2 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.

The heart of every research project—the axis around which the entire research endeavor revolves—is the problem. The first step in the research process is to identify the problem with unwavering clarity and to state it in precise and unmistakable terms.

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 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 essentially electronic photocopies of articles that have appeared in academic journals and similar sources.

Some research projects are intended to enhance basic knowledge about our physical, biological, psychological, or social world or to 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 are intended to address issues that have immediate relevance to our society's 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* (e.g., Cochran-Smith & Lytle, 1993; Mertler, 2009; Mills, 2011).

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 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 nurses is more effective than another may enhance psychologists' understanding of how, in general, people acquire new knowledge and 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* 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 do not 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, and filling them is perhaps the greatest joy of learning. But self-enlightenment is not the primary 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. *Simply 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 to make sense of 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 human being 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 might exist.

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 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 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, 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 (1979), a Nobel laureate who investigated causes of the human body's rejection of other people's transplanted organs and tissues, gave wise advice to the young scientist:

> 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. 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 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 from 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 two 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, attributing them to natural "sloshing" as a result of the earth's movement through space, rather than (more accurately) 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—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 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, 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 what is already known in the 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 urge you, too, 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 that 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 written 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 at conferences or elsewhere.

Some beginning researchers—including many students—are reluctant to approach well-known scholars for fear that these scholars do not 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. *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 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 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 12). 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, 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

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. 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 exactly 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, you are 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.

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 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.* Novice researchers 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 40,000 public and private secondary schools. These schools, north to south, extend from Alaska to 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, thus requiring more than 20,000 visitation days. The number of school days in the average school year is 180, so under the best of circumstances it would take more than one hundred years for John to gather the data. And even if John had exceptional longevity—not to mention exceptional persistence—the financial outlay for his project would be exorbitant.

"But," John explains, "I plan to gather the data by mail with a questionnaire." Fine! Each letter to the more than 40,000 schools, with an enclosed questionnaire and a return postage-paid envelope, would probably cost at least a dollar just for the postage. 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, one or more follow-up mailings would be required. And we would need to figure in the cost of envelopes, stationery, photocopying, and data analysis.

A faster and less expensive option, of course, would be to conduct the survey by e-mail. In that case, John would need to track down the name and chief administrator of every one of those 40,000-plus schools. How long might it take him to do that? And how many of his e-mail messages might end up in a chief administrator's spam filter and thus never be read?

Obviously, John did not intend to survey every secondary 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. In the academic community, a basic rule prevails: *Absolute honesty and integrity are assumed in every statement a scholar makes.*

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, 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 the researcher is likely to be any more meticulous and precise in gathering and interpreting data. Such uncertainty and misgivings are 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. Another difficulty is this one: A researcher *talks about the problem* but never actually *states what the problem is.* Using 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 task 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 study it? Furthermore, as noted in Chapter 1, it is quite difficult to prove something definitively, beyond a 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 100% 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 you will see later in the chapter, 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 two more specific psychological factors (depression and 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.

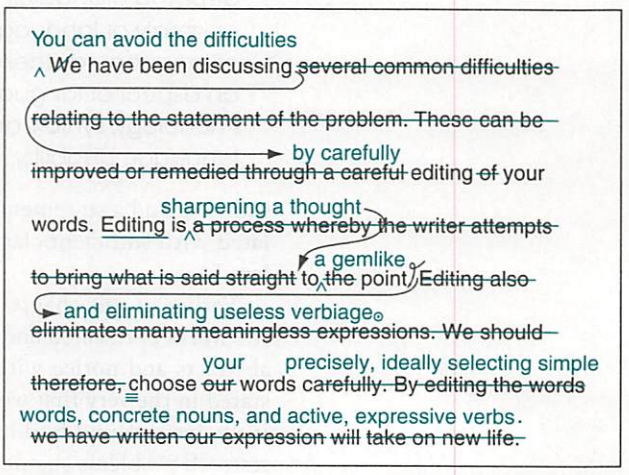
5. *Edit your work.* You can avoid the difficulties we have been discussing by carefully editing your words. *Editing* is sharpening a thought to a gemlike point and eliminating useless verbiage. Choose your words precisely, ideally selecting simple words, concrete nouns, and active, expressive verbs.

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 41 words, yielding a reduction of about 40% and a great improvement in clarity and readability. Figure 2.1 shows the original version and how it was edited. The three lines under the *c* in *choose* means that the first letter should be capitalized. We present some of the common editing marks when we discuss editing in more detail in Chapter 5.

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

FIGURE 2.1

Editing to clarify your writing: An example



thought to a gemlike point." As we edited, we also pinned down what good word choice might involve.

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

✔ CHECKLIST

Evaluating the Research Problem

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

_____

_____

_____

_____

_____

_____ 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.

_____

_____

_____

_____

_____

_____ 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:

_____

_____

_____

_____

_____

Dividing the Research Problem into Subproblems

Most research problems are too large or 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.

As noted in Chapter 1, the subparts of the main problem are called **subproblems**. By viewing the main problem through its subproblems, the researcher frequently gets a better idea of how to approach the entire research endeavor. So always think of a problem in terms of its component parts.

Subproblems versus Pseudo-Subproblems

The researcher must distinguish subproblems that are an integral part of the main problem from things that look like problems but are really nothing more than procedural issues. The latter, which we might call *pseudo-subproblems,* involve decisions the researcher must make before he or she can resolve the research problem and its subproblems. Consider the following as examples:

- What is the best way to choose a sample?
- How large should a representative sample of a population be?
- What instruments or methods should be used to gather the data?
- What statistical procedures should be used to analyze the data?

Deal with pseudo-subproblems forthrightly by making a reasonable decision about them and then get on with the solution of your research problem. To deal with pseudo-subproblems, you must decide whether (a) a little common sense and some creative thinking might help in solving your "problem" or (b) you simply lack the knowledge to address the difficulty. In the latter case, you have four options:

1. Turn to the index of this text to see whether your pseudo-subproblem regarding sample selection, instrumentation, statistical analysis, or some other issue is discussed.
2. Carefully peruse the "For Further Reading" lists at the end of each chapter in this book to see whether they contain any references that might help you, and consult general research methods books in your discipline.
3. Go to a library—preferably a college or university library—and search for books using such keywords as "Research" and either "Methods" or "Methodology." Find these books in the library stacks and look in the indexes for possible discussions of your pseudo-subproblems. Also search your library's online databases to find potentially relevant journal articles. If your library does not have certain periodicals, you can typically obtain any article you need through interlibrary loan.
4. Seek the suggestions and advice of more experienced researchers in your field. Recall a point made in Chapter 1: One of the most effective strategies for using the human mind is *collaborating with other minds.*

Characteristics of Subproblems

Following are four key characteristics of subproblems.

1. *Each subproblem should be a completely researchable unit.* A subproblem should constitute a logical subarea of the larger research undertaking. Each subproblem might be researched as a separate subproject within the larger research goal. The solutions to the subproblems, taken together, are then combined to resolve the main problem.

It is essential that each subproblem be stated clearly and succinctly. Often a subproblem is stated in the form of a question. A question tends to focus the researcher's attention more directly on the research target of the subproblem than does a declarative statement. As we have seen, a questioning, open-minded attitude is the mark of a true researcher.

2. *Each subproblem must be clearly tied to the interpretation of the data.* Just as is true for the main problem, each subproblem should involve interpretation as well as collection of data. This fact may be expressed as a part of each subproblem statement, or it may be reflected in a separate but related subproblem.

3. *The subproblems must add up to the totality of the problem.* After you have stated the subproblems, check them against the statement of the main problem to see that (a) nothing in excess of the coverage of the main problem is included and (b) all significant areas of the main problem are covered by the subproblems.

4. *Subproblems should be small in number.* If the main problem is carefully stated and properly limited to a feasible research effort, the researcher will find that it usually contains two to six subproblems. Sometimes the inexperienced researcher will come up with as many as 10, 15, or 20 subproblems. When this happens, a careful review of the problem and its attendant subproblems is in order. If you find yourself in this situation, you should study the individual subproblems to see whether (a) some are actually procedural issues (pseudo-subproblems), (b) some might reasonably be combined into larger subproblems, or (c) the main problem is more complex than you originally believed. If the last of these is true, you may want to reconsider whether the solution to the overall research problem is realistically achievable given the time and resources you have.

Identifying Subproblems

Novice researchers often have difficulty identifying the subproblems within the main problem. You should begin with the problem itself. If the problem is correctly written, you will be able to detect subproblem areas that can be isolated for further study. The old axiom that the sum of the parts equals the whole applies here. All of the subproblems must add up to the total problem.

You can use either paper and pencil or brainstorming software to help you identify your subproblems. We briefly describe each of these strategies.

Taking a Paper-and-Pencil Approach

Using this approach, you write the problem on paper and then box off the subproblem areas. More specifically, you might follow these steps:

1. Copy the problem on a clean sheet of paper, leaving considerable space between the lines.
2. Read the problem critically to discover the areas that require in-depth treatment before the problem can be resolved.
3. Make sure every subproblem contains a word that indicates the necessity to interpret the data within that particular subproblem (e.g., *analyze, discover, compare).* Underline this word.
4. Arrange the entire problem, which will now have the subproblems boxed off, in a graphic that shows the research structure of the problem. You now have a structure of the whole research design.

This procedure for finding subproblems should work for any problem in any academic discipline. We use a problem in musicology to illustrate the technique. More specifically, we take the problem of the motets of Palestrina. As presented earlier in the chapter, this problem is as follows:

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

Let's first delete the factual matter, such as life-span dates and the fact that the two men were contemporaries. These facts merely help in giving a rationale for certain elements within the problem. Modified to reflect its essential parts, the motet problem becomes the following:

> The purpose of this study will be *to analyze* the motets of Palestrina written between 1575 and 1580 to discover their distinctive contrapuntal characteristics, *to analyze* the same characteristics in the motets of William Byrd written between 1592 and 1597, and to determine what *a comparison of these two analyses* may reveal.

Notice that we have broken up the "will contrast them with" phrase in the original statement into two distinct tasks, *analyzing* Byrd's motets in the same manner that Palestrina's motets have been analyzed, and *comparing* the two analyses. The three italicized phrases in the revised problem statement reflect three subproblems, each of which involves interpretation of data that is necessary for resolving the main research problem.

Let's now arrange the problem so that we may see precisely what the design will be. Figure 2.2 is a graphic depiction of the problem. We have divided the problem into three subproblems. The first and second of these have the same general structural configuration: The analytical aspect of the subproblem is stated in the upper box and the purpose of the analysis is stated in the lower box. Addressing the third subproblem involves comparing the analyses conducted for the two preceding subproblems to determine what similarities and differences may exist. The last of the three subproblems—the comparison step—resolves the original main problem: characterizing Palestrina's motets.

Using Brainstorming Software

Some computer software facilitates the process of breaking problems into subproblems. Computer programs such as Inspiration, BrainStorm, and MindJet allow you to brainstorm research ideas and construct graphic networks of interrelated concepts, terms, and principles. For example, in Inspiration, you put the main problem, idea, or concept inside a box or oval in the middle of your computer screen. As you brainstorm other, related ideas, you put those on the screen as well, and you draw (and perhaps label) arrows to represent how various ideas are interconnected. You can break each concept or problem into subparts and, if helpful, break down each subpart even further. The process is fast and flexible, and you can save and print your final

FIGURE 2.2
A structural representation of the Palestrina-Byrd problem

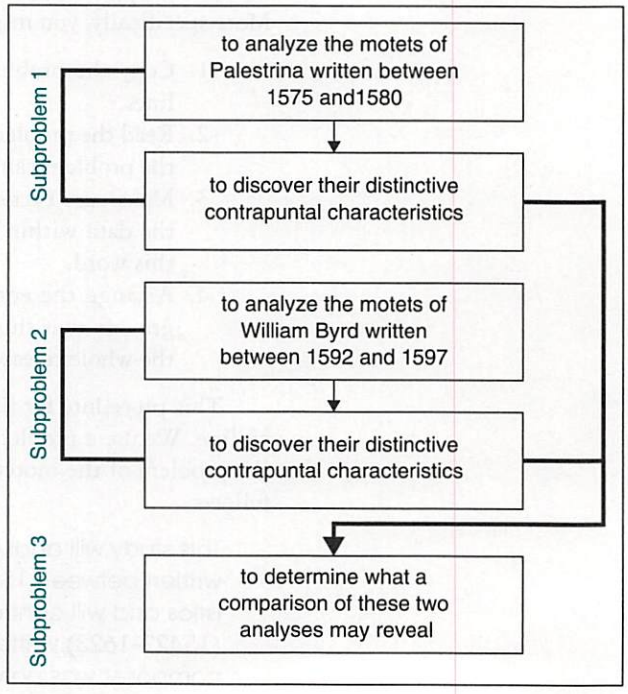


diagram (Figure 3.1, presented in Chapter 3, is an example). Some brainstorming software, such as Inspiration, also allows you to convert your diagram into an outline that lists major topics and various levels of subtopics.

Every Problem Needs Further Delineation

Up to this point, we have been discussing only the problem and its subparts. The statement of the problem establishes the goal for the research effort. The subproblems suggest ways of approaching that goal in a manageable, systematic way. But a goal alone is not enough. To comprehend fully the meaning of the problem, we need other information as well. Both the researcher and those reading the research proposal should ultimately have a clear understanding of every detail of the process.

In any research endeavor, the researcher should eliminate any possibility of misunderstanding by

- *Stating the hypotheses and/or research questions:* Describing the specific hypotheses being tested or questions being asked.
- *Delimiting the research:* Fully disclosing what the researcher intends to do and, conversely, does not intend to do.
- *Defining the terms:* Giving the meanings of all terms in the statements of the problem and subproblems that have any possibility of being misunderstood.
- *Stating the assumptions:* Presenting a clear statement of all assumptions on which the research will rest.

Taken as a whole, these elements comprise *the setting of the problem.* We look at each of them in more detail in the following sections. We also include a section titled "Importance of the Study," as this topic is frequently discussed in dissertations and other research reports.

Stating the Hypotheses and/or Research Questions

We previously discussed hypotheses in Chapter 1. There we pointed out that hypotheses are intelligent, tentative guesses about how the research problem might be resolved. *Research questions* are somewhat different in that, in and of themselves, they do not offer speculative answers related to the research problem. Hypotheses are essential to experimental research (see Chapter 9), whereas research questions are more common in many forms of qualitative research (see Chapter 6). Both hypotheses and research questions provide guidance for the kinds of data the researcher should collect; they also suggest how the researcher should analyze and interpret those data. It is not unusual for a researcher to form hypotheses *and* ask questions related to a research problem.

Research hypotheses and questions may originate in the subproblems. Often a one-to-one correspondence exists between the subproblems and their corresponding hypotheses or questions, in which case there are as many hypotheses or questions as there are subproblems.

Certainly the data from a research study can (and should) answer each research question, and they may support or not support each research hypothesis. But notice how we just said that the data may *support* or *not support* each research hypothesis; we intentionally did *not* say that the data would "prove" a hypothesis. Hypotheses are nothing more than *tentative propositions set forth to assist in guiding the investigation of a problem or to provide possible explanations for the observations made.*

A researcher who deliberately sets out to prove a hypothesis does not have the objective, impartial open-mindedness so important for good research. The researcher might bias the procedure by looking only for those data that would support the hypothesis (recall the discussion of *confirmation bias* in Figure 1.3 of Chapter 1). Difficult as it may be at times, we must let the chips fall where they may. Hypotheses have nothing to do with proof. Rather, their acceptance or rejection depends on what the data—and the data alone—ultimately reveal. If you discover that your data do not support your research hypothesis, do not let such an outcome disturb you. It merely means that your educated guess about the outcome of the investigation was incorrect.

Distinguishing Null Hypotheses from Research Hypotheses

Because we can never really prove a hypothesis, we often set out to cast doubt on—and therefore to *reject*—an opposite hypothesis. For example, imagine that a team of social workers believes that one type of after-school program for teenagers (Program A) is more effective in reducing high school dropout rates than is another program (Program B). The team's research hypothesis is:

> Teenagers enrolled in Program A will graduate from high school at a higher rate than teenagers enrolled in Program B.

Because the social workers cannot actually prove their hypothesis, they instead try to discredit an opposite hypothesis:

> There will be no difference in the high school graduation rates of teenagers enrolled in Program A and those enrolled in Program B.

If, in their research, the social workers find that there *is* a substantial difference in graduation rates between the two programs—and in particular, if the graduation rate is higher for youth in Program A—they can reject the "no difference" hypothesis and therefore have, by default, supported their research hypothesis.

When we hypothesize that there will be *no* differences between groups, *no* consistent relationships between variables, or, more generally, *no* patterns in the data, we are forming a **null hypothesis**. Null hypotheses are used primarily during statistical analyses; we support a research hypothesis by showing, statistically, that its opposite—the null hypothesis—probably is *not* true. Accordingly, we examine null hypotheses again in our discussion of statistics in Chapter 11.

Identifying the Variables under Investigation

Integral to a researcher's hypotheses and/or research questions are the variables that are the focus of investigation. We have occasionally used the term *variable* in earlier discussions in this chapter and in Chapter 1, but without defining it. We do so now: A **variable** is any quality or characteristic in a research investigation that has two or more possible values. For example, variables in studies of how well seeds germinate might include amounts of sun and water, kinds of soil and fertilizer, presence or absence of various parasites and microorganisms, genetic makeup of the seeds, speed of germination, and hardiness of the resulting plants. Variables in studies of how effectively children learn in classrooms might, for example, include instructional methods used; teachers' educational backgrounds, emotional warmth, and beliefs about classroom discipline; and children's existing abilities and personality characteristics, prior learning experiences, reading skills, study strategies, and achievement test scores.

Whenever a research project involves a possible cause-and-effect relationship, we are, of course, looking at the extent to which one variable (the hypothesized *cause)* influences another variable (the hypothesized *effect).* A variable that the researcher studies as a possible cause of something else—in many cases, this is one that the researcher directly manipulates—is called an **independent variable**. A variable that is potentially influenced by the independent variable—that "something else" just mentioned—is called a **dependent variable,** because it is influenced by and so to some extent *depends* on the independent variable. In research in the social sciences and education, the dependent variable is often some form of human behavior. In medical research, it might be people's physical health or well-being. In agricultural research, it might be quality or quantity of a particular crop. In general, a cause-and-effect relationship can be depicted like this:

Independent variable → Dependent variable

To illustrate the two kinds of variables, let's take an everyday situation. One hot summer morning you purchase two identical cartons of chocolate ice cream at the supermarket. When you arrive home, you put one carton in your refrigerator freezer but absentmindedly leave the other one on the kitchen counter. You then leave the house for a few hours. When you return home, you discover that the ice cream on the counter has turned into a soupy mess. The ice

cream in the freezer is still in the same condition it was when you purchased it. Two things vary in this situation. One, the temperature at which the ice cream is stored, is the independent variable. The other, the consistency of the ice cream, depends on the storage temperature and therefore is the dependent variable.

Now let's consider an example in medical research. Imagine that you want to compare the relative effectiveness of two different drugs that are used to treat high blood pressure. You take a sample of 60 men who have high blood pressure and randomly assign each man to one of two groups: The men in one group take one drug, and the men in the other group take the other drug. Later, you compare the blood pressure measurements for the men in the two groups. In this situation, you are manipulating the particular drug that each man takes; the drug, then, is the independent variable. Blood pressure is the dependent variable: It is presumably influenced by the drug taken and so its measured value depends to some extent on the drug.

A hypothesis or research question may occasionally specify other variables as well. For example, a **mediating variable** (also known as an *intervening variable)* might help explain why a certain independent variable has the effect that it does on a dependent variable. In particular, the independent variable influences the mediating variable, which in turn influences the dependent variable. Thus, the independent variable's influence on the dependent variable is an *indirect one,* as follows:

Independent variable → Mediating variable → Dependent variable

For example, consider the common finding that people who are confident in their ability to perform a particular new task do, on average, actually perform it better than less-confident people, even if the two groups of people had the same ability levels prior to performing the task. Looking at the situation from a simple independent-and-dependent-variables perspective, the situation would be depicted this way:

Confidence level (independent variable)	→	Performance quality (dependent variable)

But *why* does this relationship exist? One likely mediating variable is that highly confident people exert more effort in performing the new task than do people with less confidence (e.g., Bandura, 1997; Schunk & Pajares, 2005). The mediating variable, then, is *amount of effort,* as follows:

Confidence level → Amount of effort (mediating variable) → Performance quality

Still another variable of potential interest is a **moderating variable,** a variable that, while not intervening between the independent and dependent variables, influences the nature and strength of their relationship. For example, consider the fact that, on average, children from very-low-income homes are more likely to have difficulties in adolescence and adulthood; for instance, compared to their financially more advantaged peers, they are less likely to complete high school and more likely to get in trouble with the law. Yet some very poor youngsters are resilient to their circumstances: They do quite well in life, sometimes going on to become physicians, lawyers, college professors, or other successful professionals. One factor that apparently increases the odds of resilience—in other words, it *reduces* the cause-and-effect relationship between childhood poverty and later problems—is a warm, supportive mother (Kim-Cohen, Moffitt, Caspi, & Taylor, 2004). Maternal warmth is a moderating variable: It affects the *nature of the relationship* between family income level and adult problems, like so:

	Maternal warmth (moderating variable) ↓	
Childhood income level (independent variable)	→	Problems later in life (dependent variable)

The distinction between mediating and moderating variables is an important but often confusing one; even some experienced researchers get them confused (Holmbeck, 1997). A helpful way to keep them straight is to remember that an independent variable may potentially influence a mediating variable but does *not,* in and of itself, influence a moderating

variable. For example, in the earlier *mediating variable* example, a high confidence level might increase the amount of effort exerted, but in the *moderating variable* example, we would certainly not suggest that having a low income increases (i.e., causes) a mother's warmth toward her children. Rather, moderating variables provide potential *contexts or conditions* that alter—that is, they *moderate*—an independent variable's effects. When researchers refer to *risk factors* or *protective factors* in their research reports, they are talking about moderating variables—variables that affect the likelihood that certain cause-and-effect relationships will come into play.

Not all research studies require explicit identification of independent and dependent variables and any mediating or moderating variables under consideration. Such variables are most often identified in quantitative research, especially in experimental studies (see Chapter 9) and certain kinds of descriptive studies (see Chapter 8).

Identifying independent and dependent variables is often quite helpful both in choosing (a) an appropriate research design and (b) an appropriate statistical analysis. However, an important caution is in order here. In particular, *identifying independent and dependent variables does* ***not*** *guarantee that the research data will support the existence of a cause-and-effect relationship.* We return to this point in the discussion of correlational research in Chapter 6.

CONCEPTUAL ANALYSIS EXERCISE Identifying Independent, Dependent, Mediating, and Moderating Variables

Following are eight proposed research problems. Each one of them implies one or more independent variables and one or more dependent variables. Some of them also imply one or more mediating or moderating variables. Identify the independent and dependent variables—and, if applicable, any mediating and/or moderating variables—in each problem. We warn you that some of these scenarios may challenge you, as the writer's hypotheses may lie well below the surface of the words. We encourage you, then, to try to put yourself in each researcher's mind and guess what the person is probably thinking about a possible cause-and-effect relationship in the phenomenon under investigation. The answers appear after the "For Further Reading" list at the end of the chapter.

1. In this study, I will examine the possible effects of regular physical exercise on the health and longevity of laboratory rats.
2. In this study, I will investigate the extent to which placing recycling bins in convenient locations in classroom buildings affects college students' recycling behaviors.
3. In this study, I will examine the relationship between amount of cell phone use while driving and the frequency of car accidents.
4. I propose to study the degree to which test anxiety may influence test performance by increasing the frequency of distracting thoughts.
5. This investigation will examine the extent to which a supportive student–teacher relationship reduces the negative emotional impact of peer bullying on a child's emotional well-being.
6. I will investigate the degree to which male and female adolescents choose gender-stereotypical careers in three different countries: Canada, Lebanon, and Japan.
7. This study will investigate the extent to which a particular tumor-suppressing gene reduces the risk of getting melanoma [a potentially deadly form of skin cancer] after a history of frequent exposure to sunlight.
8. In this study, I will investigate the possible relationship between body mass index and psychological stress, as well as two more specific psychological factors (depression and anxiety) that might underlie such a relationship. (You previously saw this problem statement in the guidelines for "Stating the Research Problem" earlier in the chapter.)

Delimiting the Research

The statement of the research problem describes precisely what the researcher intends to do. But it is also important to know precisely what the researcher does *not* intend to do. What the researcher is not going to do is stated in the *delimitations.* The limits of the problem should be as carefully bounded for a research effort as a parcel of land is for a real estate transfer.

Research problems typically emerge from larger contexts and larger problem areas. The researcher can easily be beguiled and drawn off course by addressing questions and obtaining data that lie beyond the precincts of the problem under investigation. For instance, in the Palestrina-Byrd problem, it is possible that, because the two men were contemporaries, Byrd may have met Palestrina or at least come in contact with some of his motets. Such contact may have been a determinative influence on Byrd's compositions. But given how the problem has been stated, the researcher does not need to be concerned with *influences* on the motets of the two composers. He or she should be primarily interested in the *characteristics* of the motets, including their musical style, musical individualism, and contrapuntal likenesses and differences. Study the contrapuntal characteristics—that is what a researcher of this problem will do. What the researcher will *not* do is become involved in any data extraneous to this goal—no matter how enticing or interesting such an exploratory safari might be.

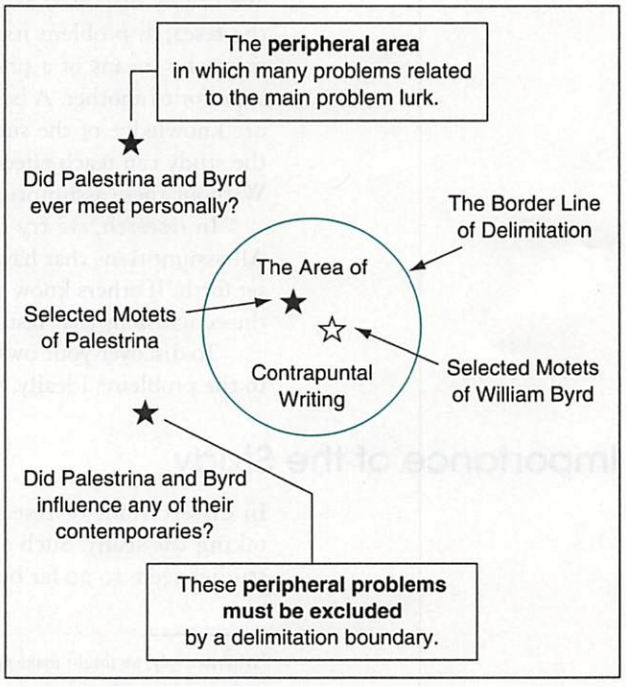
Only a researcher who thinks carefully about the problem and its focal center can distinguish between what is relevant and what is not relevant to the problem. All irrelevancies to the problem must be firmly ruled out in the statement of delimitations. Figure 2.3 may make the matter of delimitations more understandable.

Defining Terms

What precisely do the terms in the problem and the subproblems mean? For example, if we say that the purpose of the research is to analyze the harmonic characteristics of motets, what are we talking about? What are *harmonic characteristics?* Without knowing explicitly what a term means, we cannot evaluate the research or determine whether the researcher has carried out what was proposed in the problem statement.

Sometimes novice researchers rely on dictionary definitions, which are rarely either adequate or helpful. Instead, each term should be defined *as it will be used in the researcher's project.* In defining a term, the researcher makes the term mean whatever he or she wishes it to mean

FIGURE 2.3
Delimitation of a problem



within the context of the problem and its subproblems. Other individuals who read the researcher's research proposal or report must know how the researcher defines the term. Those individuals won't necessarily agree with such a definition, but as long as they know what the researcher means when using the term, they can understand the research and appraise it appropriately.

A formal definition contains three parts: (a) the *term* to be defined; (b) the *genera,* or the general class to which the concept being defined belongs; and (c) the *differentia,* the specific characteristics or traits that distinguish the concept being defined from all other members of the general classification. For example, *harmonic characteristics* (the term to be defined) might be defined as *the manner* (the genera) in which tonal values are combined *to produce individualized polyphonic patterns associated with the works of a particular composer* (the differentia: telling what particular "manner" the researcher means).

The researcher must be careful to avoid *circular definitions,* in which the terms to be defined are used in the definitions themselves. For instance, if we were to define *harmonic characteristics* in a circular manner, we might describe them as "those characteristics that derive from the harmonic patterns found in the works of a particular composer." Here the words *characteristics* and *harmonic* are used to define harmonic characteristics, giving others little or no assistance in understanding what the researcher means by the term.

Especially when talking about phenomena that have no cut-and-dried, easy-to-pinpoint manifestation in the physical world, it is often helpful to include an **operational definition.** That is, the researcher defines a characteristic or variable in terms of how it will be measured in the research study. For instance, a researcher might, for purposes of his or her study, define *intelligence* as a score on a certain intelligence test or define *popularity* as the number of peers who specifically identify an individual as being a desirable social partner. As another example, let's return to the first scenario in the earlier Conceptual Analysis Exercise: examining the possible effects of regular physical exercise on the health and longevity of laboratory rats. Longevity is easily defined and measured: It is simply the length of a rat's life span in days or some other unit of time. Somewhere in the research proposal, however, the researcher will need to be more specific about how he or she will define and measure physical exercise and health, thereby providing operational definitions for these terms. For example, physical exercise might involve putting a treadmill in some rats' cages but not in others. Health might be measured in any number of ways—for instance, through measurement of hypertension or analyses of blood or hair samples.

Stating the Assumptions

We briefly discussed assumptions in Chapter 1. Assumptions are so basic that, without them, the research problem itself could not exist. For example, suppose we are attempting to determine, by means of a pretest and a posttest, whether one method of classroom instruction is superior to another. A basic assumption in such a situation is that the pretest and posttest measure knowledge of the subject matter in question.[1] We must assume, too, that the teacher(s) in the study can teach effectively and that the students are capable of learning the subject matter. Without these assumptions, our research project would be meaningless.

In research, we try to leave nothing to chance in order to prevent any misunderstandings. All assumptions that have a material bearing on the problem should be openly and unreservedly set forth. If others know the assumptions a researcher makes, they are better prepared to evaluate the conclusions that result from such assumptions.

To discover your own assumptions, ask yourself: What am I taking for granted with respect to the problem? Ideally, your answer should bring your assumptions into clear view.

Importance of the Study

In dissertations or research reports, researchers frequently set forth their reasons for undertaking the study. Such a discussion may be especially important in a research proposal. Some studies seem to go far beyond any relationship to the practical world. Of such research efforts

[1] Alternatively, we might make no such assumption; instead, we might set out to determine the *validity* of the tests as measures in this situation. We discuss the nature of validity of measurement in Chapter 4.

one inwardly, if not audibly, asks, "Of what *use* is it? What *practical value* does the study have?"

Contemplating the six manned moon explorations conducted between 1969 and 1972, many citizens asked, "What good is it? What's the use of it all? How will spending all this money on space flights benefit anyone?" Perhaps those engaged in space research did not set forth clearly and succinctly enough the reasons the missions were undertaken. Only now are we beginning to appreciate the practical value of those early missions.

Ordering the Topics in a Research Proposal

To some degree, you may find a one-to-one correspondence between the discussions in this text and the sequence of topics that typically appear in a research proposal or research report. In any document, the first order of business is to present the problem and its setting. Generally, the document opens with a statement of the problem for research. This is followed by subproblems, hypotheses, and questions presented in a logical order.

Once the problem and its component parts have been articulated, the remaining items comprising the setting of the problem are presented, typically including a statement of delimitations, definitions of terms, and assumptions. A discussion of the importance of the study might have its own section or, alternatively, might be integrated into early paragraphs that introduce the research problem.

In a proposal or research report, such items often comprise the first chapter or section. The document then generally continues with a discussion of investigations that others have done, usually titled "Review of the Related Literature" or something of that nature. We discuss this topic in the next chapter.

PRACTICAL APPLICATION Writing the First Sections of a Proposal

In a checklist earlier in this chapter, you stated your main problem for research. In doing so, you took the first step in creating a research proposal. Now you can add the subproblems and identify the setting of the problem by doing the following exercise.

1. *State the subproblems.* On a blank sheet of paper, write the research problem statement you developed earlier. Allow considerable space between the lines. Now inspect your problem carefully and do the following:
   a. Within the problem, box off those areas that must receive in-depth treatment if the problem is to be fully explored. Consecutively number the boxed-in areas.
   b. Underline the words that indicate your intention to interpret the data (e.g., *analyze, compare*).
   c. Below the problem, which has been thus treated, write the several subproblems of your study in complete sentences. Make sure each subproblem includes a word that reflects data interpretation.
2. *Write your hypotheses/questions.* Read again what we have said about hypotheses and research questions in this chapter. Then write your own hypothesis/question related to each of your subproblems.
3. *Write the delimitations.* Review an earlier section of the chapter, titled "Delimiting the Research." Now write down the specific things that your own research project will *not* address.
4. *Write the definitions of terms.* Before writing your definitions, reread the section "Defining the Terms" earlier in the chapter. After writing your definitions, it may be helpful to box in the specific parts of each definition, labeling each box as "term," "genera," or "differentia." (Delete these labels in the final draft of your proposal.)

5. *Write the assumptions.* Reread the section "Stating the Assumptions." Now write a list of the specific assumptions you will be making as you design and carry out your research project—perhaps assumptions related to the people you will be studying, your measurement techniques, the relevance (or nonrelevance) of the context in which you will be conducting your study, and so on.
6. *Describe the importance of the study.* In a short paragraph or two, explain why your study is important. Eventually you may want to move this discussion to an earlier point in your proposal where you introduce your topic and provide an overall context for it (more on this point in Chapter 5). For now, however, keeping it in a separate section with its own label can help you remember that *talking* about your study's importance is important in its own right.
7. *Type your proposal.* Ideally, use word processing software so that you will easily be able to make future edits (there will be many!). Set margins at least an inch wide, and double-space the entire document; double spacing makes proofreading easier and allows room for handwritten edits.

Now that you have written the first sections of a proposal, reflect on your proposed project using the following checklist.

✔ CHECKLIST

Evaluating Your Proposed Research Project

_____ 1. Have you read enough literature relevant to your topic to know that your research project is worth your time and effort?

- Will the project advance the frontiers of knowledge in an important way?

_____

_____

- Have you asked an expert in your field to advise you on the value of your research effort?

_____

_____

_____ 2. Have you looked at your research problem from all angles to minimize unwanted surprises?

- What is good about your potential project?

_____

_____

- What are the potential pitfalls of attempting this research effort?

_____

_____

_____ 3. What research procedure will you follow?

- Do you have a tentative plan to review the literature?

_____

_____

- Do you have a tentative plan for data collection?

_____

_____

- Do you have a tentative plan for data analysis?

_____

_____

- Do you have a tentative plan to interpret the data you collect?

_____

_____

_____ 4. What research tools are available for you to use? Make a list and check their availability. Determine how you will use them.

_____

_____

_____

_____

_____ 5. Ask two or three peers to read your proposal. Do they understand what you are proposing to do? What questions do they have? What concerns do they express?

- I have discussed this plan with _____

_____, and _____.

- They have the following questions and concerns:

_____

_____

_____

PRACTICAL APPLICATION Reappraising a Proposed Research Problem

In this chapter we have given you numerous suggestions for identifying an appropriate problem or question for your research. Because the problem is the center and driving force of any research project, we have devoted considerable space to its discussion. We cannot overemphasize that if the problem is not correctly selected and stated, you may put considerable time, energy, and resources into an endeavor that is much less than what it could be.

GUIDELINES Fine-Tuning Your Research Problem

Earlier in the chapter we presented guidelines for identifying and stating an appropriate research problem. Here we offer a few general suggestions for fine-tuning the problem you have identified:

1. *Conduct a thorough literature review.* You have presumably already looked at some of the literature related to your research problem. A next critical step is to make sure you know enough about your topic that you can ask important questions and then make solid decisions about how you might answer them through your research endeavor. You may find that you need to revise your research plan significantly once you have delved deep into the literature related to your topic. We address strategies for conducting an in-depth literature review in the next chapter.

2. *Try to see the problem from all sides.* What is good about this potential project? What is not? Try to take a critical view of what you are proposing to do. Such a perspective will help minimize unwanted surprises.

3. *Think through the process.* Once you have brought your research problem into clear focus, imagine walking through the whole research procedure, from literature review through data collection, data analysis, and interpretation. You can gain valuable insights as you mentally walk through the project. Pay close attention to specific bottlenecks and pitfalls that might cause problems later on.

4. *Use all available tools and resources at your disposal.* Remember that research is always a learning experience. Allow time for learning about new tools or for learning how to use old tools in new ways.

5. *Discuss your research problem with others.* Frequently, beginning researchers need to clarify their problem statement. One good way to do this is to present it to other people in as clear a fashion as possible. If they do not understand, further explanation and clarity are needed. One can learn a great deal from trying to explain something to someone else.

6. *Show the first draft of your written proposal to others for their examination and critique.* Do not hide it because you are afraid someone else may not like your idea or may want to steal it from you. Rarely will either of these events happen.

Continually ask for feedback from others. Ask other people questions about your research problem, and ask them to ask *you* questions about it. Do not be overly discouraged by a few individuals who may get some sense of satisfaction from impeding the progress of others. Many great discoveries have been made by people who were repeatedly told that they could not do what they set out to do.

7. *Remember that your project will take time—lots of time.* All too often, we have had students tell us that they anticipate completing a major research project, such as a thesis or dissertation, in a semester or less. In the vast majority of cases, such a belief is unrealistic. Consider all the steps involved in research: formulating a research problem, conducting the necessary literature search, collecting and interpreting the data, describing what you have done in writing, and improving on your research report through multiple drafts. If you think you can accomplish all of these things within 2 or 3 months, you are almost certainly setting yourself up for failure and disappointment. We would much rather you think of any research project—and especially your first project—as something that is a valuable learning experience in its own right. As such, it is worth however much of your time and effort it takes to do the job well.

8. *Remember that the first drafts of whatever you write will almost certainly not be your last ones.* Good researchers continually revise their thinking and, as a result, their writing. Furthermore, as mentioned in Chapter 1, writing about one's project often helps to clarify and enhance one's thinking. So get used to writing . . . and rewriting . . . and rewriting once again.

Nevertheless, by putting your problem statement on paper early in your research project, you have begun to focus your research efforts.

In this chapter we have given you numerous suggestions for identifying an appropriate problem or question for your research. Because the problem is the center and driving force of any research project, we have devoted considerable space to its discussion. We cannot overemphasize that if the problem is not correctly selected and stated, you may put considerable time, energy, and resources into an endeavor that is much less than what it could be.

For Further Reading

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ANSWERS TO THE CONCEPTUAL ANALYSIS EXERCISE "Identifying Independent, Dependent, Mediating, and Moderating Variables" on page 42:

1. The words "effects of . . . on" tell us the direction of a hypothesized cause-and-effect relationship. Amount of physical exercise is the independent variable. Health and longevity are two dependent variables.
2. Placement of recycling bins is the independent variable; for example, the researcher might vary the number of recycling bins available and/or their proximity to classrooms and high-student-traffic areas. Recycling behavior is the dependent variable; for example, the researcher might count the number of recyclable objects (aluminum cans, sheets of paper, etc.) found in recyclying bins each day or week.
3. The problem statement uses the term *relationship* without necessarily implying that this is a cause-and-effect relationship; for instance, the statement does not include a term such as *influence* or *affect.* However, we can reasonably guess that the researcher is hypothesizing that cell phone usage *increases the risk of* an accident, in which case amount of cell phone use is the independent variable and accident rate is the dependent variable. (To some degree, car accidents lead to cell phone use as well—someone in an accident is likely to call a family member or 911—but that cause-and-effect relationship hardly seems worthy of a research study.)
4. Don't let the sequence of variables mentioned in the problem statement lead you astray here. Text anxiety is the independent variable; test performance is the dependent variable. The third variable mentioned—distracting thoughts—is hypothesized to be the mediating variable: Level of anxiety (independent variable) affects the degree to which one has distracting thoughts (mediating variable), which in turn affects test performance (dependent variable).
5. The word *impact* implies a possible causal connection between bullying (independent variable) and emotional well-being (dependent variable). The nature of a student's relationship with his or her teacher can influence the impact of bullying; thus the student–teacher relationship is a moderating variable.
6. The problem statement includes no words to suggest the direction of a relationship. Certainly, however, career choices can*not* affect one's gender, so any possible causal relationship must go in the other direction: from gender (independent variable) to career choice (dependent variable). The comparative aspect of the problem statement suggests that the researcher suspects that gender might have more of an influence on career choice in some countries (presumably those that adhere to traditional ideas about occupations appropriate for men and women) than in others. Country of residence, then, would be a moderating variable affecting the strength of the gender–career choice relationship.
7. The cause-and-effect relationship between frequent exposure to sunlight (independent variable) and melanoma (dependent variable) is well established in the medical literature. The presence or absence of a particular gene is hypothesized to be a moderating variable: The chances of sunlight leading to melanoma may be reduced—that is, the cause-and-effect relationship may be considerably weaker or possibly nonexistent—if a person has the tumor-suppressing gene.
8. Once again the problem statement talks only about a *relationship,* without using verbs such as *cause, affect,* or *influence* to imply causation. However, the mention of two psychological factors that *underlie* the relationship suggests that the researcher is assuming that either body mass index affects psychological stress or vice versa. Although the problem statement does not clarify which of these two variables is the independent variable and which is the dependent variable, two other variables—levels of depression and anxiety—are apparently hypothesized to be *mediating variables.* Perhaps a higher body mass index (independent variable) increases depression and anxiety (mediating variables) that, in turn, increase psychological stress (dependent variable). Or perhaps, instead, greater psychological stress (independent variable) increases depression and

anxiety (mediating variables) that, in turn, lead to more food consumption and/or less physical exercise (two more, unstated and presumably unmeasured mediating variables), which in turn increase body mass index (dependent variable).

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