CHAPTER 1

Picking a Question

Perhaps the most critical step in doing field biology is picking a question. Tragically, it's the thing that you are expected to do first, when you have the least experience. For example, it helps to get into grad school if you appear 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 this forced focus is difficult or even painful.

The question that you pick should reflect your goals as a biologist. If you are a new grad student, your short-term goal might be nothing more than to succeed in grad school. However, it's important to look farther down the road even as you're beginning. A common mid-term goal is getting your first job. For most jobs—those at research universities, small liberal arts colleges, federal agencies, nonprofit organizations—search committees will want to see a strong record of research and publication even if you won't be expected to conduct research or publish a lot on the job. Box 1 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.) We consider strategies for getting different kinds of jobs in ecology in chapter 7. Achieving a goal like getting a first job also demands that you build a mid-term plan for your research. 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 are important determinants of the abundance or distribution of species.

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 bothered to stop and 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 certain subdiscipline of biology, or how we manage a habitat or species. Such long-term goals can provide a yardstick with which to evaluate your choice of project. Your long-term goals should suit you and not necessarily your major advisor (who may consider a nonacademic career a waste of time) and not necessarily your parents (who may try to convince you that a conceptual thesis will leave you unemployable). Refer to the "How to Get a Job" section of chapter 7 to begin the difficult work of untangling your goals from theirs.

From the beginning, consider your short-, mid-, and long-term goals as you pick your research question. Push yourself to pose a question that both satisfies your goals

Box 1. *The importance of research for people who aspire to non-research careers*

Even if a career in research is not part 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 give you insights into ecology that are extremely difficult to get anywhere else.

- Doing experiments yourself 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 own thinking, which allows you to analyze reports and articles critically and to teach the information to others more effectively.
- Writing up your results teaches even strong writers how to write more efficiently, concisely, and clearly.

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

and will be of broad interest to others. At the same time, don't let the quest for the perfect question keep you from making tangible research progress. Figure out how narrow or broad you want your research question to be. You should recognize that if you answer a very specific question, your results may be considered important by only a very small community. Academics are more likely to get enthused about a more general question. On the other hand, it is also possible to ask a question that is too general (theoretical), so you should ask yourself if your answer will reflect reality for at least one actual species. Having a model organism in mind will keep you more grounded in reality and increase the size of your audience.

If your question is very specific, ask whether you can generalize from your results. You may find yourself answering a specific, non-conceptual question about fisheries biology, restoration, and so on if you receive funding from an applied source. It may not be possible to couch your question in more conceptual terms. If so, you may be able to ask a complementary, more general question as well. For example, your specific question might be which animals visit a particular night-blooming flower. More general (and interesting) questions might be which visitors succeed at pollinating the flower and what qualities of the flower and visitor make pollination more likely. The answer to these latter questions will be compelling to a wider audience.

Not only should your question be of broad conceptual interest, but it should also 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 study 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, one useful way to begin is to repeat an experiment or a study that captured your attention and imagination. Sometimes repeating a published study as a jumping off place will keep you from getting stuck and will inspire you to move in an exciting new direction.

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' expectation of novelty and your funding source's demands to answer the specific question for which they are giving you money. 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.

So you're looking for questions that are specific yet general and novel yet relevant to your goals. You could fret about this for years. Don't obsess about thinking up the perfect study before you are willing to begin (see box 2). 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, but you will learn more by watching, tweaking, and thinking about your system. In addition, it is not realistic to expect yourself to sit at your desk and conjure up the perfect study that will revolutionize the field. Revolutionary questions don't get asked in a vacuum; they evolve. You start asking one question, hit a few brick walls, get exposed to some ideas or observations that you hadn't previously considered, and pretty soon you're asking very different questions that are better than your initial naïve ones. Most projects don't progress as we originally conceived them.

Box 2. *Advice on picking questions for three types of ecologists*

There are three kinds of ecologists:

- The perfectionists who can't get started,
- The jackrabbits who have a lot of energy and want to get started before thinking through their goals, and
- Those who are 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 get 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 step back and ask which of these questions is most likely to advance the field and, even more importantly, to inspire enduring passion in you. And if you are a person who is just right, don't get a swelled head about it.

It is fine to start by asking a relatively "small" 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 goose eggs. These eggs are difficult to find and highly seasonal. So, you could conduct a small pilot experiment with three cartons of 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. 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 small pilot "dabbles."

Fieldwork is a hard business, and many of the factors associated with failure or success are beyond your control. You should ask whether your ideas are feasible—are you likely to get an answer to the questions that you pose? Do you have the resources and knowledge to complete the project? To deal with the reality that field projects are hard to pull off, we suggest that you try several pilot studies simultaneously. 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 much more difficult than those 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 attempted to ask similar questions but were less fortunate in the systems that they chose to investigate. Since most field projects don't work, try several possibilities and follow the leads that seem the most promising. Don't get discouraged about the ones that don't work. Successful people never tell you about the many projects they didn't pull off. You should feel fortunate if two out of seven 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. You've heard the old saying, "If you have a job you love, you will never have to work a day in your life." Pick a project that is intellectually stimulating *to you.* You are the one who has to be jazzed 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.

There are two approaches to picking a project: starting with the question or starting with the system. The difference between these two is actually smaller than it sounds because you generally have to bounce between both concerns to come out at the end 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.

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. Perhaps you are interested in this hypothesized relationship because if it is true, it could provide a sound rationale for conserving biodiversity, and if it is not generally true, ecologists should not attempt to 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 of 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 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 near the road. Your study organisms or processes should be common enough for you to get good 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 subject it to 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 close to your home. 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 an annual 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 implicit argument seems to be that the only places where we can still learn about nature are those that have not been altered by human intervention. We wonder if any such places really exist. 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 worked in some uninspiring places.)

One danger to guard against is trying to shoehorn a system to fit your pet hypothesis. If you start with a question, make sure you are willing to look around for the right system for that question and that you are 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.

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. Often one organism becomes a model for one kind of question, but it has not been explored for others. For example, the ecologies of lab darlings *Drosophila* and *Arabidopsis* are poorly known in the field. If everyone has used a system to ask one kind of question, there may be a lot of background natural history known about that system, but nobody has thought to ask the questions that you have. If you have a system but need a question, try reading broadly (and quickly) to get a sense of the kinds of questions that are exciting and interesting to you.

If you don't already have a system in mind but want to start out by taking this direction, try going to a natural area and spending a few days just looking at what's there. Generate a list of systems and questions in your notebook that you can mull over and prioritize later. Another useful approach is to start with a natural pattern you observe. First quantify that pattern. 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 places aestivating? Is there variation between individuals? Are the snails in some microenvironments bigger than those in others? Are bigger snails more active? 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 on individuals and on other organisms?

Even if the pattern you observe in your scouting has been described before, there are likely to be many great projects available. 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 test 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 that show it? Does the pattern affect their population dynamics? Does it affect the behaviors of other 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). More recently, effects of bird predation have been found to vary dramatically from one tree species to another (Singer et al. 2012). As another example, 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. Pulses of dead cicada adults stimulate soil microbes and alter plant communities (Yang 2004). In short, there are still many interesting unanswered questions even in well-known systems.

Sometimes ecologists are constrained by funding sources or by labs that work on one set of organisms. If so, all of the good questions may appear to have already been addressed. Again, consider asking questions about the ecological consequences of what everyone else works on. For example, if you work in a lab where everyone works on the morphological changes in an herbivore that are induced by exposure to various predators, one more demonstration of an induced response may not be very novel. Perhaps you can ask what the fitness consequences of the different morphologies may be. Alternatively, try turning the question on its head and ask how predators and competitors respond to different morphologies of the herbivores.

Once you have selected a question and collected some preliminary data so that you know it is feasible to answer the question, next think about how to answer it as completely as possible. One complete story will be more compelling and satisfying than a haphazard collection of loosely related pieces. Prioritize the questions that flesh out your best story and the questions that you can feasibly answer. See chapter 8 for suggestions about organizing your research into one compelling story.

Here are some additional questions that could make your study more complete.

1. Consider alternative hypotheses to produce the patterns and results that you observe (see chapter 4).
2. Think about whether the phenomenon that you are studying applies generally. For instance, you may want to repeat your studies that gave interesting results at other field sites. You might also want to repeat them with other species.
3. Explore whether your phenomenon operates at realistic spatial and temporal scales. For instance,

if you conducted a small-scale experiment, do your results apply at the larger scales where the organisms actually live (see chapter 3)?

4. If possible, work at levels both below (mechanisms) and above (consequences) the level of your pattern. What ecological mechanisms could generate the pattern that you observe? What other organisms or processes could the pattern affect?

You may not be able to answer all of these questions, but 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.

Coming up with research questions can be intimidating. You'll produce better ideas if you separate idea generation from critique. Our parents, teachers, friends, society have taught us to censor our thoughts and inclinations. Failing to do so leads to so much humiliation that, as Foucault's panopticon tells us, we repress ideas that may be "incorrect" before we are even aware of them (Foucault 1977). In order to generate new ideas, we need to temporarily turn off the censor in our heads. Be willing to hang with the dumb ideas that you will inevitably come up with, because the really great ideas stand on the shoulders of the dumb ones. Creative people in all fields tend to share two traits: an ability to tolerate ambiguity (the messiness of complex problems) and a willingness to take risks and sometimes fail (Feist 1998, Martinsen 2011).

Box 3. *Generating ideas*

One technique we like is sometimes called iterative writing (Ian refers to it as "the Acid Trip"). This exercise may sound flaky, but we have found it works well for us, and even students who aren't from California have found it helpful and fun.

Before you begin, prepare for the activity:

Gather two large pieces of paper, a few different pens, and a highlighter marker.

On the first piece of paper, write a question you'd like to consider—for example, "What is my research question?" or "How should I spend this field season?" or "What do my results mean?"

Now you are ready to do the two parts that will help you generate interesting responses to the question: get relaxed and write out your ideas.

First, relax. Believe it or not, certain relaxation techniques have been shown to increase the originality and quantity of ideas (Colzato et al. 2012). We like to do a whole-body relaxation that involves consciously considering regions of the body ("Relax your toes and feet and ankles"; then "Relax your calves and your knees"; and so on).

Once you've worked your way up through your neck and face, "wake up" only your writing hand.

Now begin responding to the question you wrote at the top of the page. You are the only one who needs to see this work, so be as open to whatever comes out of your pen as possible. Remember to shut off the censor—write everything that comes into your head, whether it seems related or not. There may be times when your thoughts run dry and you can't think of anything that seems worth writing.

Box 3. Continued

We've found ourselves tempted to give up when this happens. But the process works best if you continue writing, even if you write "I don't know what to write" or rewrite something you've already written. Better yet, take a risk and write something really crazy. You'll soon find yourself generating more ideas again.

After you've done this for about 10 minutes, grab the highlighter and intuitively highlight words or phrases that seem appealing to you. Don't overthink this—keep it loose. Transfer those highlighted words and phrases to the second sheet of paper.

Use the new words and phrases to continue writing. Sometimes you will find yourself continuing to answer your original question, and other times you will go off in a new direction; it doesn't matter.

After another 10 minutes or so, repeat the process of transferring phrases and using them as a springboard for new writing if it seems productive.

When you feel like you have finished (between 20 and 40 minutes), set down your pen and slowly "wake" back up.

After you have completed this relaxation and iterative writing process, get up and take a break. When you're ready (20 minutes or two days later), go back to critique what you've written. Some of your ideas will be off the point, others will be pretty goofy, but hopefully you will have generated a few cool ideas that you otherwise wouldn't have had. These can be shared with other people, if you're so inclined. Of course, this is just the beginning, since your ideas will need to get filled out.

If you think this technique sounds good but you can't stand dealing with the handwritten output, you can do a modified version on your computer. Download a free version of OmmWriter, a program that clears your screen of everything but a vague, snowy landscape. It allows you to type but not to format, so you can't distract yourself by italicizing Latin names, or creating headings or subheadings. Just type whatever comes into your head without judging it yet. We recommend that you don't even look at the words you're typing, to avoid triggering your judgments or correcting your spelling.

We like to do another version of this technique that is a real crowd-pleaser with graduate students despite (or because of) its unconventionality. In this version, you draw out your ideas instead of writing them. Get out a large sheet of paper and your colored markers again. Write your question at the top of the paper and do a relaxation. This time, *illustrate* your responses to the question. Draw first; only use words as a last resort. Don't worry about making the pictures look real—a stick figure of the dingoes you study is fine. The key to this technique is to push yourself to draw more than you expect to. It works best if you do it well into a stage of discomfort. Keep asking, "What else can I possibly add to this?" Again, don't overthink this stage—just let the ideas keep coming out.

It is important to push criticism aside when you are generating ideas, but you'll need to come back to your writings and drawings if you want those new ideas to pay off. The next stage is to refine those ideas into a meaningful research plan.

Much of our academic training involves memorizing and critiquing the ideas of other people. But science doesn't move forward unless we generate new ideas. We are not born with creative and original thinking skills—we cultivate them. For example, we have found that we can come up with new and richer ideas by using an exercise in iterative writing that we describe in box 3. The technique encourages you to take low-stakes risks.

Another essential way to generate creative ideas is to allow your organisms to redirect 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!