3

The Problem: The Heart of the Research Process

The problem or question is the axis around which the whole research effort revolves. The statement of the problem must first be expressed with the utmost precision; it should then be divided into more manageable subproblems. Such an approach clarifies the goals and directions of the entire research effort.

To identify and define important terms included in this chapter, go to the Activities and Applications section in Chapter 3 of MyEducationalResearchLab, located at www.myeducationlab.com. Complete Activity 1: Defining Key Terms.

The heart of every research project is the problem. It is paramount to the success of the research effort. To see the problem with unwavering clarity and to state it in precise and unmistakable terms is the first requirement in the research process.

Finding Research Projects

Problems in need of research are everywhere. To get an idea of typical research projects for doctoral dissertations, go to the reference room at your university library, open any volume of *Dissertation Abstracts International*—most university libraries also have these abstracts in an online database—and look at the dissertation abstracts in your academic discipline. To get an online sample of recent published research studies in your area of interest, go to Google Scholar at www.scholar.google.com; type a topic in the search box and then click on some of the titles that pique your curiosity.

Some research projects are intended to enhance basic knowledge about the physical, biological, psychological, or social world or to shed light on historical, cultural, or aesthetic phenomena. For example, a psychologist might study the nature of people's cognitive processes, and an ornithologist might study the mating habits of a particular species of birds. Such projects, which can advance human beings' theoretical conceptualizations about a particular topic, are known as **basic research**.

Other research projects are intended to address issues that have immediate relevance to current practices, procedures, and policies. For example, a nursing educator might compare the effectiveness of different strategies for training future nurses, and an agronomist might study the effects of various fertilizers on the growth of sunflowers. Such projects, which can inform human decision making about practical problems, are known as **applied research**. Occasionally applied research involves addressing questions in one's immediate work environment, with the goal of solving an ongoing problem in that environment; such research is known as *action research* (e.g., Cochran-Smith & Lytle, 1993; Mills, 2007).

Keep in mind, however, that the line between basic research and applied research is, at best, a blurry one. Answering questions about basic theoretical issues can often inform current practice in the everyday world; for example, by studying the mating habits of a particular species of birds, an ornithologist might lead the way in saving that species from extinction. Similarly, answering questions about practical problems may enhance theoretical understandings of particular phenomena; for example, the nursing educator who finds that one approach to training

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nurses is more effective than another may enhance psychologists' understanding of how, in general, people learn new skills.

Regardless of whether you conduct basic or applied research, a research project is likely to take a significant amount of your time and energy, so whatever problem you study should be *worth* that time and energy. As you begin the process of identifying a suitable research problem to tackle, keep two criteria in mind. First, your problem should address an important question, such that the answer can actually "make a difference" in some way. And second, it should advance the frontiers of knowledge, perhaps by leading to new ways of thinking, suggesting possible applications, or paving the way for further research in the field. To accomplish both of these ends, your research project must involve not only the collection of data but also the *interpretation* of those data.

Some problems are not suitable for research because they lack the "interpretation of data" requirement; they do not elicit a mental struggle on the part of the researcher to force the data to reveal their meaning. Following are four situations to avoid when considering a problem for research purposes:

1. *Research projects should not be a ruse for achieving self-enlightenment.* All of us have large holes in our education, and filling them is perhaps the greatest joy of learning. But self-enlightenment is not the purpose of research. Gathering information to know more about a certain area of knowledge is entirely different from looking at a body of data to discern how it contributes to the solution of the problem.

A student once submitted the following as the statement of a research problem:

> The problem of this research is to learn more about the way in which the Panama Canal was built.

For this student, the information-finding effort would provide the satisfaction of having gained more knowledge about a particular topic, but it would *not* have led to *new* knowledge.

2. *A problem whose sole purpose is to compare two sets of data is not a suitable research problem.* Take this proposed problem for research:

> This research project will compare the increase in the number of women employed over 100 years—from 1870 to 1970—with the employment of men over the same time span.

A simple table completes the project (*Historical Statistics, 1975*).

	1870	1970
Women employed	13,970,000	72,744,000
Men employed	12,506,000	85,903,000

The "research" project involves nothing more than a quick trip to the library to reveal what is already known.

3. *Calculating a correlation coefficient between two sets of data to show a relationship between them is not acceptable as a problem for research.* Why? Because the basic requirement for research is ignored: a human mind struggling with data. What we see here is a proposal to perform a statistical operation that a computer can do infinitely faster and more accurately than a person can. A correlation coefficient is nothing more than a statistic that expresses how closely two characteristics or other variables are associated with each other. It tells us nothing about *why* the association exists.

Some novice researchers think that their work is done when they collect data and, by using a simple statistical procedure, find that two variables are closely related. In fact, their work is *not* done at this point; it has only begun. For example, many researchers have found a correlation between the IQ scores of children and those of their parents. In and of itself, this fact has very little usefulness. It does, however, suggest a problem for research: What is the *cause* of the relationship between children's and parents' intelligence test scores? Is it genetic? Is it environmental? Is it a combination of both genetics and environment?

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4. *Problems that result in a yes or no answer are not suitable problems for research.* Why? For the same reason that merely finding a correlation coefficient is unsatisfactory. Both situations simply skim the surface of the phenomenon under investigation, without exploring the mechanisms underlying it.

"Is homework beneficial to children?" That is no problem for research, certainly not in the form in which it is stated. The researchable issue is not whether homework is beneficial, but wherein the benefit of homework, if there is one, lies. Which components of homework are beneficial? Which ones are counterproductive? If we knew the answers to these questions, then teachers could structure homework assignments with more purpose and greater intelligence—and thereby promote the learning of children—more effectively than they do now.

There is so much to learn and so many important questions being generated each day that we should look for significant problems and not dwell on those that will make little, if any, contribution. Peter Medawar (1979), a Nobel laureate who investigated causes of the human body's rejection of organs and tissues transplanted from other human beings, gave wise advice to the young scientist when asked about conducting research:

> It can be said with complete confidence that any scientist of any age who wants to make important discoveries must study important problems. Dull or piffling problems yield dull or piffling answers. It is not enough that a problem should be "interesting"—almost any problem is interesting if it is studied in sufficient depth. (p. 13)

Good research, then, begins with identifying a good question to ask—ideally a question that no one has ever thought to ask before. In our minds, researchers who contribute the most to our understanding of the physical, biological, psychological, and social worlds are those who pose questions that lead us into entirely new lines of inquiry. To illustrate, let's return to that correlation between the IQ scores of children and those of their parents. For many years, psychologists bickered about the relative influences of heredity and environment on intelligence and other human characteristics. They now know not only that heredity and environment *both* influence virtually every aspect of human functioning but also that they *influence each other's influences* (for a good, down-to-earth discussion of this point, see Lippa, 2002). Rather than ask the question "How much do heredity and environment each influence human behavior?" a more fruitful question—one that's fairly new on the scene—is "How do heredity and environment interact in their influences on behavior?"

PRACTICAL APPLICATION Identifying and Describing the Research Problem

How can the beginning researcher formulate an important and useful research problem? Here we offer guidelines both for identifying a particular problem and for describing it in precise terms.

GUIDELINES Finding a Legitimate Problem

As a general rule, appropriate research projects don't fall out of trees and hit you on the head. You must be sufficiently knowledgeable about your topic of interest to know what projects might make important contributions to the field. Following are several strategies that are often helpful for novice and expert researchers alike.

1. *Look around you.* In many disciplines, questions that need answers—phenomena that need explanation—are everywhere. For example, let's look back to the early 17th century, when Galileo was trying to make sense of a variety of earthly and celestial phenomena. For example, why did large bodies of water (but not small ones) rise and fall in the form of tides twice a day? Why did sunspots consistently move across the sun's surface from right to left, gradually disappear, and then, about 2 weeks later, reappear on the right edge? Furthermore, why did sunspots usually move in an upward or downward path as they traversed the sun's surface, while only

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occasionally moving in a direct, horizontal fashion? Galileo correctly deduced that the various "paths" of sunspots could be explained by the facts that both the earth and sun were spinning on tilted axes and that (contrary to popular opinion at the time) the earth revolved around the sun rather than vice versa. Galileo was less successful in explaining tides, attributing them to the natural "sloshing" that would take place as the earth moved through space rather than to the moon's gravitational pull (Sobel, 2000).

We do not mean to suggest that novice researchers should take on such monumental questions as the nature of the solar system or oceanic tides. But smaller problems suitable for research exist everywhere. Perhaps you might see them in your professional practice or in everyday events. Continually ask yourself questions about what you see and hear: Why does such-and-such happen? What makes such-and-such tick? and so on.

2. *Read the literature.* One essential strategy is to find out what things are already known about your topic of interest; little can be gained by reinventing the wheel. In addition to telling you what is already known, the existing literature is likely to tell you what is *not* known in the area—in other words, what still needs to be done. For instance, your research project might

- Address the suggestions for future research that another researcher has identified
- Replicate a research project in a different setting or with a different population
- Consider how various subpopulations might behave differently in the same situation
- Apply an existing perspective or theory to a new situation
- Explore unexpected or contradictory findings in previous studies
- Challenge research findings that seem to contradict what you know or believe to be true (Neuman, 1994)

Reading the literature has other advantages as well. It gives you a theoretical base on which to generate hypotheses and build a rationale for your study. It provides potential research methodologies and methods of measurement. And it can help you interpret your results and relate them to what is already known in the field. (We address strategies for finding and reviewing related literature in Chapter 4.)

3. *Attend professional conferences.* Many researchers have great success finding new research projects at national or regional conferences in their discipline. By scanning the conference program and attending sessions of interest, they can learn "what's hot and what's not" in their field. Furthermore, conferences are a place where novice researchers can make contacts with experts in their field—where they can ask questions, share ideas, and exchange e-mail addresses with more experienced and knowledgeable individuals.

Some beginning researchers, including many students, are reluctant to approach well-known scholars at conferences, for fear that these scholars don't have the time or patience to talk with novices. Quite the opposite is true: Most experienced researchers are happy to talk with people who are just starting out. In fact, they may feel flattered that you are familiar with their work and that you would like to extend or apply it in some way.

4. *Seek the advice of experts.* Another simple yet highly effective strategy for identifying a research problem is simply to ask an expert: What needs to be done? What burning questions are still out there? What previous research findings seemingly don't make sense? Your professors will almost certainly be able to answer each of these questions, as will other scholars you may meet at conferences or elsewhere.

5. *Choose a topic that intrigues and motivates you.* As you read the professional literature, attend conferences, and talk with experts, you will uncover a number of potential research problems. At this point, you need to pick just *one* of them, and your selection should be based on what you personally want to learn more about. Remember, the project you're about to undertake will take you many months, quite possibly a couple of years or even longer. So it should be something that you believe is worth your time and effort—even better, one you are truly passionate about. Peter Leavenworth, at the time a doctoral student in history, explained the importance of choosing an interesting dissertation topic this way: "You're going to be married to it, so you might as well enjoy it."

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6. *Choose a topic that others will find interesting and worthy of attention.* Ideally, your work should not end with a thesis, dissertation, or other unpublished research report. If your research adds an important piece to what human beings know and understand about the world, then you will, we hope, want to share your findings with a larger audience. In other words, you will want to describe what you have done at a regional or national conference, publish an article in a professional journal, or both (we'll talk more about doing such things in Chapter 12). Conference coordinators and journal editors are often quite selective about the papers they accept for presentation or publication, and they are most likely to choose those papers that will have broad appeal.

Future employers, too, may make judgments about you, at least in part, based on the topic you have chosen for a thesis or dissertation. Your résumé or curriculum vitae will be more apt to attract their attention if, in your research, you are pursuing an issue of broad scientific or social concern or, more generally, a hot topic in your field.

GUIDELINES Stating the Research Problem

As noted earlier, the heart of any research project is the problem. At every step in the process, successful researchers ask themselves: What am I doing? For what purpose am I doing it? Such questions can help focus your efforts toward achieving your ultimate purpose for gathering data: to resolve the problem.

Researchers get off to a strong start when they begin with an unmistakably clear statement of the problem. After identifying a research problem, therefore, you must articulate it in such a way that *it is carefully phrased and represents the single goal of the total research effort.* Following are some general guidelines to help you do just that:

1. *State the problem clearly and completely.* Your problem should be so clearly stated that anyone who reads English can read and understand it. If the problem is not stated with such clarity, then you are merely deceiving yourself that you know what the problem is. Such self-deception will cause you difficulty later on.

You can state your problem clearly only when you also state it completely. At a minimum, you should describe it in one or more *grammatically complete sentences*. As examples of what *not* to do, following are some meaningless half-statements—verbal fragments that only hint at the problem. Ask yourself whether you understand exactly what each student researcher plans to do.

From a student in sociology:

Welfare on children's attitudes.

From a student in music:

Palestrina and the motet.

From a student in economics:

Busing of schoolchildren.

From a student in social work:

Retirement plans of adults.

Unfortunately, all four statements lack clarity. It is imperative to think in terms of specific, researchable goals expressed in complete sentences. We take the preceding fragments and develop each of them into one or more complete sentences that describe a researchable problem.

Welfare on children's attitudes becomes:

What effect does welfare assistance to parents have on the attitudes of their children toward work?

Palestrina and the motet becomes:

This study will analyze the motets of Giovanni Pierluigi da Palestrina (1525?–1594) written between 1575 and 1580 to discover their distinctive contrapuntal

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characteristics and will contrast them with the motets of his contemporary William Byrd (1542?–1623) written between 1592 and 1597. During the periods studied, each composer was between 50 and 55 years of age.

Busing of schoolchildren becomes:

What factors must be evaluated and what are the relative weights of those several factors in constructing a formula for estimating the cost of busing children in a midwestern metropolitan school system?

Retirement plans for adults becomes:

How do retirement plans for adults compare with the actual realization, in retirement, of those plans in terms of self-satisfaction and self-adjustment? What does an analysis of the difference between anticipation and realization reveal for a more intelligent approach to planning?

Notice that, in the full statement of each of these problems, the areas studied are carefully limited so that the study is of manageable size. The author of the Palestrina–Byrd study carefully limited the motets that would be studied to those written when each composer was between 50 and 55 years of age. A glance at the listing of Palestrina's works in *Grove's Dictionary of Music and Musicians* demonstrates how impractical it would be for a student to undertake a study of all the Palestrina motets. He wrote 392 of them!

2. *Think through the feasibility of the project that the problem implies.* Students sometimes rush into a problem without thinking through its implications. It's great to have ideas. It's much better to have practical ideas. Before your enthusiasm overtakes you, consider the following research proposal submitted by John:

This study proposes to study the science programs in the secondary schools in the United States for the purpose of . . .

Let's think about that. The United States has more than 24,000 public and private secondary schools. These schools, north to south, extend from Alaska to the tip of Florida; east to west, from Maine to Hawaii. Certain practical questions immediately surface. How does John intend to contact each of these schools? By personal visit? Being very optimistic, he might be able to visit two schools per day—one in the morning and one in the afternoon. That would amount to more than 12,000 visitation days. The number of school days in the average school year is 180, so it would take more than 66 years for John to gather the data. Furthermore, the financial outlay for the project would be exorbitant; if we conservatively estimated $125 for daily meals, lodging, and travel, John would be spending $1.5 million just to collect the data!

"But," John explains, "I plan to gather the data by mail with a questionnaire." Fine! Each letter to the 24,000 schools, with an enclosed questionnaire and a return postage-paid envelope, would probably cost at least a dollar just for the postage. Thus, the total postage cost for letters to all the schools would be at least $24,000. And we mustn't overlook the fact that John would need a second and perhaps a third mailing. A 50% return on the first mailing would be considered a good return. But, for the nonreturnees, a follow-up mailing would be needed, at a cost of another $12,000. That would bring the mailing bill to approximately $36,000. And we haven't even figured in the cost of envelopes, stationery, photocopying, and data analysis. All in all, we are talking about a project that would cost well over $40,000.

Obviously, John did not intend to send surveys to every school in the United States, yet that is what he wrote that he would do.

3. *Say precisely what you mean.* When you state your research problem, you should say exactly what you mean. You cannot assume that others will be able to read your mind. People will always take your words at their face value: You mean what you say. That's it.

Your failure to be careful with your words can have grave results for your status as a scholar and a researcher. In the academic community, a basic rule prevails: *Absolute honesty and integrity are assumed in every statement a scholar makes.*

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Look again at John's problem statement. We could assume that John means to fulfill precisely what he has stated (although we would doubt it, given the time and expense involved). Had he intended to survey only *some* schools, then he should have said so plainly:

> This study proposes to survey the science programs *in selected secondary schools throughout the United States*.

Or, perhaps he could have limited his study to a specific geographical area or to schools serving certain kinds of students. Such an approach would give the problem constraints that the original statement lacked and would communicate to others what John intended to do—what he realistically *could* commit to doing. Furthermore, it would have preserved his reputation as a researcher of integrity and precision.

Ultimately, an imprecisely stated research problem can lead others to have reservations about the quality of the overall research project. If a researcher cannot be meticulous and precise in stating the nature of the problem, others might question whether such a researcher is likely to be any more meticulous and precise in gathering and interpreting the data. Such uncertainty and misgivings are very serious indeed, for they reflect on the basic integrity of the whole research effort.

We have discussed some common difficulties in the statement of the problem, including statements that are unclear or incomplete and statements that suggest impractical or impossible projects. Here's another difficulty: Occasionally, a researcher *talks about the problem* but never actually *states what the problem is*. Under the excuse that the problem needs an introduction or needs to be seen against a background, the researcher launches into a generalized discussion, continually obscuring the problem, never clearly articulating it. Take, for example, the following paragraph that appeared under the heading "Statement of the Problem":

> The upsurge of interest in reading and learning disabilities found among both children and adults has focused the attention of educators, psychologists, and linguists on the language syndrome. In order to understand how language is learned, it is necessary to understand what language is. Language acquisition is a normal developmental aspect of every individual, but it has not been studied in sufficient depth. To provide us with the necessary background information to understand the anomaly of language deficiency implies a knowledge of the developmental process of language as these relate to the individual from infancy to maturity. Grammar, also an aspect of language learning, is acquired through pragmatic language usage. Phonology, syntax, and semantics are all intimately involved in the study of any language disability.

Can you find a statement of problem here? Several problems are suggested, but none is articulated with sufficient clarity that we might put a finger on it and say, "There, that is the problem."

Earlier in this chapter, we invited you to go to *Dissertation Abstracts International* to see how the world of research and the real world of everyday living are intertwined. Now return to those abstracts and notice with what directness the problems are set forth. The problem should be stated in the very first words of an abstract: "The purpose of this study is to. . ." No mistaking it, no background buildup necessary—just a straightforward plunge into the business at hand. All research problems should be stated with the same clarity.

4. *State the problem in a way that reflects an open mind about its solution.* In our own research methods classes, we have occasionally seen research proposals in which the authors state that they intend to *prove* that such-and-such a fact is true. For example, a student once proposed the following research project:

> In this study, I will prove that obese adults experience greater psychological distress than adults with a healthy body mass index.

This is not a research question; it is a presumed—and quite presumptuous!—*answer* to a research question. If this student already knew the answer to her question, why was she proposing to

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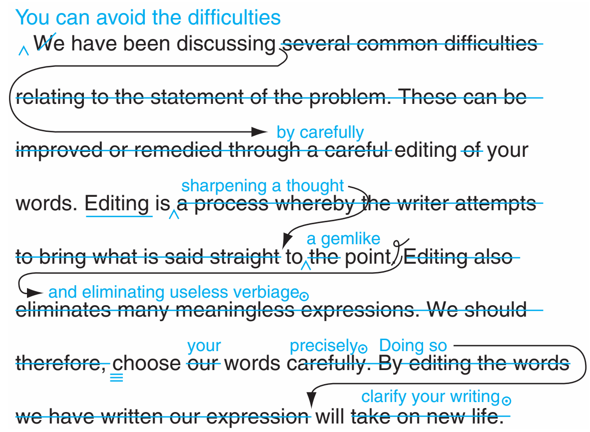
FIGURE 3.1
Editing to clarify your writing: An example



study it? Furthermore, as we noted in Chapter 1, it is quite difficult to prove something definitively, beyond the shadow of a doubt. We can certainly obtain data consistent with what we believe to be true, but in the world of research we can rarely say with one hundred percent certainty that it is true.

Good researchers try to keep open minds about what they might find. Perhaps they will find the result they hope to find, perhaps not. Any hypothesis should be stated as exactly that—a *hypothesis*—rather than as a foregone conclusion. As we will see shortly, hypotheses certainly do have their place in a research proposal. However, they should not be part of the problem statement.

Let's rewrite the preceding research problem, this time omitting any expectation of results that the research effort might yield:

> In this study, I will investigate the possible relationship between body mass index and psychological stress, as well as more specific psychological factors (e.g., depression, anxiety) that might underlie such a relationship.

Such a statement clearly communicates that the researcher is open-minded about what she may or may not find.

5. *Edit your work.* You can avoid the difficulties we have been discussing by carefully editing your words. *Editing* is sharpening a thought to a gemlike point and eliminating useless verbiage. Choose your words precisely. Doing so will clarify your writing.

The sentences in the preceding paragraph began as a mishmash of foggy thought and jumbled verbiage. The original version of the paragraph contained 71 words. These were edited down to 37 words. This is a reduction of about 50% and a great improvement in readability. Figure 3.1 shows the original version and the way it was edited. The three lines under the *c* in *choose* means that the first letter should be capitalized. When we discuss editing in more detail in Chapter 6, we'll present some of the common editing marks and what they mean.

Notice the directness of the edited copy. We eliminated unnecessarily wordy phrases—"relating to the statement of the problem," "a process whereby the writer attempts to bring what is said straight to the point"—replacing the verbosity with seven words: "sharpening a thought to a gemlike point."

Editing almost invariably improves your thinking and your prose. Many students think that any words that approximate a thought are adequate to convey it to others. This is not so. Approximation is never precision.

The thought's the thing. It is clearest when it is clothed in simple words, concrete nouns, and active, expressive verbs. Every student would do well to study how the great writers and poets set their thoughts into words. These masters have much to say by way of illustration to those who have trouble putting their own thoughts on paper.

The following checklist can help you formulate a research problem that is clear, precise, and accurate.

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✔ **CHECKLIST**

Evaluating the Research Problem

_____ 1. Write a clear statement of a problem for research.

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_____ 2. Review your written statement and ask yourself the following questions:

- Is the problem stated in a complete, grammatical sentence?
- Is it clear how the area of study will be limited or focused?
- Is it clear that you have an open mind about results that the research effort might yield?

_____ 3. On the basis of your answers to the questions in item 2, edit your written statement.

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_____ 4. Look at your edited statement and reflect on the following questions:

- Does the answer to this problem have the potential for providing important and useful answers and information?
- Will the result be more than a simple exercise in gathering information, answering a yes/no question, or making a simple comparison?
- Is the problem focused enough to be accomplished with a reasonable expenditure of time, money, and effort?

_____ 5. Looking at the statement once more, consider this: Is the problem really what you want to investigate?

_____ 6. Show other research students your work. Ask them to consider the questions listed in items 2 and 4 and then to give you their comments. With your compiled feedback, edit and rewrite your problem statement once again:

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Dividing the Research Problem Into Subproblems

Most research problems are too large or too complex to be solved without subdividing them. The strategy, therefore, is to divide and conquer. Almost every problem can be broken down into smaller units. From a research standpoint, these units are easier to address and resolve.

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