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# The Craft of Research

FOURTH EDITION

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**QUICK TIP****Creating a Writing Group**

A downside of academic research is its isolation. Except for group projects, you'll read and write mostly alone. But it doesn't have to be that way. Look for someone other than your instructor or adviser who will talk with you about your progress, review your drafts, even pester you about how much you've written. That might be a generous friend, but even better is another writer so that you can comment on each other's ideas and drafts.

Best of all is a group of four or five people working on their own projects who meet regularly to read and discuss one another's work. Early on, each meeting should start with a summary of each person's project in this three-part sentence: *I'm working on X because I want to find out Y, so that I (and you) can better understand Z* (more about this in 3.4). As your projects advance, develop an opening "elevator story," a short summary of your project that you could give someone on the way to a meeting. It should include your research question, your best guess at an answer, and the kind of evidence you expect to use to support it. The group can then follow up with questions, responses, and suggestions.

Don't limit your talk to just your story, however. Talk about your readers: Why should they be interested in your question? How might they respond to your argument? Will they trust your evidence? Will they have other evidence in mind? Such questions help you plan an argument that anticipates what your readers expect. Your group can even help you brainstorm when you bog down. Later the group can read one another's outlines and drafts to imagine how their final readers will respond. If your group has a problem with your draft, so will those readers. But for most writers, a writing group is most valuable for the discipline it imposes. It is easier to meet a schedule when you know you must report to others.

Writing groups are common for those writing theses or dissertations. But the rules differ for a class paper. Some teachers think that a group or writing partner provides more help than is appropriate, so be clear what your instructor allows.

**3**

## From Topics to Questions

In this chapter, we discuss how to find a topic among your interests, refine 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 new to research, the freedom to pick your own topic can seem daunting. Where do you begin? How do you tell a good topic from a bad one? Inexperienced researchers typically wonder, *Will I find enough information on this topic to write about it?* To their surprise they often compile too much information, much of it not very useful. They do so because their topic lacks focus. Without that focus, any evidence you assemble risks appearing to your readers as little more than a mound of random facts. As you begin a research project, you will want to distinguish a topic from a subject. A subject is a broad area of knowledge (e.g., climate change), while a topic is a specific interest within that area (e.g., the effect of climate change on migratory birds). However, finding a topic is not simply a matter of narrowing your subject. A topic is an approach to a subject, one that asks a *question* whose answer solves a *problem* that your readers care about.

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 question that only a single researcher wants to scratch. If you feel that itch, start scratching. But at some point, you must decide

### **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 we would gain by knowing. At least at the moment.

whether the answer to your question solves a problem significant to some community of 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.

#### **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. They may also believe they lack the expertise for the project. However, 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 others, including your teacher. 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. But also ask yourself: *What interests me about this topic? What would interest others?*

#### **3.1.1 Finding a Topic in a 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 you look like 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 and explore their research potential. Do this:

- In the library, look up your topic in a general guide such as *CQ Researcher* and skim the subheadings. In an online database such as Academic Search Premier, you can explore your topic through subject terms. If you have a more narrow focus, you can do the same with specialized guides such as *Women's Studies International*. While some libraries will have copies of general and specialized guides on the shelf, most now 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 websites 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 there in a reliable source. Few experienced researchers trust *Wikipedia*, so *under no circumstances cite it as a source of evidence* (unless your topic is *Wikipedia* itself).
- Remember, at this point you are exploring a topic to spur your thinking and to see if that topic is viable. With that in mind, you can also find ideas in blogs, which discuss almost every contentious issue. Since most issues are usually too big for a research paper, look for posts that take a position on narrow aspects of 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 theirs, try to narrow yours. If you can't, browse through journals and websites until your topic 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. Often topics find them as they become immersed in a field. If that is not yet the case, 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 in your field, not just for articles, but also for conference announcements, calls for papers, and

reviews. Skim recent articles in your library's online databases in your field (e.g., the MLA International Bibliography).

- Investigate the resources that your library is particularly rich in. If, for example, it (or a library nearby) holds a collection of rare papers on an interesting topic, you have found not only a topic but a way into it. Many unexpected finds await discovery in your library's archives.

## 3.2 FROM A BROAD TOPIC TO A FOCUSED ONE

The most useful way to think about a topic is as a starting place for your research. (The word "topic" comes from *topos*, which is Greek for "place.") From this starting place, you can head off in a particular direction and thus narrow an overly broad topic into a productively 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 down:

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<sub>topic</sub> → There is free will in Tolstoy's novels.<sub>claim</sub>
- (2) The history of commercial aviation<sub>topic</sub> → Commercial aviation has a history.<sub>claim</sub>

In reality, (1) and (2) are not topics at all because they do not lead anywhere. 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*<sub>topic</sub> → In *War and Peace*, Tolstoy describes three battles in which free will and inevitability *conflict*.<sub>claim</sub>
- (4) The contribution of the military in developing the DC-3 in the early years of commercial aviation<sub>topic</sub> → In the early years of commercial aviation, the military *contributed* to the way the DC-3 *developed*.<sub>claim</sub>

Such claims may at first seem thin, but you'll make them richer as you work through your project. And that's the point: these topics are actually paths to pursue when devising your project.

Caution: Don't narrow your topic so much that you can't find information on it. Too much information is available on *the history of commercial aviation* but too little (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 the 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 sto-

ries conflict with one another and with what historians think really happened, and so on. Then they dump it all into a paper that concludes, *Thus we see many differences and similarities between . . .*

Many high school teachers would reward such a paper with a good grade, because it shows that the writer can focus on a topic, find information on it, and assemble that information 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 the first generation of video games will read *anything* new about them, no matter how trivial. Serious researchers, however, do not document information for its 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 focused topic is not to find all the information you can on it, but to formulate questions that direct you to just that information you need to answer them.

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. This inventory of possible questions will help to direct your search activities and enable you to make sense of information you find. (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 Presidents' 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 4.3.2 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 also post questions to the list if they are specific and narrowly focused.

### 3.3.7 Evaluate Your Questions

After asking all the questions you can think of, 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 question:

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

A question like this gives direction to your research (and helps avoid the gathering of endless information). And it begins to imagine readers who will judge whether your question is significant.

### 3.4 THE MOST SIGNIFICANT QUESTION: SO WHAT?

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 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 five 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 *beginning* your research without a good answer to that third question—*Why does this matter?*—but you face a problem when you *finish* your research 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 exploring the Internet and skimming bibliographical guides (see 3.1). If you still draw a blank, try these steps.

**FOR GENERAL INTEREST TOPICS**

- 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.
- 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?
- 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?
- Wander through a shopping mall or store, asking yourself, *How do they make that?* Or, *I wonder who thought up that product?*
- Leaf through a Sunday newspaper, especially its features sections. Skim reviews of books or movies, in newspapers or on the Internet.
- Browse a large magazine rack. Look for trade magazines or those that cater to specialized interests. Investigate whatever catches your interest.
- Tune into talk radio or interview programs on TV until you hear a claim that you disagree with. Or find something to disagree with on the websites connected with well-known talk shows. See whether you can make a case to refute it.
- Use an Internet search engine to find websites related to your topic. These include blogs maintained by individuals and organizations. You'll get hundreds of hits, but look only at the ones that surprise you.

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

#### FOR TOPICS FOCUSED ON A PARTICULAR FIELD

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

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

## 4 From Questions to a Problem

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

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

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

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

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

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

### 4.1 UNDERSTANDING RESEARCH PROBLEMS

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

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

#### 4.1.1 Practical Problems: What Should We Do?

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

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

**PRACTICAL PROBLEM:** The chain on my bicycle broke.

**RESEARCH PROBLEM:** Can I find a bike shop that will replace it?

**RESEARCH SOLUTION:** Here it is: Cycle Source, 1401 East 55th Street.

**PRACTICAL SOLUTION:** Walk over to get my bike fixed.

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

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

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

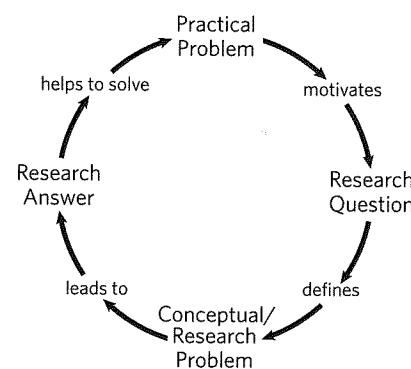
eliminate or at least mitigate the condition creating these tangible costs.

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

#### 4.1.2 Conceptual Problems: What Should We Think?

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

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



The term *problem* thus has a special meaning in the world of research, one that sometimes confuses beginners. In our everyday

world, a problem is something we try to avoid. But in academic research, a problem is something we seek out, even invent if we have to. Indeed, a researcher without a good conceptual or research problem to work on faces a bad practical problem because without one a researcher is out of work.

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

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

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

#### 4.2 UNDERSTANDING THE COMMON STRUCTURE OF PROBLEMS

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

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

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

##### 4.2.1 The Nature of Practical Problems

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

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

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

###### 1. Its condition:

I missed the bus.

The ozone layer is thinning

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

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

Many will die from skin cancer.

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

The ozone layer is thinning.

*So what?*

You answer with the cost of the problem:

A thinner ozone layer exposes us to more ultraviolet light.

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

Too much ultraviolet light can cause skin cancer.

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

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

#### 4.2.2 The Nature of Conceptual Problems

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Q 1 *helps you answer* Q 2

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

#### 4.2.4 Connecting Research to Practical Consequences

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

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

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

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

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

Try that test on this applied astronomy problem:

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

The answer would seem to be *Yes*.

Now try the test on the Alamo problem:

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

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

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

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

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

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

As you may have guessed, we are deeply committed to pure research, but also to applied—so long as the research is done well

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

#### 4.3 FINDING A GOOD RESEARCH PROBLEM

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

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

##### 4.3.1 Ask for Help

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

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

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

##### 4.3.2 Look for Problems as You Read

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

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

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

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

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

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

#### 4.3.3 Look at Your Own Conclusion

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

#### 4.4 LEARNING TO WORK WITH PROBLEMS

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

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

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

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