course, even if you follow the above steps very closely, your dissertation project may still not work in the end. That may be because you were not lucky (plenty of accidents can ruin an experiment or a field season), or perhaps because this isn’t your cup of tea after all. That’s a judgment call you’ll have to make on your own.

#### Recommended readings:

- Booth, V. (1993). *Communicating in Science: Writing a Scientific Paper and Speaking at Scientific Meetings*. Cambridge, England, Cambridge University Press.
- Feibelman, P. J. (1993). *A Ph.D. is not enough! A Guide to Survival in Science*. Reading, MA, Addison-Wesley.
- Heiberger, M. M. and J. M. Vick (1996). *The Academic Job Search Handbook*. Philadelphia, PA, University of Pennsylvania Press.
- Lanyon, S. M. (1995). "How to design a dissertation project." *BioScience* 45: 40-42.
- Rosenblum, W. I. (1997). "What is an author?" *Academe* nov-dec: 34-38.

*Text by Massimo Pigliucci, available at [www.genotypebyenvironment.org](http://www.genotypebyenvironment.org)*