The Craft of Research

THIRD EDITION

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CHAPTER THREE

From Topics to Questions

In this chapter we discuss how to find a topic among your interests, narrow it to a manageable scope, then question it to find the makings of a problem that can guide your research. If you are an experienced researcher or know the topic you want to pursue, 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 might seem frustrating—so many choices, so little time. At some point, you have to settle on a topic. But you can't jump from picking a topic to collecting data: your readers want more than a mound of random facts. You have to find a reason better than a class assignment not only for you to devote weeks or months to your research, but for your readers to spend any time reading about it. You'll find that better reason when you can ask a *question* whose answer solves a *problem* that you can convince readers to care about. That question and problem are what will make readers think your report is worth their time. They also focus your research and save you from collecting irrelevant data.

In all research communities, some questions are "in the air," widely debated and researched, such as whether traits like shyness or an attraction to risk are learned or genetically inherited. But other questions may intrigue only the researcher: *Why do cats rub their faces against us? Why does a coffee spill dry up in the shape of a ring?* That's how a lot of research begins—not with a big question that attracts everyone in a field, but with a mental itch about a small one that only a single researcher wants to scratch. If you feel that itch, start scratching. But at some point, you must decide whether the answer to your question solves a problem significant to a teacher, to other researchers, or even to a public whose lives your research could change.

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 the next chapter to it. But before you can frame your research problem, you have to find a topic that might lead to one. So we'll start there, with finding a topic.

QUESTION OR PROBLEM?

You may have noticed that we've been using the words *question* and *problem* almost interchangeably. But they are not quite the same. Some questions raise problems; others do not. A question raises a problem if not answering it keeps us from knowing something more important than its answer. For example, if we cannot answer the question *Are there ultimate particles?* we cannot know something even more important: the nature of physical existence. On the other hand, a question does not raise a problem if not answering it has no apparent consequences. For example, *Was Abraham Lincoln's right thumb longer than his nose?* We cannot think of what would we gain by knowing. At least at the moment.

3.1 FROM AN INTEREST TO A TOPIC

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

If you can work on any topic, we offer only a cliché: start with what most interests you. Nothing contributes to the quality of your work more than your commitment to it.

3.1.1 Finding a Topic in a General Writing Course

Start by listing as many interests as you can that you'd like to explore. Don't limit yourself to what you think might interest a teacher or make him think you're a serious student. Let your ideas flow. Prime the pump by asking friends, classmates, even your teacher about topics that interest them. If no good topics come to mind, consult the Quick Tip at the end of this chapter.

Once you have a list of topics, choose the one or two that interest you most. Then do this:

- In the library, look up your topic in a general bibliography such as the *Readers' Guide to Periodical Literature* and skim the subheadings. If you have a more narrow focus, look into specialized guides such as the *American Humanities Index*. Most libraries have copies on the shelf; many subscribe to their online equivalents, but not all of them let you skim subject headings. (We discuss these resources in chapter 5 and list several in the appendix.)
- On the Internet, Google your topic, but don't surf indiscriminately. Look first for Web sites that are roughly like sources you would find in a library, such as online encyclopedias. Read the entry on your general topic, and then copy the list of references at the end for a closer look. Use Wikipedia to find ideas and sources, but always confirm what you find in a reliable source. Few experienced researchers trust Wikipedia, so *under no circumstances cite it as a source of evidence* (unless your topic is the Wikipedia itself).
- You can also find ideas in blogs, which discuss almost every contentious issue, usually ones too big for a research paper. But look for posts that take a position on narrow aspects of the larger issues: if you disagree with a view, investigate it.

3.1.2 Finding a Topic for a First Research Project in a Particular Field

Start by listing topics relevant to your particular class *and* that interest you, then narrow them to one or two promising ones. If the topic is general, such as *religious masks*, you'll have to do some random reading to narrow it. But read with a plan:

- Skim encyclopedia entries in your library or online. Start with standard ones such as the *Encyclopaedia Britannica*. Then consult specialized ones such as the *Encyclopedia of Religion* or the *Stanford Encyclopedia of Philosophy*.

- Skim headings in specialized indexes, such as the *Philosopher's Index*, *Psychological Abstracts*, or *Women's Studies Abstracts*. Use subheadings for ideas of how others have narrowed your topic.
- Google your topic, but not indiscriminately. Use Google Scholar, a search engine that focuses on scholarly journals and books. Skim the articles it turns up, especially their lists of sources.

When you know the general outline of your topic and how others have narrowed it, try to narrow yours. If you can't, browse through journals and Web sites until it becomes more clearly defined. That takes time, so start early.

3.1.3 Finding a Topic for an Advanced Project

Most advanced students already have interests in topics relevant to their field. If you don't, focus on what interests you, but remember that you must eventually show why it should also interest others.

- Find what interests other researchers. Look online for recurring issues and debates in the archives of professional discussion lists relevant to your interests. Search online and in journals like the *Chronicle of Higher Education* for conference announcements, conference programs, calls for papers, anything that reflects what others find interesting.
- Skim the latest issues of journals on your library's new arrivals shelf, not just for articles, but also for conference announcements, calls for papers, and reviews. Skim the most recent articles in your library's online database.
- Investigate the resources that your library is particularly rich in. If, for example, it (or one nearby) holds a collection of rare papers on an interesting topic, you have not only found a topic but a way into it. Before you settle on a topic, on the other

hand, be sure your library has at least some relevant sources. If not, you may have to start over.

3.2 FROM A BROAD TOPIC TO A FOCUSED ONE

At this point, your biggest risk is settling on a topic so broad that it could be a subheading in a library catalog: *spaceflight*; *Shakespeare's problem plays*; *natural law*. A topic is probably too broad if you can state it in four or five words:

Free will in Tolstoy

The history of commercial aviation

A topic so broad can intimidate you with the task of finding, much less reading, even a fraction of the sources available. So narrow it:

Free will in Tolstoy	→	The conflict of free will and inevitability in Tolstoy's description of three battles in *War and Peace*
The history of commercial aviation	→	The contribution of the military in developing 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 *developing*. Those nouns are derived from verbs expressing actions or relationships: *to conflict*, *to describe*, *to contribute*, and *to develop*. Lacking such "action" words, your topic is a static thing.

Note what happens when we restate static topics as full sentences. Topics (1) and (2) change almost not at all:

(1) Free will in Tolstoy$_{topic}$ → There is free will in Tolstoy's novels.$_{claim}$

(2) The history of commercial aviation$_{topic}$ → Commercial aviation has a history.$_{claim}$

But when (3) and (4) are revised into full sentences, they are closer to claims that a reader might find interesting.

(3) The *conflict* of free will and inevitability in Tolstoy's *description* of three battles in *War and Peace*$_{topic}$ → In *War and Peace*, Tolstoy *describes* three battles in which free will and inevitability *conflict*.$_{claim}$

(4) The *contribution* of the military in *developing* the DC-3 in the early years of commercial aviation$_{topic}$ → In the early years of commercial aviation, the military *contributed* to the way the DC-3 *developed*.$_{claim}$

Such claims may at first seem thin, but you'll make them richer as you work through your project.

Caution: Don't narrow your topic so much that you can't find data on it. Too many data are available on *the history of commercial aviation* but too few (at least for beginning researchers) on *the decision to lengthen the wingtips on the DC-3 prototype for military use as a cargo carrier.*

3.3 FROM A FOCUSED TOPIC TO QUESTIONS

Once they have a focused topic, many new researchers make a beginner's mistake: they immediately start plowing through all the sources they can find on a topic, taking notes on everything they read. With a promising topic such as *the political origins of legends about the Battle of the Alamo*, they mound up endless facts connected with the battle: what led up to it, histories of the Texas Revolution, the floor plan of the mission, even biographies of generals Santa Anna and Sam Houston. They accumulate notes, summaries, descriptions of differences and similarities, ways in which the stories conflict with one another and with what historians think really happened, and so on. Then they dump it all into a report that concludes, *Thus we see many differences and similarities between . . .*

Many high school teachers would reward such a report with a good grade, because it shows that the writer 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* college course, such a report falls short if it is seen as just a pastiche of vaguely related facts. If a writer asks no specific *question* worth asking, he can offer no specific *answer* worth supporting. And without an answer to support, he cannot select from all the data he *could* find on a topic just those relevant to his answer. To be sure, those fascinated by Elvis Presley movie posters or early Danish anthropological films will read *anything* new about them, no matter how trivial. 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.

So the best way to begin working on your specific topic is not to find all the data you can on your general topic, but to formulate questions that point you to just those data that you need to answer them.

You can start with the standard journalistic questions: *who, what, when,* and *where,* but focus on *how* and *why*. To engage your best critical thinking, systematically ask questions about your topic's history, composition, and categories. Then ask any other question you can think of or find in your sources. Record all the questions, but don't stop to answer them even when one or two grab your attention. (And don't worry about keeping these categories straight; their only purpose is to stimulate questions and organize your answers.) Let's take up the example of masks mentioned earlier.

3.3.1 Ask about the History of Your Topic

- How does it fit into a **larger developmental context**? Why did your topic come into being? *What came before masks? How were masks invented? Why? What might come after masks?*
- What is its own **internal history**? How and why has the topic itself changed through time? *How have Native American masks changed? Why? How have Halloween masks changed? How has the role of masks in society changed? How has the booming market for kachina masks influenced traditional design? Why have masks helped make Halloween the biggest American holiday after Christmas?*

3.3.2 Ask about Its Structure and Composition

- How does your topic fit into the **context of a larger structure or function as part of a larger system**? *How do masks reflect the values of different societies and cultures? What roles do masks play in Hopi dances? In scary movies? In masquerade parties? How are masks used other than for disguise?*
- How do its parts **fit together as a system**? *What parts of a mask are most significant in Hopi ceremonies? Why? Why do some masks cover only the eyes? Why do few masks cover just the bottom half of the face? How do their colors play a role in their function?*

3.3.3 Ask How Your Topic Is Categorized

- How can your topic be **grouped into kinds**? *What are the different kinds of masks? Of Halloween masks? Of African masks? How are they categorized by appearance? By use? By geography or society? What are the different qualities of masks?*
- How does your topic **compare to and contrast with** others like it? *How do Native American ceremonial masks differ from those in Japan? How do Halloween masks compare with Mardi Gras masks?*

3.3.4 Turn Positive Questions into Negative Ones

- *Why have masks* not *become a part of other holidays, like President's Day or Memorial Day? How do Native American masks* not *differ from those in Africa? What parts of masks are typically* not *significant in religious ceremonies?*

3.3.5 Ask *What If?* and Other Speculative Questions

- How would things be different if your topic never existed, disappeared, or were put into a new context? *What if no one ever wore masks except for safety? What if everyone wore masks in public? What if it were customary to wear masks on blind dates? In marriage ceremonies? At funerals? Why are masks common* *in African religions but not in Western ones? Why don't hunters in camouflage wear masks? How are masks and cosmetic surgery alike?*

3.3.6 Ask Questions Suggested by Your Sources

You won't be able to do this until you've done some reading on your topic. Ask questions that **build on agreement**:

- If a source makes a claim you think is persuasive, ask questions that might extend its reach. *Elias shows that masked balls became popular in eighteenth-century London in response to anxieties about social mobility. Did the same anxieties cause similar developments in Venice?*
- Ask questions that might support the same claim with new evidence. *Elias supports his claim about masked balls with published sources. Is it also supported by letters and diaries?*
- Ask questions analogous to those that sources have asked about similar topics. *Smith analyzes costumes from an economic point of view. What would an economic analysis of masks turn up?*

Now ask questions that reflect **disagreement**:

- *Martinez claims that carnival masks uniquely allow wearers to escape social norms. But could there be a larger pattern of all masks creating a sense of alternative forms of social or spiritual life?*

(We discuss in more detail how to use disagreements with sources in 6.4.)

If you are an experienced researcher, look for questions that other researchers ask but don't answer. Many journal articles end with a paragraph or two about open questions, ideas for more research, and so on (see p. 63 for an example). You might not be able to do all the research they suggest, but you might carve out a piece of it. You can also look for Internet discussions on your topic, then "lurk," just reading the exchanges to understand the kinds of questions those on the list debate. Record questions that spark your interest. You can post questions on the list if they are specific and narrowly focused. But first see whether the list welcomes questions from students. (If you can't find a list using a search engine, ask a teacher or visit the Web site of professional organizations in your field.)

3.3.7 Evaluate Your Questions

When you run out of questions, evaluate them, because not all questions are equally good. Look for questions whose answers might make you (and, ideally, your readers) think about your topic in a new way. Avoid questions like these:

- Their answers are settled fact that you could just look up. *Do the Inuit use masks in their wedding ceremonies?* Questions that ask *how* and *why* invite deeper thinking than *who, what, when,* or *where,* and deeper thinking leads to more interesting answers.
- Their answers would be merely speculative. *Would church services be as well attended if the congregation all wore masks?* If you can't imagine finding hard data that might settle the question, it's a question you can't settle.
- Their answers are dead ends. *How many black cats slept in the Alamo the night before the battle?* It is hard to see how an answer would help us think about any larger issue worth understanding better, so it's a question that's probably not worth asking.

You might, however, be wrong about that. Some questions that seemed trivial, even silly, have answers more significant than expected. One researcher wondered why a coffee spill dries up in the form of a ring and discovered things about the properties of fluids that others in his field thought important—and that paint manufacturers found valuable. So who knows where a question about cats in the Alamo might take you? You can't know until you get there.

Once you have a few promising questions, try to combine them into larger ones. For example, many questions about the Alamo story ask about the interests of the storytellers and their effects on their stories: *How have politicians used the story? How have the storytellers' motives changed? Whose purposes does each story serve?* These can be combined into a single more significant question:

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

With only a topic to guide your research, you can find endless data and will never know when you have enough (much less what to do with it). To go beyond fact-grubbing, find a question that will narrow your search to just those data you need to answer it.

3.4 FROM A QUESTION TO ITS SIGNIFICANCE

Even if you are an experienced researcher, you might not be able to take the next step until you are well into your project, and if you are a beginner, you may find it deeply frustrating. Even so, once you have a question that holds your interest, you must pose a tougher one *about* it: *So what? Beyond your own interest in its answer, why would others think it a question worth asking?* You might not be able to answer that *So what?* question early on, but it's one you have to start thinking about, because it forces you to look beyond your own interests to consider how your work might strike others.

Think of it like this: What will be lost if you *don't* answer your question? How will *not* answering it keep us from understanding something else better than we do? Start by asking *So what?* at first of yourself:

> So what if I don't know or understand how butterflies know where to go in the winter, or how fifteenth-century musicians tuned their instruments, or why the Alamo story has become a myth? So what if I can't answer my question? What do we lose?

Your answer might be *Nothing. I just want to know.* Good enough to start, but not to finish, because eventually your readers will ask as well, and they will want an answer beyond *Just curious.* Answering *So what?* vexes all researchers, beginners and experienced alike, because when you have only a question, it's hard to predict whether others will think its answer is significant. But you must work toward that answer throughout your project. You can do that in three steps.

3.4.1 Step 1: Name Your Topic

If you are beginning a project with only a topic and maybe the glimmerings of a good question or two, start by naming your project:

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

Fill in the blank with your topic, using some of those nouns derived from verbs:

> I am studying the *causes* of the *disappearance* of large North American mammals . . .

> I am working on Lincoln's *beliefs* about *predestination* and their *influence* on his *reasoning* . . .

3.4.2 Step 2: Add an Indirect Question

Add an indirect question that indicates what you do not know or understand about your topic:

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

1. I am studying the causes of the disappearance of large North American mammals
   2. **because I want to find out whether they were hunted to extinction . . .**

1. I am working on Lincoln's beliefs about predestination and its influence on his reasoning
   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/whether* clause, you state why *you* are pursuing your topic: to answer a question important to you.

If you are a new researcher and get this far, congratulate yourself, because you have moved beyond the aimless collection of data. But now, if you can, take one step more. It's one that advanced researchers know they must take, because they know their work will be judged not by its significance to them but by its significance to others in their field. They must have an answer to *So what?*

3.4.3 Step 3: Answer *So What?* by Motivating Your Question

This step tells you whether your question might interest not just you but others. To do that, add a second indirect question that explains why you asked your first question. Introduce this second implied question with *in order to help my reader understand how, why, or whether*:

1. I am studying the causes of the disappearance of large North American mammals
    2. because I want to find out whether the earliest peoples hunted them to extinction
        3. **in order to help my reader understand whether native peoples lived in harmony with nature or helped destroy it.**

1. I am working on Lincoln's beliefs about predestination and their influence on his reasoning
    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 is the indirect question in step 3 that you hope will seize your readers' interest. If it touches on issues important to your field, even indirectly, then your readers should care about its answer.

Some advanced researchers begin with questions that others in their field already care about: *Why did the giant sloth and woolly mammoth disappear from North America?* Or: *Is risk taking genetically based?* But many researchers, including at times the three of us, find that they can't flesh out the last step in that three-part sentence until they finish a first draft. So you make no mistake *begin-*

ning your research without a good answer to that third question—*Why does this matter?*—but you face a problem when you *finish* it without having thought through those three steps at all. And if you are doing advanced research, you *must* take that step, because answering that last question is your ticket into the conversation of your community of researchers.

Regularly test your progress by asking a roommate, relative, or friend to force you 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—*I am working on the topic of . . .*
   2. what you don't know about it—*because I want to find out . . .*
      3. why you want your reader to know and care about it—*in order to help my reader understand better . . .*

In the following chapters, we return to those three steps and their implied questions, because they are crucial not just for finding questions, but for framing the research problem that you want your readers to value.

QUICK TIP: *Finding Topics*

If you are a beginner, start with our suggestions about skimming bibliographical guides (3.1). 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 travel? Surf the Internet, finding out all you can about your destination. 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, old cars. If you can't browse 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. 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 news reader, look through the list of "alt" newsgroups for one that interests you. 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 Web sites connected with well-known talk shows. See whether you can make a case to refute it.

9. Use an Internet search engine to find Web sites 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 simplistic or just wrong? A common practice that you find pointless or irritating? Do research to make a case against it.

11. What courses will you take in the future? What research would help you prepare for them?

FOR TOPICS FOCUSED ON A PARTICULAR FIELD

If you have experience in your field, review 3.1.2–3.

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. 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 issues 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 Web sites of departments at major universities, including class sites. Also check sites of museums, national associations, and government agencies, if they seem relevant.

CHAPTER FOUR

From Questions to a Problem

In this chapter we explain how to turn a question into a problem that readers think is worth solving. If you are an advanced researcher, you know how essential this step is. But if you are new to research, understanding its importance may prove challenging. If you feel lost, skip to chapter 5, but we hope you'll stay with us, because what you learn here will be essential to all your future projects.

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

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

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

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

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

4.1 DISTINGUISHING PRACTICAL AND RESEARCH PROBLEMS

Finding the significance of a problem is hard, even for experienced researchers. Too many researchers at all levels write as if their only task is to answer a question that interests them alone. They fail to understand that their answer must solve a problem that others in their community think needs a solution. To understand how to find that question and its significance, though, you first have to know what research problems look like.

4.1.1 Practical Problems: What Should We Do?

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

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

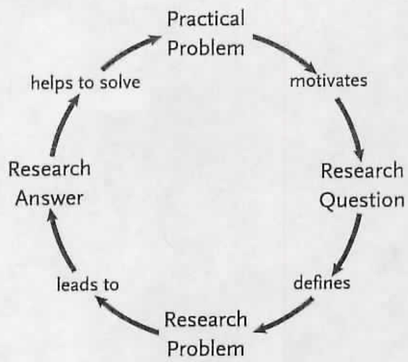
PRACTICAL PROBLEM: My brakes are screeching.
RESEARCH PROBLEM: Can I find a brake shop in the yellow pages to fix them?
RESEARCH SOLUTION: Here it is. The Car Shoppe, 1401 East 55th Street.
PRACTICAL SOLUTION: Drive over to get them fixed.

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

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

To solve either of those *practical* problems, someone first had to solve a research problem that improved their *understanding*. Then on the basis of that better understanding, someone had to decide what to *do* to solve the practical problem, then report their research so that their solution could be shared and studied.

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



4.1.2 Academic Research Problems: What Should We Think?

Solving a practical problem usually requires that we first solve a research problem, but it's crucial to distinguish *practical* research problems from *conceptual* ones:

- A *practical* problem is caused by some condition in the world (from spam to losing money in Omaha to terrorism) that makes us unhappy because it costs us time, respect, security, pain, even our lives. We solve a practical problem by *doing* something (or by encouraging others to do something) that eliminates the cause of the problem or at least ameliorates its costs.
- In academic research, a *conceptual* problem arises when we simply do not *understand* something about the world as well as we would like. We solve a conceptual problem not by doing something to change the world but by answering a question that helps us understand it better.

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

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

When they do, they create a big practical problem for themselves, because without a research question to answer, with only a topic to guide their work, 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, usually throwing in everything, just to be on the safe side. So it's not surprising they feel frustrated when a reader says of their report, *I don't see the point here; this is just a data dump.*

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

4.2 UNDERSTANDING THE COMMON STRUCTURE OF PROBLEMS

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

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

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

4.2.1 The Nature of Practical Problems

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

To be part of a practical, tangible problem, a condition can be anything, so long as it imposes intolerable costs. Suppose you win a million dollars in the lottery 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 the lottery turns out to be a Big Problem.

To state a practical problem so that others understand it clearly, you must describe both its parts.

1. Its condition:

   I missed the bus.

   The hole in the ozone layer is growing.

2. The **costs** of that condition that make you (or your reader) unhappy:

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

   Many will die from skin cancer.

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

The hole in the ozone layer is growing.
So what?

You answer with the cost of the problem:

A bigger hole exposes us to more ultraviolet light.

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

Too much ultraviolet light can cause skin cancer.

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

Practical problems like cancer are easy to grasp because when people have it, we don't ask *So what?* In academic research, however, your problems will usually be conceptual ones, which are harder to grasp because both their conditions and costs are not palpable but abstract.

4.2.2 The Nature of Conceptual Problems

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

- The condition of a practical problem can be *any* state of affairs whose cost makes you (or better, your reader) unhappy.
- The condition of a conceptual problem, however, is *always* some version of *not knowing* or *not understanding* something.

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

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

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

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

- The **cost** of a practical problem is always some degree of unhappiness.

A conceptual problem does not have such a tangible cost. In fact, we'll call it not a cost but a *consequence.*

- The **consequence** of a conceptual problem is a *second* thing that we don't know or understand because we don't understand the first one, *and that is more significant, more consequential than the first.*

You express that bigger lack of understanding in the indirect question in step 3 of that formula:

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

All this may sound confusing, but it's simpler than it seems. The condition and the consequence of a conceptual problem are both questions: Q1 and Q2. But there are two differences: (1) the answer to the first question helps you answer the second, and (2) the answer to the second question is more important than the answer to the first.

Q1 *helps you answer* → **Q2**

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

Problem + Significance

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

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

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

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

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

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

Those outside an academic field often think that its specialists ask ridiculously trivial questions: *How did hopscotch originate?* But they fail to realize that researchers want to answer a question like that so that they can answer a *second*, more important one. For those who care about the way folk games influence the social development of children, the conceptual consequences of not knowing justifies the research. *If we can discover how children's folk games originate, we can better understand how games socialize children, and before you ask, once we know that, we can better understand . . .*

4.2.3 Distinguishing "Pure" and "Applied" Research

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

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

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

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

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

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

4.2.4 Connecting a Research Problem to Practical Consequences

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

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

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

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

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

Try that test on this applied astronomy problem:

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

The answer would seem to be *Yes*.

Now try the test on the Alamo problem:

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

We may see a connection, but it's a stretch.

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

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

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

Most research projects in the humanities and many in the natural and social sciences have no direct application to daily life. But as the term *pure* suggests, many researchers value such research more than they do applied. They believe that the pursuit of knowledge "for its own sake" reflects humanity's highest calling—to know more, not for the sake of money or power, but for the transcendental good of greater understanding and a richer life of the mind. As you may have guessed, the three of us are deeply committed to pure research, but also to applied—so long as the research is done well and is not corrupted by malign motives. For example, there is a threat to both pure and applied research in the biological sciences, where profits not only determine the choice of some research problems, but color how some researchers reach their solutions: *Tell us what to look for, and we'll provide it!* That raises an ethical question that we touch on in our afterword on ethics.

"Pure" applied research is the humanities

ANTICIPATING A TYPICAL BEGINNER'S MISTAKE

No one can solve the world's great problems in a five- or even a fifty-page paper. But you might help us better understand a *small part* of one, and that can move us closer to a practical solution. So if you care deeply about a practical problem, such as destructive forest fires, carve out of it a conceptual question that is small enough to answer but whose answer 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 spread of forest fires?* The right answer to a small question moves us closer to solving a big problem than a big answer that doesn't work.

4.3 FINDING A GOOD RESEARCH PROBLEM

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

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

4.3.1 Ask for Help

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

If you are free to work on any problem, look for a small one that is part of a bigger one. Though you won't solve the big one, your small piece of it will inherit some of its larger significance. (You will also educate yourself about the problems of your field, no small benefit.) Ask your teacher what she is working on and whether you can work on part of it. But a warning: Don't let her suggestions define the limits of your research. Nothing discourages a teacher more than a student who does *exactly* what is suggested *and no more.* Teachers want you to use their suggestions to *start* your thinking, not *end* it. Nothing makes a teacher happier than when you use her suggestions to find something she never expected.

4.3.2 Look for Problems as You Read

You can also find a research problem in your sources. Where in them do you see contradictions, inconsistencies, incomplete explanations? Tentatively assume that other readers would or should feel the same. Many research projects begin with an imaginary conversation while reading another's report: *Wait a minute, he's ignoring . . .* But before you set out to correct a gap or misunderstanding, be sure it's real, not your own misreading. Countless research papers have refuted a point that no one ever made. Before you correct a source, reread it carefully. (In 6.4 we list several common "moves" that writers make to find a problem in a source, variations on *Source thinks X, but I think Y.*)

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

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

> And just as the soldier's peacetime experience influenced his battlefield performance, so must the experience of the officer corps have influenced theirs. Indeed, a few commentators after

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

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

4.3.3 Look at Your Own Conclusion

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

4.4 LEARNING TO WORK WITH PROBLEMS

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

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

By now, all this talk about airy academic research may seem disconnected from a world in which so many people labor so hard at getting ahead or keeping others down. But in business and government, in law and medicine, in politics and international diplomacy, no skill is valued more highly than the ability to recognize a problem, then to articulate it in a way that convinces others both to care about it and to believe it can be solved, especially by you. If you can do that in a class on medieval Tibetan rugs, you can do it in an office on Main Street, Wall Street, or on Queen's Road in Hong Kong.

QUICK TIP: *Manage the Unavoidable Problem of Inexperience*

We all feel anxious when we start work in a field whose basic rules we don't entirely understand, much less those tacit rules that experienced researchers follow but don't explain to others because they're taken for granted. And to our surprise, we feel a newcomer's anxiety again when we begin a new kind of project on a new topic. We three authors have felt those anxieties, not just starting out, but long after our hair had grayed. You can't avoid feeling overwhelmed and anxious at times, but there are ways to manage it:

- Know that uncertainty and anxiety are natural and inevitable. Those feelings don't signal incompetence, only inexperience.
- Get control over your topic by writing about it along the way. Don't just retype or photocopy sources: write summaries, critiques, questions, responses to your sources. Keep a journal in which you reflect on your progress. The more you write, no matter how sketchily, the more confidently you will face that intimidating first draft.
- Break the task into manageable steps and know that they are mutually supportive. Once you formulate a good question, you'll draft and revise more effectively. The more you anticipate how you will write and revise a first draft, the more effectively you will produce it.
- Count on your teachers to understand your struggles. They want you to succeed, and you can expect their help. (If they don't help, look for others who will.)
- Set realistic goals. You do something significant when you wind up your project feeling it has changed just what *you* think and that your readers think you did it well, even if they don't agree with your claims.

- Most important, recognize the struggle for what it is—a learning experience. To overcome the problems that all beginners face, do what successful researchers do, especially when discouraged: review your plan and what you've written, then press on, confident that it will turn out OK. Perhaps only "OK—considering," but probably a lot better than that.