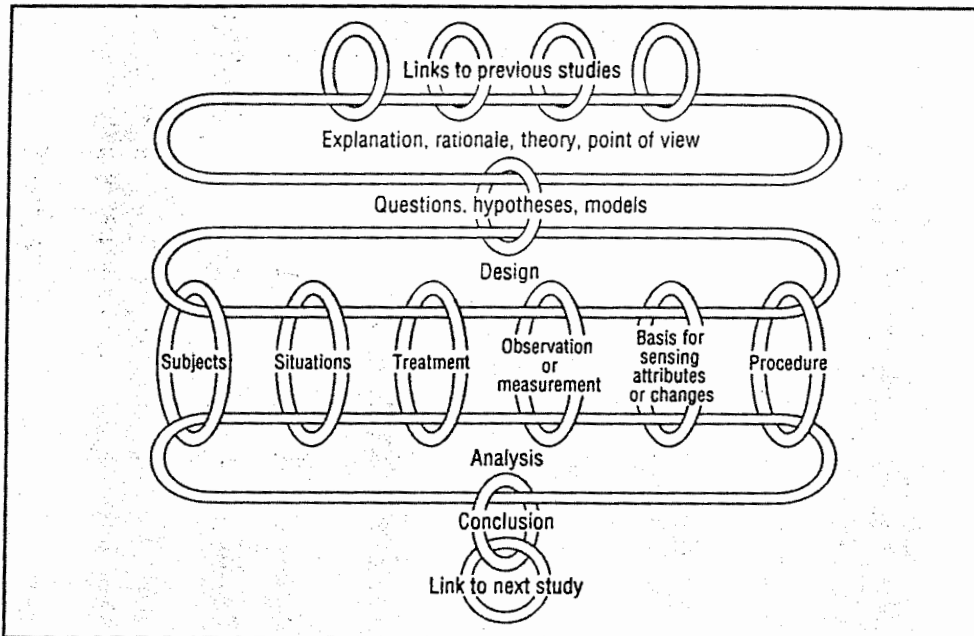


CHAPTER

6

Finding Research Problems



KRATHWOHL, D.R. (1993). Methods of Educational and Social Science Research. New York: Longman.

Someone once said that the difference between a scientist and an artist is the difference between discovery and creation. Another genius, some say, could have discovered Planck's constant—for it was there to be discovered—but only Beethoven could have written Beethoven's sonatas. I think that Chet Carlson's achievement was more like Beethoven's.

Sol Linowitz, former chairman of the board of Xerox Corp., commemorating the fiftieth anniversary of the invention of the xerography process by Chet Carlson (Byrne, 1988).

A question well stated is half solved!

Anonymous

OVERVIEW

Although problem selection is one of the most important of the researcher's tasks, it is often also among the most difficult. Some guidelines and suggestions are presented in this chapter.

Problem selection is usually accompanied by an explanation or a rationale, some notion of why the problem is worth working on. These contribute heavily to the first links of the chain of reasoning.

The chapter also reviews the lore surrounding creativity and discusses the criteria of a good problem.

CHAPTER CONTENTS

The Problem of the Problem	73	Experiences	85
Problem Finding and Research Method	73	Use Others' Data to Answer New Questions	86
Success-enhancing Problem-finding Behaviors	75	Use New Techniques, Instruments, or Models	87
Fill the Mind with the Best Possible Relevant Material	75	Translate Significant Ideas from Other Languages	87
Breaking the Mind-set	76	Criteria of a Good Problem	88
Enhancing Creativity	78	Interest: A Necessary but Not Sufficient Condition	88
Formulating the Problem as a Written Statement	81	Basis in Theory	89
Explaining Ideas to Other People	82	Some Impact in Its Field	90
Other Useful Suggestions	83	Originality and Creativity	90
Potential Sources of Research Problems	85	Feasibility	91
Draw Suggestions from Other People's Research	85	Researchable Questions	93
Keep a Log of Ideas and		Summary	93

THE PROBLEM OF THE PROBLEM¹

Though the choice of problem is the most important decision each researcher makes, it is really a gamble! There is no certain way of telling at the outset whether the investment of time, energy, and resources will yield any return—with luck, a reputation can be made. But is it luck that some persons are known as good researchers? No more so than that some people can make an honest living from the stock market, generally win at cards, or beat the odds at a racetrack.

There has been little research on problem finding, but what there is suggests that some individuals are consistently better at it than others (Getzels, 1982). All of us know individuals who are particularly creative; I have known some from whom I got a good idea every time I talked with them. But Osborn and others involved with industrial training have data that suggest that creativity can be increased (Osborn, 1959; Parnes, 1967). Therefore, most of us can benefit from advice.

Clearly, some individuals improve their odds of being successful by combining knowledge of the situation with their ability to recognize an opportunity and by carrying thought into action. Indeed, studies in the past have shown that relatively few researchers account for the bulk of useful work: 10 percent of scientists account for fully 50 percent of published research, and 10 percent of published research accounts for 40 percent of the citations in books and articles (Pletz, 1965; Price, 1963). There is little reason to believe the current situation differs. So often in reading research we find that it focuses on something in a familiar situation to which we hadn't paid attention earlier. We might have noticed it, but to our regret, we did not act on it as the author had. Modifying such behaviors, as well as learning some of the behaviors that enhance problem-finding capacity, will improve research capabilities.

How can this book help with a creative process that by its very definition defies being tied to formulas or routine processes? We can describe behavior and activities characteristic of productive researchers and suggest some creativity enhancement techniques. You can then choose the behaviors that seem personally effective. Further, we can describe the criteria of good problems so that when one appears, you will recognize it. Finally, we can note the kinds of problems that are not amenable to research methods. These topics outline the plan of this chapter.

PROBLEM FINDING AND RESEARCH METHOD

Some persons prefer to find a problem, get it well defined, proceed to gather data, analyze it, and write it up. Each step follows from the next in a deductive

1. This is the title of an excellent chapter by Getzels (1982) on problem finding. He is one of the few people who have done research on problem finding and formulation.

sequence. Research is often visualized in this way. Although a deductive stance can be used with any research method, it is usually associated with the quantitative method. Individuals who adopt this way of working start with considerable time invested in problem finding, problem redefinition, and the literature search (the topic of the next chapter). As we have previously noted, other researchers prefer to immerse themselves in a situation of interest and let the research project emerge as they explore what is there. This is an inductive approach that is usually associated with qualitative research but can in fact be used with any method. Data collection can begin as soon as what might be called an "orienting question" is identified—one that focuses and directs our attention, although that focus can be quite broad. It may be simply a situation of interest. In the Hoffmann-Riem study presented in chapter 2, the problem began as "how parents view adoption" and ended up as "how adoptive parents normalize their lives."

Incidentally, *for most of us, research method follows problem choice*. Once we know what to study, we adapt it to a method of investigation with which we personally feel comfortable. Novice researchers often work through the details of method before they are clear on what the problem is. Working back and forth between problem and method is fine if you don't go too far with method before the problem is clear. To a certain extent, working on method helps formulate the problem, and vice versa. It is an iterative process.

The heading "Problem Finding" suggests that it is a one-time process, that a problem, once defined at the outset, does not change. Nothing could be further from the truth. In qualitative methods, problem finding is a continuous process. There is the possibility of the problem's being redefined right up through data analysis. This is also true of quantitative methods. If something new and interesting shows up in data analysis, the whole focus of the problem can shift, sometimes to a phenomenon quite different from the study's original one. But even if it doesn't shift, problem redefinition usually continues into the final stages of the study. This doesn't show in the report of the study; it is written as a deductive chain of reasoning. But such reformulation is frequently an important part of the research process. After all, only as completion of the study informs us sufficiently about the phenomenon do we understand the really important questions to ask.

- Problem finding and the redefinition of the problem and its focus are continuing processes.
- The best questions often become known only at the end of the research, when the phenomenon is better understood.
- Inductive or deductive approaches can be used with any method. The relation of problem finding to data collection and analysis is typically a function of method.
- In the typical case, a clear conception of the problem emerges earlier in quantitative methods than in qualitative methods, where it is sometimes delayed into the data-analysis stage.

SUCCESS-ENHANCING PROBLEM-FINDING BEHAVIORS

We shall describe behaviors characteristic of productive researchers. These points are drawn from autobiographies of researchers, from observation, and from the advice of people who have studied creativity.

Filling the Mind with the Best Possible Relevant Material

Filling the mind with the most relevant material from one's area of interest is one of the very best ways to find a good problem. This allows the unconscious mind to process the material. The unconscious is an amazing tool. While we are sleeping or working on other things, it organizes the material and presents us with insights that often escape us when we are too wrapped up in achieving a goal. But the unconscious must have something to process, hence the necessity for a prepared mind.

Discovery Favors the Prepared Mind. As a child, you knew your backyard, alley, or room better than anybody. You proudly showed parents or siblings things to which they had never paid attention—an interestingly shaped hole, a place where some animal lived, a spot that formed the silhouette of a person significant in your life. Such knowledge comes from long, intimate contact with the situation. Research is no different; there is no substitute for knowing the territory. Fleming's discovery of penicillin was accidental, but his prior work prepared him to recognize the breakthrough when it appeared. He noticed that his bacterial cultures seemed not to grow where there was mold on the plate and wondered why this occurred. No doubt this had happened to other investigators. Fleming, however, being curious and having worked intensely with bacterial cultures, asked why and, acting on that query, made an important discovery.

Have you had the experience of learning a new word that you thought was quite rare? Once you learned it, you were surprised at how often you heard it thereafter! It must have been there before; it is a matter of the prepared mind. So dig into what is already known about a phenomenon; immerse yourself in the literature and explore each of its important facets. If that does not suggest the desired research problem, you will be sufficiently familiar with the area that when the unusual appears, you can recognize and act on it.

Read the Writings of the Seminal Minds in the Field. Productive scientists seem to read more deeply into the historical background of their problems. They read the original versions, not digests, of the best minds in the field. Often those minds will have sensed something that was not well enough understood in their time to explain clearly. In other instances, they took a problem as far as they could at the time, but now you could take it further. Reading such accounts against the background of what has happened more recently often gives a new and useful perspective and meaning. For instance, Campbell and Stanley (1963), who made important contributions to understanding social

science experimentation, start with a tribute to W. A. McCall (1923), whose then 40-year-old book described many of the concepts they use in their exposition.

Behaviors that enhance problem finding include these:

- Filling the mind with the best possible relevant material. Discovery favors a mind prepared to recognize the unusual aspects of a field.
- Reading the writings of the seminal minds in the field, both past and present.
- Delving into the historical background of the problem to learn which approaches have and have not proved fruitful.

Breaking the Mind-set

We have all been exposed to problems that require us to think about them differently in order to solve them. Remember the game of passing the scissors? One person passes scissors to another, saying, "I'm passing it to you crossed" or "I'm passing it to you uncrossed." Players unfamiliar with the game are puzzled; no matter how they position the scissors, they can do it correctly only by accident, if at all. They are concentrating on the scissors. Only when they realize that "crossed" or "uncrossed" refers to the position of the person's legs when the scissors are passed do they break the mind-set of concentrating on the scissors. Breaking the mind-set, viewing an area or a problem differently, is often the secret to an important piece of research. The following are often useful mindset-breaking tools.

Read Actively (Anticipate the Author; Don't Just Follow Passively). Active reading is one of the most important skills to develop. *Anticipate* where the material is going; *project* the argument that is being fashioned instead of passively following it. We process what we read more thoroughly if we underline or make marginal comments. This reduces reading speed and allows time to think ahead to where the argument is going. As we foresee what is coming, we will often find that the author zigs where we zagged. If our logic is correct to that point, why the zig? Here is a question worth pondering; a zag might have been a more profitable course to follow. We have to retrace our steps carefully to be sure, but many new leads are discovered by active reading.

Along these same lines, actively search for inconsistencies in the argument. Look for gaps where existing ideas do not adequately account for the phenomena. This may call for revision of existing explanations or even for new ones. For example, Merton (1959) notes that regularities in cultural behavior are typically thought to result from prescriptions by cultural norms. Yet, he notes, "Men have higher suicide rates than women, for example, even when the cultural norms do not invite males to put an end to themselves" (p. xxiii). Apparently, there are regularities that result from something besides

cultural norms, and the concept of cultural norms must be reworked to specify what kind of behavior lies beyond them.

Talk to Specialists in the Field. Experts who have worked in a field for a long time have built up their own conceptions from their experiences. Although researchers who have compared the problem solving of experts and novices note little difference in strategies, they find a difference in the repertory of experiences organized in long-term memory. Chase and Simon's (1973) study of chess players illustrates this nicely. Grandmasters and masters were asked to reconstruct the positions of 22 chess pieces after viewing them for five seconds. When the positions were taken from actual games, experts could place 81 percent of them without error, whereas novices could correctly position only 33 percent. But when pieces were arranged at random, experts were no better than novices, placing only three to five pieces correctly. Experts apparently identified games in terms of patterns they had learned or experienced instead of memorizing the position of individual pieces. But note the specificity of their knowledge; their capabilities would not apply to another subject matter. Research in this field suggests that these long-term memory patterns are subject-matter-specific, so choose an expert in your field.

If you ask specialists to tell you about a problem or area, they may, to communicate easily with you, fall back on textbook formulations. Instead, try your ideas on them so that they can see your problem through your framework. They may be able to react to it intuitively in terms of their experience—especially in reconceptualizing the problem and in making connections to other areas of work that may not be readily apparent. Experts are also useful sources of pertinent things to read (despite the advances in computer searches, *the human mind is still the best retrieval device*—see the comment regarding invisible colleges on p. 114 for ways to tap it).

Assess the experts' reactions carefully, however. Some persons discourage creative ideas that weren't original with them. And some, for whatever reason, often lack of time, may not grasp your problem and react superficially. With these caveats in mind, you will find that experts can be extremely valuable and save you much time, especially by helping you avoid false and unproductive leads.

Challenge Assumptions. When reading past research, examine the assumptions on which the arguments are based. Are they reasonable? Could we make less restrictive ones? What would be the result? If we change the assumptions, does this lead to different consequences? Consider the problem of the mentally handicapped. If we assume that they learn essentially as does everyone else, but more slowly, then given sufficient time and motivation, they could achieve normally. The consequences of this view are to give the individuals more time, to isolate them in classes where the competition is less intense, and to motivate them to achieve. A different assumption is that the conceptual structures into which they fit what they learn are not the complex ones that others can use. This assumption leads to the search for simplified conceptual structures they can learn that will result in learning approximating that of more normal children. Examining the assumptions about why slow learners are handi-

capped results in quite different consequences for remediation, each of which can be tested for validity. (See the application problems at the end of this chapter for other examples.)

Look for New Ways to Tease the Problem Apart. Psychologist Daniel Kahneman suggests a trick for questions about behavior that he claims derives from Lewinian psychology. Instead of asking, "Why does a person behave this way?" he asks, "Why doesn't he behave otherwise?" Instead of asking why a person is hostile in a particular setting, he asks, "Why isn't he more hostile?" "Why isn't he less hostile?" The kinds of answers made available by this reformulation are radically different from those derived from "Why is he hostile?" It may be much easier to remove the factors driving him to greater hostility and uncovered by the question "Why isn't he less hostile?" than to suppress the hostile behavior by manipulating forces that push him to be less hostile than he was. Sometimes it also helps to switch the focus of attention consciously from the result to the process of getting there.

Another way of teasing the problem apart is to look for concepts that have not been effective in differentiating important aspects of a phenomenon. Merton (1959) notes that concepts used to describe a phenomenon have often taken us as far as they will stretch. We need new concepts and new differentiations to take us further. For example, at one time, psychology talked about self-concept as though it were a single entity; one felt positively or negatively about oneself. Later, we came to realize there are different self-concepts and that we can talk about a self-concept of ability (one's capability in solving academic problems) and even self-concepts in different subject matters. Thus the term *self-concept* has come to be highly differentiated, and there are now several books available dealing with these different meanings (e.g., Wylie, 1979).

Breaking our initial view of a problem, our mind-set, is often critical to problem finding. The likelihood of breaking a mind-set is greater if we follow these suggestions:

- Read actively, anticipating where the author is heading.
- Analyze the approach the author is using and synthesize it with that of others' and our own prior knowledge.
- Challenge the assumptions that undergird a particular approach to a problem.
- For concepts that do not adequately differentiate their important aspects, look for new ways to ask the question that better target the area of interest.

Enhancing Creativity

Suggestions for the enhancement of creativity nearly always include breaking the mind-set, but they also include harnessing the unconscious, organizing material into suggestive patterns, and reducing the censorship of ideas.

Harness the Unconscious. There comes a point when we have read enough to have a flavor of what has been done, but new approaches have not suggested themselves. Here it is well to recognize that our minds do not always do their best work when we are consciously pushing at a problem. William Safire gives the first rule of holes: "When you are in a hole, stop digging!" Then let the unconscious mind take over. Read Raudsepp's (1977) recitation of the testimony of the greats on this score:

Dostoevsky found that he could dream up his immortal, moving stories and characters while doodling. Brahms found that ideas came effortlessly only when he approached a state of deep daydreaming. And César Frank is said to have walked around with a dreamlike gaze while composing, seemingly unaware of his surrounding. . . .

John Dewey stated, "I do not think it can be denied that an element of reverie, of approach to a state of dream, enters in the creation of a work of art. . . . Indeed, it is safe to say that creative conceptions . . . come only to persons who are relaxed to the point of reverie. . . ."

Thomas Alva Edison also knew the value of "half-waking states." Whenever confronted with what seemed an insurmountable hitch defying all efforts, he would stretch out on his workshop couch and let fantasies flood his mind. (pp. 27–28)

Poincaré (1913) concluded that the unconscious mind collates and sorts random possibilities among pertinent variables at a rate that defies the efforts of the conscious mind.

We must all find our own best means of commanding the muse, but the unconscious is an important resource too rarely emphasized. Some people are helped by daydreaming, a reverie in which the mind floats over the problem, rejecting no possibilities. Some adopt a kind of half-awake, half-asleep posture. Still others get their best ideas at night and keep paper and pencil at hand to record ideas immediately, lest they be unable to retrieve their thoughts upon becoming fully awake. Whatever your means, use the unconscious; it is one of the most powerful tools of creativity available.

Organize Material into Suggestive Patterns. There are many ways of doing this, using the themes suggested earlier of filling the mind; letting the unconscious work on it and then analyzing and synthesizing the products; and repeating this cycle until a satisfactory solution is found. One set of steps outlined by Zwicky (1969) extend what Allen (1962) called morphological analysis:

1. Without evaluation, transfer all the material about the problem onto cards (3-by-5-inch cards cut in half are a good size)—ideas for solving the problem, achievements desired, names of persons involved, books that might be consulted, and so on.
2. Disregarding order, lay the cards out in blocks three cards wide and four cards deep. Read the cards rapidly four or five times; this transfers the ideas into your subconscious mind. For the next half hour or so, leave the cards and occupy your mind as completely as possible with other matters.
3. Study the cards and categorize them into friendly or congenial groups. Five hundred cards might reduce to 20 or 30 such groups. Place a title

card in a distinguishing color on each group; we'll call each group a "component."

4. Treat each component as you did steps 2 and 3, reducing to four to seven groups by creating more inclusive categories with titles we'll call "parameters." (Was it chance that Zwicky chose seven as the maximum? Or did he sense intuitively what memory research later showed?—in general, the mind can only handle about seven things at a time.)
5. Reduce the number of components in each parameter to seven or fewer. Prioritizing of parameters or components may be necessary to reduce the possibilities to a manageable number.
6. List each parameter, followed by its components, on a separate strip of paper, and move the strips alongside one another to suggest different combinations from which solutions may emerge.

Having such a model to follow may have value in that all the possible options are covered. Elstein, Shulman, and Sprafka (1978, 1990) have shown that in medical problem solving, having a model, or heuristic, to follow increases effectiveness.

Reduce the Censorship of Ideas. What is typically called "brainstorming" involves admitting possibilities for examination that would normally be rejected by typical problem-solving processes. Popularized by Osborn (1959), it consists of assembling a group of people to attack the problem with four basic rules of interaction: criticism is ruled out, freewheeling is welcomed, quantity is wanted, and combination and improvement of previous suggestions are sought. At one time a fad, this technique is still useful. It may lead to time-consuming consideration of impossible suggestions yet may free individuals to consider desirable ones that would otherwise have been discarded. It is sometimes particularly useful to think of analogies, such as "How is this phenomenon like an animal?" Once the bulk of the ideas has emerged, they are sifted to select the best ones for further development. Sometimes these, in turn, become the focus of brainstorming sessions and the process is repeated.

Creativity is an essential ingredient of good problem finding. Creativity may be enhanced by employing these three tactics:

1. Harnessing the unconscious, one of the most powerful of all the creativity tools.
2. Organizing the material into patterns that are suggestive of relationships.
3. Using "brainstorming" or a similar technique under which the censorship of ideas is reduced: permit no criticism, seek the largest number of ideas, encourage the combination and enhancement of ideas, and evaluate for quality only at the end of the activity.

Formulating the Problem as a Written Statement

Trying to set down our thoughts involves both clarifying and organizing. As Merton (1959) puts it, try formulating questions that register our "dimly felt sense of ignorance" (p. xxvi). Writing enforces a discipline that helps articulate half-formed ideas. Something happens between the formation of an idea and its appearance on paper, a latency that somehow results in the clarification and untangling of our thinking. Writing helps bring unconscious processing to light as articulated synthesized statements—just what we are seeking! When we are reading widely, we cram the ideas into our memory, often without checking them against what is already there; even contradictory material may exist side by side. Writing makes us confront these internal inconsistencies and put together relationships.

Sometimes continued work at a problem pays off. Witness Albert Schweitzer in a new translation of his autobiography:

For months I lived in a continual state of mental agitation. Without the least success, I concentrated—even during my daily work at the hospital—on the real nature of affirmation of life and of ethics and on the question of what they have in common. . . . I saw the concept that I wanted to attain before me, but I could not . . . formulate it. While in this mental condition I had to undertake a long journey on the river. . . . Slowly we crept upstream. . . . Lost in thought I sat on the deck of the barge, struggling. . . . *I covered sheet after sheet with disconnected sentences merely to keep myself concentrated on the problem. . . .* Late on the third day, . . . there flashed upon my mind, unforeseen and unsought, the phrase, "reverence for life." The iron door yielded. The path in the thicket became visible. (Schweitzer, 1990, p. 155; italics added)

What struck me about this passage was the italicized sentence. It is so typical of good writers that even when blocked, they persist with provisional tries, seeking to formulate what they are after. Schweitzer "covered sheet after sheet with disconnected sentences" until he succeeded. The problem doesn't always yield, but the effort is worth making.

Slowing the writing process may help with difficult formulation. I can type when I know what I want to write, but I must write with a pen when I'm struggling, and as a last resort, a fountain pen seems to work better than a ballpoint. Note that each method takes progressively longer to form the words on paper. I can hold longer internal discussions with myself about what comes next, do a memory search for the right concept or word, and still get it down without unduly interrupting the flow of thought. This is important. Poor writers are often so taken up with grammar, spelling, or even forming words that their thinking may be interrupted to the point where they have difficulty remembering where their sentence was going.

In the preface we noted that internal processing is the name of the game, processing that results in "chunking" material into meaningful collections. The networking of these chunks makes connections that bring to mind new material. Further, the "chunks" of experts are larger and more complex than

those of novices. Artists seem to have learned this process intuitively since they often spend large amounts of time practicing and rehearsing their material before producing a masterpiece. Sinclair Lewis developed notebooks that described his characters and their complete setting before he wrote a novel—their personalities, what they wore, even maps of the community and floor plans of the buildings. Did he refer to the notebooks when writing? I don't know, but I suspect that advance rehearsal chunked this material so effectively that he had little need to. Similarly, before Andrew Wyeth did his Helga pictures, sketch after sketch was discarded on the floor, some of which he even proceeded to walk on. They were chunks transferred to his mind for use in later drawings. Darwin carefully indexed the books he read and organized the material into portfolios that he consulted at the beginning of each new project (Steiner, 1984).

So everyone uses chunks in problem solving, and the best writers and thinkers find that it takes work and time to build those chunks and their relational network. Maybe that is one of the differences between the greats and the not-so-greats: the willingness to do the work that is involved in building and relating the chunks that go into a masterwork.

Explaining Ideas to Other People

Similar to writing is trying to explain our ideas to someone else, ideally an uninformed but intelligent observer. It is said that the best way to learn something is to teach it. In trying to communicate clearly with someone who is unfamiliar with your area of expertise, an idea must be formulated with a clarity that makes no assumptions and avoids jargon. In starting at the beginning to explain a concept, you may recognize aspects that you take so much for granted that they escaped your focus. Examination of those assumptions may provide new insights and lead to ways of reconfiguring the question or problem. But talking isn't enough; you must get everything down in writing while it is still fresh and you are still enthusiastic about the idea! You'll write much better sooner than later; indeed, later you may have difficulty in recapturing the idea.

Problem perspective is enhanced if we see material from a different angle or in a different context. This an intelligent observer can do better than we can for ourselves. When we are close to a problem and emotionally involved in it, we miss things that are obvious to a naive observer. An observer can maintain psychological distance from the problem. Scheerer (1963), for example, assigned subjects randomly as observers and workers. The workers were to solve a problem that required use of a missing piece of string. A string that they could use was present in the form of a hanger for a wall calendar. Only half of the workers—but all of the observers—broke the mind-set of the string as hanger and solved the problem.

Talking with others also may restore a sense of excitement and sometimes competition. Determining the structure of DNA was a race between Watson's Cambridge laboratory and others, among them Linus Pauling's at the Univer-

sity of California. A sense of competition was heightened through a visit of Pauling's son to discuss their parallel progress (Watson, 1968).

Making provisional tries at formulating a problem in spoken or, especially, written form is a very useful behavior in problem formulation, for four reasons:

1. It makes available the articulated and synthesized relationships formed by the unconscious.
2. It facilitates the "chunking" of material and the networking of those chunks. Building the chunks of an expert takes time and effort but is probably one of the major contributors to excellent work.
3. It provides material to which an observer can react from a different perspective and without one's own emotional biases and pre-conceptions.
4. Interaction with others not only may provide new insights but may also restore a sense of excitement.

Other Useful Suggestions

Learn Your Most Productive Working Conditions. Become aware of the conditions that make you productive. For instance, there is probably an optimal level of motivation. At a higher level, you may be unable to stay focused long enough to allow patterns to be perceived. Administrators, in particular, are inclined to think that if a little motivation is good, more must be better; this is not necessarily so.

Where and when you work can be important. Find a place without too many distractions. Many productive writers set aside a regular time for writing, staying at it during that period with provisional tries, whether they are blocked or not.

Patterns of writing are particularly likely to be unique to each person. Outlining used to be considered a *sine qua non* by many English teachers. Neil Simon, the famous playwright, was advised to try it:

"I . . . tried to make it go that way. It wouldn't! I did it 20 times! That is not the way to write a play. . . . Because that is not the way life is. You don't know what the end is going to be so you don't twist and push it. It just carries you along, somehow, predetermined by your character." (Rosner and Abt, 1970, p. 363)

Clearly, outlining was not for him, nor may it be for others, but there is some pattern that is better for each of us, and we must find it.

Don't Close the Problem Definition Too Quickly. Getzels and Csikszentmihalyi (1976) found the most creative solutions among artists who kept the problem open longer. They suggest that solutions must be discovered by interaction

with the elements that constitute it—mucking around in the problem. Superficial solutions are also likelier to be rejected if closure is delayed.

When Having Trouble Focusing, Move to Basic Questions. Research problems frequently grow out of the common interests or annoyances of daily work. But this often forces the researcher to look for solutions where too little is known about the phenomenon. For example, in trying to get a focus on the problem of how to induce teachers to engage in in-service training, a researcher chooses teacher centers. But what to study about such centers? He could wander over many different aspects of them with no more guidance than that. In such instances, it helps to ask more basic questions: Why do teachers seek training in the first place? What purposes does it serve besides improved teaching? Does it enhance social functions, pay improvement, chances for leadership? These begin to approach in-service training from a coherent viewpoint about the purposes it serves; we can then begin to think more reasonably about designing teacher centers around those purposes. Every problem is part of a causal chain as described in the first few pages of chapter 12. Sometimes it helps to work backward in the causal chain to earlier stages.

Trim Away Your Entry to a Problem as Soon as It No Longer Fits. When the development of a human fetus is traced, there is always considerable surprise that it seems to go through all the developmental stages of a previous evolution, for example, developing useless gills, which then atrophy and become something else. As problem statements develop, their introductory statement tends to grow, retaining the problem's developmental history, recapitulating useless aspects that no longer contribute to the current problem. It is useful to us in that it retraces our thinking and gets us into the problem. But sometimes it is simply excess baggage. Other people can usually see this more easily than we can. It also is more apparent after the passage of time. The sooner this excess material is trimmed away, the stronger and more clearly we can develop the problem.

Some individuals find themselves rewriting the introduction to their problem every time they leave the work for a period of time. Not only is this likely to result in an introduction that needs to be trimmed, but it tends to be unproductive labor. Write where you are ready to write rather than writing linearly. You'll have to work to fit the pieces together, but you are less likely to be blocked. Rather than leaving the work at a point where a section is complete, stop at a point that cries out for completion and you know what you plan to do next. It will be easier to pick up at that point.

Suggestions for enhancing problem finding include these:

- Learn the conditions under which you are most productive.
- Keep the problem definition open and fluid until you are satisfied with the way it has been shaped.

- Move back to basic questions about a phenomenon if you are having trouble focusing.
- Trim away old entry statements to the problem so that the current one can be developed clearly and forcefully.

POTENTIAL SOURCES OF RESEARCH PROBLEMS

Draw Suggestions from Other People's Research

A review of the literature in an area of interest is the most common way to search for a problem. In reading the literature, examine the suggestions for further research in articles, at the end of dissertations, in critiques of other people's research, and especially in research reviews. Research reviews may provide only the most obvious questions, but their authors are in an especially good position to give an overall perspective. Good questions are likelier to be encountered than full-blown research suggestions. Remember, the more you know about an area, the more questions you have about it and the better you can differentiate central from tangential ones.

There is another side to these suggestions that you must keep in mind, however. Researchers may reserve their best and most practical suggestions for themselves and include in their "next steps" section only "pie in the sky" ones that they don't see a way to handle. There may be hidden problems in the suggestions that are apparent to them because of the work just done. They will discuss such difficulties if you contact them personally but may not have gone into those aspects in their writing. A parallel to the "Peter principle" (Peter, 1969) applies to researchers: they often carry a line of investigation as far as it is profitable.² By contacting a researcher who has dropped an area, you may learn that it was dropped because of the attraction of new research. But the information may also save you from rediscovering a difficulty at first hand.

Keep a Log of Ideas and Experiences

Immersion in a situation of interest is one of the best ways to learn where there is research potential. Consider a pilot observational study in a role that permits you to learn. Enlist as a teacher's aide, "shadow" a social worker or administrator, work as a custodian or maintenance worker—try any of a variety of roles that allow access, preferably as unobtrusively as possible. This

2. The Peter principle states that persons are promoted to positions calling for higher levels of skill until they reach a level for which they are not competent. In a similar manner, researchers often work on a problem until their skills of attacking it will carry them no further.

stage can be both exciting and frustrating—exciting because of all that is new and interesting, frustrating because there are so many leads to follow. Simultaneously reading about the situation will bring new meaning to what you are reading and new understanding to what you are observing; reading and observation each inform the other. Student teachers grow tremendously in their observation of classrooms from observing who interacts with whom, how individuals play games with one another, how some individuals manifest insecurity in their overt action and others mask it. But simply observing is not enough: you must process, think, compare and contrast observations. That means keeping a log.

Keeping a log of your work and ideas is a tradition honored more in the natural than the social sciences. Past researchers made a fetish of keeping a research notebook. Thomas Edison kept copious notes on all that went on in the laboratory because they kept ideas from getting lost. Research managers usually keep notebooks to catch the ideas that flash into their minds. Often the difference between the person who is credited with an idea and one who ends up saying, "I thought of that long ago!" is that the former captured the idea and acted on it. Further, keeping a log has all the advantages discussed in the section on formulating the problem as a written statement.

Use Others' Data to Answer New Questions

Data banks are rife with records waiting to be built into significant research studies. Computerization makes access easy once the codes that facilitate labeling and interpreting the data are obtained. Data from longitudinal studies, large-scale surveys, and major social experiments are frequently available to researchers. Directories of databases (such as Williams, Lannom, and Robins, 1985) display the wide array of available opportunities.

Coleman's widely quoted studies of public and private schools (Coleman, Hoffer, and Kilgore, 1982) grew out of routine data collection by the U.S. Department of Education and exemplify the kinds of studies that can be extracted from these files. Considering the tremendous sums invested in gathering these data, the possibility of using them has obvious attractions. There are many approaches: using new or more appropriate methods to reanalyze the data, tracking a subgroup over time, partitioning the data to determine how deeply certain trends reach, or combining subgroups or even data from different studies to see whether a trend emerges.

A few words of warning are in order, however. First, most old research hands would suggest that having a question and then searching for useful data is more likely to result in a significant study than the other way around. Otherwise, you may too quickly compromise problem quality to fit available data. Second, anyone who has gathered field data quickly learns the variety of conditions that can compromise data quality and introduce anomalies. If possible, talk to the original investigators or examine any available records that bear on data quality.

Use New Techniques, Instruments, or Models

New techniques of analysis open up new avenues of investigation and permit reanalysis of significant data. When Carl Rogers began recording counseling interviews and categorizing the data, he noticed a pattern in the negative affect that had not been apparent earlier (Rogers, 1951). Over successive counseling sessions negative self-references initially rose but then declined with problem resolution. This began a significant era in counseling-methods research made possible by the advent of magnetic recording. It allowed interview statements to be carefully categorized and coded. Bales (1950) devised interaction analysis, a method for recording and analyzing the interaction of individuals in groups. It led not only to considerable fruitful research on how groups work but also to spinoff instruments for use in analyzing classroom behavior. Both groups and classrooms had previously been researched only with relatively crude judgmental scales. New instruments permit problems to be examined that were not previously reachable. Indeed, one indication of progress in a field is the accessibility of its phenomena by measures. (The University of Chicago's social science research building has Lord Kelvin's motto carved over the door: "When you cannot measure your knowledge is meager and unsatisfactory.")

Metaphors, analogies, and models are particularly helpful in examining problems. Education has typically been looked on as a necessary function for maintaining a culture and an informed electorate. When it began to be looked at as an investment, using economic terms and models, a new perspective was gained that was especially useful to third-world countries seeking to catch up to the rest quickly. Van den Haag (1956) used the idea as his dissertation and showed the very interesting consequences if all higher education were viewed as an investment and individuals were required to pay the full cost of instruction (he concluded that all fields except the humanities should pay their way). Homeostasis is another example that, used as a model for stable systems, has spread through nearly every field and is the basis for the technology of systems theory.

Translate Significant Ideas from Other Languages

Although the United States and other English-speaking countries have been leaders in data-based social science, there is a long tradition of thoughtful examination of such problems in other countries. It has resulted in some of our most important conceptualizations—Durkheim's and Freud's, for example. Finding such material and bringing it into mainstream English-language literature is important. This requires sufficient knowledge of a foreign language that you can both recognize significant ideas in that language and translate them accurately enough so as to be useful to others. Piaget's work with children was available to those who had mastered French years before his work became popular in English-speaking countries. Such lags still exist. The introduction of such ideas in understandable form is a real service and can lead to significant advances in a field.

Potential sources of problems to research include these:

- Suggestions made by others as they finish a study or review an area of research
- Problems growing out of our logs of activities in exploring an area
- Databases resulting from routine collection or past research studies
- The application of a new technique, instrument, or model to old approaches or their use to open up new ones
- Ideas in other languages and from other cultures with general applicability

CRITERIA OF A GOOD PROBLEM

The process of problem finding is similar to the actions of a camera buff with a new zoom lens. She goes to an area that interests her and starts with a distance wide-angle shot, surveying the landscape. As she sees something of interest, she zooms in to explore it and see if there is anything there. If there isn't, she zooms out again, exploring other facets. In time, she'll scramble to a new vantage point, looking at the scene from a new angle, again zooming in and out in a search of the nooks and crannies of the landscape. This sets her off, scrambling over rocks and hillocks for better and new views. But when is the picture just right? When does she stop the search and start composing the picture? That is the function of discussing the criteria of a good problem, so that you'll recognize one when you see it. A good problem is (1) of interest, (2) embedded in theory, (3) likely to have impact, (4) original in some aspect, and (5) feasible—within your conceptual, resource, ethical, and institutional limits. Add to these Teplin's tongue-in-cheek suggestions (Youngstrom, 1990, p. 7):

- The Goldilocks test: Is the research question so broad it's untenable, so narrow it's dull—or is it just right?
- The five-year test: A five-year-old should be able to understand the purpose of the project.
- The blood test: People besides your blood relatives should want to read the research results.

Interest: A Necessary but Not Sufficient Condition

For most researchers, interest is the prime qualification, for it provides the motivation to work on the problem. As one doctoral student put it: "It's your baby, so it better be one you can love when you are up with it at night!" (Grant, 1986). Professors' files are full of projects that failed this test and the many doctoral ABD (all but dissertation) candidates are further testimony to its

importance. Clearly, it is one necessary condition; the other is feasibility. But beside these two, a problem should have as many of the following characteristics as possible.

Basis in Theory

The impact of isolated studies is trivial. But a study can contribute to explanations and significant ideas. It can provide the base of data for understanding them, for contradicting and correcting, modifying, extending, or in other ways interacting with them. Then a study's impact is multiplied. As it affects the network of previous findings, it becomes embedded with those ideas and shares in their implications and effects. Problems that either build new rationale and theory or affect previous work are less likely to get lost and more likely to have impact.

The power of Skinner to sway people to behaviorism lay not in his individual studies of learning, though these were important in building the base. Rather it lay in the rationale he built around these findings, which had important implications for explaining much of human activity; he even used the theory to suggest how language develops (Skinner, 1957).

Similarly, Piaget, whose formal experimentation must be considered minimal and whose large-scale research is nonexistent, proposed a set of stages of development that had implications for teaching children. It was the power of his theory, his explanation of phenomena, that resulted in his impact. Think of others who have had an impact on social science, and almost without exception, it is the power of their ideas that is the dominating factor. Empirical research not related to that body of thinking tends to be isolated from it and to have less impact.

Perhaps you are asking what is meant by theory. Simply put, we mean an explanation of behavior that makes good logical sense and either is consistent with the research and explanations that preceded it or convincingly negates or modifies them. Discussion of what constitutes a good theory could fill the rest of this book. Many social scientists agree that we don't have the kind of grand and precise theories that natural scientists are seen as having; some would argue that we don't have anything worthy of the name. Be that as it may, nearly all would agree that ideas that unify a variety of findings and assimilate them into a cohesive and interrelated body, as behaviorism does, for instance, are most useful. When we consider the myriad things that could be researched in a situation, theories help us find the significant variables. They suggest research directions and help locate points where research is needed to bolster arguments. They provide a network into which new findings can be integrated; the extent to which such findings fit the network tends to support or weaken our faith in them. Good problems are strengthened when they relate to theory.

When choosing or developing theory, be guided by what Yvonna Lincoln, in a speech at the American Educational Research Association convention, called the Coco Chanel principle: "Simple is always elegant, ultimately timeless and usually in fashion. Parsimony is prettier!" When

choosing among explanations, choose the simplest that adequately covers the data.

Some Impact in Its Field

Beyond interest and feasibility, the criterion most researchers consider most important is impact. We have already considered one way in which you can have impact on your field, but there are other aspects to consider. Indeed, some persons have considerable difficulty finding a problem because they are not satisfied with what they perceive as the potential impact of the outcomes.

In the context of program evaluation, Cronbach (1982) describes impact with the term *leverage*, but his ideas are relevant to research as well. "Leverage refers to the influence that reducing a particular uncertainty has on decisions" (p. 226); this may be uncertainty about whether the relationship exists or uncertainty about its generality. After the study is completed, "leverage is directly visible in the response of the community to the evidence" (p. 226). A social worker is concerned because the content of in-service training is determined by the supervisor instead of the workers themselves. She does a study to show that the training is more effective when planned and implemented by the workers than by the supervisor. Her intent is that supervisors will be deterred and workers will be empowered in the determination of in-service training. But such an intent ignores the realities of responsibility and administrators' perceptions of their roles and is likely to have little impact—leverage—in changing the situation.

Similarly, a study intended to show increased effectiveness of instruction with smaller classes will have greater impact if the increased cost of schooling is related to the value of what is achieved. But a study that shows that a teacher can be more effective by using certain behaviors may have considerable impact if the cost and difficulty of learning those behaviors are low. Hence impact must be gauged by an accurate understanding of the dynamics of the situation in which change is intended and determination of responses to such questions as, "Why hasn't it changed?" "Would it change in the light of new evidence?" "What would it take to change it?"

Originality and Creativity

A good problem reflects some of the originality and creativity of its author. As Morris Klein says: "I think that in research you want to satisfy your own ego. You want to know you did it before the other fellow." (Rosner and Abt, 1970, p. 99). Yet the hard fact of the matter is that we all stand on each other's shoulders. The competitive spirit provides a useful drive, but it gets in the way when it blinds us to our dependence on others. Graduate students often refuse problems they did not invent in a kind of "second adolescence" in which they want to be independent and show they can do things themselves (Krathwohl, 1988). It helps if they recognize their adolescentlike behavior and gain perspective on the help being offered. Researchers can make problems their

own by adding just enough of their own thinking to another's problem to get an "investment" in it.

How much originality is enough? It is impossible to say; the negative extremes are easier to specify. For example, some researchers try so hard to be original that they make their problem overly complex and overlook ways of simplifying. Others seem too ready to accept other researchers' ideas without trying to break the problem apart for themselves. None of us starts from scratch; it is important to find that middle ground and be comfortable with it.

Feasibility

A good problem is feasible if (1) it lends itself to investigation with the instruments and techniques that are either available or can be invented, (2) it is within the capability of the investigator's available or acquirable experience and skills, and (3) it can be accomplished within whatever social, ethical, and resource limits must be observed. As a criterion, feasibility is obviously critical, yet novice investigators, in their zeal, often feel that the only way to have impact is to choose a topic well beyond their capacity in terms of size, complexity, or required skills. This also is part of the "second adolescence" phenomenon—"I can do it, don't tell me I must cut it down, don't demean me in that way!" Such advice to reduce the scope of a study is in no way intended to devalue the person. Indeed, the hope is to keep the person from a position of self-devaluation. But unless the advice is viewed in perspective, it can be perceived incorrectly.

Surely for the novice investigator, the difficulty of doing a project itself is sufficient. Adding the task of developing a new instrument and showing its validity or mastering a new statistical or analytic technique and convincing the audience of its superiority markedly increases the burden. Each of these activities is big enough to be a research project in itself and is better treated as such. Until you have had sufficient experience, combining two such large projects into one should be avoided.

Project difficulty also comes in the form of complexity. To keep the problem within their grasp, researchers frequently shy away from problems perceived as too difficult or complex. The 4-minute mile was a boundary once thought to be beyond the capacity of the human body to exceed. Yet once broken by one man who believed he could do it, it has been exceeded many times. An amazing capacity of the human mind is the extent to which it can be stretched by concentration. There is the mistaken impression that the "greats" can pick up a problem of considerable complexity and work with it at any time. Yet in talking with such people, we realize that this is a myth. At the time of their contribution, they made a significant investment of time and effort and stayed with the problem almost continuously. Raising questions about it later usually requires a period for refamiliarization—sometimes more effort than they are willing to exert, and so they'll say they have moved on to other problems. Thus problems perceived as beyond our grasp may not be, if we are willing to spend the time and effort needed to master them.

Motivation is clearly critical, and it is more likely to be greater if what

is required contributes to some later goal as well as the current one. Unfortunately, novices sometimes choose problems requiring skills that have little relevance to the area in which they hope to excel; for example, people-oriented individuals who try to master complicated numeric techniques, electronic equipment, or software programming with little relevance to their future occupation may find their motivation waning.

Social, Ethical, Institutional, and Resource Limitations. All studies must be done within limits, for example:

- The research time required cannot be tolerated by a busy clinic.
- Leaving the control group without treatment may not be permitted by anxious parents.
- Prying into people's value structures, political affiliations, or sex lives may not be warranted by the value of the information gained in relation to the possible unpleasantness or perceived harm to the subjects.
- The cost to investigate the required number of subjects to do a study well may be beyond the resources of an investigator.

Prime considerations are what an institution will allow, what a community deems appropriate, and what ethical constraints the profession places on research. Codes of ethics in many professions provide guidelines to protect subjects from harm. Every federally supported research project must be approved by a human subjects protection committee that determines if there is the possibility of discomfort or harm and if so, if it is justifiable. Many institutions require approval by this committee even for projects without federal funding. (See chapter 25.) Finally, there are limits on our own time, funds, and energy, which, though somewhat flexible, have boundaries that we must find and observe. So feasibility is important in terms of not being intimidated by apparent limitations yet also acknowledging institutional realities.

Here are some questions to ask yourself about your problem:

- Is it of sufficient interest that I will continue to be motivated through to its completion?
- Is it embedded in theory so that it is part of a network of propositions and explanations?
- Will it have some impact on the field?
- Has it an element of originality and creativity about it?
- Is it feasible in terms of my acquired or acquirable knowledge and skills, as well as being within my social, ethical, institutional, and resource limitations?

Researchable Questions

Be sure your question is researchable; not all questions are. The most common nonresearchable problems are those that show what *ought* to be done—children *ought* to be able to read the classics by the sixth grade, clients *should* be permitted to find their own solutions in therapy, there *should* be a free market in education with the government paying for whatever means of achieving an education pupils and parents choose. Note the italicized words: *ought* and *should*. Nobody can show that something ought to be done. "Ought" or "ought not" involves a value judgment! Research can be helpful by showing the consequences if something were or weren't done. Then somebody else can decide whether those consequences merit saying, "Yes, it *ought* to be done!"

Research can also determine how well such propositions are supported in a given community. It can determine the consequences of a particular course of action such as letting a client or a pupil have the right of choice. It can show what will happen if one tries to have sixth graders read the classics or evaluate a program intended to enable them to do so. All such questions will unearth evidence that may help a decision maker determine an "appropriate" choice, but research cannot directly affirm or deny a value proposition.

Research can help a decision maker to determine the implications of something that is desirable or desired, but it can never determine what *ought* to be—that is a value judgment.

SUMMARY

Choice of problem is the most important decision a researcher makes. Though some researchers are better at this than others, problem-finding skills can be learned. Discovery favors the prepared mind. Reading actively, reading widely, reading the seminal minds in the field, talking to specialists, challenging assumptions, looking for new ways to tease the problem apart and break the conventional way of looking at it—in short, filling the mind with the grist that allows the unconscious to sort matters out and organize thoughts—all are highly conducive to finding new research ideas. In addition, use techniques to enhance creativity such as those that organize material into suggestive patterns and reduce the censorship of ideas (brainstorming). Formulate the problem as a written statement and/or explain it to someone else. Learn your most productive working conditions. Don't close the problem definition too quickly, and when having trouble focusing, trace your problem back to more basic questions.

A log of your ideas, kept over the years, is often an excellent source of suggestions. Other researchers' data can sometimes be used; they don't

necessarily have to be new. New techniques, instruments, and models often suggest possibilities and extensions of past research. Other languages and cultures often hold possibilities that await discovery and development.

Good problems are of enough interest to motivate you to carry them to completion, are typically embedded in theory, are likely to have some impact on the field, have an element of originality or creativity about them, and are feasible. Though feasibility in terms of personal skills often stretches further than you might initially think, the problem must be researchable within the ethical and institutional limits and the resources available. Research cannot affirm or deny a value judgment. It can only show the consequences of that position and the extent of support for or against it, which may be helpful to decision makers in determining policy.

Obviously, one of the most important sources of material to develop a "prepared mind" is the work of others. For this we need library skills, to which we turn in the next chapter.

ADDITIONAL READING

Getzels (1982)

Merton (1959)

APPLICATION PROBLEMS

1. Johnson (1978), drawing on the theories of Carl Jung, theorized that a person's psychological style is defined by how he or she makes decisions. He developed a two-dimensional decision-making scheme based on the way information is gathered (systematically or spontaneously) and on the way data are analyzed; that is, internally (the individual needs to think about it first) or externally (the individual needs to discuss it with someone). From this Johnson surmised that individuals could be classified into four personality types: spontaneous external, spontaneous internal, systematic external, and systematic internal. If you were interested in researching psychological styles, how could you use Johnson's theory to develop a research problem?

2. Assume that you are a graduate student in educational psychology interested in "intelligence." You have read widely on the

topic and know that over the years, a number of theories have been advanced about its nature. These include such concepts as Thurstone's seven primary mental abilities; Spearman's *g*, or general intelligence factor; Guilford's structure of intellect model, and the idea of fluid versus crystallized intelligence championed by Cattell and Horn. To which of these should you look as the potential source of a research problem?

3. A master's student in nursing is interested in the care of brain-injured patients. She has focused on a disorder called unilateral neglect, which leaves a patient unaware of one side of his or her body. The student wishes to investigate the degree to which such patients could carry out ordinary activities of daily living and what the implications of this would be for their nursing care. How might she proceed?

4. A doctoral student in the field of educational technology is working with an adviser who has gained international recognition for his instructional model for selecting and sequencing content. The student has identified a set of motivational strategies to add to the model and is considering their verification in an instructional setting as a

dissertation topic. However, the student wonders if this topic is sufficiently original, since she did not develop the original model. What would be your advice to her?

Compare your answers with those on pages 703–704.

SUGGESTED EXERCISE

Beginning with this chapter, it is suggested that you choose a topic or a problem that interests you and use it throughout the rest of the book to gain familiarity with the content of each chapter. This chapter and the next, on the review of the literature, might be combined to help find a topic of particular interest that you may be willing to stay with.

You might try some of the techniques described in this chapter to stimulate your creativity. You might try actively reading some of the references found for your topic, using the material in the next chapter. See whether there is a member of the invisible college in your school or college or whether you can locate one at a nearby institution. Get together with a small group and brainstorm. Have one person in the group play the role of observer to help you reflect on the process and make sure you follow the rules. Have this person summarize progress every 10 minutes or so or when there is a good breaking point, and have her point out any breeches of the rules. Try a morphological analysis. Read extensively and fill your mind with material; then concentrate on something else for a while and see what develops. Even if all this is effective, also try working at your project by yourself, filling a sheet of paper with ideas or whatever comes to mind to keep your mind engaged on the

problem. Experimenting with these different approaches will help you learn what works best for you.

By applying the content of this and each succeeding chapter to your topic, you could easily end up with a proposal for a study that has been subjected to analysis from the standpoint of a number of skills of research and has been thought about in relation to various research methods. You'd know which skills were applicable and useful for your project. You'd have compared the potential of the various research methods for it. What could make for a stronger proposal for a research study?

Many students in the two-semester course in which this book was pioneered came out of it with proposals ready to go to their committee for final refinement. It gave them a big start on their doctoral program and helped save them from ABD status. By doing these application exercises and using the suggestions in *How to Prepare a Research Proposal* (Krathwohl, 1988), you can similarly benefit. See especially the two final chapters, which were designed specifically for doctoral students, the next-to-last giving different perspectives on the meaning of the dissertation and the last reviewing the steps involved in doing a dissertation, with suggestions and advice.