Chapter Four
From Questions to Problems

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

In the last chapter, we described how to find in your interests a topic, how to find in that topic questions to research, and then how to signal the significance of your answer by describing its rationale:

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

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

4.1 Problems, Problems, Problems

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

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

4.1.1 Practical Problems and Research Problems

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

> *Practical Problem: My brakes have started screeching.*
>
> *Research Question: How can I get them fixed right away?*
>
> *Research Problem: I need to find a nearby garage in the Yellow Pages.*
>
> *Research Answer: The Car Shoppe, 1401 East 55th St.*
>
> *Application to Practical Problem: Call to see when they can fix them.*

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

> *I want to impress a potential employer. How do I find a good restaurant? Look in a city guide. Woodlawn Tap. I take her there, and I hope she thinks I've got style.*
>
> *The National Rifle Association presses me to oppose gun control. Will I lose if I don't? Take a poll. My constituents support gun control. Now decide whether to reject the NRA's request.*
>
> *Costs are up at the Omaha plant. What has changed? Compare personnel before and after. More turnover now. If we improve training and morale, our workers stay with us. OK, let's see if we can afford to do it.*

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

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

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

4.1.2 Distinguishing Practical Problems and Research Problems

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

- A practical problem originates in the world and exacts a cost in money, time, happiness, etc. You solve a practical problem by changing something out there in the world, by doing something.

But before you can solve a practical problem, you may have to pose and solve a research problem.

- A research problem originates in your mind, out of incomplete knowledge or flawed understanding. You might pose a research problem because you have to solve a practical problem, but you do not solve that practical problem merely by solving the research problem. You might apply the solution of that research problem to the solution of a practical problem, but you solve your research problem not by changing anything in the world but by learning more about something or understanding it better.

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

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

4.1.3 Distinguishing Problems and Topics

There is a second reason that beginning and even intermediate researchers struggle with this notion of "problem." Experienced researchers often talk about their research problem in a shorthand way that seems to describe it just as a topic:

I'm working on adult measles, or on early Aztec pots, or on the mating calls of Wyoming elk.

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

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

4.2 The Common Structure of Problems

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

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

4.2.1 Practical Problems

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

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

- a CONDITION that needs to be resolved

Condition: I missed the bus.
The hole in the ozone layer is growing.

- COSTS of that condition that you don't want to bear

Cost: I may lose my job because I will be late for work.
Many will die from skin cancer.

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

Benefit: If 1 can catch the bus, I save my job.
If we fix the ozone hole, we save many lives.

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

For a practical, tangible problem, the condition can be literally anything even a seeming stroke of luck, if it has a cost: You win the lottery. That might not seem like a problem, but what if you owed a loan shark $5,000,000 and your name gets in the paper? Winning the lottery could then cost you more than you won: someone finds you, takes your money, and breaks your leg.

4.2.2 Research Problems

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

Conditions.

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

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

Costs.

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

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

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

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

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

4.2.3 When a Research Problem Is Motivated by a Practical Problem

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

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

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

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

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

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

4.2.4 Distinguishing "Pure" and "Applied" Research

In much academic writing, we don't try to explain the cost of our ignorance by showing how our research will improve the world. Rather, we show how, by not

knowing or understanding one thing, we and our readers cannot understand some larger and more important matter that we have an interest in understanding better. When the solution to a research problem has no apparent application to a practical problem, but only to the scholarly interests of a community of researchers, we call that research "pure" as opposed to "applied."

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

But for astronomers, their not knowing that number is part of a "pure" research problem of great significance to them. Until they know that quantity, they can't calculate another that is much more important—the total mass of the universe. If they could calculate the mass of the universe, they might discover something more important still: whether it will keep expanding until it peters out into oblivion, collapse back on itself to explode again into a new universe, or settle into an eternally steady state. Knowing the number of stars in the sky may not help solve any tangible problem in the world, but for those astronomers (and maybe some theologians), that number represents a gap in their knowledge that exacts a great cost: it keeps them from understanding something more significant—the future of existence. (Of course, if you have an interest in knowing whether existence has a future, then perhaps you can see how not knowing how many stars are in the sky could be part of a problem for you as well.)

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

Pure Research Problem:

> Topic: I am studying the density of light and other electromagnetic radiation in a small section of the universe,
>
> Question: because I want to find out how many stars are in the sky,
>
> Rationale: in order to understand whether the universe will expand forever or contract into a new Big Bang.

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

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

Applied Research Problem:

> Topic: I am studying the difference between readings from the Hubble telescope in orbit above the atmosphere and readings for the same

stars from the best earthbound telescopes,

Question: because I want to find out how much the atmosphere distorts measurements of light and other electromagnetic radiation,

Rationale: in order to measure more accurately the density of light and other electromagnetic radiation in a small section of the universe.

4.2.5 Is Your Problem Pure or Applied?

You distinguish between a pure and applied research problem by the consequences you name in the statement of its rationale (step 3). In pure research, the consequences are conceptual and the rationale defines what you want to know; in applied research, the consequences are tangible and the rationale defines what you want to do.

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

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

Topic: I am studying the differences among various nineteenth-century versions of the story of the Alamo.

Question: because I want to find out how politicians used stories of great events to shape public opinion,

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

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

In order to formulate an effective applied research problem, you have to show that the rationale named in step 3 is plausibly connected to the question named in step 2. You can test this by working back from the rationale. Ask yourself this question:

b. If my readers want to achieve the goal of [state your objective from Step 3]
c. would they think that the way to do that would be to find out [state your question here from Step 2]?

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

Try this test on the applied astronomy problem:

a. If my readers want to measure more accurately the density of electromagnetic radiation in a section of the universe,
b. would they think that the way 10 do that would be to find out how much the atmosphere distorts measurements of it?

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

Now try the test on the Alamo problem:

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

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

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

Topic: I am studying differences among nineteenth- century versions of the story of the Alamo,

Question: because I want to find out how politicians used stories of great events to shape public opinion,

Rationale: in order to show how politicians use elements of popular culture to advance their political goals.

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

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

Topic: I am studying the differences among various nineteenth-century versions of the story of the Alamo,

Question: because I want to find out how politicians used stories of great events to shape public opinion,

Rationale: in order to understand how politicians use elements of popular culture to advance their political goals,

Significance: so that we will know more about protecting ourselves from unscrupulous politicians and become better citizens.

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

4.3 Finding a Research Problem

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

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

4.3.1 Ask for Help

Do what experienced researchers do when they are not clear about the problem they think they are investigating: talk to people.

Talk to your teachers, relatives, friends, neighbors—anyone who might be interested in your topic and your question. Why would anyone need to answer your question? What would they do with an answer? What further questions might your answer raise?

If you are free to select your own topic, you might look for one that is part of a larger problem in your field. You will be unlikely to solve it, but if you can slice

off a piece of it, your project will inherit some of its significance. (You will also be educating yourself about the problems of your field, no small dividend.) Ask your instructor what he is working on and ask to work on part of it.

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

4.3.2 Look for Problems as You Read

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

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

Once you think you have found a real puzzle or error, try to do more than merely point it out. If a source says X and you think Y, you have a research problem only if you can show that readers who go on believing X will misunderstand something more important yet.

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

> *And just as the soldier's peacetime experience influenced his battlefield performance, so must the experience of the officer corps have influenced theirs. Indeed, a few commentators after the Russo-Japanese War blamed the Russian defeat on habits acquired by officers in the course of then economic chores. In any event, to appreciate the service habits of Tsarist officers in peace and war, we need a structural—if you will,*

an anthropological—analysis of the officer corps like that offered here for enlisted personnel [our emphasis].

4.3.3 Look for Problems in What You Write

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

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

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

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

4.3.4 Use a Standard Problem

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

4.4 The Problem of the Problem

Your teachers understand that you are not a professional, but they believe it important that you develop and practice the habits of mind of a serious researcher. They want to see you do more than just accumulate facts about a topic, then summarize and report them. They want you to formulate a problem that you (and

perhaps even they) have a stake in seeing solved. You take your first step toward real research when you recognize a question that is significant to you, a question that you want to answer just for your own satisfaction, to satisfy your own desire to know more, to resolve a discrepancy, to settle a contradiction, regardless of whether anyone else cares. If you can do that much in your earliest research, if you can find some puzzle that you care about resolving, you have achieved something quite significant that will gratify your teachers.

Eventually, though, as you move on to advanced work, when you decide that you have reason to share your new knowledge and understanding with others, you will have to take this next step: You must try to understand what your readers consider interesting questions and problems, the costs they perceive resulting from a gap in their knowledge or flaw in their understanding. You take the biggest step of all when you not only know the kind of problem that your readers like to see solved, but can persuade them to entertain problems of a new kind. No one ever takes all three steps the first time out.

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

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

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