# *The Craft of Research*

**SECOND EDITION**

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CHAPTER THREE

# From Topics to Questions

***In this chapter we discuss how to explore your interests to find a topic, narrow it to a manageable scope, question it to find the makings of a problem, then turn it into a problem that guides your research. If you are an experienced researcher or already know what topics you want to pursue and why, you might skip to chapter 4. But if you are starting your first project, you will find this chapter useful.***

If you are free to research any topic that interests you, that freedom can be frustrating—so many choices, so little time. At some point, you have to settle on a topic, but beyond a topic, you also have to find a reason beyond your assignment to devote weeks or months pursuing it and writing up what you find, then to ask readers to spend their time reading your report.

As we've said, your readers expect you to do more than just mound up and report data; they expect you to report it in a way that continues the ongoing conversation between writers and readers that creates a *community* of researchers. To do that, you must select from all the data you find just those data that support an answer to a question that solves a problem your readers think needs solving. In all research communities, some problems are already "in the air," widely debated and deeply researched, such as whether personality traits like shyness or an attraction to risk are genetically inherited or learned. But other questions may intrigue only the researcher: *Why do cats rub their faces against us? Why do the big nuts end up at the top of the can?* That's how a lot of research begins—not with a "big" question known to everyone in a field, but with a mental itch that only one researcher feels the need to scratch.

If you have such an itch, good. But as we've said (and will say again), at some point, you have to decide whether the answer to your private question is also significant to others: to a teacher, colleagues, other researchers, or even to a public whose lives your research could change. At that point, you aim not just to answer a question, but to pose and solve a *problem* that others also think is worth solving.

Now that word *problem* is itself a problem: commonly, a problem means trouble, but among researchers it has a meaning so special that we devote all of the next chapter to it. It raises issues that few beginning researchers are able to resolve entirely and that can vex even advanced ones. But before you can address a research problem, you have to find a topic that might lead to one. We'll start there, with finding a topic.

## 3.1 FROM AN INTEREST TO A TOPIC

Most of us have more than enough interests to pursue, but beginners often find it hard to locate among theirs a topic focused enough to support a research project. A research topic is an interest defined narrowly enough for you to imagine becoming a local expert on it. That doesn't mean that you already know a lot about it or that you will have to learn more about it than your professor has. You just want to know more than you do now.

If your assignment leaves you free to explore any topic within reason, we can offer only a cliché: Start with what interests you most deeply. Nothing contributes to the quality of your work more than your commitment to it. Start by listing two or three interests that you'd like to explore. If you are undertaking a research project in a course in a specific field, skim a recent textbook, talk to other students, or consult your teacher. You might try to identify an interest based on work you are doing or will do in a different course.

If you are still stuck, you can find help either on the Internet or in your library. The Internet may seem the easier way, but it's more likely to lead you astray, especially if you are new to research. Start with the standard guides:

- For a project in a general writing course, start in the library. Look at the headings in a general bibliography such as the *Reader's Guide to Periodical Literature.* If you already have a general focus, use more specialized guides such as the *American Humanities Index* or the *Chicano Index.* (We discuss using these resources in chapter 5 and list many of them on pp. 298–315.)

Scan headings for topics that catch your interest. They will provide not only possible topics, but up-to-date references on them. If you already have an idea for a topic, you can check out the Internet, but if you have no idea what you are looking for, what you find there may overwhelm you. Some indexes are available online, but most don't let you skim only subject headings.

- For a first research project in a particular field, skim headings in specialized indexes, such as the *Philosopher's Index,* the *Psychological Abstracts,* or *Women's Studies Abstracts.*

Once you identify a general area of interest, use the Internet to find out more about it and to help you narrow your topic. (If you are really stuck, see the Quick Tip at the end of this chapter.)

- If you are doing an advanced research project, you might look first for what resources are easily available *before* you settle on a topic.

If you pick a topic and then discover that sources are hard to find, you may have to start over. If you *first* identify resources available in your library or on the Internet, you can plan your research more efficiently, because you will know where to start.

At first, you may not know enough about a general interest like *the use of masks in religious and social contexts* to turn it into a focused topic. If so, you have to do some reading to know what to think about it. Don't read randomly: start with entries in a general encyclopedia, then look at entries in a specialized encyclopedia or dictionary, then browse through journals and websites until you have a grip on the general shape of your topic. Only then will you be able to move on to these next steps.

## 3.2 FROM A BROAD TOPIC TO A FOCUSED ONE

At this point, you risk settling on a topic so broad that it could be a subheading in an encyclopedia: *Space flight, history of; Shakespeare, problem plays; Natural kinds, doctrine of.* A topic is usually too broad if you can state it in four or five words:

| | |
|---|---|
| Free will in *War and Peace* | The history of commercial aviation |

With a topic so broad, you may be intimidated by the idea of finding, much less reading, even a fraction of the sources available. So you have to narrow it, like this:

| | | |
|---|---|---|
| Free will in *War and Peace* | → | The conflict of free will and historical inevitability in Tolstoy's description of three battles in *War and Peace* |
| The history of commercial aviation | → | The crucial contribution of the military in the development of the DC-3 in the early years of commercial aviation |

We narrowed those topics by adding words and phrases, but of a special kind: *conflict, description, contribution,* and *development.* Those nouns are derived from verbs expressing actions or relationships: *to conflict, to describe, to contribute,* and *to develop.* Without such words, your topic is a static thing—*free will in War and Peace, the history of commercial aviation.* But when you use nouns derived from verbs, you move your topic a step closer to a *claim* that your readers might find significant.

Note what happens when these topics become statements. Topics (1a) and (2a) change almost not at all:

| TOPIC | | CLAIM |
|---|---|---|
| 1a. Free will and historical inevitability in Tolstoy's *War and Peace*? | → | There is free will and historical inevitability in Tolstoy's *War and Peace*. |
| 2a. The history of commercial aviation | → | Commercial aviation has a history. |

Topics (1b) and (2b), on the other hand, are closer to claims that a reader might find interesting:

| | | |
|---|---|---|
| 1b. The *conflict* of free will and historical inevitability in Tolstoy's *description* of three battles in *War and Peace* | → | *In War and Peace*, Tolstoy *describes* three battles in a way that makes free will *conflict* with historical inevitability. |
| 2b. The *crucial contribution* of the military in the *development* of the DC-3 in the early years of commercial aviation | → | In the early years of commercial aviation, the military *crucially contributed* to the way the DC-3 *developed*. |

Such claims will at first seem weak, but you will develop them into more specific ones as you develop your project.

A more specific topic also helps you see gaps, puzzles, and inconsistencies that you can ask about when you turn your *topic* into a research *question* (more about that in a moment). A specific topic can also serve as your working title, a short answer when someone asks you what you are working on.

Caution: Don't narrow your topic so much that you can't find enough data on it:

| TOO MANY DATA AVAILABLE | TOO FEW DATA AVAILABLE |
|---|---|
| The history of commercial aviation | The decision to lengthen the wingtips on the DC-3 prototype because the military wanted to use the DC-3 as a cargo carrier |

## 3.3 FROM A FOCUSED TOPIC TO QUESTIONS

In taking this next step, researchers often make a beginner's mistake: they rush from a topic to a data dump. Once they hit on a topic that feels promising, something like *the political origins and uses of legends about the Battle of the Alamo,* they go straight to searching out sources—different versions of the story in books and films, Mexican and American, nineteenth century and twentieth. They accumulate a mound of summaries of the stories, descriptions of their differences and similarities, ways in which they conflict with what modern historians think happened. They write all that up and conclude, "Thus we see many interesting differences and similarities between . . ."

Most high school teachers would give such a report a passing grade, because it shows that the student can focus on a topic, find data on it, and assemble those data into a report—no small achievement for a first project. But in any advanced course, including a first-year writing course in college, such a report falls short because it offers only random bits of information. If the writer asks no *question* worth pondering, he can offer no focused answer worth reading. Readers of research reports don't want just information; they want the answer to a question worth asking. To be sure, those fascinated by a topic often feel that *any* information about it is worth reading for its own sake: collectors of Japanese coins or Elvis Presley movie posters will read anything about them. Serious researchers, however, do not report data for their own sake, but to support the answer to a question that they (and they hope their readers) think is worth asking.

The best way to find out what you do not know about a topic is to barrage it with questions. First ask the predictable ones of your field. For example, a historian's first questions about the Alamo stories would concern their sources, development, and accuracy. Also ask the standard journalistic questions *who, what, when,* and *where,* but focus on *how* and *why*. Finally, you can systematically ask four kinds of analytical questions, about the composition, history, categorization, and values of your topic. Record the questions, but don't stop for answers. (And don't worry about fitting the questions into the right categories; use the categories only to stimulate you to ask them and to organize their answers.)

### 3.3.1 Identify the Parts and How They Interrelate

- What are the parts of your topic, and how do they relate to one another?

  In stories about the Alamo, what are the themes, the plot structure, the main characters? How do the characters relate to the plot, the plot to the actual battle, the battle to the characters, the characters to one another?

- How is your topic part of a larger system?

  How have politicians used the story? What role does it have in Mexican history? What role does it have in U.S. history? Who told the stories? Who listened? How does their nationality affect the story?

### 3.3.2 Trace Its Own History and Its Role in a Larger History

- How and why has your topic changed through time, as something with its own history?

  How have the stories developed? How have different stories developed differently? How have audiences changed? How have the storytellers changed? How have their motives to tell the stories changed?

- How and why is your topic an episode in a larger history?

  How do the stories fit into a historical sequence of events? What caused them to change? How did they affect national identity in the United States? In Mexico? Why have they endured so long?

### 3.3.3 Identify Its Characteristics and the Categories that Include It

- What kind of thing is your topic? What is its range of variation? How are instances of it similar to and different from one another?

  What is the most typical story? How do others differ? Which is most different? How do the written and oral stories differ from the movie versions? How are Mexican stories different from those told in the States?

- To what larger categories can your topic be assigned? How does that help us understand it?

  What other stories in U.S. history are like the story of the Battle of the Alamo? In Mexican history? How do the stories compare to other mythic battle stories? What other societies produce similar stories?

### 3.3.4 Determine Its Value

- What values does your topic reflect? What values does it support? Contradict?

  What moral lesson does the story teach, if any? Whose purposes does each story serve? Who is praised? Who blamed? Why?

- How good or bad is your topic? Is it useful?

  Are some stories better than others? More sophisticated than others? What version is the best one? The worst one? Which parts are most accurate? Which least?

### 3.3.5 Evaluate Your Questions

When you run out of questions (or think, *Enough!*), it's time to evaluate them. First, set aside questions whose answers you could look up in a reference work. Questions that ask *who, what, when,* or *where* are important, but they may ask only about matters of settled fact (though not always). Questions that ask *how* and *why* are more likely to invite deeper research and lead to more interesting answers.

Next, try to combine smaller questions into larger, more significant ones. For example, several Alamo questions revolve around the issue of the interests of the storytellers and their effects on the stories:

> How have politicians used the story? What role does it have in U.S. history? How have the storytellers changed? How have their motives to tell the stories changed? How did the stories affect national identity in the United States? How do the stories compare to other mythic battle stories? Is its moral lesson worth teaching? Whose purposes does each story serve?

Many of these can be combined into a larger, more significant question:

> How and why have tellers of the Alamo story given a mythic quality to the event?

Once you settle on a question or two, you have a guide to doing your research more systematically. A question narrows your search to only those data you need for its answer. And once you have an answer you think you can support, you know it's time to stop hunting. But when you have only a topic, the data you can find on it are, literally, endless; worse, you will never know when you have enough.

Through all this, though, the most important goal is to find questions that challenge you or, better, arouse your intense curiosity. Of course, you can't be sure where any particular question will lead, but this kind of questioning can send you in directions you never imagined, opening you up to new interests, new worlds of research. Finding good questions is an essential step in any project that goes beyond fact-grubbing. With one or two in mind, you are ready for the next steps.

## 3.4 FROM A MERELY INTERESTING QUESTION TO ITS WIDER SIGNIFICANCE

Even if you are an experienced researcher, you might not be able to take this next step until you are well into your project. If you are a beginner, you may feel that this step is still deeply frustrating even when you've finished it. Nevertheless, once you have a question that grabs your interest, you must pose a tougher question: *Why should this question also grab my readers? What makes it worth asking?*

Start by asking, *So what?* At first, ask it for yourself:

> So what if I don't know or understand how snow geese know where to go in the winter, or how fifteenth-century violin players tuned their instruments, or why the Alamo story has become myth? So what if I can't answer those questions?

Eventually, you will have to answer this question not just for yourself but for your readers. Finding its answer vexes all researchers, beginners and experienced alike, because it's so hard to predict what will really interest readers. Instead of trying to answer instantly, though, you can work toward an answer in three steps.

### 3.4.1 Step 1: Name Your Topic

If you are just beginning a project, with only a topic and maybe the glimmerings of a few good questions, describe your topic in a sentence as specific as you can make it (glance back at pp. 43–45):

> I am trying to learn about (working on, studying) ________.

Fill in the blank with your topic. Be sure to use some of those nouns based on verbs or adjectives:

I am studying *diagnostic processes* in the *repair* of cooling systems.

I am working on Lincoln's *beliefs* about *predestination* in his early speeches.

### 3.4.2 Step 2: Add a Question

As soon as you can, add to that sentence an indirect question that specifies something that you do not know or understand about your topic but want to:

1. *I am studying X*
   2. *because I want to find out* who/what/when/where/whether/why/how ______.

1. *I am studying* diagnostic processes in the repair of cooling systems
   2. *because I am trying to find out how* expert repairers diagnose failures.

1. *I am working on* Lincoln's beliefs about predestination in his early speeches
   2. *because I want to find out how* his belief in destiny influenced his understanding of the causes of the Civil War.

When you add that *because-I-want-to-find-out-how/why* clause, you state why you are pursuing your topic: to answer a question important to you.

If you are doing one of your first research projects and you get this far, congratulate yourself, because you have framed your project in a way that moves it beyond the kind of aimless collection and reporting of data that afflicts too much research. But now go one step more, if you can.

### 3.4.3 Step 3: Motivate Your Question

This step is a hard one, but it lets you know whether your question is not just interesting to you but possibly significant to others. To do that, add another indirect question, a bigger and more general one that explains why you are asking your first question.

Introduce this second implied question with *in order to help my reader understand how, why,* or *whether:*

1. *I am studying* diagnostic processes in the repair of cooling systems
   2. *because I am trying to find out how* expert repairers analyze failures,
      3. *in order to help my reader understand how* to design a computerized system that can diagnose and prevent failures.

1. *I am working on* Lincoln's beliefs about predestination in his early speeches
   2. *because I want to find out how* his belief in destiny and God's will influenced his understanding of the causes of the Civil War,
      3. *in order to help my reader understand how* his religious beliefs may have influenced his military decisions.

It's your answer to the third step that will give you a claim on your readers' interest. If that larger question touches on issues important to your field, even indirectly, then you have reason to think that your readers should care about its answer, and so care about your answer to the smaller, prior question you raise in step 2.

A few researchers can flesh out this whole pattern even before they start gathering data, because they are working on a well-known question, some widely investigated problem that others in their field are already interested in. In fact, advanced researchers often begin their research with questions that others have asked before but not answered thoroughly, or maybe even correctly. But many researchers, including at times the three of us, find that they can't flesh out these steps until they're nearly finished. And too many write up their research results without having thought through these steps at all.

At the beginning of your project, you may not be able to get past the first step of naming your topic. But regularly test your progress by asking a roommate, relative, or friend to *force* you to question your topic and to flesh out those three steps. Even if you can't take them all confidently, you'll know where you are and where you still have to go.

To summarize: Your aim is to explain

1. what you are writing about—your topic: *I am studying . . .*

2. what you don't know about it—your question: *because I want to find out . . .*

3. why you want your reader to know about it—your rationale: *in order to help my reader understand better . . .*

If you are just beginning serious research, don't be discouraged if you never get past that second step. As long as your question is interesting to *you*, plow ahead. Your teacher should be satisfied, because you have changed the terms of your project from simply gathering data to asking and answering a question.

If you are a graduate student doing advanced research, however, you *must* take that last step, because answering that last question will help you create the relationship you are working to establish with the rest of your research community. It's your ticket into the conversation.

In the following chapters, we will return to those three steps and their implied questions, because as you'll see, they are crucial not just for finding good specific questions that you want to answer, but for finding and then expressing the problem that you want your readers to recognize and value.

## ☞ QUICK TIP: *Finding Topics*

If you have experience in your field but are stuck for a topic, you can find one with some quick research. Read recent articles and review essays and, if they are available, recent dissertations. Look closely at the conclusions: they often suggest further lines of research. You can also browse the archives of an Internet discussion list in your field: look for points of current controversy.

But if you are a beginner and your teacher has not suggested specific topics, start with our suggestions about skimming bibliographical guides (pp. 298–315). If you still draw a blank, try these steps.

### FOR GENERAL TOPICS

1. What special interest do you have—sailing, chess, finches, old comic books? The less common, the better. Investigate something about it you don't know: its origins, its technology, how it is practiced in another culture, and so on.

2. Where would you like to go? Surf the Internet, finding out all you can about it. What particular aspect surprises you or makes you want to know more?

3. Wander through a museum with exhibitions that appeal to you—artworks, dinosaurs, automobiles. If you can't get there in person, browse a "virtual museum" on the Internet. Stop when something catches your interest. What more do you want to know about it?

4. Wander through a shopping mall or store, asking yourself, *How do they make that?* or, *I wonder who thought up that product?*

5. Leaf through a Sunday newspaper, especially its features sections, until something catches your eye. Skim reviews of books or movies, in newspapers or on the Internet.

6. Browse a large magazine rack. Look for trade magazines or those that cater to specialized interests. Investigate whatever catches your interest.

7. If you can use an Internet newsreader, look through the list of "alt" newsgroups until you find one that sounds interesting. Read the posts, looking for something that surprises you or that you disagree with.

8. Tune into talk radio or interview programs on TV until you hear a claim you disagree with. Or find something to disagree with on the websites connected with well-known talk shows. See whether you can make a real case to refute it, instead of just shouting back.

9. Use an Internet search engine to find websites about something people collect. (Narrow the search to exclude dot-com sites.) You'll get hundreds of hits, but look only at the ones that surprise you.

10. Is there a common belief that you suspect is much too simplistic, or just plain wrong? Or a common practice that you detest? Don't just pronounce the belief or practice wrong, but instead probe for something you can show about it that might lead others to reconsider.

## FOR TOPICS FOCUSED ON A PARTICULAR FIELD

1. Browse through a textbook of a course that is one level beyond yours or a course that you know you will have to take some time in the future. Look especially hard at the study questions.

2. 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.

3. Ask your instructor about the most contested issue in your field.

4. Find an Internet discussion list in your field. Browse its archives, looking for matters of controversy or uncertainty.

5. Surf the websites of departments at major universities, including class websites. Also check sites of museums, national associations, and government agencies, if they seem relevant.

CHAPTER FOUR

# *From Questions to Problems*

***In this chapter we explain how to frame your project as a problem that readers want to see solved, an essential step for advanced researchers. If you are attempting your first research project, this chapter may prove difficult. (You can find more help on problems in our discussion of introductions in chapter 14.) If you feel lost, you can skip to chapter 5, but we hope that you will stay with it. You'll learn important steps you can take now, and will certainly need in the future.***

In the last chapter, we described how to find a topic in your interests, how to narrow it, then to question it. We suggested that you identify the significance of your questions 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 as a researcher.

When you move from step 1 to 2, you stop being a mere data collector, because you are now motivated not by aimless curiosity (by no means a useless impulse), but by a desire to understand something better. That second step also helps you develop an increasingly sophisticated relationship with your readers. When you move from step 2 to 3, you focus your project on the significance of that understanding, at least for yourself. But you can join a *community* of researchers only when you can see that significance from your readers' point of view. With that last step, you change your intention from merely discovering and understanding something for yourself to *showing* and *explaining* something to others, a move that makes a stronger claim on readers and so creates a stronger relationship with them.

## 4.1 PROBLEMS, PROBLEMS, PROBLEMS

That third step is hard for everyone, even experienced researchers. Too many write as if they do their job by answering a question that happens to interest them. They fail to understand that their answer must also solve a *problem* that is significant to their community of readers. But researchers often cannot start their project knowing exactly what problem they will finally solve. Many start with just a hunch, a puzzle, something they want to know more about; it's not until they are well into their research, sometimes even their drafting, that they finally figure out what problem they have solved. So don't feel uneasy if early in your project you do not yet know exactly the significance of your question. But you can begin planning your research knowing (or at least hoping) that a good one is out there somewhere.

To understand how to find that good question and then communicate its significance, though, you first have to know what a research problem really is.

### 4.1.1 Practical Problems and Research Problems

Everyday research usually begins not with dreaming up a topic but with solving a practical problem that has just landed on you, a problem that, left unresolved, means trouble. When the solution is not obvious, you ask questions whose answers you hope will help you solve it. But to answer them, you must pose and solve a problem of another kind, a *research* problem defined by what you do not know or understand, but feel you must before you can solve your practical problem.

This process of addressing practical problems is familiar. It typically looks like this:

PRACTICAL PROBLEM: My brakes have started screeching.
RESEARCH QUESTION: Where can I get them fixed right away?

RESEARCH PROBLEM: Find the Yellow Pages and look up closest brake shop.
RESEARCH ANSWER: The Car Shoppe, 1401 East 55th Street.
APPLICATION: Call to see when they can fix them.

It's a pattern common in every part of our lives:

The National Rifle Association presses me to oppose gun control. *Will I lose my election if I don't?* Take a poll. *A majority of my constituents support gun control.* Now decide whether to reject the NRA's request.

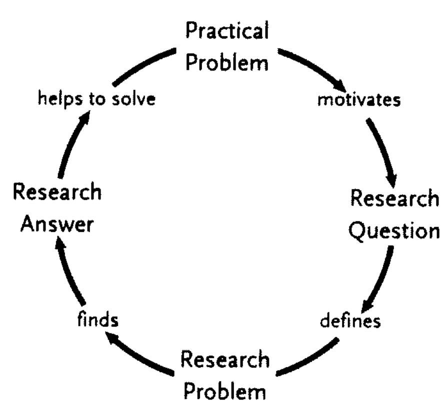
Costs are up at the Omaha plant. *What has changed?* Compare personnel before and after. *More turnover now.* If we improve training and morale, our workers stay with us. OK, let's see if we can afford to do it.

Problems like those rarely require us to write up a solution. We write only when we have to convince others that we've found and solved a problem important to *them:*

To manager of Omaha plant: Costs are up in Omaha because we have too much turnover. Employees see no future in their jobs and are quitting after a few months. To retain workers, we must upgrade their skills so they will want to stay. Here is a plan . . .

But before anyone could solve the *practical* problem of rising costs, someone had to pose and solve a *research* problem defined by not knowing why they were rising. Only then can they decide what to *do* about it.

Graphically, the relationship between practical and research problems looks like this:



### 4.1.2 Distinguishing Practical Problems and Research Problems

Though solving a practical problem usually requires that we solve a research problem as well, it is crucial to distinguish between them, because we solve and write about them in different ways.

- A *practical* problem is caused by some condition in the world, from e-mail spam to terrorism, that makes us unhappy because it costs us time, money, respect, security, pain, even our lives. You solve a practical problem by *doing* something that changes the world by eliminating the causes that lead to its costs, or by encouraging others to do so.

- A *research* problem is motivated not by palpable unhappiness, but by incomplete knowledge or flawed understanding. You solve it not by changing the world but by understanding it better.

Though a research problem is often motivated by a practical problem, you don't solve the practical problem just by solving the research one. The manager of the Omaha plant might know the answer to the research question *Why are costs rising?* but still struggle to solve the practical problem *How do we improve training?*

The term *problem* thus has a special meaning in the world of research, one that sometimes confuses beginners. In our everyday world, a practical problem is something we try to avoid. But in the academic world, a research problem is something we eagerly seek out, even inventing one, if we have to. Indeed, a researcher without a good research problem has a bad practical one, because with no research problem to work on, she has nothing to do.

There is a second reason inexperienced researchers sometimes struggle with this notion of a research problem. Experienced researchers often talk about their research problems in shorthand. When asked what they are working on, they respond with what sounds like one of those general topics we warned you about in the last chapter: *adult measles, early Aztec pots, the mating calls of Wyoming elk.*

As a result, some beginners think that having a topic to read about is the same as having a problem to solve. But when they do, they have a big practical problem, because without a research problem, they lack the focus set by the need to answer a particular question. So they gather data aimlessly and endlessly, with no way of knowing when they have enough. Then they struggle to decide what to include in their report and what not, finally throwing in everything they have, 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; this is just a data dump.* To avoid that judgment, you need a problem to focus your attention on those particular data that will help you solve your problem. That means first understanding how problems work.

## 4.2 THE COMMON STRUCTURE OF PROBLEMS

Practical problems and research problems have the same basic structure. Both have two parts:

1. a situation or *condition,* and
2. the undesirable consequences of that condition, *costs* you don't want to pay.

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

### 4.2.1 The Nature of Practical Problems

A flat tire is a typical practical problem, because it is (1) a condition in the world (the flat) that (2) exacts on you a tangible cost that you don't want to pay (not getting to work on time or missing a dinner date). But suppose you were bullied into the date and would rather be anywhere else. In that case, the flat has no significant cost; in fact, since it turns out to be a benefit, it is not a problem at all, but a solution. No cost, no problem.

For a practical, tangible problem, the condition can be literally anything, even winning the lottery. Suppose you win a million dollars but owe a loan shark two million, and your name gets in the paper. He finds you, takes your million, and breaks your leg. Winning a million turns out to be a Big Problem.

To pose a practical problem, you must be able to describe both its parts:

- its **condition**

  I missed the bus.

  The hole in the ozone layer is growing.

- the **costs** of that condition that make you (or someone) unhappy

  I will be late for work and may lose my job.

  Many will die from skin cancer.

But now a crucial caution: *Your readers* will judge the significance of a problem not by its cost to *you*, but by its cost to *them*. So you must try to frame your problem from their point of view. To do that, imagine that when you pose the condition of your problem, your reader responds, *So what?* For example,

> The hole in the ozone layer grew last year.
> *So what?*

You answer with the cost of the problem:

> A bigger hole in the ozone means more ultraviolet light hitting the earth.

Suppose the other person again says, *So what?*, and you respond with a further cost:

> Too much ultraviolet light can give people skin cancer.

If, however improbably, he again asks, *So what?*, you have failed to convince him that the problem is not just yours, but his as well. We acknowledge that a problem exists only when we stop saying, *So what?*, and instead say, *Oh no! What do we do about that?*

Practical problems like cancer are easy to grasp because they always have palpable consequences. In the academic world, however, you probably will work more with research problems, which are harder to grasp because both their conditions and costs are always abstract.

### 4.2.2 The Nature of Research Problems

Practical and research problems have the same structure, but their conditions and costs differ in important ways:

- The condition of a practical problem can be any state of affairs whose cost makes you (or someone) unhappy. The condition of a research problem, on the other hand, is *always* some version of *not knowing* or *not understanding* something.

You can identify conditions by working though the formula in chapter 3. The second step states 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 local Texas politicians.*

That's why we emphasized the value of questions. They force you to consider what you don't know or understand but want to.

- The cost of a practical problem is unhappiness. The consequence of a research problem, on the other hand, *is something else* we or, more important, our readers don't know or understand, but is *more* significant, *more* consequential than the ignorance or misunderstanding named by the condition. This, too, we can express as a question.

You identify these consequences 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 how regional self-images influence national politics.*

All this may sound confusing, but it's simpler than it seems. When you move from questions to problems, you only translate that formula for working out the significance of a question *to you* into a way to find its significance *to your readers.*

It works like this: The first part of a research problem is something you don't know but want to. You can phrase that as a direct question:

> How many stars are in the sky?
>
> How have romantic movies changed in the last fifty years?

Now imagine someone asking, *So what if you can't answer that question?* What do you say? You answer by stating *something else* you don't know until you answer the first question, something that the other person should also want to know. 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 question*

If you think that finding an answer to that second question is important, you've stated a cost that makes your research problem worth pursuing, and if your reader thinks so too, you're in business.

But what if your potential readers might again ask, *So what?*

> So what if I don't know whether our cultural depictions of romantic love have changed?

You will just have to pose a yet larger question whose answer depends on answering the previous ones, an answer that should be even more significant to your readers:

> If we can't answer the question of how our cultural depictions of romantic love have changed in the last fifty years,*second question* then we can't answer a more important one yet: How is our culture shaping the expectations of young men and women concerning marriage and families?*consequence/larger, more important question*

If you imagine that reader again asking, *So what?*, you might be tempted to think, *Wrong audience.* But if that's the audience you're stuck with, you will have to try again.

To those outside an academic field looking in, researchers sometimes seem to pose a question so narrowly that outsiders think it is ridiculously trivial: *So what if we don't know how hopscotch originated?* Yet for those few who care about the way folk games influence the social development of children, the cost of not knowing justifies the research. *What do you mean? If we can discover how children's folk games originate, we can learn something about how they socialize themselves. . . .*

### 4.2.3 Distinguishing "Pure" and "Applied" Research

When the solution to a research problem has no apparent application to any practical problem in the world, but only to the scholarly interests of a community of researchers, we call the research *pure.* When the solution to a research problem does have practical consequences, we call the research *applied.*

You can tell whether a research problem is pure or applied by looking at the last of the three steps in defining your project. Does it refer to knowing or doing?

1. **Topic:** I am studying the density of light and other electromagnetic radiation in a small section of the universe
    2. **Question:** because I want to find out how many stars are in the sky,
        3. **Significance:** in order to help readers understand whether the universe will expand forever or contract into a new big bang.

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

In an applied research problem, the second step also refers to knowing, but that third step refers to *doing:*

1. **Topic:** I am studying the difference between readings from the Hubble telescope in orbit above the atmosphere and readings for the same stars from earthbound telescopes

2. **Question:** because I want to find out how much the atmosphere distorts measurements of light and other electromagnetic radiation,
    3. **Practical Significance:** so that *astronomers can use data from earthbound telescopes to measure more accurately the density of electromagnetic radiation.*

That is an applied problem because astronomers can *do* what they need to—measure light more accurately—only when they *know* how much atmospheric distortion to account for.

### 4.2.4 Connecting a Research Problem to Practical Consequences

Some less experienced researchers are uncomfortable with pure research because its costs—merely not knowing something—are so abstract. Since they are not yet part of a community that cares about the answers to their questions, they feel that their findings aren't good for much. So they try to cobble a practical cost onto their conceptual research question to make it seem more significant:

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

Most readers are likely to think that connection is a bit of a stretch.

To formulate a useful applied research problem, you have to show that the answer in step 2 plausibly leads to step 3. Ask yourself this question:

(a) If my readers want to achieve the goal of ______________ [state your objective from step 3],

(b) would they think that a good way to do that would be to find out ______________? [state your question from step 2]

Try that test on the 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 a good way to do so would be to find out how much the atmosphere distorts measurements of it?

Since astronomers have piles of data from earthbound telescopes that could be adjusted for atmospheric distortion, the answer would seem to be *Yes*.

Now try the test on the Alamo problem:

> (a) If my readers want to achieve the goal of helping people protect themselves from unscrupulous politicians,
> (b) would they think a good way to do that would be to find out how nineteenth-century politicians used stories of great events to shape public opinion?

Again, that feels like a stretch.

If you really think that the answer to your research problem can apply to a practical one, formulate your problem as the pure research problem it is, then *add* your application as a fourth step:

1. **Topic:** I am studying the differences among various nineteenth-century versions of the story of the Alamo
    2. **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 elements of popular culture to advance their political goals,
            4. **Potential Practical Application:** so that readers can better protect themselves from unscrupulous politicians.

When you state your problem in your introduction, it's usually best to formulate it as a purely conceptual research problem whose significance is based on conceptual consequences. Unless your assignment includes the question of practical applications, save them for your conclusion. (For more on introductions and conclusions, see chapter 14.)

Most research projects in the humanities and many in the natural and social sciences have no direct application to daily life. In fact, as the word *pure* suggests, many researchers value pure research more highly than they do applied. They believe that the pursuit of knowledge "for its own sake" reflects humanity's highest calling—to know more and understand better, not for the sake of money or power, but for the good that understanding itself brings. As you may have guessed, the three of us support both the pure and the practical—so long as the research is done well and is not corrupted by dishonest or malign motives.

A threat to both pure and practical research today, especially in the biological sciences, is that profits from patents not only determine the choice of research problems, but also color their solutions: *Tell us what to look for, and we'll provide it!* That raises the kind of ethical question that we touch on later (pp. 285–88).

### A Typical Beginner's Mistake

For some beginners, especially in classes that study significant practical problems, research problems never feel practical enough, not even when they have obvious applications. So they try to force their project into the practical domain. That's usually a mistake. No one can solve the world's great problems in a five- or even a fifty-page paper. But a good researcher might help us understand those problems better, which gets us closer to a solution. So if you care deeply about a practical problem, such as the increasing frequency of highly destructive forest fires in the West, carve out of it a research question that you can answer and that might ultimately contribute to a practical solution:

> How important are fires to the ecological health of a forest?
> How do local fire codes affect the susceptibility of buildings to fire damage?

Choose one of the smaller questions, knowing that small answers to small questions sometimes lead to great solutions.

## 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 on a problem whose solution makes the rest of us see the world in a new way. We can all learn to recognize a good problem when we bump into it, or it bumps into us (or when it's already a live issue). But researchers often begin a project without being entirely clear as to what their problem is. Sometimes they hope only to define it more clearly. Indeed, those who find a new problem or manage to clarify an old one often win more fame and (sometimes) fortune than those who solve a problem already defined. Some researchers have even gotten credit for *disproving* a plausible hypothesis that they had hoped to prove. So don't be discouraged if you can't formulate your problem fully at the outset of your research. Few of us can. But thinking about it early will save you hours of work along the way—and perhaps avoid panic toward the end.

Here are some ways you can aim at a problem from the start.

### 4.3.1 Ask for Help

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

If you are free to select your own problem, look for a small one that is part of a bigger one. Though you are unlikely to solve the big one, your piece of it will inherit some of its significance. (You will also educate yourself about the problems of your field, no small dividend.) Ask your teacher what she is working on and whether you can work on part of it. But a warning: If your teacher helps you define your problem and gives you leads on sources, do not let those suggestions define the limits of your research. Nothing discourages a teacher more than a student who does *exactly* what is suggested, *and nothing more.* In that situation, the teacher probably wants you to do some research that will help her find out something she didn't know or understand, such as better sources and new data.

### 4.3.2 Look for Problems as You Read

You can find a research problem if you read critically. As you read a source, where do *you* detect contradictions, inconsistencies, incomplete explanations? If you are not satisfied with an explanation, if something seems odd, confused, or incomplete, tentatively assume that other readers would or should feel the same way. Many research projects begin in an imaginary conversation that a researcher has with another's report: *Wait a minute, he's ignoring . . .*

But before you set out to correct a gap, error, or misunderstanding, be sure it is real, not just your own misreading. Reread your source carefully and generously. Countless research papers have aimed to refute a point that no writer ever made.

Once you think you have found a real puzzle or error, do more than just point it out. If a source says X and you think Y, you have a research problem only if you can show that those who go on believing X will misunderstand something even more important. (For the most common kinds of contradictions, see our Quick Tip, pp. 72–74.)

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 daily life of the nineteenth-century Russian peasant influenced his performance in battle:

> 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 gives us both the problem that this writer set out to solve and a new one waiting for someone to tackle.

### 4.3.3 Look for the Problem that Your Claim Solves

Critical reading can also help you discover a good research problem in your own early drafts. Writers almost always do their best thinking in the last few pages of a draft. It is often only then that they begin to formulate a final claim that they did not dream of when they started out. If in an early draft you arrive at an unanticipated claim, ask yourself what questions it might answer. Paradoxical as it might seem, you may well find a solution to a problem that you have not yet posed. Your task is to figure out what that problem is. Chances are, you can work backward to formulate a better, more interesting problem than the one that got you started.

## 4.4 SUMMARY: THE PROBLEM OF THE PROBLEM

Your teachers will assume that you are not an expert researcher, but they want you to start developing and practicing the mental habits of one. They want you to do more than just accumulate and report facts about a topic that happens to interest you. They want you to formulate a question that you think is worth answering and pose a problem that you think is worth solving, regardless of who else cares.

Eventually, though, as you move to advanced work, you have to share your new knowledge and understanding with others. At that point, you must understand what *your readers* think are interesting questions and problems. As we've emphasized, they base that judgment on the costs *they* pay as a result of not knowing or understanding something. And the step we all dream of is not only finding the kind of problem readers want to see solved, but persuading them to think seriously about a problem none of them has ever thought of. No one takes all three steps the first time out. Just about all of us get to the first one: *What am I interested in discovering?* Most of us get to the second: *What might my readers be interested in?* Few of us get to the third: *How can I get* *them to realize they are asking the wrong questions?* But those of us who don't get there do not necessarily fail, because we can measure our success by how well our readers think we answer questions they already care about. The worst response you can get from a reader is not *I don't agree,* but *I don't care.*

By now, all this airy talk about academic research may seem disconnected from a world in which so many people labor so hard at getting ahead or keeping others down. But when research problems in the world are pursued honestly, they are structured *exactly* as they are in the academic world. And 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 that others should take seriously, then to articulate that problem in a way that convinces them to care. If you can do that in a class in Chinese history, you can do it in a business or government office down the street or in Hong Kong.