- Is there a common belief that you suspect is simplistic or just wrong? A common practice that you find pointless or irritating? Do research to make a case against it.
- What courses will you take in the future? What research would help you prepare for them?

FOR TOPICS FOCUSED ON A PARTICULAR FIELD
If you have experience in your field, review 3.1.2–3.

- Browse through a textbook of a course that is one level beyond yours or a course that you know you will have to take. Look especially hard at the study questions.
- Attend a lecture for an advanced class in your field, and listen for something you disagree with, don't understand, or want to know more about.
- Ask your instructor about the most contested issues in your field.
- Find an Internet discussion list in your field. Browse its archives, looking for matters of controversy or uncertainty.
- Surf the websites of departments at major universities, including class sites. Also check websites of museums, national associations, and government agencies, if they seem relevant.

4 From Questions to a Problem

In this chapter, we explain how to turn a question into a problem that readers think is worth solving. If you are an advanced researcher, you know how essential this step is. If you are new to research, we hope to convince you of its importance, because what you learn here will be essential to all your future projects.

In the last chapter, we suggested that you can identify the significance of your research question by fleshing out this three-step formula:

1. **Topic:** I am studying ________
2. **Question:** because I want to find out what/why/how ________,
3. **Significance:** in order to help my reader understand ________.

These steps describe not only the development of your project but your own development as a researcher.

- When you move from step 1 to 2, you are no longer a mere data collector but a researcher interested in understanding something better.
- When you then move from step 2 to 3, you focus on why that understanding is *significant*.

That significance might at first be just for yourself, but you join a community of researchers when you can state that significance *from your readers' point of view*. In so doing, you create a stronger relationship with readers because you promise something in return for their interest in your report—a deeper understanding of something that matters to *them*. At that point, you have posed a *problem* that they recognize needs a solution.

4.1 UNDERSTANDING RESEARCH PROBLEMS

Too many researchers at all levels write as if their task is to answer a question that interests themselves alone. That's wrong:

to make your research matter, you must address a problem that others in your community—your readers—also want to solve. To understand why, you have to understand what research problems look like. And to do that, you have to understand two other kinds of problems, what we'll call practical problems and conceptual problems.

4.1.1 Practical Problems: What Should We Do?

Everyday research usually begins not with dreaming up a topic to think about but with a practical problem that if you ignore it means trouble. When its solution is not obvious, you have to find out how to solve it. To do that, you must pose and solve a problem of another kind, a *research* problem defined by what you *do not know or understand* about your practical problem.

It's a familiar task that typically looks like this:

PRACTICAL PROBLEM: The chain on my bicycle broke.
RESEARCH PROBLEM: Can I find a bike shop that will replace it?
RESEARCH SOLUTION: Here it is: Cycle Source, 1401 East 55th Street.
PRACTICAL SOLUTION: Walk over to get my bike fixed.

Problems like that are in essence no different from more complicated ones.

- The National Rifle Association is lobbying me to oppose gun control. *How many votes do I lose if I refuse?* Do a survey. *Most of my constituents support gun control.* I can reject the request.
- Costs are up at the Omaha plant. *What changed?* Hire a consulting firm to figure it out. *Increase in turnover.* If we improve training and morale, our workers will stick with us.

Put in general terms, a *practical* problem is caused by some condition in the world (from spam to losing money in Omaha to terrorism) that troubles us because it costs us time, money, respect, security, opportunity, even our lives. We solve a practical problem by *doing* something (or by encouraging others to do something) to

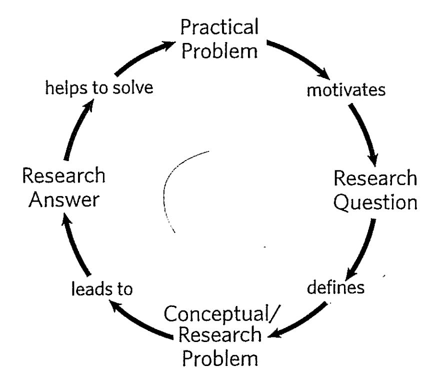
eliminate or at least mitigate the condition creating these tangible costs.

But to know what to do, someone first has to *understand* something better. That politician being lobbied by the NRA, for example, needs to know how his constituents feel about gun control so he can decide where he stands; the managers of the Omaha plant need to know the cause of their increasing costs so they can address it.

4.1.2 Conceptual Problems: What Should We Think?

That need for knowledge or understanding raises a conceptual problem. In research, a *conceptual* problem arises when we do not understand something about the world as well as we would like. We solve a conceptual problem not by doing something to change the world but by answering a question that helps us understand it better.

We usually answer these questions through research, which is why conceptual problems are also called research problems: the word *conceptual* describes their condition and costs or consequences; the word *research* refers to how we solve them. Graphically, the relationship between practical and conceptual or research problems looks like this:



The term *problem* thus has a special meaning in the world of research, one that sometimes confuses beginners. In our everyday world, a problem is something we try to avoid. But in academic research, a problem is something we seek out, even invent if we have to. Indeed, a researcher without a good conceptual or research problem to work on faces a bad practical problem because without one a researcher is out of work.

Inexperienced researchers sometimes struggle with these notions because experienced researchers often talk about their work in shorthand. When asked what they are working on, they often answer with what sounds like one of those general topics we warned you about: *adult measles, mating calls of Wyoming elk, zeppelins in the 1930s.* As a result, beginners sometimes think that having a topic to read about is the same as having a problem to solve.

When they do, they create a big practical problem for themselves, because with only a topic to guide their work, they gather data aimlessly and endlessly. Without a specific question to answer, they have no way of knowing when they have enough. When they write, they struggle to decide what to include, usually throwing in everything just to be on the safe side. So it's not surprising that they feel frustrated when a reader says, *I don't see the point here; this is just a data dump.*

To avoid that judgment, you need a problem that focuses you on finding just those data that will help you solve it. It might take a while to figure out what that problem is, but from the outset you have to think about it. That begins with understanding how conceptual problems work.

4.2 UNDERSTANDING THE COMMON STRUCTURE OF PROBLEMS

Practical problems and conceptual problems have the same two-part structure:

- a situation or *condition*, and
- undesirable *consequences* caused by that condition, *costs* that you (or, better, your readers) don't want to pay

What distinguishes them is the nature of those conditions and costs.

4.2.1 The Nature of Practical Problems

Consider a flat tire. Ordinarily, it would be a practical problem, because it is (1) a condition in the world (the flat) that imposes (2) a tangible cost that you don't want to pay, like missing a dinner date. But suppose you were bullied into the date and would rather be anywhere else. In that case, the benefit of the flat is more than its cost, so the flat is not a problem but a solution to the bigger problem of an evening spent with someone you don't like. Low cost, big benefit, no problem.

On the other hand, suppose the police set up a sting in which they lure criminals out of hiding by announcing that they have won the lottery. Ordinarily, winning the lottery is not a problem, but here it is, because it has a tangible cost: arrest.

A practical problem has two parts: a condition, which can be anything that imposes intolerable costs, and those costs. To state a practical problem so that others understand it clearly, you must describe both of its parts.

1. Its **condition**:

   I missed the bus.

   The ozone layer is thinning

2. The **costs** of that condition that you (or your reader) don't like:

   I'll be late for work and lose my job.

   Many will die from skin cancer.

But a caution: When you write, readers judge the significance of your problem not by the cost *you* pay, but by the cost *they* pay if you don't solve it. So what *you* think is a problem they might not. To make your problem their problem, you must frame it from *their* point of view, so that they see its costs to *them*. To do that, imagine that when you pose the condition part of your problem, your reader responds, *So what?*

The ozone layer is thinning.

So what?

You answer with the cost of the problem:

> A thinner ozone layer exposes us to more ultraviolet light.

Suppose he again asks, *So what?*, and you respond with the cost of more ultraviolet light:

> Too much ultraviolet light can cause skin cancer.

If, however improbably, he again asks, *So what?*, you have failed to convince him that *he* has a problem. We acknowledge a problem only when we stop asking *So what?* and say, instead, *What do we do about it?*

Practical problems like cancer are easy to grasp because they are concrete: when someone has cancer, we don't ask, *So what?* In academic research, however, your problems will usually be conceptual ones, which are harder to grasp because both their conditions and costs are abstract.

4.2.2 The Nature of Conceptual Problems

Practical and conceptual problems have the same two-part structure, but they have different kinds of conditions and costs.

- The condition of a practical problem can be *any* state of affairs that has a tangible cost for you or, better, for your readers.
- The condition of a conceptual problem, however, is *always* some version of not knowing or not understanding something.

You can identify the condition of a conceptual problem by completing that three-step sentence (see 3.4): The first step is *I am studying/working on the topic of* ________. In the second step, the indirect question states the condition of a conceptual problem, what you do not know or understand:

> I am studying stories of the Alamo, because I want to understand **why voters responded to them in ways that served the interests of Texas politicians.**

That's why we emphasize the value of questions: they force you to state what you don't know or understand but want to.

The two kinds of problems also have two different kinds of costs.

- The **cost** of a practical problem is always some tangible thing or situation we don't like.

A conceptual problem does not have such a tangible cost. In fact, we'll emphasize this difference by calling the cost of a conceptual problem its *consequence.*

- The **consequence** of a conceptual problem is a particular kind of ignorance: it is a lack of understanding that keeps us from understanding something else even more significant. Put another way, because we haven't answered one question, we can't answer another that is more important.

Researchers often choose projects simply because they are curious. In fact, that's how most of us first become interested in the subjects we study. But to make your research matter to others, you have to say more than *Here is something I find interesting.* You have to show them how solving your problem helps them solve theirs. You do that by explaining your problem's consequence.

You express a problem's consequence in the indirect question in step 3 of our formula:

> I am studying stories of the Alamo, because I want to understand why voters responded to them in ways that served the interests of local Texas politicians, in order to help readers understand the bigger and more important question **of how regional self-images influence national politics.**

All of this may sound confusing, but it's simpler than it seems. The condition and the consequence of a conceptual problem are questions that relate to each other in two ways:

- The answer to the first question (Q1) helps you answer the second (Q2).
- The answer to the second question (Q2) is more important than the answer to the first (Q1).

Q1 *helps you answer* **Q2**

Here it is again: The first part of a conceptual or research problem is something you don't know but want to. You can phrase that gap in knowledge or understanding as a direct question: *How have romantic movies changed in the last fifty years?* Or as an indirect question: *I want to find out how romantic movies have changed in the last fifty years.*

Now imagine someone asking, *So what if you can't answer that question?* You answer by stating *something else more important* that you can't know until you answer the first question. For example:

> If we can't answer the question of how romantic movies have changed in the last fifty years,*condition/first question* **then we can't answer a more important question: How have our cultural depictions of romantic love changed?***consequence/larger, more important second question*

If you think that it's important to answer that second question, you've stated a consequence that makes your problem worth pursuing, and if your readers agree, you're in business.

But what if you imagine a reader again asking, *So what if I don't know whether we depict romantic love differently than we did?* You have to pose a yet larger question that you hope your readers will think is significant:

> If we can't answer the question of how our depictions of romantic love have changed,*second question* **then we can't answer an even more important one: How does our culture shape the expectations of young men and women about marriage and families?***consequence/larger, more important question*

If you imagine that reader again asking, *So what?*, you might think, *Wrong audience.* But if that's the audience you're stuck with, you just have to try again: *Well, if we don't answer that question, we can't . . .*

Those outside an academic field often think that its specialists ask ridiculously trivial questions: *How did hopscotch originate?*

But they fail to realize that researchers want to answer a question like that so that they can answer a second, more important one. For those who care about the way folk games influence the social development of children, the conceptual consequences of not knowing justifies the research. *If we can discover how children's folk games originate, we can better understand how games socialize children, and, before you ask, once we know that, we can better understand . . .*

4.2.3 Distinguishing "Pure" and "Applied" Research

We call research *pure* when it addresses a conceptual problem that does not bear directly on any practical situation in the world, when it only improves the understanding of a community of researchers. We call research *applied* when it addresses a conceptual problem that does have practical consequences. You can tell whether research is pure or applied by looking at the last of the three steps defining your project. Does it refer to knowing or doing?

1. **Topic:** I am studying the electromagnetic radiation in a section of the universe
   2. **Question:** because I want to find out how many galaxies are in the sky,
      3. **Significance:** in order to help readers *understand* whether the universe will expand forever or eventually collapse into a point.

That is pure research, because step 3 refers only to understanding.

In applied research, the second step still refers to *knowing* or *understanding*, but that third step refers to *doing*:

1. **Topic:** I am studying how readings from the Hubble telescope differ from readings for the same stars measured by earthbound telescopes
   2. **Question:** because I want to find out how much the atmosphere distorts measurements of electromagnetic radiation,
      3. **Practical Significance:** so that astronomers can use data from earthbound telescopes *to measure* more accurately the density of electromagnetic radiation.

That problem calls for applied research because only when astronomers *know* how to account for atmospheric distortion can they *do* what they want to—measure light more accurately.

4.2.4 Connecting Research to Practical Consequences

Some inexperienced researchers are uneasy with pure research because the consequence of a conceptual problem—merely not knowing something—is so abstract. Since they are not yet part of a community that cares deeply about understanding its part of the world, they feel that their findings aren't good for much. So they try to cobble a practical cost onto a conceptual question to make it seem more significant:

1. **Topic:** I am studying differences among nineteenth-century versions of the Alamo story
   2. **Research Question:** because I want to find out how politicians used stories of such events to shape public opinion,
      3. **Potential Practical Significance:** in order to protect ourselves from unscrupulous politicians.

Most readers would think that the link between steps 2 and 3 is a bit of a stretch.

To formulate a good applied research project, you have to show that the answer to the indirect question in step 2 *plausibly* helps answer the indirect question in step 3. Ask this question:

(a) If my readers want to achieve the goal of ________ [state your objective from step 3],
(b) would they think that they could do it if they found out ________?
[state your question from step 2]

Try that test on this applied astronomy problem:

(a) If my readers want to use data from earthbound telescopes to measure more accurately the density of electromagnetic radiation,
(b) would they think that they could if they knew how much the atmosphere distorts measurements?

The answer would seem to be *Yes.*

Now try the test on the Alamo problem:

(a) If my readers want to protect themselves from unscrupulous politicians,
(b) would they think they could if they knew how nineteenth-century politicians used stories about the Alamo to shape public opinion?

The answer would probably be *No.* We may see a connection, but it's a stretch.

If you think that the solution to your conceptual problem *might* apply to a practical one, formulate your project as pure research, then *add* your application as a *fourth* step:

1. **Topic:** I am studying how nineteenth-century versions of the Alamo story differ
   2. **Conceptual Question:** because I want to find out how politicians used stories of great events to shape public opinion,
      3. **Conceptual Significance:** in order to help readers understand how politicians use popular culture to advance their political goals,
         4. **Potential Practical Application:** so that readers *might* better protect themselves from unscrupulous politicians.

When you state your problem in your introduction, however, present it as a purely conceptual research problem whose significance is in its conceptual consequences. Then wait until your conclusion to suggest its practical application. (For more on this, see chapter 16.)

Most research projects in the humanities and many in the natural and social sciences have no direct application to daily life. But as the term *pure* suggests, many researchers value such research more than they do applied research. They believe that the pursuit of knowledge "for its own sake" reflects humanity's highest calling: to know more, not for the sake of money or power, but for the transcendental good of greater understanding and a richer life of the mind.

As you may have guessed, we are deeply committed to pure research, but also to applied—so long as the research is done well and is not corrupted by malign motives. For example, the potential for profit might compromise the integrity of both pure and applied research in the biological sciences, because it can influence not only what problems some researchers choose to address but also their solutions: *Tell us what to look for, and we'll provide it!* Such situations raise ethical questions that we touch on in our afterword, "The Ethics of Research."

4.3 FINDING A GOOD RESEARCH PROBLEM

What distinguishes great researchers from the rest of us is the brilliance, knack, or just dumb luck of stumbling over a problem whose solution makes all of us see the world in a new way. It's easy to recognize a good problem when we bump into it, or it bumps into us. But researchers often begin a project without being clear about what their real problem is. Sometimes they hope just to define a puzzle more clearly. Indeed, those who find a new problem or clarify an old one often make a bigger contribution to their field than those who solve a problem already defined. Some researchers have even won fame for *disproving* a plausible hypothesis that they had set out to prove.

So don't be discouraged if you can't formulate your problem fully at the outset of your project. Few of us can. But thinking about it early will save you hours of work along the way (and perhaps panic toward the end). It also gets you into a frame of mind crucial to advanced work. Here are some things you can do to identify and refine a good problem.

4.3.1 Ask for Help

Do what experienced researchers do: talk to colleagues, teachers, classmates, relatives, friends, neighbors—anyone who might be interested. Why would anyone want an answer to your question? What would they do with it? What new questions might an answer raise?

If you are free to work on any problem, look for a small one that is part of a bigger one. Though you won't solve the big one, your small piece of it will inherit some of its larger significance.

(You will also educate yourself about the problems of your field, no small benefit.) If you are a student, ask your teacher what she is working on and whether you can work on part of it. Don't let her suggestions define the limits of your research. Nothing discourages a teacher more than a student who does *exactly* what is suggested *and no more.* Teachers want you to use their suggestions to *start* your thinking, not *end* it. Nothing makes a teacher happier than when you use her suggestions to find something she never expected.

4.3.2 Look for Problems as You Read

You can also find research problems in your sources. Where in them do you see contradictions, inconsistencies, incomplete explanations? Tentatively assume that other readers would or should feel the same. Many research projects begin with an imaginary conversation with the author of a source: *Wait a minute, he's ignoring...* But before you set out to correct a gap or misunderstanding, be sure it's real, not just your own misreading. Countless research papers have refuted a point that no one ever made. Before you correct a source, reread it carefully. (In 6.3 we list several common "moves" that writers make to find a problem in a source, variations on *Source thinks X, but I think Y.*)

Once you think you've found a real puzzle or error, do more than just point to it. If a source says X and you think Y, you may have a research problem, but only if you can show that those who think X misunderstand some larger issue as well.

Finally, read the last few pages of your sources closely. That's where many researchers suggest more questions that need answers. The author of the following paragraph had just finished explaining how the life of nineteenth-century Russian peasants influenced their performance as soldiers:

> And just as the soldier's peacetime experience influenced his battlefield performance, so must the experience of the officer corps have influenced theirs. Indeed, a few commentators after the Russo-Japanese War blamed the Russian defeat on habits acquired by officers in the

> course of their economic chores. In any event, to appreciate the service habits of Tsarist officers in peace and war, *we need a structural—if you will, an anthropological—analysis of the officer corps like that offered here for enlisted personnel.* [our emphasis]

That last sentence offers a new problem waiting for you to tackle.

4.3.3 Look at Your Own Conclusion

Critical reading can also help you discover a good research problem in your own drafts. We often do our best thinking in the last few pages that we write, because there we formulate claims we did not anticipate when we started. If in an early draft you arrive at an unanticipated claim, ask yourself what question it might answer. Paradoxical as it might seem, you may have answered a question that you have not yet asked, and thereby solved a problem that you have not yet posed. Your task is to figure out what that problem might be.

4.4 LEARNING TO WORK WITH PROBLEMS

Experienced researchers dream of finding new problems to solve. A still bigger dream is to solve a problem that no one even knew they had. But that new problem isn't worth much until others think (or can be persuaded) that it needs solving. So the first question an experienced researcher should ask about a problem is not *Can I solve it?* but *Will readers think it should be solved?*

No one expects you to do all that the first time out. But you should begin to develop mental habits that will prepare you for that moment. Research is more than just accumulating and reporting facts. Try to formulate a question that *you* think is worth answering, so that down the road, you'll know how to find a problem that *others* think is worth solving. Until you can do that, you risk the worst response a researcher can get: not *I don't agree*, but *I don't care.*

By now, all this talk about airy academic research may seem disconnected from what some call the "real world." But in business and government, in law and medicine, in politics and international

diplomacy, no skill is valued more highly than the ability to recognize a problem, then to articulate it in a way that convinces others both to care about it and to believe it can be solved, especially by you. If you can do that in a class on Byzantine pottery, you can do it in an office on Main Street, Wall Street, or Queen's Road in Hong Kong.

QUICK TIP **Manage the Unavoidable Problem of Inexperience**

We all feel anxious when we start work in a new field whose values, concerns, and ways of thinking and arguing we don't entirely understand. In fact, we authors still experience that newcomer's anxiety again when we begin new kinds of projects on new topics. You can't avoid experiencing that feeling at times, but there are ways to manage it:

- *Know that uncertainty and anxiety are natural and inevitable.* Those feelings don't signal incompetence, only inexperience.
- *Get control over your topic by writing about it along the way.* Don't just retype or photocopy sources: write summaries, critiques, questions, responses to your sources. Keep a journal in which you reflect on your progress. This kind of writing not only helps you understand what you read but stimulates your thinking about it. The more you write early on, no matter how sketchily, the easier it will be to face that intimidating first draft.
- *Break the task into manageable steps and know that they are mutually supportive.* Once you formulate a good question, you'll draft and revise more effectively. The more you anticipate how you will write and revise a first draft, the more effectively you will produce it.
- *If you are a student, count on your teachers to understand your struggles.* They want you to succeed, and you can expect their help. (If they don't help, look for others who will.)
- *Set realistic goals.* You do something significant when you wind up your project feeling that it has changed just what *you* think and that your readers think you did it well, even if they don't agree with your claims.
- *Most important, recognize the struggle for what it is—a learning experience.* To overcome the problems that all beginners face, do what successful researchers do, especially when discouraged: review your plan and what you've written, then press on, confident that it will turn out OK. Perhaps only "OK—considering," but probably a lot better than that.

5 From Problems to Sources

If you are a new researcher and expect to find most of your sources in your library or on the Internet, this chapter will help you develop a plan for your research. If you are more experienced, you might skip to the next chapter.

If you have not yet formulated a research question, you may have to spend time reading generally on your topic to find one. But if you have a question and at least one promising answer (the philosopher C. S. Peirce called it a *hypothesis on probation*), you can start looking for data to test it.

To do that efficiently, you need to have a plan. If you plunge into any and all sources on your topic, you risk losing yourself in an endless trail of books and articles. To be sure, aimless browsing can be fun, even productive. We indulge in it a lot. Many important discoveries have begun in a chance encounter with an unexpected idea. But if you have a deadline, you need more than luck to find good sources in time: you have to search systematically for those sources that will help you advance your research project or, just as usefully, challenge you to improve it. In this chapter, we discuss different ways you can use sources in your research, how you can find useful sources, and how you can winnow your sources to a manageable number. In the next chapter, we focus on how to use sources in your writing.

5.1 THREE KINDS OF SOURCES AND THEIR USES

Sources are conventionally categorized into three kinds: primary, secondary, and tertiary. Their boundaries are fuzzy, but knowing these categories can help you plan your research.