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Developing the Thesis or Dissertation Proposal

Some Common Problems

The general purposes and broad format of the proposal document have now been presented. There remain, however, a number of particular points that cause a disproportionate amount of difficulty in preparing proposals for student-conducted research. In some cases, the problems arise because of real difficulty in the subtle and complex nature of the writing task. In other cases, however, the problems are a consequence of confusion, conflicting opinions, and ambiguous standards among research workers themselves and, more particularly, among university research advisors.

As with many tasks involving an element of art, it is possible to establish a few general rules to which most practitioners subscribe. Success in terms of real mastery, however, lies not in knowing, or even following, the rules but in what the student learns to do within the rules.

Each student will discover his or her own set of special problems. Some will be solved only through practice and the accumulation of experience. While wrestling with the frustrations of preparing a proposal, you should try to remember that the real fascination of research lies in its problematic nature, in the search for serviceable hypotheses, in selecting sensitive means of analyzing data, and in the creative tasks of study design.

Some of the problems graduate students face cannot be solved simply by reading about them. What follows, however, is an effort to alert you to the

most common pitfalls, to provide some general suggestions for resolution of the problems, and to sound one encouraging note: consultation with colleagues and advisors, patience with the often slow process of "figuring out," and scrupulous care in writing will overcome or circumvent most of the problems encountered in preparing a research proposal. In the midst of difficulty, it is useful to remember that problems are better encountered when developing the proposal than when facing a deadline for a final copy of the report.

The problems have been grouped into two broad sections: "Before the Proposal: First Things First" and "The Sequence of Proposing: From Selecting a Topic to Forming a Committee." Each section contains a number of specific issues that may confront the student researcher and provides some rules of thumb for use in avoiding or resolving the attendant difficulties. You should skim through the two sections selectively, because not all the discussions will be relevant to your needs. Chapter 4 ("Content of the Proposal: Important Considerations"), Chapter 6 ("Style and Form in Writing the Proposal"), and Chapter 7 ("The Oral Presentation") deal with specific technical problems and should be consulted after completing a review of what follows here.

Before the Proposal: First Things First

Making Your Decision: Do You Really Want to Do It?

The following idealized sequence of events leads to a thesis or dissertation proposal.

1. In the process of completing undergraduate or master's level preparation, the student identifies an area of particular interest in which he or she proposes to concentrate advanced study.
2. The student selects a graduate institution that has a strong reputation for research and teaching in the area of interest.
3. The student identifies an advisor who has published extensively and regularly chairs graduate student research in the area of interest.
4. Based on further study and interaction with the advisor, the student selects and formulates a question or hypothesis as the basis for a thesis or dissertation.

Because we do not live in the best of all possible worlds, few students are able to pursue the steps of this happy and logical sequence. For a variety of reasons, most students have to take at least one of the steps in reverse. Some

even find themselves at the end of several semesters of study just beginning to identify a primary area of interest, in an institution that may be less than perfectly appropriate to their needs, and assigned to an advisor who has little or no experience in that particular domain. For this unfortunate state of affairs, we offer no easy solution. We do believe that one significant decision is, or should be, available to the student—the decision to do, or not to do, a research study. Faced with conditions such as those described above, if the option is available, the more rational and educationally profitable course may be to elect not to undertake a research study. You can determine whether this option is available before the school is selected, or at least before the program of study is selected.

There are sound reasons to believe that experience in the conduct of research contributes to graduate education. There also are good and substantial reasons to believe that other kinds of experiences are immeasurably more appropriate and profitable for some students. The question is, "Which experience is right for you?"

If you are, or think you might be, headed for a career in scholarship and higher education, then the decision is clear. The sooner you begin accumulating experience in research activities, the better. If you are genuinely curious about the workings of the research process, interested in combining inquiry with a career of professional service, or fascinated by the problems associated with a particular application of knowledge to practice, again the decision is clear. An experience in research presents at least a viable alternative in your educational plans.

Lacking one of these motives, the decision should swing the other way, toward an option more suited to your needs. Inadequately motivated research tends not to be completed or, worse, is finished in a pedestrian fashion far below the student's real capacity. Even a well-executed thesis or dissertation may exert a powerful negative influence on the graduate experience when it has not been accepted by the student as a reasonable and desirable task.

One problem touches everyone in graduate education, faculty and students alike—the hard constraints of time. Students want to finish their degree programs in a reasonable period of time. The disposition or circumstances of some, however, may define reasonable time as "the shortest possible time." Others find the thought of any extension beyond the standard number of semesters a serious threat to their sense of adequacy. For students such as these, a thesis or dissertation is a risky venture.

Relatively few research studies finish on schedule, and time requirements invariably are underestimated. Frequent setbacks are almost inevitable. This is one aspect of the research process that is learned during the research

experience: Haste in research is lethal to both quality of the product and worth of the experience. If you cannot spend the time, deciding to initiate a research project endangers the area of inquiry, your advisor, your institution, your education, your reputation, and any satisfaction you might take in completing the task. In short, if you can't afford the time, then don't do it at all.

Choosing Your Turf: Advisors and Areas

Once a firm decision has been made to write a thesis or dissertation, the choice of an advisor presents a less difficult problem. Here, area of interest dictates selection because it is essential to have an advisor who is knowledgeable. Further, it always is preferable to have one who is actively publishing in the domain of interest.

Competent advisement is so important that a degree of student flexibility may be required. It is far better for students to adjust their long-range goals than to attempt research on a topic with which their advisor is completely unfamiliar. It may be necessary for the thesis or dissertation to be part of the advisor's own research program. As long as the topic remains within the broad areas of student interest, however, it is possible to gain vital experience in formulating questions, designing studies, and applying the technology and methods of inquiry that are generic to the domain.

It is desirable for student and advisor to interact throughout the development of the proposal, beginning with the initial selection and formulation of the question. On occasion, however, the student may bring an early stage proposal to a prospective advisor as a test of his or her interest or to encourage acceptance of formal appointment as advisor. Experience suggests that this strategy is most likely to produce immediate results if the proposal is in the primary interest area of the advisor. If the proposal involves replication of some aspect of the advisor's previous research, the student may be amazed at the intensity of attention this attracts.

Finding Your Question: What Don't We Know That Matters?

Before launching into the process of identifying a suitable topic for inquiry, we suggest a short course of semantic and conceptual hygiene. The purpose of this small therapy is to establish a simple and reliable set of terms for thinking through what can sometimes be a difficult and lengthy problem—what do I study?

All research emerges from a perceived problem, some unsatisfactory situation in the world that we want to confront. Sometimes the difficulty rests

simply in the fact that we don't understand how things work and have the human itch to know. At other times, we are confronted by decisions or the need for action when the alternatives or consequences are unclear. Such perceived problems are experienced as a disequilibrium, a dissonance in our cognition. Notice, however, they do not exist out in the world, but in our minds.

That may sound at first like one of those "nice points" of which academics are sometimes fond, but for the purposes of a novice researcher, locating the problem in the right place and setting up your understanding of exactly what is unsatisfactory may represent much more than an arbitrary exercise. Thinking clearly about problems, questions, hypotheses, and research purposes can prevent mental logjams that sometimes block or delay clear identification of what is to be investigated.

The novice will encounter research reports, proposals, and even some well-regarded textbooks that freely interchange the words "problem" and "question" in ways that create all sorts of logical confusion (as in "The question in this study is to investigate the problem of . . ." or "The problem in this study is to investigate the question of . . ."). The problem is located alternately in the world or in the study, the distinction between problems and questions is unclear, and what is unsatisfactory in the situation is not set up as a clear target for inquiry.

We suggest that you be more careful as you think through the question of what to study. Define your terms from the start and stick with them, at least until they prove not to be helpful. The definitions we prefer are arbitrary, but it has been our experience that making such distinctions is a useful habit of mind. Accordingly, we suggest that you use the following lexicon as you think and begin to write about your problem.

Problem—the experience we have when an unsatisfactory situation is encountered. Once carefully defined, it is that situation, with all the attendant questions it may raise, that can become the target for a proposed study. Your proposal, then, will not lay out a plan to study the problem but will address one or several of the questions that explicate what you have found "problematic" about the situation. Note that in this context neither situation nor problem is limited to a pragmatic definition. The observation that two theories contradict each other can be experienced as a problem, and a research question may be posed to address the conflict.

Question—a statement of what you wish to know about some unsatisfactory situation, as in the following: What is the relation between . . . ? Which is the quickest way to . . . ? What would happen if . . . ? What is the location of . . . ? "What is the perspective of . . . ? As explained below, when cast in a precise, answerable form, one or several of these questions will become the mainspring for your study—the formal research question.

Purpose—the explicit intention of the investigator to accumulate data in such a way as to answer the research question posed as the focus for the study. The word "objective" is a reasonable synonym here. Although only people can have intentions, it is common to invest our research design with purpose (as in "The purpose of this study is to determine the mechanism through which . . . ").

Hypothesis—an affirmation about the nature of some situation in the world. A tentative proposition set up as a convenient target for an investigation, a statement to be confirmed or denied in terms of the evidence.

Given this lexicon, the search for a topic becomes the quest for a situation that is sufficiently unsatisfactory to be experienced as a *problem*. The proposal has as its *purpose* the setting up of a research *question* and the establishment of exactly how (and why) the investigator intends to find the answer, thereby eliminating or reducing the experience of finding something problematic about the world. Problems lead to questions, which in turn lead to the purpose of the study and, in some instances, to hypotheses. Table 3.1 shows the question, purpose, and hypotheses for a study. Note that the hypotheses meet the criteria established in Chapter 1 and are the most specific.

The research process, and thus the proposal, begins with a question. Committed to performing a study within a given area of inquiry and allied with an appropriate advisor, students must identify a question that matches their interests as well as the resources and constraints of their situation. Given a theoretically infinite set of possible problems that might be researched, it is no small wonder that many students at first are overwhelmed and frozen into indecision. The "I can't find a problem" syndrome is a common malady among graduate students, but fortunately one that can be cured by time and knowledge.

Research questions emerge from three broad sources: logic, practicality, and accident. In some cases, the investigator's curiosity is directed to a gap in the logical structure of what already is known in the area. In other cases, the investigator responds to the demand for information about the application of knowledge to some practical service. In yet other cases, serendipity operates and the investigator is stimulated by an unexpected observation, often in the context of another study. It is common for several of these factors to operate simultaneously to direct attention to a particular question. Personal circumstance and individual style also tend to dictate the most common source of questions for each researcher. Finally, all the sources depend on a more fundamental and prior factor—thorough knowledge of the area.

Table 3.1 Problem, Question, Purpose, and Hypotheses

Problem—Extensive teacher planning of lessons requires large investments of time and energy, and often must compete with other important responsibilities—both professional and personal.

Question—Is the amount or kind of lesson planning done by teachers positively related to student in-class learning behaviors such as time-on-task?

Purpose—The purpose of this study is to examine the relationships between several categories (types) of teacher lesson planning and student time-on-task in a high school automobile mechanics class.

Hypotheses (Note that directional hypotheses are used for Hypotheses 1-3 and that even Hypothesis 4, stated in the null form, could be based on data from a pilot study.)

1. The number of teacher lesson planning decisions that relate to design and use of active learning strategies will be positively related to student time-on-task when those lessons are implemented.
2. The number of class management planning decisions related to particular lesson components will be positively related to student time-on-task when those components are implemented.
3. Teacher lesson planning decisions that require students to wait for the availability of tools or work sites will be negatively related to student time-on-task when those lessons are implemented.
4. The total number of teacher planning decisions (irrespective of category) will not be related to student time-on-task when those lessons are implemented.

It is this latter factor that accounts for the "graduate student syndrome." Only as one grasps the general framework and the specific details of a particular area can unknowns be revealed, fortuitous observations raise questions, and possible applications of knowledge become apparent. Traditional library study is the first step toward the maturity that permits confident selection of a research question. Such study, however, is necessary but not sufficient. In any active area of inquiry, the current knowledge base is not in the library—it is in the invisible college of informal associations among research workers.

The working knowledge base of an area takes the form of unpublished papers, conference speeches, seminar transcripts, memoranda, dissertations in progress, grant applications, personal correspondence, telephone calls,

and electronic mail communications, as well as conversations in the corridors of conference centers, restaurants, hotel rooms, and bars. To obtain access to this ephemeral resource, the student must be where the action is.

The best introduction to the current status of a research area is close association with advisors who know the territory and are busy formulating and pursuing their own questions. Conversing with peers, listening to professorial discussions, assisting in research projects, attending lectures and conferences, exchanging papers, and corresponding with faculty or students at other institutions are all ways of capturing the elusive state of the art. In all of these, however, the benefits derived often depend on knowing enough about the area to join the dialogue by asking questions, offering a tangible point for discussion, or raising a point of criticism. In research, as elsewhere, the more you know, the more you can learn.

Although establishing a network of exchange may seem impossible to young students who view themselves as novices and outsiders, it is a happy fact that new recruits generally find a warm welcome within any well-defined area of intensive study. Everyone depends on informal relationships among research colleagues, and this rapport is one source of sustaining excitement and pleasure in the research enterprise. As soon as you can articulate well-formulated ideas about possible problems, your colleagues will be eager to provide comment, critical questions, suggestions, and encouragement.

In the final process of selecting the thesis or dissertation problem, there is one exercise that can serve to clarify the relative significance of competing questions. Most questions can be placed within a general model that displays a sequence of related questions—often in an order determined by logic or practical considerations. Smaller questions are seen to lead to larger and more general questions, methodological questions are seen necessarily to precede substantive questions, and theoretical questions may be found interspersed among purely empirical questions. The following is a much simplified but entirely realistic example of such a sequential model. It begins with an everyday observation and leads through a series of specific and interrelated problems to a high-order question of great significance.

OBSERVATION: Older adults generally take longer than young adults to complete cognitive tasks, but those who are physically active seem to be quicker mentally, especially in tasks that demand behavioral speed.

1. What types of cognitive function might be related to exercise?
2. How can these cognitive functions be measured?
3. What are the effects of habitual exercise on one of these types of cognitive function—reaction time?

4. Are active older adults faster on a simple reaction time task than sedentary older adults?
5. Are active older adults faster on a more complex reaction time task, such as choice reaction time, than older sedentary adults?

QUESTION: What effect does habitual exercise have on choice reaction time in older adults?

By making the twists and turns of speculation visible in the concrete process of sequential listing, previously unnoticed possibilities may be revealed or tentative impressions confirmed. In the simple example given above, the reader may immediately see other questions that could have been inserted or alternative chains of inquiry that branch off from the main track of logic. Other diagrammatic lists of questions about exercise and cognitive function might be constructed from different but related starting points. One might begin, for example, with the well-established observation that circulation is superior in older individuals who exercise regularly. This might lead through a series of proximal experiments toward the ultimate question, "What is the *mechanism* by which exercise maintains cognitive function?"

Building such diagrams will be useful for the student in several other ways. It is a way of controlling the instinct to grab the first researchable question that becomes apparent in an area. Often such questions are inferior to what might be selected after more careful contemplation of the alternatives. A logical sequence can be followed for most questions, beginning with "What has to be asked first?" Once these serial relationships become clear, it is easier to assign priorities.

In addition to identifying the correct ordering and relative importance of questions, such conceptual models also encourage students to think in terms of a series of studies that build cumulatively toward more significant conclusions than can be achieved in a one-shot thesis or dissertation. The faculty member who has clear dedication to a personal research program can be a key factor in attracting students into the long-term commitments that give life to an area of inquiry.

Researchable questions occur daily to the active researcher. The problem is not finding them but maintaining some sense of whether, and where, they might fit into an overall plan. Although this condition may seem remote to the novice struggling to define a first research topic, formulating even a modest research agenda can be a helpful process. The guidance of a sequential display of questions can allow the student to settle confidently on the target for a proposal.

The Sequence of Proposing: From Selecting a Topic to Forming a Committee

A Plan of Action: What Follows What?

Figure 3.1 presents a plan of action for developing a proposal. It can be useful for the novice if one central point is understood. A tidy, linear sequence of steps is not an accurate picture of what happens in the development of most research proposals. The peculiar qualities of human thought processes and the serendipity of retrieving knowledge serve to guarantee that development of a proposal will be anything but tidy. Dizzying leaps, periods of no progress, and agonizing backtracking are more typical than is a continuous, unidirectional flow of events. The diagram may be used to obtain an overview of the task, to establish a rough time schedule, or to check retrospectively for possible omissions, but it is not to be taken as a literal representation of what should or will happen.

To say that development of a proposal is not a perfectly predictable sequence is not to say, however, that it is entirely devoid of order. Starting at the beginning and following a logical sequence of thought and work has some clear advantages. When the proposal has been completed, a backward glance often indicates that a more orderly progression through the development steps would have saved time and effort.

For instance, although the mind may skip ahead and visualize a specific type of measure to be used, Step 11 ("Consider alternative methods of data collection") should not be undertaken until Step 6 ("Survey relevant literature") is completed. Many methods of measurement may be revealed and noted while perusing the literature. Sometimes suggestions for instrumentation materialize in unlikely places or in studies that have been initially categorized as unlikely to yield information concerning measurement. Additionally, reported evidence of the reliability and validity of the scores from alternative procedures will be needed before any final selection can be made. Thus, a large commitment of effort to consideration of alternative methods can be a waste of time if it precedes a careful survey of the literature.

For simplicity, many important elements have been omitted from Figure 3.1. No reference is made to such pivotal processes as developing a theoretical framework, categorizing literature, or stating hypotheses. Further, the detailed demands that are intrinsic to the writing process itself, such as establishing a systematic language, receive no mention. What are presented are the obvious steps of logic and procedure—the operations and questions that mark development toward a plan for action. Finally, the reader who begins to make actual use of the diagram will find that the sequence of steps at

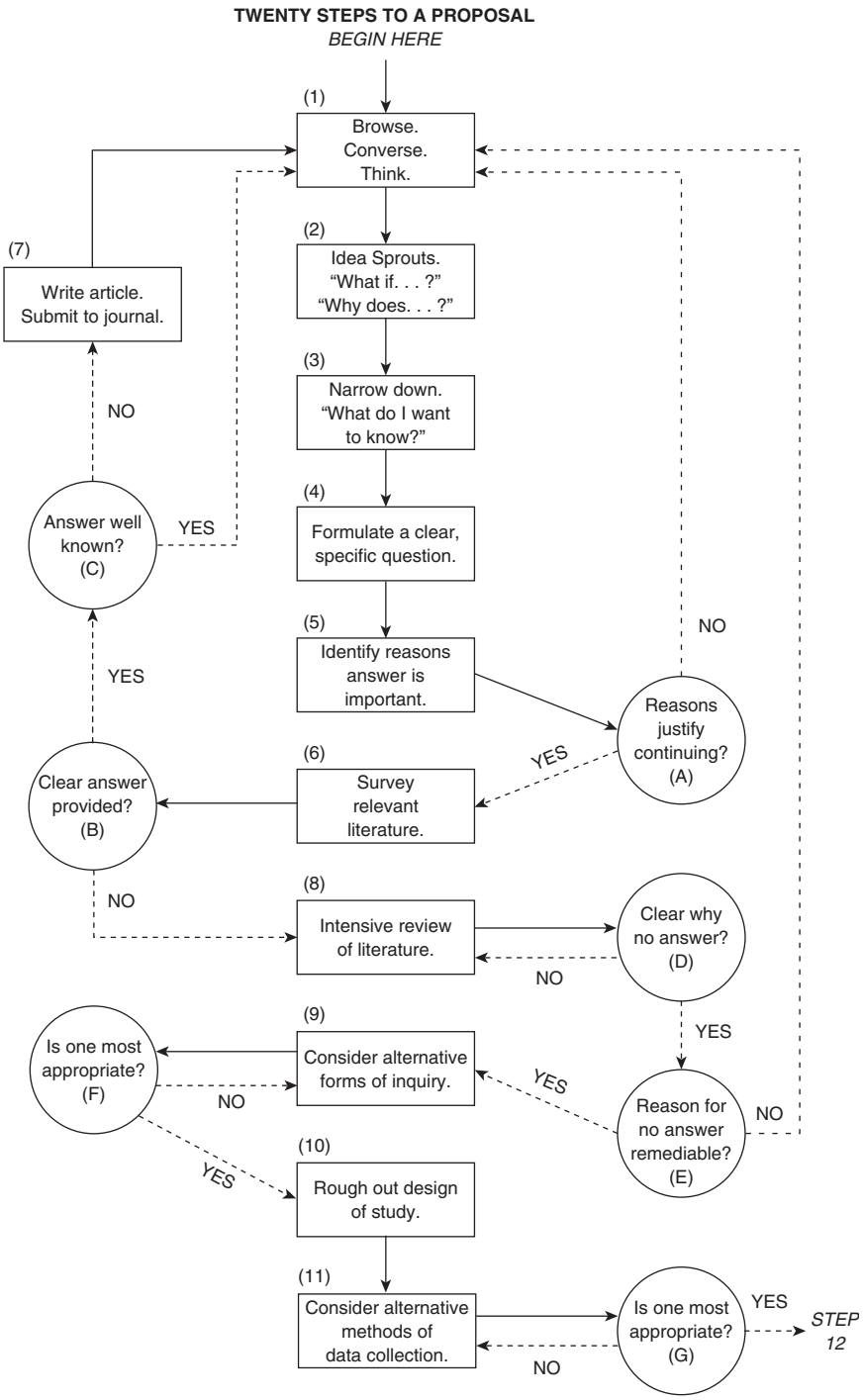
several junctures leads into what appear to be circular paths. For example, if at Question F a single form of inquiry does not present itself as most appropriate, the exit line designated "NO" leads back to the previous procedural step of considering alternative forms of inquiry. The intention in this arrangement is not to indicate a trap in which beginning researchers are doomed forever to chase their tails. In each case, the closed loop suggests only that when questions cannot be answered, additional input is required (more study, thought, or advice), or that the question itself is inappropriate to the case and must be altered.

For the most part, Figure 3.1 is self-explanatory. We have assumed that students will be working with, and obtaining advice from, their advisor as they navigate the various steps. In the pages that follow, however, we have selected a few of the steps and questions for comment, either because they represent critical junctures in the proposal process or because they have proven particularly troublesome for our own advisees. It will be helpful to locate in the diagram sequence each of the items selected for discussion so that the previous and succeeding steps and questions provide a frame for our comments.

Step 3: Narrow down. "What do I want to know?" Moving from general to specific is always more difficult for the beginner than is anticipated. It is here that the student first encounters two of the hard facts of scientific life: logistic practicality and the perverse inscrutability of seemingly simple events. Inevitably, the novice must learn to take one small step, one manageable question, at a time. In other words, the proposal must conform in scope to the realistic limitations of the research process itself. At their best, research tools can encompass only limited bits of reality; stretched too far, they produce illusion rather than understanding.

It may be important to think big at first, to puzzle without considering practicality, and to allow speculation to soar beyond the confines of the sure knowledge base. From such creative conceptual exercises, however, the researcher must return to the question, "Where, given my resources and the nature of the problem, can I begin?" Delimiting questions such as "In which people?" "Under what conditions?" "At what time?" "In what location?" "By observing which events?" and "By manipulating which variables?" serve the necessary pruning function.

Step 5: Identify reasons answer is important. This step places the proposed research in scientific-societal perspective. The study should contribute to the generation or validation of a theoretical structure or subcomponent or relate to one of the several processes by which knowledge is used to enhance



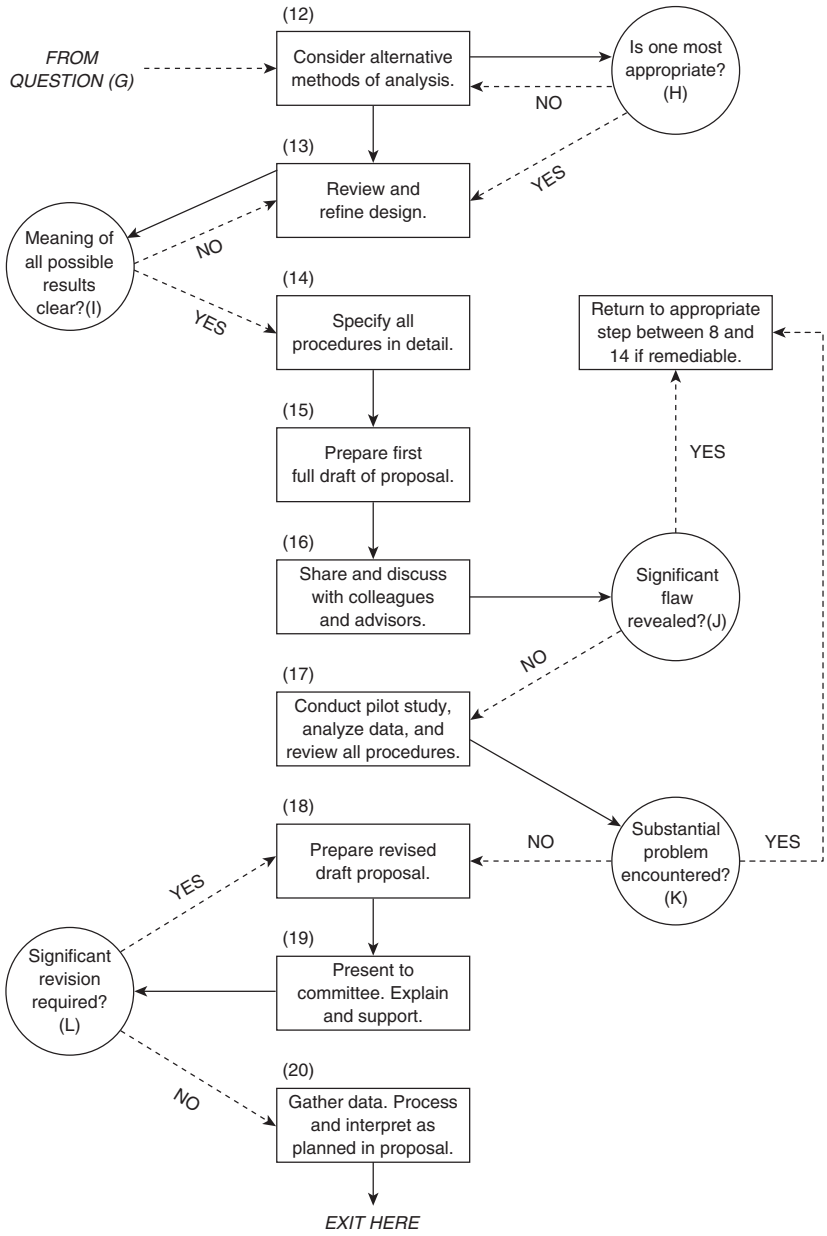


Figure 3.1 Twenty Steps to a Proposal

NOTE: Boxes represent major procedural steps, and unbroken lines trace the main sequence of those steps. Circles represent the major questions to be confronted, and broken lines lead to the procedural consequences of the alternative YES or NO answers.

professional practice. The trick here is to justify the question in terms appropriate to its nature. Inquiry that is directed toward filling a gap in the structure of knowledge need not be supported by appeals to practical application (even though later events may yield just such a return). Inquiry that arises directly from problems in the world of practice need not be supported by appeals to improve understanding of basic phenomena (even though later events may lead to this). Each kind of question has its own correct measure of importance. The task of distinguishing the trivial from the substantive is not always easy; do not make it even more difficult by attempting to apply the wrong standard.

Question A: Reasons justify continuing? In examining a list of reasons that support the importance of a question, the issue of worth may be viewed from several dimensions: worth to the individual contemplating the answer and worth to a profession, to the academic community, and ultimately to society. Question A, "Reasons justify continuing?" is the question that the researcher must answer in terms of personal interests and needs. The world is full of clearly formulated and specific questions that may not, once seen in their formal dress, seem worth the effort of answering. Because researchers are human, perfectly legitimate questions may seem dull, interesting veins of inquiry may peter out into triviality, and well-defined issues may fail to suit for no better reason than a clash with personal style. On the other hand, some questions are supported by the researcher's immediate need to enhance teaching in a vital subject area or to quench curiosity about a long-held hunch.

The basic rule is to be honest before proceeding. If you really don't care about answering the question, it may be better to start again while the investment still is relatively small.

Step 6: Survey relevant literature. A preliminary scanning of the most obvious, pertinent resources, particularly reviews of the literature, is a way of husbanding time. It is far better to abandon a line of thought after several weeks of selective skimming than to work one's way via slow, thorough digestion of each document to the same conclusion after several months of effort.

Conscientious students sometimes feel vaguely guilty about such quick surveys. Keeping in mind the real purpose, which is to identify questions that already have satisfactory answers, is one way of easing such discomfort.

Question E: Reason for no answer remediable? In some cases, the literature contains an empty area because the state of technology, the available

knowledge framework, ethical considerations in completing the study, or the logistic demands peculiar to the question have made it impossible or unreasonable to conduct appropriate forms of inquiry. So long as the gap in knowledge seems to exist because no one has yet defined the question or become interested in pursuing the answer, it is reasonable to proceed. There are other reasons for empty or ambiguous areas in the literature, however, and they signal caution before proceeding.

Question I: Meaning of all possible results clear? The tighter the logic, the more elegant the theoretical framework, the more closely the design is tailored to produce clarity along one dimension—in short, the better the quality of the proposal—the greater the risk that the proposer will be lured into an unfortunate presumption: that the result of the study is known before the data are in hand. That student researchers sometimes are confronted by the stunning news that their treatment produced a reverse effect is in itself neither surprising nor harmful. Being unable to make an intelligent interpretation of such a situation, however, is unfortunate and in most cases avoidable.

Unanticipated results raise a fundamental question that the investigator must confront. Does the finding truly reflect what is resident in the data, or is it only an artifact of the analysis? If there is any doubt about the appropriateness of the analysis, particularly if the procedures were not perfectly aligned with the research question, the latter possibility must be considered. If reexamination of the analysis provides no accounting for findings that are sharply incongruent with expectations, another explanation must be sought. All of this is made more difficult if the possibility of discrepant findings has never been contemplated. A strong proposal, constructed in an orderly, step-by-step sequence, will enhance the likelihood that you can manage the unexpected with at least a degree of dignity.

Through serious consideration of alternative outcomes at the time of constructing the proposal, it may be possible to include elements in the study that will eliminate ambiguity in some of the most likely results. One method of anticipating the unexpected is to follow through the consequences of rejecting or failing to reject each hypothesis of the study. If the hypothesis was rejected, what is the explanation? How is the explanation justified by the rationale for the study? What findings would support the explanation? Conversely, if the findings of the study fail to provide a basis for rejection, what explanations are to be proposed? At the least, some careful preliminary thought about alternative explanations for each possible result will serve as a shield against the panic that produces such awkward post hoc interpretations as "no significant differences were observed because the instruments employed were inadequate."

Step 16: Share and discuss with colleagues and advisors. There is a well-known syndrome displayed by some who attempt research, characterized by the inclination to prolong the period of writing the final report—indefinitely. Some people simply cannot face what they perceive to be the personal threat implied in opening their work to challenge in the public arena. These individuals are terribly handicapped and only rarely can become mature, productive scholars. An early sign of this is seen in students who cannot bring themselves to solicit advice and criticism for their proposals.

Sometimes students experience severe criticism because they present their ideas before they have been sufficiently developed into a conceptual framework that represents careful preparation. Many professors avoid speculative conversations about "half-baked" ideas that have just arrived in a blinding flash of revelation to the student. Few professors, however, refuse a request for advice concerning a proposal that has been drafted as the culmination of several weeks of hard thought, research, and development. Even at that, having one's best effort devastated by pointed criticism can be an agonizing experience. Nevertheless, the only alternative is to persist in error or ignorance, and that is untenable in research.

If you are fortunate enough to be in a department that contains a vigorous community of inquiring minds, with the constant give and take of intellectual disputation, the rough and tumble soon will be regarded as a functional part of producing good research. The novice will solicit, if not always enjoy, the best criticism that can be found.

The notion that it is vaguely immoral to seek assistance in preparing a proposal is at best a parody of real science and at worst, as in the form of an institutional rule, it is a serious perversion arising from ignorance. Research may have some game-like qualities, but a system of handicaps is not one of them. The object of every inquiry is to get the best possible answer under the circumstances, and that presumes obtaining the best advice available. It is hoped that the student will not be held to any lesser standard.

It should be obvious that students, after digesting and weighing all the criticism received, must still make their own choices. Not all advice is good, and not all criticism is valid. There is only one way to find out, however, and that is to share the proposal with colleagues whose judgments one can respect, if not always accept.

The process of proposal development is enhanced if you obtain advice at various steps and do not wait until the end to solicit feedback. We strongly recommend working with your advisor and committee in ways that help you move steadily forward. For example, at Step 4 consulting your advisor about possible research questions may help you refine them and may assist you in finding relevant literature. At Steps 9 through 12, short, focused meetings

where you are prepared to discuss specifics may be particularly beneficial. Advice and constructive criticism are best when received in small doses and integrated throughout the proposal process.

Step 19: Present to committee. Explain and support. Presentation of your proposal may take place before a thesis or dissertation committee on an occasion formally sanctioned by the graduate school, or at an informal gathering in the advisor's office. In either instance, the purpose served and the importance assumed will depend on both local traditions and the relationships that have evolved to that point among the chairperson, committee members, and student.

If, for example, the chairperson has closely monitored the developing proposal and is satisfied that it is ready for final review and approval, the nature of the meeting is shaped accordingly. In addition, if other committee members have consulted on the proposal at various stages of writing, the meeting may serve primarily as an occasion for final review and a demonstration of presentation skills, rather than evaluation, extensive feedback, and judgment. When these conditions do not apply, the meeting assumes far greater significance, in itself, and the length and nature of the presentation will be affected.

Whatever the circumstances, both a prudent respect for the important function the committee members must perform and a proper desire to demonstrate the extent to which the efforts of your advisors have been effective make careful preparation and a good presentation absolutely necessary. Much of our advice about that is contained in Chapter 7. For the present purpose, we want to underscore the following points.

1. The more you can work with committee members before an official meeting, the more that meeting can focus on improving (and appreciating) your proposal—rather than just on understanding it.

2. As committee members talk with you and with each other at the meeting, it is natural that new insights and concerns will surface. So long as those are accurately recorded, and so long as there is clear provision for how the committee will manage subsequent revisions in the proposal, that process is all to your advantage. The object is not simply to get the proposal (as it stands) accepted; it is to create the best possible plan for your dissertation or thesis.

3. Where you have had to make difficult choices, accept compromises in method for pragmatic reasons, or leave some final decision(s) for a later point in time, it is best to bring such matters directly to the attention of your committee. Don't wait to be questioned. Take the initiative and lay out the problematic aspects for your advisors as you go through the presentation. You need not

make the proposed study appear to be mired in difficulty. Propose solutions and give your rationale, but never ignore or gloss over what you know requires more attention—and the help of your committee.

4. If the proposal is approved with the understanding that certain revisions or additions will be made, the best procedure is to obtain signatures on documents while at the meeting. The signed forms can then be held by your chairperson until he or she has approved the final draft.

Step 20: Gather data. Process and interpret as planned in proposal. This is the payoff. A good proposal is more than a guide to action, it is a framework for intelligent interpretation of results and the heart of a sound final report. The proposal cannot guarantee meaningful results, but it will provide some assurance that, whatever the result, the student can wind up the project with reasonable dispatch and at least a minimum of intellectual grace. If that sounds too small a recompense for all the effort, consider the alternative of having to write a report about an inconsequential question, pursued through inadequate methods of inquiry, and resulting in a heap of unanalyzable data.

Originality and Replication: What Is a Contribution to Knowledge?

Some attention already has been given to considerations that precede the proposal, the critical and difficult steps of identifying and delimiting a research topic. One other preliminary problem, the question of originality, has important ramifications for the proposal.

Some advisors regard student-conducted research primarily as an arena for training, like woodchopping that is expected to produce muscles in the person who holds the axe, but not much real fuel for the fire. Whatever may be the logic of such an assumption, students generally do not take the same attitude. Their expectations are more likely to resemble the classic dictum for scholarly research, to make an original contribution to the body of knowledge.

An all-too-common problem in selecting topics for research proposals occurs when either the student or an advisor gives literal interpretation to the word "original," defining it as "initial, first, never having existed or occurred before." This is a serious misinterpretation of the word as it is used in science. In research, the word "original" clearly includes all studies deliberately employed to test the accuracy of results or the applicability of conclusions developed in previous studies. What is not included under that rubric are studies that proceed mindlessly to repeat an existing work either in ignorance of its existence or without appropriate attention to its defects or limitations.

One consequence of the confusion surrounding the phrase "original contribution" is that misguided students and advisors are led to ignore one of the most important areas of research activity and one of the most useful forms of training for the novice researcher—replication. That replication sometimes is regarded simply as rote imitation, lacking sufficient opportunity for students to apply and develop their own skills, is an indication of how badly some students misunderstand both the operation of a research enterprise and the concept of a body of knowledge.

The essential role of replication in research has been cogently argued (Gall, Gall, & Borg, 2007). What has not been made sufficiently clear, however, is that replication can involve challenging problems that demand creative resolution. Further, some advisors do not appreciate the degree to which writing proposals for replicative studies can constitute an ideal learning opportunity for research trainees.

In direct replication, students must not only correctly identify all the critical variables in the original study but also create equivalent conditions for the conduct of their own study. Anyone who thinks that the critical variables will immediately be apparent from a reading of the original report has not read very widely in the research literature. Similarly, an individual who thinks that truly equivalent conditions can be created simply by "doing it the same way" just has not tried to perform a replicative study. Thorough understanding of the problem and, frequently, a great deal of technical ingenuity are demanded in developing an adequate proposal for direct replication.

As an alternative to direct replication, the student may repeat an interesting study considered to have been defective in sample, method, analysis, or interpretation. Here the student introduces deliberate changes to improve the power of a previous investigation. It would be difficult to imagine a more challenging or useful activity for anyone interested in both learning about research and contributing to the accumulation of reliable knowledge.

In writing a proposal for either kind of replicative study, direct or revised, the student should introduce the original with appropriate citation, make the comments that are needed, and proceed without equivocation or apology to the proposed study. Replicative research is not, as unfortunate tradition has it in some departments, slightly improper or something less than genuine research.

Given the limitations of research reports, it often is useful to discuss the source study for the replication with the original author. Most research workers are happy to provide greater detail and in some instances raw data for inspection or reanalysis. In a healthy science, replication is the most sincere form of flattery. A proposal appendix containing correspondence with the author of the original report, or data not provided in that report, often can serve to interest and reassure a hesitant advisor.

Getting Started: Producing the First Draft

The student who has never written a research proposal commonly sits in front of a desk and stares at a blank piece of paper or an empty video monitor for hours. The mind is brimming with knowledge gleaned from the literature, but how does one actually get started? The concept of "a research proposal" conjures up ideas of accuracy, precision, meticulous form, and use of a language system that is new and unpracticed by the neophyte researcher. The demands can suddenly seem overwhelming. The student should realize that this feeling of panic is experienced by nearly everyone, not only those who are new to the writing endeavor but those who are skilled as well. Fanger (1985) expressed it beautifully: "I have come to regard panic as the inevitable concomitant of any kind of serious academic writing" (p. 28). For anyone temporarily incapacitated by a blank page or empty monitor, the following suggestions may be helpful.

Make an outline that is compatible with the format selected to present the communication tasks listed in Chapter 1. An initial approval of the outline by the advisor may save revision time later. Gather the resource materials, notes, and references, and organize them into groups that correspond to the outline topics. For instance, notes supporting the rationale for the study would be in one group, and notes supporting the reliability of an instrument to be used would be in another group.

Once the outline is made and the materials gathered, tackle one of the topics in the outline (not necessarily the first) and start writing. If the section to be written is labeled "The Purpose," try imagining that someone has asked, "What is the purpose of this study?" Your task is to answer that question. Start writing. Do not worry about grammar, syntax, or writing within the language system. Just write. In this way you can avoid one of the greatest inhibitions to creativity—self-criticism so severe that each idea is rejected before it becomes reality. Remember, it is easier to correct than to create. If all the essential parts of a topic are displayed in some fashion, they can later be rearranged, edited, and couched within the language system. With experience, the novice will begin thinking in the language system and forms of the proposal. Until that time, the essential problem is to begin. Awkward or elegant, laborious or swift, there is no substitute for writing the first draft.

One way to approach writing is to use the outline feature on your word processor, which allows you to develop your outline and then go back and progressively fill in the detail under each heading. Learn to use this feature. The effort needed to learn its use will be repaid many times over. Word processing programs provide the opportunity for writers to edit, rearrange text easily, and store manuscript copy for future revisions. There is a significant

psychological advantage in the ease with which revised drafts can be produced. This encourages the author to make revisions that might otherwise be set aside under the press of limited time, and has greatly enhanced the ability of proposal writers to revise and polish their work.

Selecting Your Thesis or Dissertation Committee

Master's thesis committees vary in number from one professor to a committee of five or six faculty members. A doctoral dissertation committee typically consists of four to six members. In some instances, all committee members are from within the department of the student's major. In other instances, the committee is multidisciplinary, with faculty representing other departments on campus.

At most universities, students have some opportunity to request specific faculty members for their committee. If the student does have some freedom to exercise choice, committee membership should be designed to maximize the support and assistance available. A student interested in the study of behavioral treatment of drug abuse in young upwardly mobile women could tap the value of different faculty perspectives and skills by blending members from several departments. For this purpose, individuals with multiple interests are particularly useful. For example, a faculty member in the psychology department might be selected for both statistical competence and interest in behavior modification, someone in the school of social work might bring epidemiological expertise regarding drug usage, and a faculty member in the school of public health might be a part of the committee because of expertise in both experimental design and therapeutic compliance techniques.

Because students know from the beginning of the graduate program that faculty eventually will have to be selected for such a committee, it behooves them to be thinking about these matters during the selection of elective courses throughout the program. If a choice has to be made between two professors for an elective course, and one of them is more interested in the student's probable area of research, that may carry the day in determining which course to take. Although it is not essential that students have taken their committee members' classes, it is easier to ask a known faculty member to serve on your committee. That person is likely to take a greater interest in your work, and you have a good idea of his or her standards and methods of scholarship.