Process & Parameters
The Holy Grail: In Pursuit of the Dissertation Proposal
Michael Watts
Institute of International Studies
University of California, Berkeley

"[T]here is too little emphasis ... on what it means to do independent research."
- William Bowen and Neil Rudenstein
In Pursuit of the Ph.D. 1992

Introduction

One of the great curiosities of academia is that the art of writing a research proposal -- arguably one of the most difficult and demanding tasks confronting any research student -- is so weakly institutionalized within graduate programs. The same, incidentally, might be said of fieldwork, whether the site is a village in northern Uganda or an archive in Pittsburgh. My experience is that fieldwork has all of the aura (and anxiety) of any rite of passage. But with a difference. It is a Darwinian learning-by-doing ordeal for which there is presumed to be no body of preparatory knowledge that can be passed on in advance; those that succeed return, and those that don't are never seen again. It is perhaps for such reasons that Bowen and Rudenstein in their important book In Pursuit of the Ph.D. see the period between the end of coursework and the engagement of a dissertation topic as one of the most fraught and difficult in graduate formation. The selection of a topic they say is 'a formidable task', and students must be -- but in practice rarely are in the social sciences and the humanities -- encouraged to engage with their dissertation project in their first and second years. All of this is to say that the transition -- another rite of passage -- from course work to dissertation project is often paralyzing ("How exactly am I going to operationalize my crypto-Foucauldian study of the micro-physics of political power in San Francsico's credit unions?") and typically a source of bewilderment, anxiety and yes, even depression. It is always worth recalling the old adage that in its most demanding forms, writing and doing research, requires a state of mind and a way of being that most people in the world spend their lives trying to avoid: withdrawal, obsession, panic. This is the stuff of research and yet is it surprising how many classic monographs cover their tracks, obfuscate the mistakes, errors and panic, and forget the lived realities of working in the 'field', however defined. To be blunt: fieldwork is important, but it ain't necessarily pretty.

It is interesting to reflect on why the research proposal, and research design, has become a sort of public secret on campuses and indeed why it has become less an object of scrutiny in the last couple of decades. Perhaps the post-structural skepticism to toward method and 'truth', and the attraction of the conditions under which knowledge is produced has contributed to a sort of flight from research design. While an important consideration, I want to use this opportunity to introduce a number of issues pertaining to research design and proposal writing and to lay out in

broad terms a number of concerns and knotty problems that enter into the long and complicated process of framing, designing and conducting a researchable project...

Nuts & Bolts

This section of the site is designed as a practical guide to the proposal's various parts, including **theory**, **research question**, **research design**, **background & history**, **budgeting**, and **concepts & terminology**. You should first look at the suggested timeline indicating many of the activities that you will need to undertake in preparing to submit your proposal on time. While you may compress or extend this timeline, remember, you cannot begin too early.

01 THEORY

The proposal's theoretical section occupies a critical, but subsidiary position in the proposal's text. You must at once demonstrate disciplinary mastery, highlight critical theoretical debates, point to shortcomings in existing research and approaches, and indicate how your work will help fill the void. All without miring your proposal in a swamp of disorienting sources, sub-themes, and subtleties. While the specifics of a proposal's theoretical section must, of course, be determined by a fellowship's requirements, the following points may prove helpful in maintaining your focus and clarity.

Establish the context. The primary purpose of your theoretical review is to demonstrate your familiarity with present intellectual currents and concerns. Your review should not, however, be a general survey of the field. Your discussion must quickly situate you and your work within the context of the field's theoretical themes. If you intend to conduct cross-disciplinary research, you should highlight points of intersection between various theoretical fields and justify why you are drawing on what some skeptics may consider obscure sources. At all times, keep in mind that your theoretical review must justify your research question and help determine your research design.

Point out debates and disjuncture; expose the cracks and highlight the payoffs. Your second primary task in reviewing existing theory and literature is to justify the need for and interest in your proposed research. Justification for research can come from a variety of sources. At one level, new events or developments may justify an empirical review of a long-accepted theory's empirical foundations (e.g., why no democratization in places with a strong middle class?). You may also highlight rival claims within the literature of your field that can only be resolved through empirical work (e.g., some claim peasants are motivated by economic forces, others say they are not). Regardless, attempt to highlight seeming paradoxes or internal contradictions in the existing literature. Then demonstrate how your work will contribute to their resolution.

Privilege elegance over expansiveness. Graduate students tend to use the theory section as a thorough review of past approaches while pointing to minor subtleties and differences. Unless yours is a purely theoretical project, the theory section is intended to provide only the foundation and justification for your research, not a treatise on the theory itself. As with the rest of the proposal, aim for a clear and democratic tone that is accessible without being shallow.

Show your knowledge and expertise without being pedantic or dismissive. As with the rest of the proposal, you must demonstrate your expertise and qualifications without being dismissive of others' work and ideas. (See also the section on style for more on tone). Many students stereotype or 'straw man' past approaches in an effort to highlight weaknesses and shortcomings. Committee members may interpret the too easy dismissal of previous works as lacking respect or appreciation for the field. You also run the risk of offending committee members who are attached to a particular approach or author. For all you know, one of the people you criticize could be reviewing your proposal. As Przeworski writes, "Good proposals demonstrate awareness of alternative viewpoints and argue the author's position in such a way as to address the field broadly, rather than developing a single sectarian tendency indifferent to alternatives." Hyperbole and hubris will, needless to say, go unappreciated. Without low-balling your qualifications, avoid presenting yourself as your discipline's savior or prophet.

02 RESEARCH QUESTION

Your research question is the most critical part of your research proposal -- it defines the proposal, it guides your arguments and inquiry, and it provokes the interests of the reviewer. If your question does not work well, no matter how strong the rest of the proposal, the proposal is unlikely to be successful. Because of this, it is common to spend more time on the researching, conceptualizing and forming of each individual word of the research question than on any other part of the proposal.

To write a strong research question you will need time. Step away from your computer; consider what drew you to your topic. What about it animates and matters to you? Listen to yourself and start formulating your question by following your own interests. Remember, you will spend a lot of time researching and writing about the proposed project: if it does not interest you in the beginning, it will certainly become very difficult to write about in the end.

Next, extensively research your topic. What have people said about it? How have they framed their research? What gaps, contradictions, or concerns arise for you as you read, talk to people, and visit places?

After you have done this you can go back to your computer or note pad and start crafting the question itself. When you do, consider that a strong research question should be evocative, relevant, clear, and researchable.

The research question should be evocative.
Evocative questions are ones that catch the interest of the reviewer and draw her/him into the proposal. Equally important, they easily adhere in the reviewers' memory after reading the proposal. Questions tend to be evocative because of the ways they engage with challenging topics: they pose innovative approaches to the exploration of problems, and because of this the answers found are far from obvious. There is no single way to form a conceptually innovative question. However, some of the following qualities are common to successful proposals.

Make it timely. Evocative questions are often distilled from very contemporary social or theoretical concerns. For example, questions regarding the energy crisis, international tribunals, nationalism, or the rise of anti-globalization protests are likely to peak the interests of others because they are questions whose relevance will be clearly discernible for reviewers.

Frame it as a paradox. Frame your question around a provocative paradox. For example, why have indigenous organizations in Bolivia markedly declined while the number and quantity of funding sources has increased? Or why have violent conflicts over forest resources increased in the last ten years while the very people involved in these conflicts have become less and less dependent on forest resources for their livelihoods? There are many potential answers to these questions, and your research may ultimately challenge your own expected explanation -- but this in itself is a relevant discovery. These types of paradoxes pull the reader into the proposal and set up a situation whereby the research will fill in a provocative piece of the puzzle and make clear a much-needed broader understanding.

Take a distinctive approach. Finally, a question that approaches an old problem in a refreshingly new way, or proposes a surprising angle of analysis on a difficult dilemma, is likely to prove evocative for reviewers. This could involve a new methodology, a new conceptual approach, or the linking of two previously disparate fields of knowledge. These innovative approaches both develop confidence in the intellect of the researcher and hold promise for new understandings and insights to old and difficult questions.

The research question should be relevant.
Questions that clearly demonstrate their relevance to society, a social group, or scholarly literature and debates are likely to be given more weight by reviewers. Of course the relevance of a research question, not to mention the question of who finds it relevant, will vary widely according to the funding source. As a general rule, research is more likely to be funded if it is seen as part of a larger intellectual project or line of inquiry, not just a way for the researcher to get a degree. Below are two common ways to demonstrate this in your proposal.

Fill in the missing piece. If your proposal can lay out a given field or dilemma and then point to a specific portion that is missing in that field or dilemma -- a gap which will be filled by the answer to your research question -- your research is likely to garner a great deal of support. Reviewers will note its importance and recognize its relevance to a larger community of researchers.

Make connections. Even if you are working on a narrow topic or in a specific place, ask questions that help relate the research to broader trends, patterns, and contexts. Doing this will help show how funding a seemingly distinct research project helps fuel larger debates. For example, show how someone working in a small town in Outer Mongolia will help understand the broader process of post-Soviet economic transformations.

The research question should be clear.
Clear questions tend to be short, conceptually straightforward, and jargon-free. This does not mean they have to be overly simplistic; but save your theoretical gymnastics and abstract disciplinary language for the analysis. Work to keep your questions as lucid and simple as possible. This may be easier in some cases than in others, but some of the strongest and most

theoretically sophisticated proposals we reviewed were framed by some of the simplest, most straightforward research questions. In contrast, the most complicated questions tended to appear in proposals where the researcher seemed more interested in demonstrating his/her theoretical knowledge than in engaging the research itself. Below are simple ways to keep your question clear.

Ground the questions. Keep your questions close to the topic or place you are researching. Questions that are too abstract or obtuse make it difficult for the reader to determine your question's relevance and intent. You must still link your question to a larger context, but ground that connection in temporal and spatial specifics.

Limit variables. If a question is burdened with too many variables or too many clauses it becomes both difficult to read and difficult to research. Here are two contrasting examples from the SSRC web site: a question like "Was the decline of population growth in Brazil the result of government policies?" is much easier to understand than "Was the decline in population growth in Brazil related more to sex education, the distribution of birth control, or resource depletion?" You may talk about all these factors in your proposal, but the first question allows the reader to focus on the central aspect of your research rather than the variables surrounding it.

The research question should be researchable.
Research questions need to be clearly "doable." One of the most common rationales for rejecting proposals is that the question is simply too expansive (or expensive) to be carried out by the applicant. There are many questions that you will need to ask yourself to avoid this pitfall. Above all else, *consider your limitations.* Many very practical questions need to be considered when choosing your research question. First among them is: How long will the research take to carry out? Next, do you have the appropriate background to carry out the research? Are there ethical constraints? Is the project likely to be approved by your advisor and your university's committee for the protection of human subjects? Can you obtain the cooperation from all the necessary individuals, communities and institutions you need to answer the question you have asked? Are the costs of conducting the research more than you will be likely to raise? If I can't complete this project well, can I break it down and address the most important component? Remember that writing a research question is an iterative process and such concerns need to be carefully considered in your research design and budget.

03 RESEARCH DESIGN

Creating an effective research design is likely to be one of the most difficult and eminently useful tasks in drafting a proposal. An effective research design links abstract and stylized concepts and questions with the empirical world's complexities and challenges. A research design must at once be specific and highly flexible. It must be expansive enough to adapt these very complexities while still pointing you towards relevant data. The methods you use should be extensions of your substantive question and epistemological orientation. Contrary to some disciplinarians' claims, there is no single research model that one can or should follow. Numerous alternatives must always be considered and choices made. What follows is a set of general

principles and questions to consider in making those choices. Whether or not these questions help ensure funding, they will help guide you as you start to navigate "the field."

Identify the kind of research you intend to do. Depending on discipline, project, and personal inclination, social science research projects may contain a wide range of empirical and theoretical objectives. While most researchers hope to explore and document some form of "reality" -- something important in the real world -- the reasons for doing so vary tremendously. Identifying your normative motivations and your theoretical foundations will considerably influence how you design your research: where you go, for how long, with whom you talk, and the kind of questions you ask. Deciding if you intend to test or elaborate existing theory or are trying to build a new, grand theory, or are using existing theory in a new way, has implications in the kind of information you need to collect.

Be realistic. The world is infinitely more complicated than anything you can possibly represent in a comprehensible text, be it your proposal or dissertation. Given the technical, financial, and chronological restraints you will face in conducting your research (see fieldwork below), you are going to have to make choices. Conducting a household survey may mean that you cannot also do participant observation, an in-depth ethnography, and extensive archival research. Such questions become even more complicated when conducting research at multiple sites or with ethnically or linguistically diverse populations. Selecting and justifying a limited number of approaches will demonstrate that you have thought through your agenda and the kind of information you need to make your point. Demonstrating that you have the technical skills to execute these approaches will only make your statement stronger.

Be precise. Social scientific discourse, both methodological and substantive, is rife with neologisms and jargon. As with any concept you hope to use, you must be prepared to tease out and concretize the methods you select. If you intend to conduct open-ended interviews, you must ask a whole series of secondary questions:

> What do I want to get out of these interviews?
> With whom am I going to conduct these interviews?
> How do I know they will talk to me?
> How many interviews must I do?

The same goes for "process tracing" (e.g., what process, where do I see this process, etc.), "archival research" (what archives, what sources, what about accessibility? reliability?), or with any other approach. Not all of your answers to these questions need to go in the proposal, but demonstrating that you have considered them will only help.

Be flexible. While realism and precision require excluding some possible approaches, a research design that is too strictly curtailed raises its own set of hazards. In the words of King, Keohene and Verba, "the first-rate social scientist does not regard a research deign as a blueprint for a mechanical process of data-gathering and evaluation. To the contrary, the scholar must have the flexibility of mind to overturn old ways of looking at the world, to ask new questions, to revise research designs appropriately, and then to collect more data of a different type than originally intended" (1994:12). It may be useful to consider what you will do if you cannot access a

certain data set, speak to a particular official, or live among a certain group of villagers. Developing a research design that allows you to incorporate these contingencies will help persuade grant-makers that you are ready for what lies ahead.

As much as possible, test your methods in advance. Trying out drafts of your questionnaire, interviewing technique or skills at facilitating focus group discussions can prove invaluable. Ideally, this would be done "in the field" on a pre-dissertation trip, but most of us are not lucky enough to get such a chance. You may be surprised, however, at just how quickly you can eliminate or refine particular questions or approaches by trying them with strangers at home. Moreover, you can help see what methods you realistically think you will be able to use. Doing this ahead of time will not only save invaluable time when you get to the field, but can help you decide what methods you are most comfortable (and most competent) using. Not everyone, for example, is prepared to go undercover in a meatpacking factory as a participant-observer. Being able to specify what you are going to do, and why you are the person to do, are central to convincing potential funders that you are a worthy grantee.

Consider revising your research question; consider revising your methods. For some, research design and methodology are seen as ways of operationalizing a research question. Others, often those with more technical leanings, choose a research question that highlights their methodological prowess. There are merits to both approaches. A research question must be answerable by the methodological tools available to you, the researcher. Conversely, the methods, however sophisticated, must help you to answer a question of significance to both you and your discipline. As you consider what you can do practically, it may be worth thinking about reformulating or "spinning" your question in a way that will allow you to provide an effective answer. Similarly, as your thinking evolves and your research question changes, you must be prepared to reformulate your research design.

Cited: King, G., Keohane, R. O., Verba, S. (1994) Designing Social Inquiry. Princeton: Princeton University Press.

04 BACKGROUND & HISTORY

Whereas the theory section provides the intellectual context for your research, the background and history highlights its empirical foundations. In many ways, this section of your proposal is deceptively straightforward. On one level, the purpose of a background/history section is to give the reader the relevant facts about your topic and/or research site so that they understand the material or case that you are writing about and how it links to your theoretical question. This section must not, however, simply provide the general context, but must direct the readers' attention to the empirical details through which your research topic and questions are lived and made relevant. As such, they must not just fill in details of the place or topic you are researching, but implicitly illustrate the need for and importance of your research. There are three simple, overlapping concepts to keep in mind when writing your background or history section that will help you do to this. Engage your readers with broader themes and topics that illustrate your concepts, questions, and theory and demonstrate your knowledge and passion.

The history/background should engage your readers with broad themes and topics. This involves connecting details to concepts. The history should be easy to read and compelling both for its relevance and for its fresh approach. Few want to read the details of textile handicrafts in southern Mississippi simply to learn about weaving. If, on the other hand, you show how this craft is linked to a history of racial tensions, changing economic conditions, or gender relations, the details of handicraft cooperatives and techniques can be engrossing and make the reader want to know more.

The background/history should illustrate your concepts, questions, and theory. To do this, try to ensure a tight fit between this and the proposal's other sections. Your history should be the empirical embodiment of your theoretical section. This requires you to make explicit links between the story you tell and the questions and theoretical approach you are using. If, for example, you are writing on indigenous land rights struggles in Bolivia, you should not just include a history of events, but a history that is tightly linked to your theoretical concerns and the research question you are asking. Trace the major actors, sources of change, and point to potential outcomes. If you do this, your history section offers a chance to expound on (for the benefit of others' understanding) the broader topic through the details of your story.

The history/background should demonstrate your experience, knowledge, and passion. What you write about and how you write can reveal a great deal about your knowledge and interest in your subject. This is true in all parts of your proposal, but perhaps most so in this section. Use the background section as an occasion to show the depths of your knowledge of the topic by demonstrating your fluency in accepted understandings and literature as well as your fresh insights and approaches. You may also use this review to implicitly reveal what has drawn you to the topic in the first place. Doing this well will help convince the reader that your interest in the topic is justified and that you are likely to sustain that interest over the time required to complete the project.

As with the theoretical review, the historical and background section must be precise and measured. Too passionate, too political, or too lengthy a historical review may cause some readers to loose focus or question your capacity to be detached and analytical. You must also be careful in choosing your citations as proposal readers from your field or region are likely to look carefully at your bibliography. If you are writing on New Mexico forest politics, for example, and the classic authors and works are not cited, it will likely appear to your reviewers that you have not done your homework. Similarly, you must show that you have read authors from across the theoretical or ideological spectrum. While simply putting the "right" people in you bibliography should not be the focus of your work, it is important to demonstrate that you have done your research and that you know your field.

05 BUDGETING

Budgeting is a natural and critical extension of your research design. A realistic and detailed budget is also often a prerequisite for convincing potential funders that you know what you need to accomplish your research and how you are going to conduct it. Not all funders require a budget, and those that do often require specific formats or include particular conditions. These

need to be considered seriously or your proposal may be prematurely dismissed. Even so, some general considerations may prove useful.

Demonstrate that your budget is realistic by specifying costs for each line entry. Doing this well may include extensive and difficult research into the real costs in your country of study. Consulates or tourism offices may help, but you will probably be better off speaking to recently returned researchers who are more likely to have dealt with the problems you are going to address. Be wary of budgeting from guidebooks or past experience as prices may have skyrocketed due to inflation or monetary devaluation. Prepare for continued devaluation or possible price rises. It will also help your readers if you include subtotals of various budget categories (e.g., transportation, living expenses, supplies, and research assistants). See samples below.

Consider every possible expense. You are not just funding your research, but your life for the time you are in the field. Any expenses you incur in the field will have to be paid by someone and f you don't ask for adequate support, your research will be compromised and you may come home even poorer than when you left. When sitting down to write a budget, think about all of your daily expenses at home, from the big-ticket items like tuition and books to the more trivial expenses like taking the bus to class. Something costing as little as three dollars a day will add up to over a thousand dollars in a year. Also consider those things that you may get for free at home but will have to pay for overseas. These costs may range from things as major as health care to expenses as seemingly minor as charges for photocopies, computer use, or printing documents. When asking for money, however, be careful. Make sure that the funds you are requesting are only for expenses that will be incurred during the research period and that the items you intend to buy are allowed (e.g., many grantors will not fund computer or automobile purchases).

Compare your budget to available funds. If your actual budget is likely to be higher than that which a particular funder is likely to support, indicate how you will make up the difference. To do this, you may want to mention other fellowships for which you are applying, university support to which you are entitled, available private funds, or payments you expect to receive for consulting. If you already have other funding secured, be upfront about it and detail which funder will cover what costs. It is far better to show that you have other funding that to submit a budget request that is far below what will be realistically needed to complete your research. Similarly, you may raise suspicions if your total expenses are perfectly matched to the maximum grant on offer.

06 CONCEPTS & TERMINOLOGY

Every discipline has its own idiom replete with vocabulary, grammatical structures, and other linguistic conventions. This inevitably leads to confusion for those conducting interdisciplinary research or speaking across disciplinary boundaries. A political scientist and an anthropologist may, for example, mean very different things when they say "participation" or "order." Moreover, these fields may attach very different normative valences to particular terms. You must prevent such differences from prematurely sinking your proposal. Understanding how your language is likely to be interpreted by reviewers and being very clear about the way you use terms and

concepts will help your proposal be understood and respected across disciplines. You may also want to consider the following more specific points:

Whenever possible, avoid neologisms. The invention of new words and concepts is a necessary part of keeping social science language dynamic and current. The Academy, moreover, provides strong institutional incentives for developing new and catchy phrases. While there is no rule for when such genesis is justified, it must always be done with a great deal of care. Unless you are quite certain that what you are describing -- or what you think you are describing -- is a genuinely new phenomenon, creating new words may come across simply as "old wine in new bottles." Moreover, defining new terms may draw attention (and space) away from other concerns.

Be conceptually consistent. Social science terminology is often ill defined and sloppily used. In a proposal, such inconsistency may be grounds for dismissal. If you are using a term or idea that is open to multiple interpretations, be sure that you define the term and stick to that definition. This may sound obvious, but it is not uncommon for serious slippage to occur. If you are writing about "civil society" but mean "nongovernmental organizations" (NGOs), why not just use that term. If in some places you mean NGOs but also mean citizen choral groups, bowling clubs, etc., you will need to be clear about that from the get-go. The same can be said of a myriad of other terms (e.g., "sustainable development," "peasant," and "democracy.") Being inconsistent may not only baffle your readers, but may leave them with the impression that you don't really not what you are talking about. Perhaps most importantly, conceptual clarity and consistency prove invaluable in crafting your research design.

Carefully consider and justify typologies and categories. The creation and use of typologies -- coordinated sets of terms that provide labels for different components of the analytic domain of interest to the scholar -- is often at the heart of social scientific analysis. The way in which you categorize the social phenomena you seek to describe must, however, be linked to both your theoretical foundations and the empirical reality. Be careful, however, for even those typological schemes that appear most unproblematic often carry with them notable levels of bias, both normative and analytical (e.g., democracy vs. authoritarianism). Carefully considering the typological categories you employ can have important analytical payoffs and will ensure that you are searching for meaningful distinctions.

Below is a brief bibliography on conceptualization in general. These should not be read as the final word on any of the topics, but rather as examples of the complexities and challenges associated with conceptual clarity. They may also provide important bibliographic and historical background for your own efforts to reach a concise and researchable definition.

Works on Conceptualization

* Concepts in Social and Political Philosophy. 1973. Richard E. Flathman (ed.). New York: Macmillan.
* Encyclopedia of Democracy. 1995. Seymour Martin Lipset (ed.). Washington: Congressional Quarterly.
* Handbook of Political Science. 1975. Fred I. Greenstein and Nelson W. Polsby (eds.). Reading, MA: Addison-Wesley (Eight Volumes!).

* International Encyclopedia of the Social Sciences. 1968. David L. Sills (ed.). New York: Macmillan and the Free Press.
* The New Palgrave: A Dictionary of Economics. 1987. John Eatwell, Murray Milgate, and Peter Newman (eds.). London: Macmillan.
* Oxford Companion to Politics of the World. Joel Krieger (ed.). New York: Oxford University Press.
* The Penguin Dictionary of Human Geography. 1987. Brian Goodall. (ed.). New York: Penguin Books.
* Social Science Concepts: A Systematic Analysis. Giovanni Sartori (ed.). 1984. Beverly Hills: Sage Publications.
* "What Makes a Concept Good? A Criterial Framework for Understanding Concept Formation in the Social Sciences."1999, John Gerring. Polity. Vol. 31(3):357-393.