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From Topics to Questions

In this chapter, we discuss how to use your interests to find a topic, narrow it to a manageable scope, and then generate questions that will focus your research. If you are an advanced student and already have a dozen topics that you would like to pursue, you might skip to Chapter 4. If, however, you are starting your first project, you will find this chapter useful.

3.1 INTERESTS, TOPICS, QUESTIONS, AND PROBLEMS

IF YOU ARE FREE to pursue any research topic that interests you, that freedom may be frustrating—so many choices, so little time. Finding a topic, though, is only the first step, so do not assume that once you have a topic, you need only search for information and report what you find. Beyond a topic, you have to find a reason (other than completing your assignment) for devoting weeks or months to pursuing it and then for asking readers to spend time reading about it.

Researchers do more than just dig up information and report it. They use that information to answer a question that their topic inspired them to ask. At first, the question may intrigue the researcher alone: how good was Abe Lincoln at math? Why do cats rub their faces against us? Is there such a thing as innate perfect pitch? That's how most significant research begins—with an intellectual itch that only one person feels the need to scratch. But at some point, a researcher has to decide whether the question and its answer might be *significant*, at first to the researcher alone, but eventually to others—to a teacher, to colleagues, to an entire community of researchers.

At that point, the researcher must view his task differently: he must aim not just at answering a question, but at posing and solving a *problem* that he thinks *others* should also recognize as worth solving. That word “problem,” though, has a meaning so special in the world of research that it is the topic of the whole next chapter. It raises issues that few beginning researchers are ready to resolve entirely, and that can vex even an advanced researcher. So do not

feel dismayed if at first you cannot find in your topic a problem that others might think worth solving. But you will never even approach that point unless you strive to find in your topic a question that at least *you* think worth asking.

In this chapter, we focus on the steps leading to the formulation of a research question. How do you transform an interest into a topic for research? How do you find questions that can guide your research? Then how do you decide whether those questions and answers are worth pursuing, at first just to you, but then to your readers? The process looks like this:

1. Find an interest in a broad subject area.
2. Narrow the interest to a plausible topic.
3. Question that topic from several points of view.
4. Define a rationale for your project.

In the next chapter we address the more vexing matter of turning your questions into a research *problem*.

3.2 FROM AN INTEREST TO A TOPIC

Experienced researchers have more than enough *interests* to pursue. An interest is just a general area of inquiry that we like to explore. The three of us have our current favorites: society and language, textual coherence and cognition, ethics and research. But while beginning researchers also have interests, they sometimes find it difficult to locate among them a *topic* appropriate for academic research. A topic is an interest specific enough to support research that one might plausibly report on in a book or article that helps others to advance their thinking and understanding: the linguistic signals of social change in Elizabethan England, the role of mental scenarios in the reader's creation of coherence, the degree to which current research is motivated by under-the-counter payments.

If you are free to explore any topic within reason, we can offer only a cliché: start with what interests you most deeply. Nothing will contribute to the quality of your work more than your sense of its worth and your commitment to it. Start by listing four or five areas that you'd like to learn more about, then pick one with the best potential for yielding a topic that is specific and that might lead to good sources of data. If you are in an advanced course, you are likely to be limited to matters of interest to those in your field of study, but you can always find more by looking in a recent

textbook, talking to another student, or consulting your teacher. You might even try to identify an interest that will provide a topic for work in another course, either now or in the future.

If you are still stuck, here is a way to search for topics that might pan out: If this is your first research project in a writing course, find in the reading room of your library a general bibliographical resource such as the *Reader's Guide to Periodical Literature* or the *Bibliographic Index* (we will discuss these resources in more detail in Chapter 5 and in the Quick Tip after it). If you are an advanced student, locate a specialized index in your particular field, such as *Philosopher's Index*, *Psychological Index*, *Women's Studies Abstracts*. Now skim its headings until you find one that catches your interest. That heading will provide not only a possible topic, but also a list of sources on it.

If you are writing your first research paper in a particular field and have not yet settled on a topic, you might head over to the library to find out where its resources are particularly rich. If you pick your topic first and then after considerable searching discover that the sources are thin, you will have to start over. By identifying areas with promising resources, you learn the strengths and weaknesses of your library and can plan this and future projects more thoughtfully. (If you are really stuck, look at the Quick Tip at the end of this chapter for more suggestions.)

3.3 FROM A BROAD TOPIC TO A NARROW ONE

At this point, you risk picking a topic so broad that it could be a subheading in an encyclopedia article: "Space flight, history of"; "Shakespeare, Problem Plays"; "Natural kinds, doctrine of." A topic is probably too broad if you can state it in fewer than four or five words. If you find yourself struggling with that kind of topic, narrow it:

Free will and historical inevitability in Tolstoy's <i>War and Peace</i>	→	The conflict of free will and historical inevitability in Tolstoy's description of three battles in <i>War and Peace</i>
The history of commercial aviation	→	The contribution of the military to the development of the DC-3 in the early years of commercial aviation

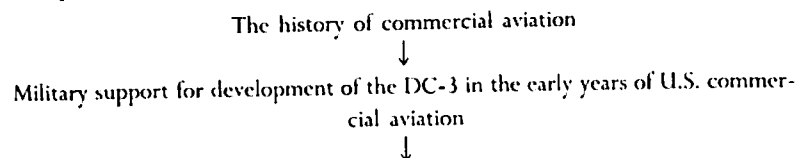
We narrowed these topics by adding modifying words and phrases. In particular, we added four nouns of a special kind: *conflict*, *description*, *contribution*, and *development*. Those nouns are special because they are each related to a verb: *conflict*, *describe*, *contribute*, and *develop*. At some point, you will have to move from a phrase that names a topic—"free will and historical inevitability in Tolstoy," "history of commercial aviation"—to a sentence that states a potential claim. If you narrow your topic by using nouns derived from verbs, you will be one step closer to a claim that could be challenging enough to interest your readers. Compare these:

Free will and historical inevitability in Tolstoy's <i>War and Peace</i>	→	There is both free will and historical inevitability in Tolstoy's <i>War and Peace</i> .
The <i>conflict</i> of free will and historical inevitability in Tolstoy's <i>description</i> of three battles in <i>War and Peace</i> .	→	Tolstoy <i>describes</i> three battles in a way that makes free will <i>conflict</i> with historical inevitability.
The history of commercial aviation.	→	Commercial aviation has a history.
The <i>contribution</i> of the military in the <i>development</i> of the DC-3 in the early years of commercial aviation.	→	The military <i>contributed</i> to the way the DC-3 <i>developed</i> in the early years of commercial aviation.

These may not be particularly interesting claims yet. But since you will build your final project out of a series of claims, you should, from the beginning, take every opportunity to work toward the kinds of claims you will eventually need.

The advantage of a specific topic is that you more easily recognize gaps, inconsistencies, and puzzles that you can question. That will help you turn your topic into a research question. (If you follow our later suggestion to begin with an index or abstract, your topic will already be restricted by its headings.)

Caution: you narrow your topic too severely when you cannot easily find sources.



The decision to lengthen the wing tips on the DC-3 prototype as a result of the military desire to use the DC-3 as a cargo carrier

3.4 FROM A NARROWED TOPIC TO QUESTIONS

Once the beginning researcher hits on a topic that feels both interesting and promising, perhaps something like "the political origins and development of legends about the Battle of the Alamo," she typically begins searching out sources and collecting information, in this case versions of the story in books and films, Mexican and American, nineteenth century and twentieth. She might then write a paper that summarizes the stories, points out differences and similarities, contrasts them with what modern historians think really happened, and concludes.

Thus there are interesting differences and similarities between . . .

In a first-year writing course, such a paper might earn a passing grade. It shows that the student can focus on a topic, find data on it, assemble those data, and present them coherently—no small achievement for a first research project. But for anyone who wants her research to *matter*, such an achievement would fall short of the mark.

While the writer may have learned something from the exercise of searching out and reporting on the Alamo stories, she offers only *information*. She asked no *question* that she or her readers might think worth asking, and so she can offer no *answer* significant enough to change how she or her readers should think about those stories or their development.

Once you have a topic to research, you should find in it questions to answer. Questions are crucial, because the starting point of good research is always what *you do not know or understand but feel you must*. Start by barraging your topic with question after question, first with the obvious standing questions of your field:

Do the legends about the Battle of the Alamo accurately reflect our best historical accounts? Do the historical accounts differ?

Ask the standard *who*, *what*, *when*, and *where* questions. Record your questions, but don't stop for their answers.

You can organize your questions from these four perspectives:

1. What are the parts of your topic and what larger whole is it a part of?
2. What is its history and what larger history is it a part of?
3. What kinds of categories can you find in it, and to what larger categories of things does it belong?
4. What good is it? What can you use it for?

(Don't worry about getting the right questions in the right categories; the categories serve only to stimulate the questions.)

3.4.1 Identify Its Parts and Wholes

- Question your topic in a way that analyzes it into its component parts and evaluates the working relationships among them:

What are the parts of stories about the Battle of the Alamo? How do they relate to one another? Who were the participants in the stories? How do the participants relate to the place, the place to the battle, the battle to the participants, the participants to one another?

- Question your topic in a way that identifies it as a working component in a larger system:

What use have politicians made of the story? What role does it have in Mexican history? What role does it have in our history? Who told the stories? Who listened? How does the nationality of the teller affect the story?

3.4.2 Trace Its History and Changes

- Question your topic in a way that treats it as a dynamic entity that changes through time, as something with its own history:

How did the battle develop? How have the stories developed? How have different stories developed differently? How have audiences changed? How have the storytellers changed? How have motives to tell the story changed? Who first told the stories? Who told them later? Who were the earliest readers and listeners? Who later?

- Question your topic in a way that identifies it as an episode in a larger history:

What caused the battle, the stories? What did the battle and the stories then cause? How do the stories fit into a historical sequence? What else was happening when the stories appeared? When they changed? What forces caused the story to change?

3.4.3 Identify Its Categories and Characteristics

- Question your topic in a way that defines its range of variation, how instances of it are like and different from one another:

What is the most typical story? How do other stories differ from it? Which one is most different? How do the written and oral stories differ from the movie versions? How are Mexican stories different from ours?

- Question your topic in a way that locates it in a larger category of things like it:

What other stories in our history are like the story of the Battle of the Alamo? What other stories are very different? What other societies have the same kinds of stories?

3.4.4 Determine Its Value

- Question your topic in regard to the value of its uses:

What good are the stories? What use has been made of them? Have they helped people? harmed them?

- Question your topic in regard to the relative value of its parts and features:

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

3.4.5 Review and Rearrange Your Answers

When you run out of questions, group them in different ways. In the Alamo example some questions relate to the development of the stories; others address their quality as fact or fiction; others highlight differences between versions (nineteenth- and twentieth-century, Mexican and American, written and movie); other questions address political issues, and so on. Such lists can provide scores of research topics. If they are freewheeling enough, they can have the exhilarating effect of opening up worlds of research.

The next step requires more careful judgment. First, identify

questions that need more than a one- or two-word answer. Questions that begin with *who*, *what*, *when*, or *where* are important, but they ask only about matters of fact. Emphasize instead questions that begin with *how* and *why*. Then decide which questions stop you for a moment, challenge you, spark some special interest. At this point, of course, you can't be sure of anything. Your answers may turn out to be less surprising than you hoped, but your task now is only to formulate a few questions whose answers *might* be both plausible and interesting.

When you've done all this, you have taken your first big step toward a project that goes beyond just collecting data. You have identified something that you don't know but want to, and what you want to know drives the earliest stages of your research. You are ready to gather data, a process we'll describe in Chapter 5. But even though you can now begin gathering data, the process of focusing your project is not yet complete.

3.5 FROM A QUESTION TO ITS SIGNIFICANCE

Even if you are an experienced researcher, you may not be able to take this next step until you are well into your project, perhaps even close to its end. And if you are a beginning researcher, you may feel this step is especially frustrating. Once you have a question, you have to ask and try to answer the further question, *So what?*

So what if I don't know or understand how snow geese know where to go in the winter, why the Titanic was designed so badly, how fifteenth-century violin players tuned their instruments, why Texans tell one story about the Alamo, Mexicans another? So what?

This question vexes all researchers, beginners and experienced alike, because to answer it, you have to know how significant your research might be not just to yourself but to others. Instead of asking that question straight out, though, you can get closer to its answer if you move toward it in steps.

3.5.1 Step 1: Name Your Topic

In the earliest stages of a research project, when you have only a topic and maybe the first glimmerings of a few good questions, try to describe your work in a sentence something like this:

I am learning about/working on/studying _____.
Fill in the blank with a few noun phrases. Be sure to include one or two of those nouns that you can translate into a verb or adjective:

I am studying *repair processes* for cooling systems.

I am working on the *motivation* of President Roosevelt's early speeches.

3.5.2 Step 2: Suggest a Question

As early as you can, try to describe your work more exactly by adding to that sentence an indirect question that specifies something about your topic that you do not know or fully understand, but want to:

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

You now have to fill in the new blank with a subject and a verb:

I am studying repair processes for cooling systems, *because I am trying to find out how* expert repairers analyze failures.

I am working on the motivation of Roosevelt's early speeches, *because I want to find out whether* presidents since the '30s have used those speeches to announce new policy.

When you can add that kind of *because-I-want-to-find-out-how/why* clause, you have defined both your topic and your reason for pursuing it. If you are doing one of your first papers and you get this far, congratulate yourself, because you have defined your project in a way that goes beyond the random collection of information.

3.5.3 Step 3: Motivate the Question

There is, though, one more step. It's a hard one, but if you can take it, you transform your project from one that interests you to one that makes a bid to interest others, a project with a rationale explaining why it is important to ask your question at all. To do that, you must add an element that explains why you are asking your question and what you intend to get out of its answer.

In Step 3, you add a second indirect question, this one introduced with *in order to understand how, why, or whether*:

1. I am studying repair processes for cooling systems,
 2. because I want to find out how expert repairers analyze failures,
 3. *in order to understand how* to design a computerized system that could diagnose and prevent failures.
1. I am working on the motivation of Roosevelt's early speeches,
 2. because I want to discover whether presidents since the '30s used those speeches to announce new policy,
 3. *in order to understand how* generating public support for national policy has changed in the age of television.

Assembled, the three steps look like this:

1. *Name your topic:*
I am studying _____,
2. *Imply your question:*
because I want to find out who/how/why _____,
3. *State the rationale for the question and the project:*
in order to understand how/why what _____.

Rarely can a researcher flesh out this pattern fully before she begins gathering information. In fact, most can't complete it until they're nearly finished. Too many, unfortunately, publish their results without having thought through these steps at all.

Even though at the beginning of your project you won't be able to state these steps fully, it is a good idea to test your progress every so often by seeing how close you can come. Better: Have someone else—roommate, relative, or friend—force you to flesh out this progression. Your evolving description will help you keep track of where you are and keep you focused on where you may still have to go.

It may be that in your first try at research you will not find a question whose answer has much significance to anyone but yourself. But do that much and you will delight your teacher. As you move through your project, though, do what you can to fill out the pattern; try to find a reason for asking your question, a way to make its answer seem significant to you, maybe even to others.

Remember, your eventual object is to explain,

- what you are writing about—your topic.
- what you don't know about it—your question.
- why you want to know about it—your rationale.

When you can achieve these three objectives, you will have articulated a motive for your project that goes beyond just meeting a requirement. You will know that you have an *advanced* research project when what follows the *in order to understand* is important not just to you but to your readers as well.

It is when we begin to consider our readers that we must change the terms of our project from posing and answering a question to posing and solving a problem, the subject of our next chapter.

QUICK TIP: FINDING TOPICS

If you are an advanced researcher, chances are that you will not have to look far for topics to research. You can focus on current research in your field, which you can find easily enough by browsing through recent articles and review essays and, if they are available, recent dissertations, especially the suggestions for future research in their conclusions. If you are less advanced, your teacher will still expect you to focus your topics on the field, though not on its most advanced state. Most teachers will either assign topics to choose from or at least indicate the kind of topics to consider.

But sometimes you will be left to find topics on your own, and if you are in a first-year writing class, you will have to find good topics without even a specific field to focus your efforts. If you have to find your own topic and have drawn a blank, try looking in these sources:

FOR TOPICS FOCUSED ON A PARTICULAR FIELD OF STUDY

1. Browse through a textbook in a course one level advanced beyond yours or from a course that you know you will have to take at some time in the future. Don't overlook the study questions.
2. Attend a public lecture in your field and listen for something you disagree with, don't understand, want to know more about.
3. Browse through the topic headings in specialized bibliographies and abstracts.
4. Browse through the *Encyclopedia of . . .* in the field you are studying.
5. Ask your instructor about the most contested issue in her field.
6. If you have access to the internet, find a specialized "list" that interests you and "lurk" (read messages sent by others) until you find debated topics.

FOR GENERAL TOPICS

1. Think of some special interest you have—sailing, gymnastics, chess, volunteer work, modern dance—and investigate its origins or how it is practiced in another culture.

2. Investigate a specific aspect of a country you'd like to visit.
3. Wander through a museum of any kind—art, natural history, automobile—until you find yourself looking at something with great interest. What more do you want to know about it?
4. Wander through a large shopping mall or store, asking yourself, *How do they make that?* or *I wonder who thought up that product?*
5. Leaf through your Sunday newspaper, especially the features sections, until you find yourself stopping to read something. If you have access to the *New York Times*, look through its feature sections and the Sunday review of books.
6. Go to a large magazine rack and browse. Buy a magazine that looks technical and interesting. Look especially for trade magazines or those that cater to highly specialized interests.
7. Look through the kind of popular magazines you find in waiting rooms, such as the *Reader's Digest*, for an article that makes a significant claim about health, society, or personal relationships that is based on alleged "evidence." Find out whether it is true.
8. Tune into interview programs on TV or talk radio until you hear something you disagree with. Then ask yourself whether you could find enough information to refute it.
9. Recall the last time you heatedly discussed some important topic and were frustrated because you didn't have the facts you needed.
10. Think of one thing that you believe but most people don't. Then ask whether it's the kind of issue on which you can find enough evidence to convince someone else.
11. Think of some common beliefs that everyone takes for granted but might not be so, such as the claim the Eskimos have scores of words for snow or that one gender is naturally better at something than the other.
12. Skim topic headings in general bibliographies, such as the *Readers' Guide to Periodical Literature*.
13. Think of a popular controversy that research could help you clarify.
14. Get together with five or six friends and brainstorm about what you would all like to know more about.

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From Questions to Problems

This chapter covers matters that beginning researchers may find difficult, perhaps even baffling. So those of you working on your first project might skip to Chapter 5. (Of course, we hope that you will rise to the challenge and read on.) For advanced students, though, what follows is essential.

IN THE LAST CHAPTER, we described how to find in your interests a topic, how to find in that topic questions to research, and then how to signal the significance of your answer by describing its rationale:

1. *Topic:* I am studying _____.
2. *Question:* because I want to find out who/how/why _____.
3. *Rationale:* in order to understand how/why/what _____.

These steps define not only the development of your project, but your own growth as a researcher. When you move from step 1 to 2, you go beyond those who merely gather information, because you are directing your project not by aimless curiosity (by no means a useless impulse), but by your need to understand something better. When you move on to step 3, you surpass beginning researchers, because you are focusing your project on the *significance*, on the *usefulness* of understanding what you do not know. When those steps become a habit of thinking, you become a true researcher.

4.1 PROBLEMS, PROBLEMS, PROBLEMS

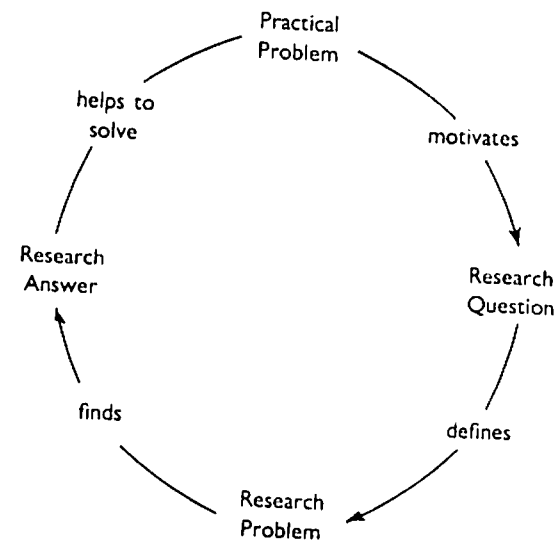
There is, though, a last step, one that is hard for even experienced researchers. You must convince your readers that the answer to your question is significant not just to you, but to *them* as well. You must transform your motive from discovering to *showing*; more importantly, from understanding to *explaining* and *convincing*.

This last step trips up even experienced researchers, because

they often think that they have done their job simply by posing and answering a question that interests them. They are only partly right: their answer must also be the solution to a *research problem* that is significant to others, either because those others already think it is significant or, as is more likely, because they can be convinced to think so. What sets you apart as a researcher of the highest order is the ability to develop a question into a problem whose solution is significant to your research community. The trick is to communicate that significance. To understand how to do that, you have to understand more exactly what we mean by a research "problem."

4.1.1 Practical Problems and Research Problems

Most everyday research begins not with finding a topic but with confronting a problem that has typically found you, a problem that left unresolved means trouble. When faced with a practical problem whose solution is not immediately obvious, you usually ask yourself a question whose answer you hope will help you solve the problem. But to find that answer, you must pose and solve a problem of another kind, a research problem defined by what it is that you do not know or understand, but feel you must. The process looks like this:



PRACTICAL PROBLEM: My brakes have started screeching.

RESEARCH QUESTION: How can I get them fixed right away?

RESEARCH PROBLEM: I need to find a nearby garage in the Yellow Pages.

RESEARCH ANSWER: The Car Shoppe, 1401 East 55th St.

APPLICATION TO PRACTICAL PROBLEM: Call to see when they can fix them.

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

- I want to impress a potential employer. *How do I find a good restaurant?* Look in a city guide. *Woodlawn Tap.* I take her there, and I hope she thinks I've got style.
- The National Rifle Association presses me to oppose gun control. *Will I lose if I don't?* Take a poll. *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.

We don't write up solutions to most such problems, but we usually have to when we want to convince others that we have solved a problem important to *them*:

To CEO: Costs are up in Omaha, because your workers see no future in their jobs and after a few months quit. You have to train new ones, which is costly. To retain workers, upgrade their skills so they will want to stay.

Before anyone could solve the *practical* problem of rising costs, though, someone had to solve a *research* problem defined by not knowing why costs were rising.

4.1.2 Distinguishing Practical Problems and Research Problems

This distinction between practical, pragmatic problems and research problems may seem to be a fine one, but it is crucial:

- A *practical* problem originates in the world and exacts a cost in money, time, happiness, etc. You solve a practical problem by changing something out there in the world, by *doing* something. But before you can solve a *practical* problem, you may have to pose and solve a *research* problem.
- A *research* problem originates in your mind, out of incomplete knowledge or flawed understanding. You might pose a research problem because you have to solve a practical problem, but you

do not solve that practical problem merely by solving the research problem. You might *apply* the solution of that research problem to the solution of a practical problem, but you solve your research problem not by changing anything in the world but by learning more about something or understanding it better.

Most medical researchers, for example, believe that before they can solve the practical problem of the AIDS epidemic, they must first solve in the laboratory a research problem posed by the puzzling mechanism of its virus. But even if medical researchers solve that research problem by discovering its mechanism, governments still have to find a way to apply that solution to the practical problem of AIDS in society.

"Problem" thus has a special meaning in the world of research, one that sometimes confuses beginning researchers who usually think of problems as "bad." Every researcher needs a "good" research problem to work on; in fact, if you don't have a good research problem, you have a practical problem that is bad indeed.

4.1.3 Distinguishing Problems and Topics

There is a second reason that beginning and even intermediate researchers struggle with this notion of "problem." Experienced researchers often talk about their research problem in a shorthand way that seems to describe it just as a topic: *I'm working on adult measles*, or on *early Aztec pots*, or on *the mating calls of Wyoming elk*.

As a result, many beginning researchers confuse having a *topic* to read up on with having a *research problem* to solve. Lacking the focus provided by the search for a solution to a well-defined research problem, they just keep gathering more and more data, not knowing when to stop. Then they struggle to find a principle for deciding what to include in their report and what not, and finally just throw in everything they have. Then they feel frustrated when a reader says, *I don't see the point; this is just a data dump*.

You risk wasting your reader's time if you cannot distinguish between a *topic* to read about and a *research problem* to solve. In the rest of this chapter, we explain what a problem is, both academic and nonacademic. We return to problems in Chapter 15, when we discuss how to state your research problem in the introduction of your paper.

4.2 THE COMMON STRUCTURE OF PROBLEMS

We have distinguished pragmatic problems and research problems, but they have the same essential structure. Both consist of two elements:

- (1) some particular situation or condition, and
- (2) its undesirable consequences, *costs* that you don't want to pay.

4.2.1 Practical Problems

A flat tire is usually a practical problem, because it is (1) a condition out there in the world that (2) may exact from you a tangible cost—perhaps missing a dinner engagement. But suppose your dinner companion bullied you into accepting the date and you would rather be anywhere else. In that case, the flat tire does not have a cost, because now you judge missing that dinner date to be a positive benefit. In fact, the flat tire is now not part of a problem, but part of a solution.

So when you think you have found a problem, be sure that you can identify and describe a situation with these two parts:

- a *condition* that needs to be resolved
 CONDITION: I missed the bus.
 The hole in the ozone layer is growing.
- *costs* of that condition that you don't want to bear
 COST: I may lose my job because I will be late for work.
 Many will die from skin cancer.

You can often rephrase negative costs in positive form, as a benefit of resolving the condition:

BENEFIT: If I can catch the bus, I save my job.
 If we fix the ozone hole, we save many lives.

The greater the consequences of the condition—either the costs of leaving it unresolved or the benefits of resolving it—the more *significant* the problem.

For a practical, tangible problem, the condition can be literally anything, even a seeming stroke of luck, if it has a cost: *You win the lottery*. That might not seem like a problem, but what if you owed

a loan shark \$5,000,000 and your name gets in the paper? Winning the lottery could then cost you more than you won: someone finds you, takes your money, and breaks your leg.

4.2.2 Research Problems

A practical problem and a research problem have the same structure, but they differ in two important ways.

Conditions. While the condition part of a practical problem can be any state of affairs, the condition of a research problem is *always* defined by a rather narrow range of concepts. It is always some version of your *not knowing* or *not understanding* something that you think that you and your readers should know or understand better.

That's why in Chapter 3 we emphasized the value of questions. Good questions are the first step to defining your research problem, because questions imply what you and your readers don't know or understand but should: *What role does genetics play in cancer? How do icebergs influence the weather? How did Latin epics influence Old English poetry? How much does the death penalty deter murder?*

Costs. The second difference is harder to grasp. It is that the consequences of a research problem might have nothing immediately to do with the world. The *immediate* cost or benefit of a research problem is always some *further* ignorance and misunderstanding that is *more* significant, *more* consequential than the ignorance or misunderstanding that defined the condition.

This idea of cost is easy to understand in a practical problem because its costs are usually palpable—pain and suffering, lost money, opportunity, happiness, reputation, and so on. The costs of a research problem, though, are that we do not know or understand something else. That's why the problem of a visit from the loan shark seems easier to grasp than the problem of not understanding the influence of Latin on Old English poetry. The costs of the first are more palpable than those of the second. But not understanding the influence of Latin on Old English poetry has costs nonetheless. If we do not understand those influences, we will not understand *something yet more significant*—what an important but puzzling poem might mean, what Old English poets knew and didn't know about other literatures, why Old English poetry is the way it is.

An advanced researcher must show that because she does not know or understand one thing, she cannot know or understand something else *more important*. She must answer the question, *So what?*

So what if I never understand the role of genetics in cancer, why cats rub their jaws against us, how bridges were built in ancient Greece? If I never find out, what greater cost do I pay in my larger knowledge or understanding?

In short, you have no research problem until you know the cost of your incomplete knowledge or flawed understanding, a cost that you define in terms of a yet greater ignorance or misunderstanding.

4.2.3 When a Research Problem Is Motivated by a Practical Problem

It is easier to identify costs and benefits of a research problem when it is motivated by a practical problem:

So what if we don't know why costs are up in Omaha? We go bankrupt. So what if we do not understand the role of genetics in cancer? Until we do, we will not know whether we can identify the genes that predispose us to cancer, when it can be predicted, or even cured.

The cost of not knowing the role of genetics in cancer is that we do not understand its cause. Or putting this in the form of a benefit, perhaps only when we understand the genetics of cancer can we cure it. Now we instantly recognize the additional costs of our ignorance and the benefits if we remedy it, because a solution to the research problem points to a solution to the practical problem.

But how can stories about the Alamo or the aesthetics of Tibetan weaving be part of a significant research problem? We see a condition clearly enough: incomplete knowledge. But what costs do we bear if we go on knowing incompletely?

So what if we don't know about the evolution of medieval plumbing or the life cycle of a rare orchid in central New Guinea? What's the cost if we never find out? Or the benefit if we do? Well, let me think . . .

It is at this point that researchers invoke the idea of "pure research" as opposed to "applied research."

Practical vs. Research Problems: A Typical Beginner's Mistake

A practical problem with its tangible conditions and costs is easier for beginning researchers to understand and more interesting to study, so they are often tempted to take on as their topic a tangible problem in the world—abortion, acid rain, homelessness. That's fine, as a starting point. But you risk a mistake if you make a problem in the world the problem you try to solve in your research. No research paper can solve the problem of acid rain, but good research might give us knowledge that could help us solve it. Research problems involve only *what we don't know or fully understand*. So write your paper not to solve the problem of acid rain, but to solve the problem that *there is something about acid rain that we don't know or understand*, something that we need to know before we can deal with it.

4.2.4 Distinguishing "Pure" and "Applied" Research

In much academic writing, we don't try to explain the cost of our ignorance by showing how our research will improve the world. Rather, we show how, by not knowing or understanding one thing, we and our readers cannot understand some *larger and more important matter that we have an interest in understanding better*. When the solution to a research problem has no apparent application to a practical problem, but only to the scholarly interests of a community of researchers, we call that research "pure" as opposed to "applied."

For example, none of your three authors knows how many stars are in the sky (or how much "dark matter"), and, candidly, we don't feel bad about not knowing. We wouldn't mind knowing, but we can't think of any cost if we never find out, or any benefit if we do. And so for us, not knowing is no problem.

But for astronomers, *their* not knowing that number is part of a "pure" research problem of great significance *to them*. Until they know that quantity, they can't calculate another that is much more important—the total mass of the universe. If they could calculate the mass of the universe, they might discover something *more important still*: whether it will keep expanding until it peters out into oblivion, collapse back on itself to explode again into a new universe, or settle into an eternally steady state. Knowing the number of stars in the sky may not help solve any tangible problem in the

world, but for those astronomers (and maybe some theologians), that number represents a gap in their knowledge that exacts a great cost: it keeps them from understanding something more significant—the future of the universe. (Of course, if you have an interest in knowing whether the universe has a future, then perhaps you can see how not knowing how many stars are in the sky could be part of a problem for you as well.)

You can tell whether a research problem is pure or applied by looking at the last of the three steps in defining your project:

Pure Research Problem:

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. *Rationale:* in order to understand whether the universe will expand forever or contract into a new Big Bang.

This is a *research* problem because its question (step 2) implies that we do not know something. This is a *pure* research problem because its rationale (step 3) implies not something that we will do, but something we do not know but should.

In an *applied* research problem, the question still implies something we want to know, but the rationale in step 3 implies something we want or need to *do*:

Applied Research Problem:

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 the best earthbound telescopes.
2. *Question:* because I want to find out how much the atmosphere distorts measurements of light and other electromagnetic radiation.
3. *Rationale:* in order to *measure more accurately the density of light and other electromagnetic radiation in a small section of the universe.*

4.2.5 Is Your Problem Pure or Applied?

You distinguish between a pure and applied research problem by the consequences you name in the statement of its rationale

(step 3). In pure research, the consequences are conceptual and the rationale defines what you want to *know*; in applied research, the consequences are tangible and the rationale defines what you want to *do*.

Perhaps one of the biggest reasons beginners have a hard time getting the hang of pure research is that its costs are entirely conceptual, and so it seems to them less like curing cancer and more like counting stars. Feeling that their findings aren't good for much, they try to cobble the solution of a research problem onto the solution of a practical problem:

If we can understand how politicians used stories about the Alamo to shape opinion in the nineteenth century, we could protect ourselves from unscrupulous politicians and be better voters today.

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. *Rationale:* in order to help people protect themselves from unscrupulous politicians and become better voters.

In some areas this is a respectable strategy, some would say a preferable one. But in our example, the writer is unlikely to convince many readers that his research on the Alamo stories can in fact improve democracy.

In order to formulate an effective applied research problem, you have to show that the rationale named in step 3 is plausibly connected to the question named in step 2. You can test this by working back from the rationale. 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 the way to do that would be to find out [state your question here from Step 2]?*

The more strongly your readers would answer "yes" to your question, the more effectively you have formulated the applied problem.

Try this test on the applied astronomy problem:

(a) *If my readers want to measure more accurately the density of electromagnetic radiation in a section of the universe,*

(b) *would they think that the way to do that would be to find out how much the atmosphere distorts measurements of it?*

Since astronomers have decades worth of data collected from high-powered telescopes on earth, their answer would seem to be *Yes*: if they can discover how much the atmosphere distorts readings, they could adjust all of their data accordingly.

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 and be better voters,*

(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?*

In this case, readers would have a harder time seeing a connection between the goal and the research. A researcher who wanted to help voters protect themselves might think of other courses of action before he turned to nineteenth-century stories of the Alamo.

A reader might think that the following question defines a good research problem, but one that is pure rather than applied:

1. *Topic*: I am studying differences among 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. *Rationale*: in order to show how politicians use elements of popular culture to advance their political goals.

At the heart of most research in the humanities and much in the sciences and social sciences are questions whose answers have no direct application to daily life. In fact, in many traditional disciplines, researchers value pure research more than they value applied research—as the word “pure” suggests. They see the pursuit of knowledge “for its own sake” as reflecting humanity’s highest calling—to know more and understand better, not for the sake of money or power, but for the sake of the good that understanding itself can bring.

If you pose a question of pure research as though you could directly apply its answer to a practical problem, your readers may think you naïve. When you pose such a question and you want to discuss the tangible consequences of its answer, formulate your problem as the pure research problem that it really is and then *add* to that problem a further possible significance:

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. *Rationale*: in order to understand how politicians use elements of popular culture to advance their political goals,
4. *Significance*: so that we will know more about protecting ourselves from unscrupulous politicians and become better citizens.

If your project is more pure than applied but you still believe that it has indirect tangible consequences, you should say so. But when you state your problem in your introduction (see Chapter 15), formulate it as a pure research problem whose rationale is based on conceptual consequences; save the possible tangible consequences for your conclusion (see Quick Tip, pp. 252–53).

4.3 FINDING A RESEARCH PROBLEM

What distinguishes great researchers from the rest of us is the brilliance, the knack, or just the good luck of stumbling upon a problem whose solution makes everyone see the world in a new way. Fortunately, the rest of us can usually recognize a good problem when we bump into it, or it into us. As paradoxical as it may seem, though, most of us begin a research project not entirely certain of what our problem is, and sometimes just clarifying the problem will be our major result. Some of the best research papers do no more than pose an important new problem in search of a solution. Indeed, finding a new problem or even clarifying an old one is often a surer way to fame and (sometimes) fortune than solving a problem already there. So do not be discouraged if you cannot formulate your problem fully at the outset of your research. Remember, though, that thinking about it early can save you wasted hours along the way and especially 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 when they are not clear about the problem they think they are investigating: talk to people. Talk to your teachers, relatives, friends, neighbors—anyone who

might be interested in your topic and your question. Why would anyone need to answer your question? What would they do with an answer? What further questions might your answer raise?

If you are free to select your own topic, you might look for one that is part of a larger problem in your field. You will be unlikely to solve it, but if you can slice off a piece of it, your project will inherit some of its significance. (You will also be educating yourself about the problems of your field, no small dividend.) Ask your instructor what he is working on and ask to work on part of it.

A warning: If your teacher helps you define your problem *before* you begin your research and gives you leads on sources, do not let those suggestions define the limits of your effort. You must find other sources, bring something of your own to the definition of the problem. Nothing more dismays a teacher than a student who does exactly what was suggested, *and nothing more*.

4.3.2 Look for Problems as You Read

You can often find a research problem if you read critically. As you read a source, where do *you* feel contradictions, inconsistencies, incomplete explanations? Where do you wish a source had been more explicit, offered more information? If you are not satisfied with an explanation, if something seems odd, confused, or incomplete, tentatively assume that other readers would or should feel that way too. Experienced researchers have the confidence to assume that when they read a passage that they do not entirely understand, then something is wrong, not with them, but with what they are reading. In fact, when they cannot quite grasp something, they predictably assume that their source is wrong and that they may have found a new problem: an error, discrepancy, or inconsistency that they can correct.

Of course, you *may* be the one who is wrong, so if you make your disagreement the center of your project, re-read your source to be sure you understand it. The problem may have been resolved in a way that your source did not state. Research papers, published and unpublished, are full of useless refutations of a point never made in the first place.

Once you think you have found a real puzzle or error, try to do more than merely point it out. If a source says X and you think

Y, you have a research problem only if you can show that readers who go on believing X will misunderstand something more important yet.

Finally, read the last few pages of your sources closely. It is there that many researchers suggest more questions that need answers, more problems in search of a solution. The author of the following paragraph had just finished explaining how the daily life of the nineteenth-century Russian peasant influenced his military performance.

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].*

4.3.3 Look for Problems in What You Write

There is another way that critical reading can help you discover and formulate a good research problem: you can read your own early drafts *critically*. When you draft, you almost always do your best thinking close to the end, in the last few pages. It is then that you begin to formulate your final claim, which can often be turned into the solution to a research problem that you have not yet completely formulated.

When you finish your first draft (we may seem to be getting ahead of ourselves here, but we warned you that doing research was not a neatly linear process), you should look closely at your last two or three pages.

1. Look first for the main point of your paper, the sentence or two that would stand as your most important claim.
2. Next look for signs that your point has resolved a puzzle, settled conflicting opinions, revealed something not previously known.
3. Now try to ask a complicated question that your main point would plausibly answer. That question should define the

condition of ignorance or misunderstanding that, lacking your answer, you and your readers will continue to suffer.

When you can do that, you have defined the condition of your research problem, what you do not know but want to. The next step is easy: Ask *So what?* The harder step is answering. But if you can find an answer, you have successfully reasoned backward from your solution to a full statement of the problem you have solved (we return to this process in Chapter 15).

4.3.4 Use a Standard Problem

Every problem is different, but most problems fall into just a few categories, many defined by a researcher disagreeing or contradicting some generally held view. When you reach a point where you think you may have the outlines of a problem, look at the Quick Tip on "contradictions" after Chapter 8. You may recognize in that list a kind of problem you can work toward.

4.4 THE PROBLEM OF THE PROBLEM

Your teachers understand that you are not a professional, but they believe it important that you develop and practice the habits of mind of a serious researcher. They want to see you do more than just accumulate facts about a topic, then summarize and report them. They want you to formulate a problem that you (and perhaps even they) have a stake in seeing solved. You take your first step toward real research when you recognize a question that is significant to *you*, a question that you want to answer just for your own satisfaction, to satisfy your own desire to know more, to resolve a discrepancy, to settle a contradiction, regardless of whether anyone else cares. If you can do that much in your earliest research, if you can find some puzzle that you *care* about resolving, you have achieved something quite significant that will gratify your teachers.

Eventually, though, as you move on to advanced work, when you decide that you have reason to share your new knowledge and understanding with others, you will have to take this next step: You must try to understand what *your readers* consider interesting questions and problems, the costs *they* perceive resulting from a gap in *their* knowledge or flaw in *their* understanding. You take the biggest step of all when you not only know the kind of problem that your readers like to see solved, but can persuade them to

entertain problems of a new kind. No one ever takes all three steps the first time out.

To work your way through all of this, you can use the three-steps we discussed in the last chapter. We change the language from *discover* to *show* and *understand* to *explain*, but the second and third steps still implicitly define your problem:

1. *Name your topic:*
I am writing about _____.
2. *State your indirect question (and thereby define the condition of your problem):*
... because I am trying to show you who/how/why _____.
3. *State how your answer will help your reader understand something more important yet (and thereby define the cost of not knowing the answer):*
... in order to explain to you how/why _____.

All this may seem disconnected from the real world, but it is not. Research problems in the world at large are structured *exactly* as they are in the academic world. In business and government, in law and medicine, no skill is more highly valued than the ability to recognize a problem important to a client, employer, or the public, and then to pose that problem in a way that convinces readers that the problem you have discovered is important to *them* and that you have found its solution. The work you are doing now is your best opportunity to prepare for the kind of work that you will have to do, at least if you hope to thrive in a world that depends not just on problem solving but on problem finding. To that end, no skill is more useful than the ability to recognize and articulate a problem clearly and concisely, an ability in some ways even more important than solving it. If you can do that in a class in medieval Chinese history, you can do it in a business or government office downtown.

