Choosing a Research Project

Dr. Paul Doughty

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1. How do they do it?

Choosing a good research project is not easy work. But yet, everybody *else* seems to have no trouble coming up with great projects and pursuing interesting, original lines of research. How do they do it? In what follows, I'll be going over some things that may help you to come up with a project that will produce top-class research and that's a pleasure to conduct. After going over some bits of general advice for coming up with a research project, I'll specifically address how students might choose the university environment that they'll be conducting the work. Many of these ideas are sort-of off-the-cuff, as there is no real "guide" to be consulted. Nevertheless, I think some general principles apply and there certainly are projects that are best avoided.

The pointers that follow will differ for students whether they're thinking of doing Honours, in the middle of a Masters or a PhD student thinking about a postdoctoral project. With experience, finding good projects just gets easier while the standard goes up as a young researcher learns the ropes of doing research.

2. What do you want?

So*what do you want*? This question can often send students running for cover. After all, wasn't avoiding this question part of the whole deal with uni?! By now, however, most students of herpetology will have discovered that pursuing science by working with herps is (i) intellectually satisfying and (ii) downright fun! So if you like studying herps, then it's certainly worth asking yourself: "how am I going to continue studying herps for a living?"

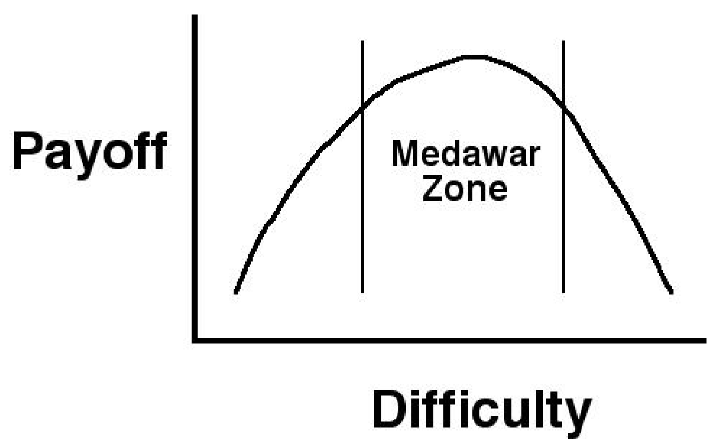
One rationale students sometimes use to avoid asking themselves this question is a kind of belief that there are "gifted" and "ordinary" students. Believing themselves to be ordinary, this can often justify neglect of thinking about a career in science. This encourages a kind of "drifting along" attitude, and (perhaps not incoincidentally) justifies slacking off too! The truth is that doing good science isn't hard once you put your mind too. And so it is with having a career in science too.

The first question you might ask yourself is: *"What kinds of questions grab my attention?"* Some people naturally gravitate towards the mechanistic side of things, whereas others are drawn towards more "ultimate" causal factors (e.g., natural selection). For example, when considering a sea turtles reproductive output, do you think of hormonal regulation or the decrement in expected lifetime reproduction? Perhaps you think of conservation-related issues, such as overharvesting of the eggs for food in some countries?

When you're at a conference such as ASH, what kinds of talks keep you on the edge of your seat and which ones put you to sleep? Take note of these basic reactions! Biology is a vast field, and given everyone's unique take on the world, we all naturally gravitate towards some areas and not others (nothing to apologise about here). Never, but never, go into an area if you're not truly interested in the issues! If you do, you could be miserable when you're doing your project. When this happens, people often generalise their experience of their project to all of biology, and this can end up with you talking yourself out of a career in biology.

A second question you may ask yourself is: *"What do you like to do?"* This question is sometimes overlooked in the pursuit of lofty scientific ideals, but is just as important as the first question. Volunteer experience can provide you an idea of what you like to do. The key is to imagine what you'll be doing day-to-day in order to get your data. For example, working on lizards versus frogs means you'll be doing very different things. Diurnal lizards get up early (but not as early as birds) and don't like cold or rainy days. If you're going to be studying breeding choruses of frogs, then you'll be spending a lot of time at night looking at a1 or 2 m spot of light by your headtorch for hours. Frogs love rain and there are many species in Australia that are winter breeders. Another example is fieldwork versus labwork. Whereas some people can't get away from the bush (with its roughness and uncertainty), other herpetologists feel on top of the world after working their pipettes and gels for a 10 hour day in the DNA lab. Good behaviourists are usually very patient, relaxed people who don't mind waiting hours to observe a single crucial behavioural act by their animal. So, what do *you* like to do?

3. Medawar Zone



A depiction of the "Medawar Zone" is shown in the figure. Now what exactly is the Medawar Zone? In reference to the figure, the Medawar Zone is the middle area that yields a high payoff of discovery with a moderate degree of difficulty. In an excellent article on creativity in research, Craig Loehle named this zone after Sir Peter Medawar, a Nobel prize-winning medical researcher who was active from the 40s to the 60s. In a book called "The Art of the Soluble", Medawar suggested that there seems to be a certain time when scientific questions seem especially ripe for answering, whereas other questions remain elusive and out-of-reach from investigation.

The Medawar Zone is all about asking the right questions at the right time. For example, microsatellites revolutionised the way people thought about sexual selection in the 80s and 90s. This was possible because the means to characterise fast-evolving DNA opened the door for behavioural ecologists interested in realised paternity.

In contrast to the Medawar Zone, projects that are very easy to carry out often yield a low payoff. This doesn't mean there's anything particularly wrong with easier projects, it's just that often they don't turn out to be all that surprising (and therefore people are less interested in them). Honours and PhD supervisors will generally try to push their students to the left of the Medawar Zone. Supervisors may do this because they don't want to risk their students' projects stuffing up and dealing with the headaches that arise for both student and supervisor. The tendency for supervisors to want to push you to the left of the Zone is not all bad. Early in your career you'll be learning the ropes of conceiving, executing and writing up research projects. This is fine for an Honours project, but PhD students should take on more difficult projects as they present more of an exciting challenge and the promise of a higher payoff.

Difficult projects can turn into smashing success stories, but these are probably best left until later in your career when you can afford the risk of a disaster. Experienced researchers also have more of an intuition of where the exciting new developments in their field are going, and can use their experience (partially based on past mistakes!) to pick a project that may yield a high payoff.

4. How to hit the Medawar Zone

One of the surest ways to consistently hit the Medawar Zone is to *be informed*. Being on top of the current debates in science puts you in a position to know where the gaps in knowledge are. After identifying the gaps, then all you need to work out is what kinds of information will fill these gaps. OK, so it's not going to be that easy. Usually the gaps in knowledge aren't filled yet because (i) the data are too difficult for more senior academics to bother getting, (ii) a lack of the right technique to get the coveted data or (iii) nobody's got around to it yet. This last option is made possible by staying up on current trends and anticipating where your field will lean to next.

Following "hot" journals is a good way to keep up with current debates. Often "trendy" areas in science just refers to areas where many people are doing lots of work because there is a need for data. Taking one kind of punk option and turning your nose up at these areas will lead you away from the action into a non-trendy area. This is fine (and attitude is a good thing in a young scientist), but there are risks involved and you may end up doing cold science. A better punk option would be to take on the underlying assumptions of these fields, or seek out those areas where you don't see eye-to-eye with the main protagonists of the new paradigms. Picking fights in science can often lead to breakthrough work because the participants are usually highly motivated individuals.

In addition to keeping up with the latest and greatest in herpetological science, *people* are a valuable source of information. For future interesting project ideas your future supervisor will obviously have a major input (for students looking towards their next degree). But your current supervisor (now that you know them a little better) can often point you into interesting directions for your next project (and lab see below). Your more senior colleagues hanging around in your department might be able to give you some leads on what's interesting, as are people whom you've met at meetings.

Good science is about discovery. It is often the case that discoveries can be made when conducting carefully controlled experiments in the lab aimed at teasing apart fine-grained hypotheses about universal phenomena. However, discovery in herpetology can often be as easy as stumbling on to cool weird stuff that herps do. For example, male wrestling contests for female access in a WA frog were described in a recent paper (Roberts et al. 1999. An.Beh.). This work was then followed up by more rigorous tests of questions that naturally arose out of the initial largely descriptive work (Byrne & Roberts 2000, Proc.Roy.Soc, Evol.).

You can find out about such potentially interesting phenomena by talking to naturalists. They can be found in old wings of your department, in amateur herp groups, in museums or reptile parks. Herpos hold vast amounts of unorganised information on herps in their heads and will gladly divulge it to you if you ask them about it. Australia is blessed with tremendous diversity in herps with relatively few people studying them. Students can take advantage of this by keeping their ears open for interesting herpetological stuff from herpos who have been in the bush and seen herps doing their thing in the bush.

5. An aside multiple projects

One way to lock on to a project that will work for you is to take a page from the process of natural selection. Early in your PhD, start two or three projects that you think will have a good chance of succeeding. After a while you can then choose which project you like the best. With this approach, usually the worst thing that happens is that you like all of the projects! If so, then you still choose the one that will yield the most interesting results. You can then scale down the time investment into the other projects or postpone them until you have some time to pursue them later.

It is possible, however, to take this too far and dilute your research efforts. Early in your career you want to develop a reputation for having made major inroads into important problems. Choosing projects that are clustered around a central theme will help this to happen

6. Choosing a research environment

For students, choosing a research environment is not easy. This is a good reason to plan about where you're going to go well before the time comes (i.e., when you're out of work and money!).

Again, your current supervisor is a goldmine of information on what labs are the most productive and that you'll be happiest in. However, bear in mind that the character of labs change with the people that inhabit them, so things might be different now than what your supervisor may think. (Solution: check it out yourself! More on this later)

By keeping up with the literature, you can get a feel for which people and labs are producing cutting-edge science and which might have cooled down a bit. It's also perfectly fine to e-mail authors of interesting papers directly about working with them. Just be sure to be brief (everyone's busy) and attach a brief CV (to let them know a bit about your talents). You can learn a lot about a person by surfing their uni's web site and reading about their work and projects there.

Funding is very important when choosing a research environment. This is especially so for molecular-type projects that may require that the funding be entirely in place before you even start. If you are dead-set on working on a project that will require lots of money, then seek out researchers with large active (i.e., funded) research programs. In labs with lots of grant money, funds can often be shunted around to help you get your project started. You can then apply for your own funds later based on the pilot study.

Several more subtle issues may also come into your decision of choosing a research environment. For example, what are the pros and cons of a younger versus an older supervisor? A younger supervisor might be more on top of current trends, but may also be busier and less likely to put you on to the choicest research projects. On the other hand, having an older supervisor might help you later if they have a strong reputation and can write a good letter of recommendation for you. Then again, older supervisors can sometimes be the heads of "factories" labs where the students know only their little bit while the chief knows the big picture.

One of the most important (and hard-to-judge) things to consider when choosing a place to study is how good a *mentor* your supervisor will be. In other words, will your supervisor go out of their way to look after your career? Do they know what's important for getting a young researcher established early in their career? Do they, will they care?! This is a hard one to judge, but their track record of turning out students that "stick" in research and their (dare I say?) personality might help you decide on this one. Feel free to ring or e-mail former students to ask them what it was like to work with this person they'll have lots to say!

Finally, the best thing to do when choosing a research environment is to *go there*! Since you'll be spending years at this new place with these new people, you may as well spend some money to check it out. In addition to meeting your supervisor, checking out the facilities and environs is important too. You'll be working alongside the people in the lab for some time, so it would be nice to get to meet them and have a chat about how they feel about working in that lab. Are they happy there? Are they up-to-date on the "big picture" in science, or do they come across cogs in the "factory" working on their little assigned piece of the puzzle? After visiting a few labs, a decision should naturally emerge from your gut instincts. The important thing is to be proactive and get out there and meet people!

7. Parting words

DO NOT PANIC. All projects have their hiccups and mini-disasters. It's important at such times to not despair and think that it's somehow a reflection of you. Doing good research is all about persevering through all the problems that can and do go wrong. Experienced researchers are used to this and see project fire-fighting as simply part of the job.

If you're not enthusiastic about your research, you'll do a poor job and eventually convince yourself that biology is not for you. You can avoid this by choosing projects you'll be happy doing.

Therefore, in your choice of research topics BE INDULGENT!!! Your parents have probably already given up on you for your foolish choice of biology over law, medical school, accounting, etc. Since you've chosen biology from deep within your heart and soul, you may as well keep choosing your research topics for the same reasons.

Finally, IT'S YOUR PARTY! Doing research is a wonderful opportunity to learn about biology and about your own abilities too. Pushing yourself during your studies pays off in terms of the research, but can also sharpen your critical and organisational skills too. This is best achieved by choosing a research project you'll be excited about and that's "yours". Good luck and happy choosing!

8. Further reading

Feibelman, P. J. 1993. A Ph.D. Is Not Enough. Addison-Wesley, Reading, MA.

Loehle, C. 1990. A guide to increased creativity in research inspiration or perspiration? BioScience 40:123-129.

Medawar, P. B. 1967. *The Art of the Soluble.* Oxford Univ. Press.

Web resource: http://online.anu.edu.au/BoZo/Scott/Studentresources.html

http://online.anu.edu.au/BoZo/Scott/DoughtyProject.html