CHAPTER 1

Picking a Question

Perhaps the most critical step in doing field biology is picking a question. Tragically, as a young ecologist, 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 all want to see a strong record of research and publication even if you will not 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

Box 1. *The importance of research for people who aspire to nonresearch 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 the scientific method into your own thinking, which allows you to analyze reports and articles critically and to teach the information to others more effectively.
- Writing a thesis 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 truly immersing yourself in your research. And besides, it's fun.

communicating effectively. (They may also want to see other qualifications and experiences, such as teaching, etc.) Being a productive researcher demands a midterm plan for your research. For example, this might include solving a particular problem in conservation, such as whether a single large or several small reserves are more beneficial for amphibian diversity in your re-

gion. More conceptual mid-term goals 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.) You should push yourself to pose a question that satisfies your goals and will be of broad interest. Some long-term goals that you might want to try out include attempting to influence how we think about or practice a subdiscipline of biology or how we manage a habitat or a crop. Having these goals in mind can provide a yardstick with which to evaluate your choice of project. In other words, figure out what you care the most about before picking a project. Do you most want to save sea turtles or to find a general ecological law? A project that is exciting to someone interested in trying to save a piece of the planet may not be satisfying to someone else who is trying to change how ecologists think about larval recruitment.

Your choice of goal 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). That said, you should also recognize that if you answer a very specific

question, your results may be considered important by a very small community. Academics are more likely to get enthused about a more general question. If your question is too general (theoretical), ask yourself if it reflects 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 specific, ask whether you can generalize from your results. For example, you may choose to determine whether a specific disease causes symptoms in grapevines that are distinguishable from those caused by water stress. The answer to this question may be of considerable interest to grape growers but to no one else. Perhaps you can also ask the broader question of whether disease causes different plant responses than abiotic stress. The answer to this question will be interesting to a wider audience. Sometimes your funding may come from an applied source requiring that you answer a specific question about fisheries biology, restoration, and so on. It may not be possible to couch your question in more general terms. If so, you may be able to ask a complementary, parallel question that is more conceptual.

All projects have to be novel and original to some extent. You can't repeat work that has already been done and expect anyone to be excited about your results. We all like to hear new stories and new ideas and there is certainly a large premium placed on novelty. If you are asking the same question that has been an-

swered in other systems, 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 you are stuck thinking up a novel idea, a useful way to begin may be to repeat an experiment or a study that captured your attention and imagination. Sometimes repeating a published study as a jumping off place will allow you to get started and move in an exciting new direction.

Policy makers are much less concerned with novelty than are academics, so what we just said about novelty may not apply as much if you are funded by an agency to answer a specific policy question. This means you will be asked to balance the expectation for novelty from your academic colleagues and the demands to answer the specific question for which your funding source is giving you money. Your first priority should be to generate relevant data for the agency; however, you should keep your eyes open for alternative answers and approaches. Asking additional questions in your study system that can lead to publishable research is also well worth considering.

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. 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,

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.

Our advice differs depending upon where you fall on this continuum. 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 for a minute 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.

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 isolation; 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 conceived them.

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 often generate more excitement than bigger ones because their more modest goals can be achieved with relatively few data, much sooner. Imagine that you want to study predation rates on goose eggs. These eggs are difficult to find and highly seasonal. You could conduct a small pilot experiment with three cartons of eggs from the grocery store. Your pilot study will not provide definitive answers about goose eggs but will likely provide useful insights about how to conduct an experiment for your project. 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 in others and 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 elucidate 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, have several possibilities that you try 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. As Confucius is supposed to have said, "Choose a job you love, and 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 want 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 end up with a good question. So regardless of which 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. Alternatively you may be 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 basis or core of the hypothesis and whether previous studies have addressed these key elements. Are there novel aspects of this question that haven't been addressed yet? 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 has to be conveniently located and common enough for you to get enough replication. Ideally, it should be protected from vandalism by curious people and animals (or it

should be possible for you to minimize these risks). It should be amenable to the manipulations that you would like to subject it to and apparent enough for your observations. 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 some 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 worked out in your own backyard regardless of where you live—we can attest to this, having lived 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 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 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 use this approach, 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 that you observe. First quantify that pattern. 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 that is associated with behavioral traits? For example, are the snails in some spots active but those in other places aestivating? Is there variation among individuals? Are the snails in some microenvironments bigger than others? Are bigger snails more active? And so on. Once you have quantified these patterns, ask: (1) what mechanisms could cause the patterns that you observe? And (2) what consequences could the patterns have on individuals and on other organisms? Even if a 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, it has probably been described by many people. 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 of these. It is also less likely that its consequences have been described. Is the pattern important? Does it affect the fitness of the organisms that show it? Does it affect their population dynamics? Does it affect the behaviors of organisms that interact with it? 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 and plant fitness have only begun to be explored and are still very poorly known (Marquis and Whelan 1995). 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 (Yang 2004).

Sometimes ecologists are constrained by funding sources or by labs that work on one set of organisms. If so, all of the obvious 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. 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 3).

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. For example, think about the ecological mechanisms that could generate the pattern that you observe. Again, understanding the mechanisms will help you to predict when and where to expect the phenomenon. Think also about the potential ecological consequences of your phenomenon. What other organisms or processes could it 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 necessarily expect to address them all.

No matter how you first get started doing field biology, 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!