

The Craft of Research

SECOND EDITION

WAYNE C. BOOTH

GREGORY G. COLOMB

JOSEPH M. WILLIAMS

The University of Chicago Press, Chicago 60637
The University of Chicago Press, Ltd., London

© 1995, 2003 by The University of Chicago
All rights reserved. Published 2003
Printed in the United States of America

12 11 10 09 08 07 06 05 3 4 5
ISBN: 0-226-06567-7 (cloth)
ISBN: 0-226-06568-5 (paper)

Library of Congress Cataloging-in-Publication Data

Booth, Wayne C.

The craft of research / Wayne C. Booth, Gregory G. Colomb, Joseph M. Williams.—2nd ed.

p. cm. — (Chicago guides to writing, editing, and publishing)

Includes bibliographical references and index.

ISBN 0-226-06567-7 (cloth : alk. paper) — ISBN 0-226-06568-5 (paper : alk. paper)

I. Research—Methodology. 2. Technical writing. I. Colomb, Gregory G. II. Williams, Joseph M. III. Title. IV. Series.

Q180.55.M4 B66 2003

001.4'2—dc21

2002015184

© The paper used in this publication meets the minimum requirements of the American National Standard for Information Sciences—Permanence of Paper for Printed Library Materials, ANSI Z39.48-1992.

THE UNIVERSITY OF CHICAGO PRESS
Chicago & London

CHAPTER FOUR

From Questions to Problems

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

In the last chapter, we described how to find a topic in your interests, how to narrow it, then to question it. We suggested that you identify the significance of your questions by fleshing out this three-step formula:

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

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

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

thing to others, a move that makes a stronger claim on readers and so creates a stronger relationship with them.

4.1 PROBLEMS, PROBLEMS, PROBLEMS

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

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

4.1.1 Practical Problems and Research Problems

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

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

PRACTICAL PROBLEM: My brakes have started screeching.

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

RESEARCH PROBLEM: Find the Yellow Pages and look up closest brake shop.

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

APPLICATION: Call to see when they can fix them.

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

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

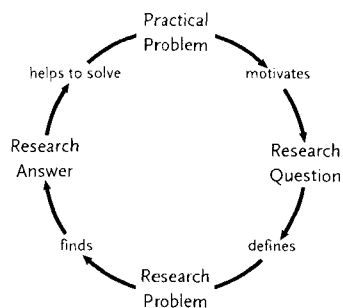
Costs are up at the Omaha plant. *What has changed?* Compare personnel before and after. *More turnover now.* If we improve training and morale, our workers stay with us. OK, let's see if we can afford to do it.

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

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

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

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



4.1.2 Distinguishing Practical Problems and Research Problems

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

- A *practical* problem is caused by some condition in the world, from e-mail spam to terrorism, that makes us unhappy because it costs us time, money, respect, security, pain, even our lives. You solve a practical problem by *doing* something that changes the world by eliminating the causes that lead to its costs, or by encouraging others to do so.
- A *research* problem is motivated not by palpable unhappiness, but by incomplete knowledge or flawed understanding. You solve it not by changing the world but by understanding it better.

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

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

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

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

4.2 THE COMMON STRUCTURE OF PROBLEMS

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

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

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

4.2.1 The Nature of Practical Problems

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

For a practical, tangible problem, the condition can be literally anything, even winning the lottery. Suppose you win a million dollars but owe a loan shark two million, and your name gets in

the paper. He finds you, takes your million, and breaks your leg. Winning a million turns out to be a Big Problem.

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

- its **condition**

I missed the bus.

The hole in the ozone layer is growing.

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

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

Many will die from skin cancer.

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

The hole in the ozone layer grew last year.

So what?

You answer with the cost of the problem:

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

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

Too much ultraviolet light can give people skin cancer.

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

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

4.2.2 The Nature of Research Problems

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

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

You can identify conditions by working through the formula in chapter 3. The second step states what you do not know or understand:

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

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

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

You identify these consequences in step 3 of our formula:

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

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

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

How many stars are in the sky?

How have romantic movies changed in the last fifty years?

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

If we can't answer the question of how romantic movies have changed in the last fifty years,¹*condition/first question* then we can't answer a more important question: How have our cultural depictions of romantic love changed?²*consequence/larger, more important question*

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

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

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

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

If we can't answer the question of how our cultural depictions of romantic love have changed in the last fifty years,¹*second question* then we can't answer a more important one yet: How is our culture shaping the expectations of young men and women concerning marriage and families?²*consequence/larger, more important question*

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

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

4.2.3 Distinguishing "Pure" and "Applied" Research

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

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

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

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

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

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

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

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

4.2.4 Connecting a Research Problem to Practical Consequences

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

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

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

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

- (a) If my readers want to achieve the goal of _____ [state your objective from step 3],
- (b) would they think that a good way to do that would be to find out _____? [state your question from step 2]

Try that test on the applied astronomy problem:

- (a) If my readers want to use data from earthbound telescopes to measure more accurately the density of electromagnetic radiation,
- (b) would they think that a good way to do so would be to find out how much the atmosphere distorts measurements of it?

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

Now try the test on the Alamo problem:

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

Again, that feels like a stretch.

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

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

When you state your problem in your introduction, it's usually best to formulate it as a purely conceptual research problem whose significance is based on conceptual consequences. Unless

your assignment includes the question of practical applications, save them for your conclusion. (For more on introductions and conclusions, see chapter 14.)

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

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

A TYPICAL BEGINNER’S MISTAKE

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

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

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

4.3 FINDING A GOOD RESEARCH PROBLEM

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

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

4.3.1 Ask for Help

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

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

her find out something she didn't know or understand, such as better sources and new data.

4.3.2 Look for Problems as You Read

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

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

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

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

And just as the soldier's peacetime experience influenced his battlefield performance, so must the experience of the officer corps have influenced theirs. Indeed, a few commentators after the Russo-Japanese War blamed the Russian defeat on habits acquired by officers in the course of their economic chores. *In any event, to appreciate the service habits of Tsarist officers in peace and war, we need a structural—if you will, an anthropological—analysis of the officer corps like that offered here for enlisted personnel.* [our emphasis]

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

4.3.3 Look for the Problem that Your Claim Solves

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

4.4 SUMMARY: THE PROBLEM OF THE PROBLEM

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

Eventually, though, as you move to advanced work, you have to share your new knowledge and understanding with others. At that point, you must understand what *your readers* think are interesting questions and problems. As we've emphasized, they base that judgment on the costs *they* pay as a result of not knowing or understanding something. And the step we all dream of is not only finding the kind of problem readers want to see solved, but *persuading* them to think seriously about a problem none of them has ever thought of. No one takes all three steps the first time out. Just about all of us get to the first one: *What am I interested in discovering?* Most of us get to the second: *What might my readers be interested in?* Few of us get to the third: *How can I get*

them to realize they are asking the wrong questions? But those of us who don't get there do not necessarily fail, because we can measure our success by how well our readers think we answer questions they already care about. The worst response you can get from a reader is not *I don't agree*, but *I don't care*.

By now, all this airy talk about academic research may seem disconnected from a world in which so many people labor so hard at getting ahead or keeping others down. But when research problems in the world are pursued honestly, they are structured *exactly* as they are in the academic world. And in business and government, in law and medicine, in politics and international diplomacy, no skill is valued more highly than the ability to recognize a problem that others should take seriously, then to articulate that problem in a way that convinces them to care. If you can do that in a class in Chinese history, you can do it in a business or government office down the street or in Hong Kong.