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CHAPTER 1

Picking a Question

To contribute to progress in ecology, you need to generate new knowledge. This is quite different than memorizing what is already known. As a result, perhaps the most critical step in doing field biology is picking a research question. Tragically, it's the thing that you are expected to do first, when you have the least experience. For example, it is helpful if your application essay for grad school appears to be focused on a particular set of questions that matches a professor's interests. However, at this stage in most students' careers, many topics sound equally interesting, so forcing yourself to focus in this way is daunting or even painful.

The gold standard: Novel, general, feasible, exciting, and not perfect

Your research question should be as novel as possible. All projects have to be original to some extent. We all like to hear new stories and new ideas, and ecologists place a large premium on novelty. If you are asking the same question that has been answered in other systems (that is, with similar organisms in analogous environments), it behooves you to think about what you can do to set your study apart from the others. That said, if you are trying to start a project and haven't yet thought of a novel question, a useful way to begin may be to repeat a study that captured your attention and imagination, but with a different organism or system. Your repeat can be quick and dirty (not too many reps, not too long term), just enough to inspire an exciting new direction for your own novel question.

Policy makers are much less concerned with novelty than academics are. If you are funded by an agency to answer a specific policy question, you will need to balance your academic colleagues' expectations of novelty and your funding source's demands to answer the specific question for which you are funded. Your first priority should be to generate relevant data for your funders; however, if possible, ask additional, complementary questions in your study system that can lead to publishable research.

In addition to novelty, ecologists like generality. That is, we get more excited about broad or theoretical questions than about specific, narrow ones. It is possible to ask a question that is too general, especially if you are building a model; in that case, ask yourself if your answer will reflect reality for at least one actual species or habitat. It is more common for students to find themselves answering an overly specific question that may be considered important by only a very small community. If possible, ask a question that has the potential to matter beyond your study system or organism. If your question is very specific, ask whether you can generalize from your results. For example, you may find yourself answering a question about managing a specific fishery, restoring a particular plant species, and so on if your funding comes from an applied source. It may not be possible to couch your question in more general terms. Instead, you may be able to ask a complementary, more conceptual question as well. For example, let's say you have been funded to determine which animals visit a particular endangered night-blooming flower. More general (and interesting) questions might be which of those animals successfully fertilize that species and what characteristics of the flower and/or the visitors make them effective pollinators. Are the traits that you identify shared by other night-blooming species? The answers to these latter questions will be compelling to a broader audience.

A relatively small question can catalyze a general question. By "small," we mean specific to your study system and with relatively little replication. Small questions will often generate more excitement for you than bigger ones because their more modest goals can be achieved with relatively few data, and much more quickly. Imagine that you want to study rates of predation on Canada goose eggs. These eggs may be difficult to find and highly seasonal. So, you could conduct a small pilot experiment with three cartons of chicken eggs from the grocery store. Your pilot study will not give you definitive answers about goose eggs but will likely provide useful insights about how to conduct that experiment. You don't need to invest an entire season on a pilot study. Do simple analyses of your data early and often. If results from the pilot study turn out as expected, they can provide a foundation for a bigger project. If the results are unexpected, they can serve as a springboard for a novel working hypothesis. Almost all of our long-term projects had their beginnings as pilot "dabbles."

A third component of a good research question is feasibility. Many of the factors associated with failure or success in field projects are beyond your control. Nonetheless, you should ask whether your ideas are feasible—are you likely to get an answer to the questions you pose? Do you have the resources and knowledge to complete the project? Your armchair answers to these questions won't necessarily be on target, but they can help you anticipate and plan for potential problems. Don't talk yourself out of doing an exciting project just because it seems challenging—think about ways to make it work.

Since most field projects don't work, try several pilot studies and follow the leads that seem the most promising. If you know that you want to ask a particular question, try it out on several systems at the same time. You'll soon get a sense that the logistics in some systems are easier than in others and that the biological details make some systems more amenable to answering particular questions. It is a lucky coincidence that Gregor Mendel worked on peas since they are particularly well suited to elucidating the particulate nature of inheritance. Other people had attempted to ask similar questions but were less fortunate in the systems that they chose to investigate. Don't get discouraged about the ones that don't work. Successful people never tell you about the many projects (journal submissions, job applications) they didn't pull off. You should feel fortunate if two out of seven projects work well.

An essential ingredient of a good project is that you feel excited about it. The people who are the most successful over the long haul are those who work the hardest. No matter how disciplined you are, working hard is much easier if it doesn't feel like work but rather something that you are passionate about. As Kong Fuzi (previously known as Confucius in the West) is supposed to have said, "If you have a job you love, you will never have to work a day in your life." Dehua Wang, a professor of zoology at Shandong University, has told us that a better translation is something like: "Those who know are not as good as those who want to know, and those who want to know are not as good as those who are driven to know." The message here is to pick a project that is intellectually stimulating, specifically *to you.* Figure out what you are really driven to know. You are the one who must be excited enough about it to do the boring grunt work that all field projects involve. You will feel much more inclined to stay out there in the pouring rain, through all the mind-numbing repetitions that are required to get a large enough sample size, if you have a burning interest in your question and your system.

So, you're looking for questions that are specific yet general, and novel yet relevant to your interests. You could fret over this for years. Don't agonize over the perfect study before you are willing to begin (see box 1). One of the most unsuccessful personality traits in this business is perfectionism. Field studies are never going to be perfect. For example, don't get stuck thinking that you need to read more before you can do anything else. Reading broadly is great,

Box 1. *Advice for three types of ecologists on picking questions*

There are three kinds of ecologists:

· The perfectionist who waits for a transformational idea before starting,
· The jackrabbit who has a lot of energy and wants to get started before thinking through their goals and their study, and
· The Goldilocks who is just right, someplace in between.

If you are a perfectionist who can't get started because you haven't thought of the perfect question, we suggest you just go out there and do it. The experience and insight (not to mention publications) that you'll get by doing an imperfect study will help you improve in the future. If you are a jackrabbit and find yourself starting a million projects, our advice is to take a moment and ask which of these questions is most likely to advance the field and, even more importantly, inspire enduring passion in you. And if you are a Goldilocks who has it just right, maybe post that you are humbled by your own success.

but you will learn more by watching, tweaking, and thinking about your system. Talk to many people about your ideas—your major professor, peers, family, and so on. It is not realistic to expect yourself to sit at your desk and conjure up the study that will revolutionize the field. Revolutionary questions don't get asked in a vacuum; they evolve. This is one reason repeating a past study (see above) can be a useful springboard. We often start asking one question, hit a few brick walls, and get exposed to some ideas or observations that we hadn't previously considered; and then pretty soon we're asking very different questions that are better than our initial, naïve ones. Most projects don't progress as we originally conceived them.

How to pick a project

There are two approaches to picking a project: starting with the question or starting with the organism or system. The difference between these two is actually smaller than it sounds, because you generally have to bounce between both concerns in order to come out the other side with a good project. So regardless of which one you start with, you need to make sure that you are satisfying a list of criteria related to both.

Starting with a question

Many successful studies start with a question. You may be interested in a particular kind of interaction or pattern for its own sake or because of its potential consequences. For example, you may be excited by the hypothesis that more diverse ecological systems are intrinsically more stable. Maybe you're interested in this hypothesized relationship because if it is generally true, it could provide a sound rationale for conserving diversity, and if it is not generally true, ecologists should not use it as a basis for conservation policy. Since many studies have considered this question, you should think about what's at the bottom of the hypothesized link between biodiversity and stability. Have previous studies addressed these key elements? Are there novel aspects to this question that haven't been addressed yet? Are there assumptions that scientists take for granted but have never tested? Even questions that have been addressed by many researchers may still have components that have yet to be asked.

If you start by asking a question, you will need to find a suitable system (that is, interacting species and their surroundings) to answer it. The system should be conveniently located. For example, if you don't have money for travel, choose a system close to home, and if you don't like to hike, choose plots close to the road. Your study organisms or processes should be common enough for you to get enough replication. Ideally, your sites should be protected from vandalism by curious people and animals (or it should be possible for you to minimize these risks). Your system should be amenable to the manipulations that you would like to do and the observations you would like to make. You can get help finding systems by seeing what similar studies in the literature have used, by asking around, or by looking at what's available at field stations or other protected sites. The appropriate system will depend upon the specific questions that you want to ask. If your question requires you to know how your treatments affect fitness, you will want to find a short-lived species rather than a charismatic but long-lived species. If your hypothesis relies upon a long history of coevolution, you should probably consider native systems rather than species that have been recently introduced. (Incidentally, there is a widespread chauvinism about working in pristine ecosystems. The unspoken assumption seems to be that the only places where we can still learn about nature are those that have not been altered by human intervention. Certainly, less disturbed places are inspiring and fun, but they also represent a very small fraction of the earth's ecosystems. There are still plenty of big questions about how nature works that can be asked in your own backyard regardless of where you live—we can attest to this, having lived in some truly uninspiring places.)

Be careful that you aren't shoehorning a system to fit your pet hypothesis. If you start with a question, look around for the right system for that question and be willing to modify your question as necessary to go where the natural history of your chosen system takes you. You cannot make your organisms have a different natural history, so you must be willing to accept and work with what you encounter.

It is also possible that you will be handed a question, particularly if you are a master's student. This has its benefits and drawbacks. You don't have to come up with your own hypothesis; on the other hand, you may not feel as much ownership of or excitement about your research. Also, you may not learn how to pose a good question, which is one of the most important skills you can take from grad school. If your major professor agrees, you may be able to add your own question as well.

Starting with a system

If you start with an organism or a system because of your interests, your funding, your major professor, whatever, you may find yourself in search of a question. Try skimming the literature broadly to get a sense of the kinds of questions that are exciting and interesting to you (see chapter 6 for reading strategies); you may be able to apply those questions to your organism or system. Often an organism becomes a model for one suite of questions but has not been explored for others. For example, the genetics of *Drosophila* and *Arabidopsis* are well studied in the lab, but their ecologies are poorly known in the field. Similarly, sometimes a system becomes popular for one type of question, but no one has asked the question you're interested in. (In those systems, previous studies may also offer you valuable background natural history.) For example, nectar was long assumed simply to be sugar water that attracted and rewarded pollinators. Tadashi Fukami and his lab knew that nectar contained diverse microbes and used microbes in nectar to develop novel models about community assembly (Peay et al. 2012). As a postdoc in this lab, Rachel Vannette then used the same system to ask questions about how these nectar microbes affected plant-pollinator interactions (Vannette et al. 2013).

Let's say you don't have an organism *or* a system. Try going to a natural area and spending a few days just looking at what's there. As you poke around, generate a list of systems and patterns. For those that interest you most, gather quick-and-dirty quantitative and qualitative data. For example, you might observe that snails are at a particular density at your study site. Next, ask whether there is natural variation in this measurement. Do some microhabitats have more snails than others? Is there natural variation associated with behavioral traits? For example, are the snails in some spots active but those in other spots aestivating? Is there variation between individuals? Are the snails in some microenvironments bigger than those in others? And so on. Once you have quantified these patterns, ask more about them. What mechanisms could cause the patterns that you observe? What consequences might the patterns have for individuals and for other organisms? Traits are shaped by natural selection, so ask questions about survival and reproductive output when you can. Using this technique during Rick's field class, students invariably come up with more good questions than they can pursue.

Mechanisms and consequences

Even if a pattern you observe in your scouting has been described before, it may still form the basis of many novel projects. If it is an important and general pattern, other people have probably noticed it too. However, it is less likely that the ecological mechanisms that cause the pattern have been evaluated. Understanding ecological mechanisms not only provides insight into how a process works, but also can tell us about its effects and where we would predict it to occur. Elucidating the mechanisms of a well-known pattern is likely to be a valuable contribution. Generate a list of potential mechanisms and then devise ways to collect evidence to evaluate the strength of each.

It is also less likely that the consequences of the pattern have been described. Does the pattern affect the fitness of the organisms involved and under what conditions? Does it affect their population dynamics? Does it affect the behaviors of organisms in the system? Answering any one of these questions is plenty for a dissertation.

Don't assume that questions have been answered just because they seem obvious. For example, thousands of studies have documented predation by birds on phytophagous insects, but the effects of that predation on herbivory rates and plant fitness went relatively unexplored for decades (Marquis and Whelan 1995, Mooney et al. 2010). Also, although periodical cicadas are the most abundant herbivores of eastern deciduous forests of North America, their interactions with their host plants and the rest of the community are largely unexplored. Several hundred years into studies of the natural history of periodical cicadas, Louie Yang (2004) found that the pulses of dead cicada adults stimulated soil microbes and altered plant communities.

Scores of ecologists have observed that individual organisms vary from one to another, but most have dismissed that variation as noise and focused only on the average tendencies. This reflects a general tendency to look for overall trends and to disregard variation. Consequently, variation among individuals is a promising source of questions. For example, recent work looking at individual variation among conspecific animals has found that it can be interesting and important in its own right (Sih et al. 2012). Similarly, the extent of spatial and temporal variation in plant traits has been found to be as impactful to herbivores as the mean values of the traits themselves (Wetzel et al. 2016). In both of these examples, innovative advances have been made by considering variation around the mean values of traits even though the trait means themselves had been well studied. In short, there are still many interesting unanswered questions even in well-known systems.

Telling a complete story

Your ultimate goal will be to tell one complete story, which will be more compelling and satisfying than a haphazard assortment of loosely related pieces. So, once you have selected a question and collected some preliminary data, think about how to develop your story as fully as possible. Keep in mind that no story is ever truly comprehensive. Here are some additional questions that could make your study more complete.

1. Think about whether the phenomenon you are studying applies generally. For instance, you may want to repeat your studies that had interesting results at other field sites or with other species.
2. If possible, work at levels both upstream (mechanisms) and downstream (consequences) of the level of your pattern. What ecological mechanisms could generate the pattern that you observe? What other organisms or processes could the pattern affect?
3. Explore whether your phenomenon operates at realistic spatial and temporal scales (see chapter 3). For instance, if you conducted an experiment at a small spatial scale, do your results apply at the larger scales where the organisms actually live?
4. Consider alternative hypotheses that could produce the patterns and results you observe (see chapter 4).

The more complete your story is, the more useful and appreciated your work is likely to be. Each of these additional questions can take a lot of time and energy, so don't expect to address them all. Prioritize the questions that flesh out your best story and the questions that you can feasibly answer.

The bigger picture: Your question should reflect your goals

The question that you pick should reflect your goals as a biologist. It's worth figuring out what your short-, mid-, and long-term goals are and then making a plan to help you achieve them. If you are a new grad student, your short-term goal might be nothing more than to succeed in grad school. Make sure you don't focus on gaining what you believe will be marketable skills at the expense of doing something you are passionate about. It's important to look farther down the road even as you're beginning. Try to pose a question that is deeply interesting to you.

A common mid-term goal is getting your first job. For most jobs (those at research universities, small liberal arts colleges, federal agencies, and nonprofit organizations), search committees want to see a strong record of research and publication even if you won't be expected to do research or publish a lot on the job. Box 2 presents a justification for this bias. Search committees want to know that you are capable of advancing the field and communicating effectively. (They may also want to see other qualifications and experiences, such as teaching, grant-writing, or outreach; see chapter 7.)

Box 2. *The importance of research for people who aspire to non-research careers*

Even if a career in research is not one of your long-term goals, it is still worth throwing yourself into the world of research while you work on your degree. The process of doing research will teach you things that are hard to absorb and integrate in any other way.

- Testing your own hypotheses helps you understand how individual biases, preconceptions, and points of view shape the ecological information that appears in textbooks.
- Over time, working on independent research helps you to incorporate scientific reasoning into your everyday thinking, which allows you to analyze reports and articles critically and to teach the information to others more effectively.
- Even if you are already a strong communicator, writing up your results will teach you how to write more efficiently, concisely, and clearly.
- Analyzing your own data is a much more compelling way to absorb important abstract ideas and analytic tools than trying to learn them from homework sets.

These and other insights and skills are virtually impossible to gain solely through reading; instead, you are more likely to learn by truly immersing yourself in your research. And besides, it's fun.

Your mid-term plan will probably revolve around a larger suite of questions than your short-term plan. For example, your plan might include solving a problem in restoration, such as how to return a particular piece of real estate to some level of ecological functioning. A more conceptual mid-term goal might involve making people rethink the interactions that drive the abundance or distribution of a taxon.

Long-term goals are harder to formulate but are at least as important. (If you don't believe this, talk to some burnt-out researchers late in their careers. Some people never stopped to figure out what they really valued and wanted to accomplish for themselves. Thinking through your big-picture, long-term goals makes doing the work more enjoyable.) Some long-term goals that you might want to try out include attempting to influence how you and others think about or practice a subdiscipline of biology, how to manage a crop, or how to recognize and mitigate some effects of climate change.

Your long-term goals should suit you and not necessarily your major professor (who may consider nonacademic goals a waste of time), and not necessarily your parents (who may try to convince you that a conceptual thesis will leave you unemployable). While you shouldn't let uncertainty about your long-term interests slow down your research progress, having long-terms goals in mind can provide a yardstick with which to evaluate your choice of project.

In summary, allow your organisms to direct your questions. Many discoveries in science are unplanned. While you are answering one question, you are likely to see things that you haven't imagined. There is some chance that nobody else has seen them either. Rather than trying to force your organisms to answer your questions, allow them to suggest new ones to you. Read broadly so that you recognize that something is novel when you stumble upon it. Above all, be opportunistic!

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