Chapter 2

The Problem: The Heart of the Research Process

The main research problem or question is the axis around which the whole research effort revolves. It clarifies the goals of the research project and can keep the researcher from wandering in tangential, unproductive directions.

Learning Outcomes

2.1 Identify strategies for choosing and refining a research problem.

2.2 Subdivide a main research problem into useful subproblems.

2.3 Recognize examples of independent, dependent, mediating, and moderating variables.

2.4 Pin down a proposed research study by (a) stating one or more hypotheses, (b) identifying variables to be examined, (c) defining terms, (d) stating assumptions, (e) identifying delimitations and limitations, and (f) explaining the study's importance.

The heart of every research project—the axis around which the entire research endeavor revolves—is the problem or question the researcher wants to address. The first step in the research process, then, is to identify this problem or question with clarity and precision.

FINDING RESEARCH PROJECTS

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

Other research projects address issues that have immediate relevance to current practices, procedures, and policies. For example, a nursing educator might compare the effectiveness of different instructional techniques 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.*

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 practices 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 the 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 nurses is more effective than another may enhance psychologists' understanding of how, in general, people acquire new knowledge and skills.

To get an online sample of recently published research studies in your area of interest, go to Google Scholar at scholar.google.com; type a topic in the search box and then click on some of the titles that pique your curiosity. As you scan the results of your Google search, especially look for items labeled as **pdf**, referring to **portable document format**; these items are often electronic photocopies of articles that have appeared in academic journals and similar sources.

You might also want to look at typical research projects for doctoral dissertations. For example, your university library probably has a section that houses the completed dissertations of students who have gone before you. Alternatively, you might go to the electronic databases in your library's catalog. Among those databases you are likely to find ProQuest Dissertations & Theses, which includes abstracts—and in many cases, the complete texts—for millions of dissertations and theses from around the world.

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* your 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 component; they don't require the researcher to go beyond the data themselves and reveal their meaning. Following are four situations to avoid when considering a problem for research purposes.

1. ***Research projects should not be simply a ruse for achieving self-enlightenment.*** All of us have large gaps in our education that we may want to fill. But mere self-enlightenment should not be the primary purpose of a research project (see Chapter 1). 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.

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

This "research" project involves nothing more than a quick trip to a government website to reveal what is already known.

3. ***Simply calculating a correlation coefficient between two related sets of data is not acceptable as a problem for research.*** Why? Because a key ingredient in true research—*making sense* of the data—is missing. 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 might exist.

Some novice researchers think that after they have collected data and performed a simple statistical procedure, their work is done. 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 is of little use. It does, however, suggest a problem for research: What is the underlying *cause* of the correlation between children's and parents' intelligence test scores? Is it genetic? Is it environmental? Does it reflect some combination of genetic heritage and environment?

4. ***Problems that result only in a yes-or-no answer are not suitable problems for research.*** Why? For the same reason that merely calculating 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, at least 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, if any, are counterproductive? If we knew the answers to these questions, then teachers could better structure homework assignments to enhance students' learning and classroom achievement.

There is so much to learn—there are so many important questions unanswered—that we should look for significant problems and not dwell on those that will make little or no contribution. When asked about conducting research, Peter Medawar, recipient of a Nobel Prize for his research on organ transplantation, gave wise advice to young scientists:

> 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. (Medawar, 1979, 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. Researchers who contribute the most to our understanding of our 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 is relatively 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 a beginning researcher formulate an important and useful research problem? Here we offer guidelines both for choosing an appropriate problem and for describing it sufficiently to focus the research effort.

GUIDELINES Choosing an Appropriate Problem

Choosing a good research problem requires genuine curiosity about unanswered questions. But it also requires enough knowledge about a topic to identify the kinds of investigations that are likely to make important contributions to one's 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. 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 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, mistakenly attributing them to natural "sloshing" as a result of the Earth's movement through space, rather than to the moon's gravitational pull.

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? What are people thinking when they do such-and-such?

2. ***Read the existing research literature about a topic.*** One essential strategy is to find out what things are already known and believed about your topic of interest—a topic we address in more detail in Chapter 3. Little can be gained by reinventing the wheel. In addition to telling you what is already known, the existing literature about a topic 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 personally know or believe to be true (Neuman, 2011)

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 offers potential research designs and methods of measurement. And it can help you interpret your results and relate them to previous research findings in your field.

As you read about other people's research related to your topic, *take time to consider how you can improve your own work because of it.* Ask yourself: What have I learned that I would (or would not) want to incorporate into my own research? Perhaps it is a certain way of writing, a specific method of data collection, or a particular approach to data analysis. You should constantly question and reflect on what you read.

We also urge you to *keep a running record of helpful journal articles and other sources.* Include enough information that you will be able to track each source down again—perhaps including the author's name, the title and year of the journal or book, key words and phrases that capture the focus of the work, and (if applicable) the appropriate library call number or Internet address. You may think you will always be able to recall where you found a helpful source and what you learned from it. However, our own experiences tell us that you probably *will* forget a good deal of what you read unless you keep a handwritten or electronic record of it.

3. ***Seek the advice of experts.*** Another simple yet highly effective strategy for identifying a research problem is 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 might contact through e-mail or meet on campus and elsewhere.

Some beginning researchers—including many students—are reluctant to approach well-known scholars 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 would like to extend or apply it in some way.

4. ***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 more experienced individuals in their field—where they can ask questions, share ideas, and exchange e-mail addresses that enable follow-up communication.

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 some 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 are about to undertake will take you many months, quite possibly a couple of years or even longer. So it should be something 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 for a while, so you might as well enjoy it."

6. ***Choose a topic that others will find interesting and worthy of attention.*** Ideally, your work should not end simply with a thesis, dissertation, or other unpublished research report. If your research adds an important piece to what the human race knows and understands about the world, then you will, we hope, want to share your findings with a larger audience. In other words, you will want to present what you have done at a regional or national conference, publish an article in a professional journal, or both (we talk more about doing such things in Chapter 13). Conference coordinators and journal editors are often quite selective about the research reports they accept for presentation or publication, and they are most likely to choose those reports that will have broad appeal.

Future employers may also 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—especially one that is currently a hot topic in your field.

7. ***Be realistic about what you can accomplish.*** Although it is important to address a problem that legitimately needs addressing, it is equally important that the problem be a *manageable* one. For example, how much time will it take you to collect the necessary data? Will you need to travel great distances to get the data? Will you need expensive equipment? Will the project require knowledge and skills far beyond those you currently have? Asking and then answering such questions can help you keep your project within reasonable, accomplishable bounds.

GUIDELINES Stating the Research Problem

Remember, 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 you 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. Thus, after identifying a research problem, 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 several general guidelines to help you do exactly that.