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This document is a Adobe Acrobat version of the entire workshop guide to writing proposals.

Original files found at http://globetrotter.berkeley.edu/DissPropWorkshop/
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THE HOLY GRAIL: 
IN PURSUIT OF THE DISSERTATION PROPOSAL

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“[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 Francisco’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.

The Funding Regime: Selection Criteria and Processes

Before I turn to the nuts and bolts of the writing process, let me say a few words about the political economy of funding and proposal writing. You will doubtless be turning to a number of funding agencies—Federal (the National Science Foundation), Foundations (Ford, Carnegie, MacArthur), small donors (AAUW), NGOs (the Aspen Institute) and research organizations like the Social Science Research Council. Each of these organizations have quite different interests, forms of governance and review, and may vary quite markedly in terms of the nature of the proposal they request (a 2 page Fulbright application versus a 15 page NSF grant). Such variability speaks directly to the need to do and consider several things:
• Identify the panoply of organizations that might consider funding a project such as your own on say military security; the Foundations register, your University research office and this website are obvious places to begin! You might also want to check out our resources page.

• Be creative and flexible is reading the rubric of each funder and the specific program in question—perhaps a program on “peace and co-operation” to consider the ways in which your own interests may be ‘packaged’ (take note: not compromised) to be eligible for program and congruent with the grant guidelines.

• Dig around to locate background information on the funding agency (what sorts of projects have they funded in the past, who is on the selection committee?).

• Take careful note of the deadlines and the requirements of each application to give yourself time to prepare your proposals (six months minimum of writing, feedback and rewriting, and request letters of support as a teacher let me say that (a very quick way to seriously piss off your overworked advisor and to undermine your credibility is to request that letter of recommendation the day before the deadline).

• Recall that all such research competitions are competitions! Getting support is competitive, and becoming more so. The consequences are severalfold. You have to give the competition your best shot (you cannot submit a piece of garbage just because the deadline comes around). You must understand that the proposal will be read by a number of experts in your field–screeners, selection committees members, program officers and the like. You have to be writing to your peers recognizing and the experts will be sitting in judgement on what you write.

• Your project will be (for better or worse) assessed against others; research monies are tight. A reviewer/screener might be reading 30 such proposals from which he/she has to ‘deselect’ 20. To stand a chance your proposal must not simply be solid and good; it must jump out of the pile. There are several ways in my experience in which proposal can jump out of the pile: one is the proposal that has a typo in the first line or has the hypothesis buried in trivial details in a footnote on page 8. I would not recommend either strategy. Your proposal must ‘grab’ the reader: a tight, compelling, well-written and clever opening paragraph does wonders (I speak from the bitter experience of reading 100 proposals a year throughout the 1980s and 1990s for SSRC, NSF and other funders). A meandering fishing expedition will endure that your proposal is heading for the wastebasket. This is crude and harsh perhaps but the conditions under which your project is reviewed demands some serious reflection.

• You only have one time only to vote. Most, but not all, programs have one deadline per year. This speaks again to giving yourself the best chance at success–allow yourself time to think, write, and plan for the deadline. You cannot begin too early.

Primary Objectives and Parameters

I am making a number of assumptions and exhibit a certain conceit in outlining the primary concerns that should inform the construction of a research proposal (as an exemplar of research design). I do this because I am assuming that most of you are in the process of doing this or thinking about it for your dissertation projects. And this is of course a formative moment in your training. I am assuming that most of you will conduct something like fieldwork and to do this you’ll need to raise research grants and hence will need a research proposal (as indeed you will for your own internal departmental and disciplinary needs). So I am going to walk through the research proposal as a way of flagging some difficulties and some issues that we all need to think about–because the process IS so difficult, demanding and drawn out. I’ll do this by telling some stories about my own experiences conducting research in West Africa (Nigeria/Senegambia), in South India (Kerala) and California (the Sacramento valley). While my interests are eclectic I’ve had a particular interest in peasants, in rural transformation, in social
movements, and in a variety of agrarian issues including household dynamics and gender questions. These interests will shape what I have to say.

Let me immediately say that I cannot possibly deal in any detail with all of the problems of research design as such: this is not an occasion for a crash course in designing surveys, training assistants, thinking about respondent bias, working through the problems of evidence, or a genealogy of hermeneutic theory. Neither is this a treatise challenging or even questioning the theoretical or disciplinary approaches you may have adopted as an economist, anthropologist or historian. Of course I have my own biases and for purposes of clarity, I might as well make them explicit now. The first is to take seriously the notion of considering a variety of methodological approaches through which one can approach a research problem—to raise the idea of multiple methods as something I would encourage you to look into. And second to emphasize some of the key moments in research design and proposal writing (for example linking evidence to a particular question) as a way of driving home the point that you need to be as clear, as self conscious and as explicit as you can be in explaining HOW you will conduct a project (you’ve arrived in rural Idaho to study the militias with your truck and gun rack, now what are you going to do?). A good research design makes your research life in the barrios of Los Angeles or the NGOs of Bogota much easier. In this sense I suppose a research proposal is a sort of security blanket given all of the unknowns associated with doing fieldwork and collecting data. And in this regard a proposal by definition pushes you to construct something more than a fishing expedition—“I’ll go and poke around and see what is there”. A good research proposal provides you with an identifiable problem, a tentative hypothesis or proposition, a road map of necessary evidences, and at least some ideas about how and where that evidence can be located and generated. To leave the warm and cuddly academic groves of Berkeley or Cambridge for the field without having thought carefully through all such matters is to invite catastrophe, or at least more confusion and anxiety—which is where most people are when they start thinking about a dissertation topic. We can all do with less of this I presume. A proposal, then, has the merit of identifying a hypothesis or a hunch or an argument or a paradox to be explained. How else could one begin? There is something worse than a bad hypothesis, idea, or proposition, and that is no hypothesis (idea/proposition) at all.

Let me start here with a brief definition of a research proposal: it is a text that links in a more or less formal way theory, method and evidence. More elaborately we could say that a question or problem is theorized in such a way that it generates evidentiary needs on the one side, and a series of means (methods) for generating, locating and assessing evidence on the other. How these pieces are articulated—for example through a comparative study of three country cases using large-n samples—represents what I would call the research design. As I have already implied, differing funders impose different requirements, needs and organizational templates; disciplines may vary in their institutional culture as regards how formal such proposals should be. The language of hypothesis testing may seem remote in some disciplines or outright anachronistic. But all of the social sciences and humanities have to grapple with the intellectual and practical problems of conducting independent research: namely that some evidence is theory laden, that some questions have particular evidentiary demands, that some methods may not be appropriate for some questions and so on. I am assuming that we are all in the business of writing narratives of differing sorts that sustain arguments, proposition, that provide differing sorts of explanations of social life.

Put in this way it all sounds straightforward and perhaps pedestrian. But of course it isn’t. It’s the most difficult thing you will do (yes, even more difficult that writing the dissertation). There are very good reasons why in their book, In Pursuit of a Ph.D., Bowen and Rudenstein emphasize ‘anxiety’, ‘paralysis’ in their account of the genesis of a research project. Now we can talk about why this is the case: the process is often loosely institutionalized, it is compounded by bad advising and poor training, and it certainly is made no easier by the profound arbitrariness of arriving at a topic. How can something predicated on logic and reason be so illogical and unreasonable? Why on earth did I choose beer-brewing co-operatives in Burundi and not national dental organizations in Des Moines? There really is no avoiding this; selecting and designing a research project is hard, exhausting and unsettling; it is also thrilling, exhilarating and exciting.
But the difficulty of designing and writing a good research proposal is unquestionably compounded by the lateness to which students come to it. Highly structured coursework, and the impending nightmares of qualifying exams and so on, typically make the planning horizon the immediate and the short term rather than three years down the way when you are stepping into the field. You cannot start thinking about your research project too early for a number of reasons. First of all the identification of a place and problem–household dynamics in northern Kenya–carries with it enormous implications as regards the skill-set that you need to acquire: language, area studies, large scale social survey design and so on—all aside from the typical theory courses that are the very stuff of graduate formation. And I think that starting as early as you can is key so that you build into your formation not simply the need to know fields, but a series of integrated needs to conduct a project (it’s sort of difficult to pick up Chinese language late in the game). Second, the process of writing a research proposal is profoundly recursive. Your proposal can change radically in the course of being put through 6-10 different drafts and through soliciting feedback from your committee, friends and peers. To expect that this process to take anything less than six months is myopic.

Thirdly, the practical “start-up” demands of conducting a project, particularly in a foreign location, is time consuming. There is ideally a need to make regular pre-dissertation visits to establish scholarly contacts, affiliations and academic networks; there is a need to scout out possible field research sites and perhaps improve language skills; and most of all a desire to test one’s primitive ideas on the local scholars who are familiar with the subject at hand. To ensure such pre-planning presupposes time and flexibility and such practical requirements can only be laboriously constructed over time.

The great value of a research proposal carefully crafted early on in one’s graduate training is that it acts as a sort of foundation upon which a program of work can be constructed; that is to say is provides an intellectual and methodological roadmap for you. To determine, for example, that you wish to study the relations between local Ecuadorian environmental NGOs and US-based transnational environmental organizations that fund them—with the idea that foreign transnational organizations shapes the agendas and practices of local green groups in specific ways—generates immediate demands for graduate training, to put yourself in other words in the best possible position to both secure funding for the project and to accomplish a well-organized and effective field project. Quite specifically, one might anticipate the student wishing to conduct this project identifying the following areas and fields as (minimally) necessary for the project:

- Spanish language training, and perhaps a local vernacular should the Ecuadorian NGOs be representative of indigenous peoples.
- Theoretical work on transnational organizations and transnational networking.
- Methodological training on interviewing and participant observation.
- Conceptual work on inter-organizational behavior, management and practice.
- Background work on environmental movements and organizations including funding, structure and governance.
- Literature searches on Ecuadorian green movements.
- Affiliations and contacts with organizations in the US and Ecuador that will provide the case studies for the study.

Most of you will be in the business producing a 10-15 page research prospectus for funding purposes—and this will provide the template for my discussion—that includes sections on theory, method, design, and plan of work. There is no one way, one narrative structure or proposal organization, to link
problem, theory, method, and evidence but I would say that there are some generic demands (“principles”) that any compelling proposal must conform to:

- Transparency
- Clarity
- Methodological Precision
- Theory-driven expectations
- Plan of Work (‘do-ability’)

By transparency, I mean that the logic by which theory, evidence and method are connected must be explicit and obvious. This implies two things. One is that the reader must be able to understand how you are designing your project and what your thinking has been about the ways in which you will approach your problem or question. Hence if you are proposing to study the nature of social and economic differentiation among peasants in northern Thailand in relation to the neo-liberal reforms then it must be clear how you are going to measure differentiation (what criteria, how many people), the means by which you will collect data appropriate to the measures you will use, and the measures you are taking to ensure that you can separate out the effects of the neo-liberal reforms on differentiation from other ‘causal’ forces (say farming ability, household size. Transparency then is simply the legibility of the process by which you construct a problem, pose a hypothesis or question, and explore the evidentiary needs of your research and the validity of your results.

Clarity refers to the need to strike a balance between the specialized lexicon of theory and discipline and the need to be able to “walk-through” a proposal in a way that the reader fully and easily grasps the internal logic of the study. Clarity does not demand a sort of linguistic or expressive dilution but rather highlights the dangers of obfuscation (what exactly is this proposal suggesting?), ambiguity and a lack of sufficient information (what exactly is the author proposing to do in the name of ethnographic fieldwork or “hanging out” in the village?). Methodological precision asserts the importance of focussing on the “how” question. This is typically the part of the proposal that funders scrutinize with particular care: and it is often that part of the proposal which students fudge or gloss over the knotty problems of evidence. How large a sample, how will the sample be selected, is representativity an issue, how can one confidently assume that data on credit will be reliable, how exactly can evidence be collected on state espionage? The key point I wish to make here is that there are lots of exciting and creative and innovative questions that we as scholars can pose but have evidentiary demands that cannot be met (i.e. they presuppose that we have access to the internal records of large transnational oil companies). This may sound perfectly obvious in the abstract but all proposals must be able to convince a reader that reliable, valid and quality information appropriate to the question can be collected under the conditions of fieldwork in an ethically responsible way.

Theoretical expectations is perhaps counter-intuitive and somewhat controversial. It is the idea that the ways in which you are couching your problem—the theoretical tradition in which you have chosen to operate—provides something more than a context for your research; it is theoretical precisely because it leads us to expect certain outcomes or specific hypotheses. One can argue over the extent to which this is predictive or overdetermines the research process. But theory must be useful—it is a sort of toolbox that you have decided to deploy—and to this extent it leads the researcher to a hunch about what is going on. The hunch may be wrong—your research will discover this—but a proposal must contain such a hunch and, through the principles outlined, convince a reader why your proposition is plausible and worth exploring. Do-ability highlights practical considerations that will shape the “fundability” of the proposal—and indeed your ability to pull off the project! It is one thing to have a theoretically brilliant and well-designed study of financial markets and transnational capital flows; it is another to have the time, money and resources to analyze vast data sets and to complete the analysis in several months.
In adhering to these principles the reader should be fully able to appreciate the nature of the problem, how the researcher is approaching his/her study, and how it is to be conducted (when, where, how). In this way, a good proposal offers the reader a clear answer to the following three questions:

- What will we learn that we do not already know?
- Why is it worth knowing?
- How will we know if they findings are valid?

All of these questions are in some measure shaped by field, by discipline and so on (validity for a rational choice analysis of collective action may be rather different from an ethnographic analysis of a social movement). But you must always keep them in mind because they represent one important set of criteria by which your project will be assessed and evaluated.

A this point let me say a word about the construction of a proposal in relation to the reader, or more precisely those scholars (reviewers, screener, selection committees) and its assessment. I have already stressed the competitiveness of the selection process and its political economy for want of a better phrase. One can of course become almost immobilized by the prospect of second-guessing what funders “need” or are looking for. Indeed there are obvious intellectual and professional costs of “donor-driven research”. Nevertheless, there are a number of narrative devices, “tricks of the trade”, and obvious “dos and don’ts” that should not be overlooked.

- **Powerful Opening**: get straight to the point; do not drift around in some aimless way. The opening paragraph is your first salvo. You must have a way of encapsulating in a few sharp, snappy sentences what this project is about.

- **Freshness/originality**: There is no simple way of making a proposal standout, and the process of crafting a research project must not be an excuse of showiness, fashion, or superficial cleverness. One way, nonetheless, of highlighting your problem is to construct your study around a puzzle, a paradox or a conundrum. The rise of political Islam has been associated with a particular social basis to recruitment and a rejection of certain liberal ideals; case X is Jordan however stands as a striking contrast. Why. Or my theory would leave you to expect that people would vote in one way but in practice did the opposite. Why is Y movement in Nigeria that attacks ethnic politics as a stain on the Federation itself has ethnic identification as its basis for political mobilization?

- **Never bury ignorance or sensitivity**: even the best plans and early proposal writing can come up short. Or alternatively the best plans are confounded but unexpected crises and risk. A student preparing to conduct fieldwork in Chiapas in 1994 obviously had to confront unexpected political and practical difficulties. The point is that there will always be absences and deficiencies in everyone’s training and knotty practical and ethical difficulties to be confronted. Never bury thee problems or attempt to hide them. Respond to them directly. If your language skills are not terrific explain your plans to improve them. If you are working in a sensitive war-zone explain why you think you can conduct work there safely without endangering the lives or yourself or others. If you are collecting large-n data of a social survey sort but have no training in survey design, how do you intend to acquire these skills (you might consider a summer intensive course at the University of Michigan, the ground-zero of survey training).

- **Security in ambition**: conducting a project is always anxiety provoking; there are always unknowns and insecurities. How could it be otherwise? One common response to the combination of practical and personal insecurities (am I the person to do this, am I up to it?) is to add more wood to the research fire; adding questions, expanding the theme (do I have enough), adding more data and so on. Insecurity breeds ambition. But this can work against ‘doability’. One of the most common refrains of the dissertation advisor or the screener is: “it’s just too big”.

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• **Self-promotion:** never be reticent about making it clear why you are the person to do this project. You have language training, work experience in the region, several pre-dissertation trips, personal connections and so on.

• **Know, Don’t tell:** There will always be unknowns in any project. Which village will I select? How will I select my snowball sample? Can I interview people on sensitive issues like credit? The tendency is to defer judgement on these issues (“I’ll figure it out when I get there”). There are good reasons perhaps for improvisation in fieldwork; things don’t work, local contingencies shapes outcomes and choices and so on. But such a logic can breed either a complacency or sense in the proposal that you have not thought through (as best you can) what you might do. Give it your best-reasoned shot; don’t obfuscate, don’t fudge.

• **Shopping:** A research design cannot be a ‘look-see’ or a shopping expedition (e.g., long lists of generally unstructured questions).

• **You rarely can be “too specific”:** any advisor would rather read a proposal that has all the details in place (even if not justified!) and all the specifics addressed than a proposal that is full of vague associations, and elaborate hand-waiving.

**Entry Points and Using Evidence**

At this point let me step back a little and reflect upon how we identify a researchable problem or question (what I shall call points of entry), and the ways in which such a question or problem can be framed (what I shall refer to as logics of inquiry)\(^1\). Often we start will an ill-defined interest that takes the form of an association or a broad relationship, ill specified and general in its articulation. For example, we might be interested in the relation between migration and intra-household dynamics, or between Hindu nationalism and “neoliberal” reforms, or between armed struggle and forms of democratization. Quite how we get to these entry points and why really does not matter—and we should not spend too much time figuring out why we are drawn to violence or gender or class conflict (though these might be interesting topics for you and your therapist). These are all important entry points—and like all entry points they leave out important sorts of middle level questions and specifics: what forms of armed struggle; what are the specific aspects of neo-liberalism and how do they have causal efficacy, what sort of evidence would we need and use to identify this or that variable.

Entry points then usually take the form of a particular sort of question or query, with the goal naturally to identify the “right” research question. Often this process is treated as one of individual choice or by a curious process of osmosis in which the field of knowledge is transmitted to the researcher, or that it emerges inexorably from the data. In practice there is of course a complex tacking back and forth between theory, question and data. One cannot over emphasize the importance of struggling to formulate a coherent—that is to say conceptually integrated and empirically grounded—research question. The question does ultimately commit or obligate the scholar in keys ways: to mastering literatures, to identifying with a theory, of plowing through sources of data and so on. All of this is likely to lead to dead ends and paralysis unless the researcher is explicit and self-conscious about the theoretical and empirical decisions one has made.

Whatever the entry point, you will need at some point to generate a specific question rooted in empirical circumstances and with a particular design and scale (perhaps a large n, perhaps a national comparison, perhaps a single village case). An entry point typically generates different sorts of questions, each if which may provide the groundwork for the elaboration of a research program. One sort of question—practical—might emerge for a student’s experience working in a non-profit or a government agency. How can an Indian NGO better delivery family planning advice to south Indian

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\(^1\) I have taken this language and the discussion that follows from an alas unpublished book project (now abandoned) on Social Science Epistemology by Professors Paul Lubeck and Bob Alford of the University of California, Santa Cruz. I am grateful to Paul Lubeck for sharing this work with me.
women in deeply patriarchal male dominated households? How might organic grape growers in Napa Valley improve their market share? My experience is that students who have strong political commitments to their research and who have returned to graduate school from say practical work on development projects in the Third World, often lean toward such action questions. They may be driven say by the frustrations of western aid projects to target particular communities or by the tensions between local NGOs and their transnational partners. But such concerns must be located with respect to a theoretical framework, and within a logic of inquiry, if they are to be action-research (that is to say a theorized and scholarly program of work with direct practical implications emerging from the object of study). Another entry point and research question is empirical. Empirical questions can also take a variety of forms: some are abstract (“how is class consciousness shaped by social interactions among persons of equal status”), some are concrete (“were Muslims less involved in the genocidal activities in Rwanda in 1991 than Catholics”), or historical (“how did the language of the 1946 strike in X differ from the same plant’s strike in 1978”). And finally some questions are theoretical: “does bureaucratic domination reduce the legitimacy of rule”? “Under what historical circumstances does social integration increase or decrease”? “How do members of militant movements construct beliefs about the meaning of life which justify suicidal acts?”

The question then becomes, how do I push this question forward, develop and refine it, convert a hunch into a research program, a proposal. There are several immediate sorts of responses to this impulse. One is to figure out a conceptual toolkit that can help you refine your question, but can also generate hypotheses or propositions to be tested or evaluated. Another is the sort of evidence that is appropriate to the questions and the means by which valid evidence can be collected. A third, is how a particular approach to linking evidence and theory is shaped by practical considerations: your limited time, energy and resources. In quality research institutions much time is rightly spent on theory and on the student having acquired a road map of theory appropriate to the discipline, and also appropriate to the selection of concepts that are relevant to the research project. Much less attention is often given to the perhaps banal and pedestrian questions of evidence: both what constitutes evidence for a particular approach to a problem (and why), and the mundane issues of acquiring such evidence however constituted. As I have already mentioned, it is customarily the “methods” section of the research proposal that is weakest. It is often weak because it is underspecified–“I shall engage in participant observation”–but also because the connections–I would say the rules–by which evidence is linked to theory or theorized claims is often opaque and unclear.

Let’s take three projects for illustrative purposes. One is a study of a farmer’s movement in India with a focus on the question of the meanings of being identified with the movement. Another examines the particular historical conjuncture out of which the Mafia was born in mid nineteenth century Sicily. A third is an analysis of strike action in relation to rational choices made by differing sorts of actors. One way to see these different sorts of questions–they might all incidentally be approached in Marxist or Weberian terms–is that they fall into one of three logics of inquiry: respectively, they are phenomenological, historical and causal. Such logics provide ways for linking theory and evidence and also help you see the sorts of choices you have to make regardless of the content of the question. The logic of inquiry does not help you answer your questions; it highlights the choices that have to be made (working in one way with one set of tool does provides limits of what can be pursued and how) and their consequences. Logics of inquiry offer you a way of formulating and reformulating your question within different approaches, and to see the choices available to you.

Let me examine each of these logics in turn as a way of showing how something about the rules linking theory and evidence, and differing logics confer differing choices and options.

Causal Logic: one broad class of procedures attempts to distinguish the relative importance of different causal factors, to discover the causal structure that explains variation in the social world. It explains variations in the attributes of different units of analysis by deploying a multivariate analysis. In order for evidence to be recognized by theory (whether Marxian, rational-choice or Foucauldian), it must be transformed into “variables”. This approach is frequently grounded in and draws strength from positivism (the model is of course the natural
sciences, the world is assumed to be knowable and real, observations can be replicated, bias controlled and the world is divisible into autonomous parts). The most important variables cannot be manipulated by the investigator who must assume that classification into subgroups substitutes for experimental manipulation. It is assumed that one can draw data from a sample and measure the variables of interest without rupturing the actual social relations among individuals and groups from which the data is drawn. A survey is the most typical quantitative example of multivariate analysis. To work, some degree of independence of the independent variables must be assumed and defined. Objectivity is the careful specification of variable and their measures, and the reporting of all relevant data and how they were gathered. The observer is assumed to be at some distance from the observed. A basic task is obviously to reduce interview bias and measurement error. A model of causal logic might be Durkheim’s study of suicide.

**Phenomenological Logic:** This is an interpretive logic of inquiry. The various theories that make use of it assume that social reality is constructed by and through symbolic and cultural interpretations, webs of meaning and signification built and used by human actors. It is typically based upon a phenomenological philosophy and is customarily associated with field observations of real life situations, participant observation, ethnographic method and secondarily the interpretation of key texts. Within this logic there is a sort of causal connection between categories in the actor’s mind and their actions; between the roles being played and the rules of the game. But as Lubeck and Alford say, the open-ended negotiated, self-conscious character of social interaction means that causation is not linear; relations are contingent and subject to continual change. Meaning symbols and discourses are the theoretical categories that identify and locate relevant evidence for analysis. Observations of actual interactions, events, movements and gestures would be the typical qualitative data. Participant observation is the method that links phenomenology to interpretive theory and to qualitative field notes as the form of evidence. Objectivity results from self-conscious checking of the observer’s perceptions and his relations to those observed. The researcher participates in social life and categories of observation cannot be separated from those activities. While such questions of meaning—for example which symbols are struggled over in political struggles of X—are associated with cultural theory, and the humanities, but there is no *a priori* reason why surveys might endeavor to collect systematic data on some symbolic questions. A model of interpretive logic might be Weber’s *Protestant Ethic*.

**Historical-Dialectical:** This is approached by be based on a historicist philosophy, and draws strength from the observation and belief that contingent sequences of events take place within an interdependent historical totality. Evidence is primarily textual and the method is to construct a narrative sensitive to conjunctures, contingencies and contradictions. Historical analysis assumes that all relationships and processes are interdependent and change over time in relation to one another. The essential concepts are *totality* (a single case changing over time), *conjunctures* (overdetermination and multiple factors changing together), and *chronology* (sequences of concrete events). Historical events are discrete moments in time that can stand in for a variety of forces at work within a totality. Theoretical categories that identify empirical units of observation are, for example, the Depression, the Great War, and the New Deal. They sum up the meaning of a particular period and each of these events is a complex totality which derives its meaning from a larger context but also becomes the mechanism for gathering and interpreting specific historical data. As Lubeck and Alford say “the ideal type example of the historical logic of inquiry focuses on a single case seen as a totality of interdependent elements which constitute each other and cannot be separate from their relations from each other. The sequences of events are contingent outcomes which cannot be attributed to separable causes”. One might say this inseparability is dialectical. A search for patterns and changes is the method linking philosophy of history to historical theory, and the unit of analysis is the global, societal or sub-societal entity that has constitutes a whole. The interplay between structural forces and conjunctural or contingent events is an intrinsic theoretical issue within the historical logic of.
inquiry. There is a sort of causation at work here too but causes are neither linear nor independent; they are interdependent and dialectical. A model might be Marx’s *Brumaire*.

These logics are abbreviated and stylized of course but I want to refer to two key points about them. First, each type of evidence for a project located with respect to one of these logics must be converted to the appropriate form recognized by the theory in order to be defined as appropriate for explanation. A causal theory only recognizes primary data that can be converted into a variable. Texts or narratives of events are key to historical logics but must be converted into variables through some sort of coding if they are to be deployed by causal logic, although this coding may be qualitative as well as quantitative. Interpretive theory may use field notes but within the historical logic they are a text and for causal analysis they must be rendered into multivariate form. Second, in practice a research project may deploy two or more of such logics of inquiry—great works typically do—and a research program may indeed be involved in using specific data in a variety of ways (if possible) to make it appropriate for different types of analysis. Whether and how for example a historical text can be converted into a variable is an important and complex question. The point I seek to emphasize however is that analyses of quite different sorts located in different theoretical traditions may all locate their study in one of these logics. Marxist, neoclassical and institutional analyses of household economic behavior may all adopt a sort of causal analysis by deploying similar sorts of multivariate data. Similarly a Marxist analysis could be located in theory in any of the logics of inquiry (though I appreciate there will be a ferocious debate over whether causal logics are consistent with some versions of Marxian political economy). The key point however is that focussing on these differing logics makes clear to you the sorts of choices that are available to you once a question has been formulated.

Once you have made your choices—your Marxian analysis of the culture of work in south Indian textile factories—you can begin to seriously explore the sorts of evidences you need and the knotty questions of validity, reliability and so on. This is not the place to work through such a complex field but I would in passing take note of a number of issues that are typically lost sight of in many of the sorts in international fieldwork-oriented projects that pass over my desk:

- **National Accounts**: virtually all dissertations addressing some aspect of development typically refer to and make use of macro-economic and national accounts data (even if the object of scrutiny is the village or the household). Yet anyone who has worked in Africa or Indonesia is acutely aware of deep problems associated with the most basic economic data (for a period in the 1980s for example the Nigeria Central Bank published no financial and monetary data; the disparities between World Bank, FAO and USDA estimates of say Senegalese food output can be enormous). All of which is to say the epistemology of numbers warrants more attention than is customarily granted to the duplicated World Bank table or the UNDP statistical roll.

- **The Archive**: the use of colonial archives has also become an almost standard part of foreign area field research (and the same can be said of many other historical sources—Missionary archives, business archives and so on—that are deployed by the social sciences). I raise this point because rarely is the question addressed in a research proposal: how can you be confident that you can derive the sorts of data you need from historical texts? This question is not only one of textual interpretation, but also of whether such information was indeed collected and whether and how it can be located! Just because you are interested in prostitution in colonial Nairobi or communal violence in colonial South India, does not mean that the archive itself (and its organization) is laid out in a fashion which will expedite the discovery, or indeed the interpretation, of the information you need. To simply invoke the archive as a source of evidence then is simply a beginning, not an end. As Luise White discovered in her book on prostitution in Kenya one needs in some way to understand the social and epistemological organization of the archive—the “colonial mind”—in order to figure out where certain sources of information might be located.

- **The Assistant**: Even though many dissertation projects have quite limited budgets, the use of assistants (for surveys, as translators) is commonplace. Much has been made in Anthropology of
course of the deployment of the “informant” or assistant. I simply want to raise here the practical dimensions of using enumerators and assistants. How in other words one recruits (from where, with what background, with what local understanding and connection) assistants, how they are to be trained, their contractual or other relation to you the Principal Investigator, their salaries and benefits; in other words the dull details of employment, and the hermeneutic complexities of a sort of intellectual intermediation (you are getting information twice removed). Whether all of this needs to be documented in a research proposal is an open question. But once again to simply indicate in a Methods section that you will make use of ’interviewers' can only raise flags unless this is framed in some way.

- **The Survey**: Much could be said about surveys and this is not the place. In lieu of a full discussion, I wish to make the following points. Survey design is an art in itself and any project involving large n samples and a survey designed by the Principle Investigation (PI) must establish that they (the PI) have the training to undertake such a project. Here the absence of such courses on many campuses is striking and the utility of summer courses at some place like the ISPCR at the University of Michigan is accordingly magnified. Second, surveys generate substantial amounts of data, and a proposal must therefore be able to address the demands and resources associated with large scale data collection, management and analysis (saying that you have put in the budget the $5000 request for a new powerful laptop will not do it!). And third, the survey (however constituted) is something that some sections of the social sciences and the humanities shy away from (“I do not collect that sort of data”, “I prefer ethnography” and so on). In keeping with the thrust of my remarks and the value of multiple methods in research design, I would encourage students to think about surveys in a variety of way, not least the fact that a survey even if it is not a central data collection device is a powerful tool for scanning, probing and assessing the landscape on which your study will be located. In other words, there can be spillover effects and insights derived from the collection of a rather mundane baseline survey. It has also been my experience that the need for systematic data—which can only be generated by a survey—may emerge in the course of a project that did not anticipate the need for such data. Being prepared for such eventualities then has a particular payoff.

**Warnings, Pathologies, and Conclusions**

Parenthetically, it is precisely that these principles are often intractable and demanding that one can better understand certain ‘pathologies’ that attend the crafting of a research proposal: the flight into high theory (to avoid the demands of ‘operationalization’) or the flight into data and empiricism (to circumvent the demands of theorizing a problem). All of this in terms feeds the necessary/inevitable panic and self-doubt that is associated with a first stab at thinking about a dissertation project. To have the opportunity—formal or informal—to share these anxieties, and to benefit from the experiences of others (in preparing a proposal, collecting data, and writing the dissertation itself) is the sort of collective experience that one would have thought they would be institutionalized in some way in every Department. And yet it rarely is. It all seems to be *ad hoc* and word of mouth. On the Berkeley campus it is almost impossible to find a course on fieldwork, ethnography, or writing a proposal. The moral of the story being: create such opportunities, seminars and courses in your own program! Organize! Organize!

Finally, I want to turn to one last issue. The research proposal that you craft is ultimately a “big hypothesis”. I mean this in at least two senses. First, you may discover in the course of your research that things are not quite what you expected; the problem of out-migration is less significant than you thought, or the ease with which you can study domestic violence has been greatly exaggerated. The second, is that the world—and the world of your research site—changes. You may find yourself in a war zone; you may get sick for long periods of time; you may simply be unable, for reasons of sensitivity, to approach a problem because of shame or embarrassment or the threat of violence. All of these sorts of contingencies—the necessary and inevitable risks and uncertainties of *doing* research -- drive home the point that the proposal—however theoretically brilliant and methodologically sound—may, and often does, confront a real world and lived experience, including it needs to be said your abilities to do what you think you can do, which demands flexibility, improvisation and an ability and willingness to go back
and think again, or tweak the research, or perhaps at its worst abandon the project. All of which is to say that the research process is dialectical and recursive; there is a complex feedback between the document you prepare (and may have received funding for) and the risks, unknowns and contradictions of actually “doing research”. Perhaps none of this can be prepared for. But even the best-laid research plans can and never should be cast in stone. It is, for this reason, that good advisors (and funders) constantly reinforce the need to write regular reports on what you have achieved, how things are going, what are the ups and downs of data collection, and for a return trip from the field at some point during your research. Standing back from the day to day grind of what you are doing—seeing the wood for the trees—is a key prerequisite for conducting a research project, and for having the vigilance and self-reflection to see where and how you might be going off the rails.

To emphasize the contingencies of research, of research in action, takes us far from where I began. The same can be said for the completion of data collection and the long and arduous process of making sense of your fieldnotes, surveys, interviews and so on; and not least of writing the bloody dissertation. Now is not the time or place to reflect upon how we organize our field notes, how we prepare for our return to the University after a spell in Africa or France, or how to begin the difficult and sometime arduous process of writing. But they are part and parcel of this complex thing called “doing research”. Writing a research proposal is of course foundational to this process. My remarks are not intended to invoke first or second order panic (or depression). But it is perhaps inevitable that making explicit the silences and absences in our training and formation in graduate programs—of actually talking about and taking seriously the business of doing independent research—raises the bar in a way that can seem simply overwhelming. But it isn’t, or need not be, and moreover doing research can be the source of enormous energy, insight and yes fun. Hang with it!!
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.
The 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 discernable for reviewer.

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.
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 lays 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 it, 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.

Background and 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 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 back-ground 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 your 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.
### Timeline

Writing an effective and persuasive proposal is a time consuming task. Although the groundwork for your research must start when you begin graduate school, here are the final stages in preparing to submit a grant proposal. While some people may be able to turn out a grant-winning proposal in a number of days, these are truly exceptional cases. Depending on whether this is your first effort, how familiar you are with your case or cases, how quickly you write and how quickly your colleagues and advisors can give you feedback, the process can take up to six months. What follows is a list of tasks that almost every writer will need to address. In developing this timeline, we have assumed that you already have a good idea of the kind of research you want to do.

In developing your own time line (and having personal deadlines is critical), consider realistically how long you will need to accomplish the tasks at hand. Also consider how best to use your faculty and friends. Doing this involves judicious requests for guidance and assistance and allowing plenty of time for them to write references and feedback. In asking for help, always be very explicit about your time line and as specific as possible about the kind of feedback or assistance you require.

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<tr>
<th>Time until due date</th>
<th>Task</th>
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<tr>
<td><strong>PHASE Ia—Prewriting</strong></td>
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<tr>
<td>26 weeks</td>
<td>Carefully review grant applications and foundation websites. Identify substantive and administrative requirements. Note due dates and submission requirements (e.g., transcripts, letters, writing samples). If possible, speak to previous grant recipient.</td>
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<tr>
<td>25-23 weeks</td>
<td>Develop a one to two page preliminary statement of potential research question, topic, and approach. Identify relevant theoretical and empirical sources; develop a preliminary bibliography.</td>
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<td>23 weeks</td>
<td>Present short version and bibliography to advisor and graduate student colleagues.</td>
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<td>22 weeks</td>
<td>Use feedback to identify areas requiring further background research or conceptualization.</td>
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<tr>
<td>21 weeks</td>
<td>Research empirical context, history, background. Refine conceptual architecture.</td>
</tr>
<tr>
<td>19 weeks</td>
<td>Write a 2-3 page document exploring various theoretical frames/justification, questions, and respective methodological approaches.</td>
</tr>
<tr>
<td>17 weeks</td>
<td>Speak to people about feasibility and academic relevance of various approaches. Also consider which methods best suited to your skills, the available data, and your disciplinary requirements.</td>
</tr>
<tr>
<td>16 weeks</td>
<td>Continue research, Peruse existing works, and further develop your understanding of historical and contemporary contexts. This should help you become familiar with the reality of your case and how your work will relate (methodologically, empirically, and conceptually) to past approaches to the topic.</td>
</tr>
<tr>
<td>14 weeks</td>
<td>Refine your question in light of the kind of data you are may be able to collect and the way such a question is embedded in a theoretical frame.</td>
</tr>
</tbody>
</table>
## PHASE Ib—Early Administration

<table>
<thead>
<tr>
<th>Weeks</th>
<th>Task</th>
</tr>
</thead>
<tbody>
<tr>
<td>20 weeks</td>
<td>Identify and contact relevant data or support sources. This could be institutions, archives, organizations, or people who may facilitate your research once you are in the field.</td>
</tr>
<tr>
<td>18 weeks</td>
<td>Begin budget research (living expenses, travel, etc.) and visa and residency requirements.</td>
</tr>
<tr>
<td>14 weeks</td>
<td>Begin research protocol process</td>
</tr>
<tr>
<td>13 weeks</td>
<td>Request transcripts (especially from other schools); Revise CV</td>
</tr>
</tbody>
</table>

## PHASE II—Focused Writing and Administration

<table>
<thead>
<tr>
<th>Weeks</th>
<th>Task</th>
</tr>
</thead>
<tbody>
<tr>
<td>13 weeks</td>
<td>Integrate research question, theoretical frame, and a provisional research design into a single document (5 pages)</td>
</tr>
<tr>
<td>12 weeks</td>
<td>Spend a week gathering your thoughts and additional 'data' you may need to complete draft</td>
</tr>
<tr>
<td>12 weeks</td>
<td>Reconnect with potential collaborators, institutions, or organizations and determine which will be useful and reliable</td>
</tr>
<tr>
<td>12-10 weeks</td>
<td>Flesh-out document into a complete draft following the grant requirements</td>
</tr>
<tr>
<td>9 weeks</td>
<td>Get feedback from colleagues</td>
</tr>
<tr>
<td>8 weeks</td>
<td>Revise draft, create a tentative budget, and submit to faculty along with C.V. or other support materials. Ask faculty advisors for letters of reference</td>
</tr>
</tbody>
</table>

## PHASE III—Editing and Submission

<table>
<thead>
<tr>
<th>Weeks</th>
<th>Task</th>
</tr>
</thead>
<tbody>
<tr>
<td>5 weeks</td>
<td>Review specific requirements for grant application.</td>
</tr>
<tr>
<td>5 weeks</td>
<td>Revise proposal to incorporate faculty suggestions</td>
</tr>
<tr>
<td>4 weeks</td>
<td>Let sit for a week</td>
</tr>
<tr>
<td>3 weeks</td>
<td>Gently remind faculty of letter due date</td>
</tr>
<tr>
<td>2.5 weeks</td>
<td>Assemble materials (transcripts, c.v.)</td>
</tr>
<tr>
<td>2 weeks</td>
<td>Review proposal, carefully edit, and finalize.</td>
</tr>
<tr>
<td>10 days</td>
<td>Get help copy editing from friends and/or colleagues</td>
</tr>
<tr>
<td>3-4 days</td>
<td>Print final copy and collect materials (allow time for last-minute catastrophes)</td>
</tr>
<tr>
<td>2-3 days</td>
<td>Submit proposal</td>
</tr>
</tbody>
</table>

**DUE DATE**
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 if 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.
Sample Budget One

Travel

Three trips to the National Archives, Washington, D.C.
@ $275 roundtrip airfare. $825.00
Ground Transportation $65.00
Per diem @ $64, for 10 days $640.00

Sub-total, Travel $1,530.00

Communications

Telephone $250.00
Postage $80.00

Sub-total, Communications $330.00

Materials and Supplies

Printing and Duplicating $150.00
Miscellaneous supplies $100.00

Sub-total, Materials and Supplies $250.00

TOTAL $2,110.00

Source: Rubin (1983)
Sample Budget Two

Estimated Resources at Present

At present, I have secured no funding earmarked specifically for the proposed project. It may become possible to upon personal savings to supplement an awarded fellowship for an amount no greater than $1,500.00

Estimated Total Expenses for Fellowship Period

The following budget indicates travel and research expenses association with the completion of the project outlines in the attached narrative. Although my fieldwork will be executed entirely within Tanzania, the project involves time spent at three discrete locations: the capital, Dar es Salaam, and two yet to be determined village settings in Rukwa district. As such, the budget details the rail, bus, and accommodation funds required for my frequent relocations. Also included are fees to obtain a cellular telephone and email address. Such services are central to the maintenance of contacts and the collection of information during my time in Western Tanzania where the telecommunications infrastructure is poorly developed and unreliable. The airfare reflects the average of quotes obtained for student tickets from a number of discount travel services. In country expenses (transportation and otherwise), are based on inquiries undertaken while previously in Tanzania.

Food and Personal Maintenance ($18/day) $4,860.00
Accomodations and Storage $3,375.00
University Fees/Tuition (Dar es Salaam) $3,000.00
Health Insurance and Medi-vac ($300/month) $2,700.00
Round-trip airfare from San Francisco to Dar es Salaam $1,400.00
Communications (e-mail, telephone)($60.00/month) $540.00
Translator/Research Assistance (Rukwa) $600.00
Ground Transportation in Tanzania $700.00
Photocopying $250.00
Survey expenses $250.00

TOTAL $17,925.00

Potential Sources of Alternate Funding

Fulbright-IIE Rejected
Institute on Global Conflict and Cooperation Pending
Institute of International Studies, University of California Pending
Joint Center for African Studies, Stanford-Berkeley Pending

Source: The preceding budget was written for an application to the Institute for the Study of World politics.
Concepts and 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, followed by links to bibliographies on specific concepts. 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**


Selected Bibliography of Specific Concepts

**Agency**


**Anarchism**


**Atrocity**


**Authoritarian Regime**


**Authority**


Balance of Power

Bureaucracy

Capacity

Citizenship


Civil Society

Change, Organizational

Class

Coercion

Commons, The
Community

Compromise

Consensus

Corporatism

Crises


Critical Junctures

Culture

Culture, Political

Decentralization

Democracy


**Development**


**Elite**


**Entitlement**


**Equality**


**Equilibrium**


**Ethics**


**Ethnicity**


Exchange


Fascism


Feminism


Freedom


Game Theory: Prisoner's Dilemma, Battle of the Sexes, and Chicken


Human Capital


Human Nature


Human Rights


Ideology


Income Distribution

Institutions


Integration


Justice


Justice, Compensatory


Justice, Criminal


Justice and Gender


Justification


Just War


Kinship


Law, Limits of


Law, Rule of

Leninism

Liberalism

Liberty

Majorities

Markets

Marxism

Morality

Nationalism

Oligarchy

Order, Political

Totalitarianism


**Trotskyism**


**Utility**


**Utopianism**


**Virtue**


**Voluntary Association**


**Welfare**

Style

You have only one chance with most grant reviewers, so the way you present your ideas is central to the success of your proposal. The people who read your proposal will sometimes be reviewing hundreds of others and are likely to be overburdened with other projects as well. Your goal is for the reviewers to be able to understand your research purpose and judge its relevance and importance without having to work to do so. Regardless of your project’s intellectual merits, a proposal that puzzles reviewers with complex syntax, ill-defined terms, or inelegant prose is not likely to win a favorable rating. With out style, your proposal’s substance is likely to be ignored. With this in mind, the following four central criteria may help you make your proposal as lucid and explicit as possible. You may also wish to refer to George Orwell’s *Politics and the English Language* (viewable at this website).

Clarity

Clarity is the most important aspect of style for proposal writing. The clearer your proposal, the easier it will be for the reader to understand and follow your logic and writing. The following are some suggestions for enhancing your proposal’s clarity. (See also “The Art of Writing Proposals” by Adam Przeworski and Frank Salomon, at the Social Science Research Council website.)

**Think first.** The first step of clear writing is clear thinking. The clearer the ideas are in your head, the clearer they will be on the page. Thinking through your proposal should leave you with a solid understanding of what you are proposing to research, how you plan to accomplish it, and its broader relevance to scholarship and to the world. After you have written a draft, leave yourself time to think it over and then, without looking at the original draft, see if you can say what is important in a clearer for accessible way. Many of the students we spoke with told us that doing this not only made the writing much easier and more satisfying, but also helped integrate the overall structure and logic of the proposal.

**Use Outlines and Sketches.** If you work well from outlines, lay out your thoughts in that structure to organize and direct a logical flow for your proposal. While you may or may not follow this outline exactly or even use it as you write (though we suggest that you do), thinking through the overall structure and logic of your proposal will help focus your writing and lead to a clearer proposal. You may also want to sketch out certain parts of the proposal or ideas before you start writing. If you are reviewing a particular concept, for example, it may help to write out what you mean by the term on its own. This allows you to test out ideas and logical connections without having to integrate them into your broader argument. If these ideas are clear to you before you write, your proposal is likely to benefit.

**Be explicit.** A proposal should not read like a mystery novel where the key to the story comes at the end. Never assume that the reader knows what you mean or where you are going with your prose. State your research question and objectives early on and make it clear that you are doing so. If you haven’t gotten to it by the first paragraph, make sure it comes on the first page. While you need to leave your position open to reconsideration, present your argument (or your hunch) in the most straightforward manner possible. It is hard to overstate this piece of advice. Many reviewers will spend only a brief few seconds searching for the main purpose of your research. Making it hard for them makes it much less likely that they will read the rest of your proposal.

**Write simply.** As the saying goes “write to express, not to impress.” This often means writing in short, simple sentences using simple language (see below). When possible, write short paragraphs that begin with informative topic sentences that tell the reviewer what you are going to do in the
paragraph; then do it. Use simple verbs and place them next to the subjects to which they pertain and, whenever possible, eliminate complex clauses or language that may be open to multiple interpretations. Use subsections and verbal maps that orient the reviewer “up front” to your direction and purpose.

**Minimize jargon.** As a general rule, keep your proposal as jargon-free as possible. Too often, language used to impress proposal reviewers obscures more than it reveals. Keep in mind that what is considered conceptual precision to one reviewer might be alienating and impenetrable to another. If you feel that there are terms that that may be foreign to some readers but are conceptually critical for your argument, briefly define them in your text to avoid alienating anyone. Try to use language that is widely recognized and used in different disciplines to improve the likelihood that a reviewer from another field or subfield will be familiar with the terms you are using. The best way to ensure your text’s ‘democratic’ credentials is to have it read by colleagues from other disciplines (see below).

**Be brief.** It enhances clarity if you convey the maximum information in the minimum number of words. For example, substitute “now” for “at this point in time,” “whether” for “whether or not,” “to” for “for the purpose of,” etc. Perform an “efficiency review” of your proposal with the aim of deleting words and phrases that are not crucial to the meaning. This helps eliminate wordiness, which gives you more room to convey important information and helps communicate your ideas more clearly.

**Share.** We highly recommend talking though and sharing your ideas with others at different stages of your proposal writing. This is absolutely essential when you have a working draft written, but talking to others is also helpful when conceptualizing and outlining your proposal. Doing so helps you see early on the potentials and pitfalls of a given project and lets you hone your argument. When choosing casual reviewers, select people who will bring different strengths and perspectives to their reading of your work. People from outside your field, those with good editing skills and those with strong theoretical and/or empirical backgrounds are likely to prove particularly useful.

**Tone**

An inappropriate tone—one of arrogance or apology—can condemn a substantively sound proposal. Remember that the reviewers are not just funding your research idea; they are investing in you and in the likelihood that you will be able to carry out the proposed research. The tone in which you express yourself is likely to influence the reviewers’ estimation of you and your capacities.

**Be confident.** Your writing should convey a respectful confidence. Expressing a measured confidence in your research and yourself is likely to enhance the reviewers’ faith in your ability to carry out a rigorous academic investigation. To do this, use straightforward language and simple verbs (avoid using the subjunctive or excessive conditional clauses). Instead of, “If I am funded I would hope to conduct interviews during the final phase of my research,” say “I will conduct interviews during the final phase of the research.” Avoid the passive voice as much as possible. The passive voice avoids specifying who or what did, does, or will do the action of the verb. The result in most cases is a less direct and often less confident tone. Rather than, “the research will be conducted over a one-year period,” write, “I will conduct the research in one year.”

**Be passionate.** Do not hide your passion for your project. There are few graduate students in the humanities and social sciences in search of fame or fortune. Indeed, most care deeply about their work, are often normatively engaged with it, and are truly excited for the opportunity to do original research. When these attitudes are expressed through your writing, they may help pique the interest of even the most jaded reviewer. Communicating this passion is easier for some then for others and must always be done carefully. It may help to start by reflecting on what made you interested in your topic.
when you first got involved and why you are still keen on the research. Do not state your feelings in the proposal directly, but express your passion in the way you frame and tell the reviewer of your story, your approach, and your work’s import. The key is to express these sentiments while maintaining a respect for the formality of the proposal format.

Avoid arrogance and apology. One of the fastest ways to estrange a reviewer is to write your proposal in an overly arrogant or apologetic tone. You must find and respect the line between being pompous and being confident; apologetic and modest; passionate and unprofessional. Make sure the information you convey about yourself is information that the reviewer really needs to know for your role in the project and is not simply thrown in to impress. Express your enthusiasm through your topic or approach, not through personal information about yourself. Do not apologize for what you do not know, but focus on what strengths you bring to the research and how you will systematically overcome your shortcomings (e.g., language training). Finally, get friends, preferably close, honest friends, to read your proposal with tone in mind and ask them for candid comments.

Coherence

Proposals are frequently the products of innumerable drafts and revisions. While the linkages between and among the sections may be clear in your head, they may not always be so evident to readers of your proposal. As you revise, concentrate on ensuring a high degree of coherence, the logical and smooth integration of the text’s various sections. For your proposal to be successful, it is essential that the research question you propose is logically linked to the methods you plan to employ, and that your theoretical frame adequately justifies the empirical cases and context which you hope to explore. And these linkages must be made explicit. The following paragraphs point to four common sources of discontinuity and disconnection. As with everything else, the only way to ensure continuity is to have others read your proposal.

Questions and Methods. Your research question (or questions) will be one of the most scrutinized sections of your proposal. Reviewers will closely consider whether the methods you propose to use are adequate to gather the information you need to answer the question(s) convincingly. We suggest that you place each question on one side of a sheet of paper and carefully map out how the methods employed will help you gather the information needed to answer each question.

Case and Theory. After taking years of course work and preparing for qualifying exams, researchers tend to organize their proposals around their theory. This can be all too apparent in the proposal itself and can result in the history or description of the research site and background seeming disconnected from the research itself. Often in these situations, a researcher will attempt to make the case or topic fit their theoretical framework too neatly exposing their ignorance of what is certainly a complex reality. Conversely, many students offer theoretical frameworks that come across as weak justifications for spending time in a place that interests the researchers. Such attempts are often very transparent and may raise a red flag to reviewers. To avoid this, carefully justify why you have chosen your case and how this selection relates to a broader theoretical debates and concerns. Similarly, make an effort to emphasize why this theoretical frame is particularly well suited to the trends and patterns unfolding in your area of interest.

Project and Time. One of the easiest ways to determine if researchers are realistic is to look at what they intend to do in the allotted time. Most first-time researchers, eager to overcome the shortcomings of past efforts, drastically overestimate what they can accomplish. Your timeline—a concrete part of your research design—must persuade the reviewer of two things. First, you must demonstrate that you have a good idea of what conditions are going to be like on the ground. If you cannot travel long distances during the rainy season, you must schedule this into your plan. Second, you must show that
you have prioritized the methods and approaches you are going to use. If answering your research question depends on a particularly kind of data, a good portion of your timeline should be dedicated to its collection. Speaking to others who have recently completed similar project or even trying out some of the methods at home will help you realistically understand the time needed to complete your proposed project. For more on this, refer to the research design page at this website.

**Budget and Project.** Quite often researchers have lofty ideas and ambitious goals, but the proposed budget appears insufficient to complete the research. Skimping on the money you ask for does not increase the chances of getting funded. Moreover, if your budget does not match the cost you will incur in your project, it conveys the impression that you do not realistically understand your research and may cause your proposal to be rejected because the project appears infeasible. If you need more money than the funding source offers, mention other sources you will be approaching for funds. Be specific about what costs you are asking a particular grantor to fund and what parts of your budget you are asking other donors to fund. To assure coherence between your budget and your proposal, be honest, realistic, and transparent in matching your budget to the actual work you will need to do to carry out the research. For more on budgeting, and sample budgets, refer to the budgeting page at this website.

**Presentation**

Academics pride themselves on their substance and seriousness. Such concerns can not, however, lead you to overlook your proposal’s physical appearance. Proposals that are easy to read are simply more inviting to the reader. After a reviewer has looked at dozens of proposals, most from qualified, competent candidates, a clearly presented proposal can literally be a sight for sore eyes.

**Readability.** Make your proposal easy and inviting to read. Although researchers commonly give a great deal of attention to the presentation of a curriculum vitae or resume, they often neglect the esthetics and presentation of a proposal. Leaving lots of white space on pages and being clear and consistent with your style headings and subheadings, boldface, underlining, capitals, or italics can help the reader to visually navigate your prose. Finding a font that is easy to read and that leaves enough spacing between letters and words will only help in this regard. Avoid narrowing your margins or reducing the font size (in the text or in the footnotes) just to squeeze more words on the page. Try cutting out words instead. Attention to these fine points of presentation can help reviewers work less to see the gist of your argument and even enhance their comprehension of your more difficult substantive concepts.

**Length.** The desired proposal length varies greatly from one foundation to another. Some want shorter more conceptual proposals while others will ask for more robust research proposals of the type included on our examples page at this website. In either case, it is important to stay within the specified page or word limit or limits. Many foundations are very strict about these page limits and some will not review proposals that exceed the requested length. Do not simply adjust the line spacing, font sizes, or margins of your proposal to fit within the allotted length. Respect the funders’ requests and shape your text so that it will fit within the required length utilizing standard margins, font sizes, and line spacing. Following the funders’ guidelines by providing a concise statement of your research agenda is only going to help you to earn a favorable review.
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1) ARCHIVAL RESEARCH

Prefatory remarks

For non-historians, archival research may seem like a standardized and transparent method of inquiry that is carried out mainly in libraries and special collections rooms. Even for some historically-oriented social scientists today, questions of “historiology” appear to be divorced from what they think historians do at and with “the archives.” But there are as many different kinds of archives as there are different agents and agencies—letter writers, households, firms, parties, unions, universities, states—that produce and save their own records. And, as with other critical sources and strategies for “reconstructing the past” and “detecting the future in the present,” we need to be reflective about the range of (mostly) unpublished and (but not always) eye-witness inventories that we use.


2) CASE STUDIES


3) ETHNOGRAPHIC METHODS

Prefatory remarks

“The process of doing ethnographic research was a kind of insiders’ information until about 1970, when Peggy Golde edited and published Women in the Field. Until then, the prevailing wisdom was that fieldwork was something to be learned by doing, that was individual and idiosyncratic, and therefore not teachable. There were accounts of fieldwork to be sure, but their role was primarily to enhance the mystique of the heroic anthropologist, braving unfamiliar customs, suspicious natives, and horrible hygiene in order to add to the Science of Men. Once you had done it, you could sit and drink beer and swap stories by the hour, but fieldwork was like combat in that you had no idea what it was like until you had done it.

Since 1970, everything has changed, and the process of fieldwork has been scrutinized repetitively (and sometimes repetitiously) from many angles. The stimuli for this seem to be two: the realization that we can learn from each other, and in fact have the obligation to share information; and the troubling thought that fieldwork, as an unsystematized process, embodies biases that we hardly realize are there. There have thus sprung up two rather different but related literatures: one on how to do it (techniques), and one on what it’s really doing (critiques).

You can read just about any ethnographic monograph from the “classic” era between the World Wars (and some of the more modern ones) and find an account of what the anthropologist did in the field. Particularly enlightening not only about the methods employed by the old masters, but also about their attitudes toward their work, are the introductory sections of Bronislaw Malinowski (1922), F. E. Evans-Pritchard (1940), Napoleon Chagnon Yanomamo (1968), or the chapters on the field from Margaret Mead’s autobiography, Blackberry Winter.

More accounts of What It Was Like, written just at the beginning of the era of methodological and critical scrutiny of the fieldwork process, can be found in Peggy Golde (1970).

Regarding technique, there are more manuals here than one can keep track of, almost all published in the 1970’s and 1980’s. These include Robert Burgess (1982); Werner Schoepfle (1987); P. F. Ellen (1984); Pertti Pelto and Greta Pelto (1979); and James Spradley (1979) and (1980).

The general critique of anthropology as a politically and socially embedded endeavor, rather than an objective science, began in earnest with two edited books published in 1974: Talal Asad’s Anthropology and the Colonial Encounter and Michelle Rosaldo and Louise Lamphere’s Woman, Culture, and Society. One questioned the colonial roots of the discipline; the other, the implicit and unexamined gender bias.
This critique grew and flourished into the 1970s and 1980s, and shows signs of abating in the 1990s with most of the issues out on the table. One branch of the critique is directed specifically at the process of ethnography, demoted and enriched from positivistic science to literary endeavor that may or may not be scientific. The most influential collection in this vein has been James Clifford and George Marcus (1986). Also noteworthy (and shorter and easier) is Clifford Geertz’s *Works and Lives*.

Recently, there have been attempts to combine critical with methodological thinking, to extract anthropologists from the epistemological bind created by the early critiques. I have found both Tony Larry Whitehead and Mary Ellen Conway’s *Self, Sex, and Gender* and *Gender in Cross-Cultural Fieldwork* (1986), and Roger Sanjek’s *Fieldnotes* (1990) to contain many valuable pieces.

A useful exercise is to read the introduction to Evans-Pritchard’s *The Nuer*. Then red critiques of Evans-Pritchard’s approach to Geertz’s *Works and Lives*, James Clifford’s “On Ethnographic Authority” in his *The Predicament of Culture*, and Renato Rosaldo’s “From the Door of his Tent,” in Clifford and Marcus’ *Writing Culture* (1986). Would you feel like doing ethnography after this? If so, how?


John Bodley, *Victims of Progress*, Mayfield Press.


James Clifford, “On Ethnographic Authority,” in *The Predicament of Culture*.


Clifford Geertz, *Works and Lives*.


Margaret Mead, *Blackberry Winter*.


Tony Larry Whitehead and Mary Ellen Conway *Gender in Cross-Cultural Fieldwork* (1986).

Tony Larry Whitehead and Mary Ellen Conway *Self, Sex, and Gender*.


4) FOCUS GROUP RESEARCH


5) ORAL HISTORIES

Oral Traditions: Theory and Method


de Heusch, Luc, The Drunken King, Indiana, 1982.


History in Africa, annual.


**Oral Traditions: Case Studies**


**Oral and Life History: Theory and Method**


Hofmeyer, Isabel, “We Spend Our Years as a Tale that is Told”, Heinemann, 1993.


**Oral and Life History: Case Studies**


Reviewing Books about Africa,” *History in Africa*, 17 (1990), 373-78.


Robertson, Claire, *Sharing the Same Bowl*, Indiana, 1984.


6) QUANTITATIVE METHODS

Overview:

Survey data, coded texts, and administrative data are the most likely examples of evidence that are typically analyzed using quantitative methods. These data are often numerous and/or reflect a probability sample of a population, allowing researchers to employ quantitative methods to draw broad descriptions, in their simplest form, or test causal arguments, in their most complex form. In both cases, the purpose of a quantitative approach is to offer generalized descriptions of patterns with some statistical estimate of the likelihood of observing that pattern. Quantitative methods can be employed at numerous points in the research process and in combination with data analyzed qualitatively, a mixed-methods approach. Researchers in Economics, Sociology, Psychology, and Geography are more likely to employ quantitative methods, but quantitative approaches are not limited to these disciplines. For example, quantitative methods might be used to provide a background description of a place about which qualitative methods are employed to gain deeper understanding. Or, quantitative methods might be employed to get a sense of the extent of a phenomenon and the degree to which the phenomenon varies across important categories, such as class, race or gender. This information is then used to inform a qualitative study design.

Typical concerns for quantitative methods are measurement (e.g., attention to reliability and validity or the appropriateness of composite indices for capturing conceptual complexity), independence of observations (e.g. spatial or temporal autocorrelation), distributions (e.g. normal, linear, or non-linear) and model specification (causality, omitted variables, outliers, and estimators/estimation techniques). Quantitative evidence can take the form of a cross-section – representing one moment in time with observations of multiple people, households, communities, or countries at that moment. Or, quantitative evidence and methodologies can take into account multiple observations over time. Or, quantitative evidence can be relational observations as in a multi-level data set (for example, evidence from individuals with additional information about their households with additional information about their communities) or a network data set (e.g. information about the extent and quality of ties between people – kinship relations or friendship networks).

Prefatory remarks

There are a number of potential bridge points between economics and other disciplines on development issues. Bridge points with anthropology include theories of the household, resource allocation and institutional formation, migration, networks, and economic integration of labor markets across international boundaries.

Two fields of economics—economic history and development economics—have so much overlap with certain areas of history as a discipline that thematic bridges are abundant. From U.S. history, three examples come to mind: women and work in the industrial revolution, economic and historical roots of slavery, and women and work since World
Bridge points between economics and political science include theories of the state, political economy of policy, government action and credibility, provision of public or social goods, coalesional formation and strategic interactions, and collective action problems.

An array of topics focusing on individual behavior - attitudes toward risk, deviant behavior, charity, child development, etc., are potential bridges with economics as well.

With respect to sociology, potential bridges include institutional formation, collective actions problems, group behavior, especially the microfoundations, industrial organization, labor unions and the contractual arrangements of labor relations and theories of the household.

Those who are not familiar with much of development economics may read a few introductory books on development such as Basu, Gills et al, or Todaro. To get more information on any particular topic, a basic reference book is the Chenery and Srinivasan edited *Handbook of Development Economics*.

Once you have formulated your quantitative theoretical model that has interesting policy implications, or you have postulated some behavioral relationships, you would like to estimate these relationships and test if your theory or its predictions are supported empirically.

It is important to statistically estimate the behavioral relationships that you have assumed in your theoretical model, otherwise you are building up bubbles on the air with your theory. Appropriate econometric techniques need to be applied to estimate your model, and to choose between models. Of course, the techniques that you can apply depend on the type of data you have. If wrong econometric techniques are applied to your valuable data that you collected with enormous efforts, you will draw wrong inference and policy conclusions. A starting point for those of you who do not have a formal course in econometrics/ statistics before is the book by Ramanathan [1992]. This book takes you through most of the econometric techniques and issues without requiring you to have much prior knowledge in the area, and the book comes with a software which is helpful for you to practice most of the empirical exercises yourself. A somewhat more advanced reference book covering most of the econometric issues is Judge, et al.

If you have historical quantitative information on certain variables, or you have aggregate macro data for a country over a long period of time, and you would like to see whether these variables are related in certain ways (you postulate the relationship), then you will be using time series analysis. For this purpose, after you master the materials in Ramanathan, read Harvey, and then Granger and Newbold.

When you have data on certain variables for a lot of households, or time and suppose you postulate a certain behavioral relationship to hold for a representative household and want to estimate your postulated relationship, you use survey data analysis. There are different
techniques available depending on the type of variables you have, the type of relationships you postulated, and the way you have collected your data. The books you may consult in this area are Maddala, Heckman and Singer, and Amamlya (in this order of difficulty).

When you have repeated observations on a few households or firms over a certain period of time, you use pooled time series cross section analysis. The basic book on this is Hsiao.

It is not enough that you specify only one model and estimate it using most appropriate statistical techniques. In fact, in most situations there are a lot of competing models that you will come up with. Then the natural question is how to decide which estimated model to choose for your final analysis of the behavioral and policy issues that you started with. These issues fall in the area known as specification testing, a basic reference in this area is Godfrey, and a more advanced text is White.


7) SURVEY RESEARCH

Overview


Planning (and Proposing) Survey Data Collection


Research Design


Qualitative Inquiry in and beyond the Design Process


Overview of Concepts in Sampling


Developing Good Questions: Fundamental Concepts


Developing Good Questions, Cont.: Measuring Behaviors, Facts, Knowledge, and Attitudes


From Questions to an Instrument: Components of a Questionnaire and Guidelines for Ordering and Formatting


Pretesting: Rationale and Overview of Pre-Field and Field Techniques


**Data Pre-Coding, Coding, Cleaning and Management**


University of Michigan Survey Research Center. ND. *Introduction to the Coding Procedures Used in the Survey Research Center.*

**Modes of Administration: Overview of Classic and Innovative Approaches and Criteria for Selection**


page 24, reading list


**Interviewer Recruitment and Training**


Fieldwork, In Principle and Practice: Strategies for Monitoring Quality


8) RESEARCH ETHICS

**Defining the Standard Realm of Ethical Issues**


Also available at http://ohrp.osophs.dhhs.gov/humansubjects/guidance/45cfr46.htm


Freed-Taylor, M. “Ethical Considerations in European Cross-National Research.” Available at http://www.unesco.org/most/ethissj.htm


Ziman, John. “Why must scientists become more ethically sensitive than they used to be?” *Science; Washington; Dec 4, 1998*


**Expanding the Standard Realm: Starting and Doing Field Research**


Patrick G. Coy. Shared risks and research dilemmas on a peace brigades international team in Sri Lanka *Journal of Contemporary Ethnography; Thousand Oaks; Oct 2001*

George A Morgan; Robert J Harmon; Jeffrey A Gliner. Ethical problems and principles in human research *Journal of the American Academy of Child and Adolescent Psychiatry; Baltimore; Oct 2001*
James E Giles. Ethics and epistemology in the twenty-first century. *Cross Currents*; New Rochelle; Fall 2001;


Do the Ends Justify the Means? The Ethics of Deception in Social Science Research. from Online Ethics Center (June 2000) Available at http://www.onlineethics.org/reseth/appe/vo11/justify.html


Shrag, B. “Commentary on Crossing Cultural Barriers: Informed Consent in Developing Countries” from Online Ethics Center. Available at http://www.onlineethics.org/reseth/appe/vol5/culturalc1.html?text


*Broadening the Standard Realm of Ethical Issues when Considering Research in International and Cross-Cultural Settings: Research Collaborations and Dissemination.*


Jones, Todd. “Interpretive Social Science and the ‘Native’s Point of View’: A Closer Look.” *Philosophy of Social Sciences* 28 (1):32-68


9) SOME EXAMPLES OF SECONDARY DATA SOURCES

Through the web it is now possible to access numerous types of data either immediately by downloading information, or relatively quickly through purchase, or slightly longer through permission from principal investigators. In any case, it is likely that it will not take much effort to find important background or critical information and statistics about your topic, country, or site of study. The following are just a few starting points and should not be viewed as inclusive of all possible data available for your analysis. Along with your own search on the web, your university’s social sciences librarian should be able to provide you with numerous data sources. A final source for finding data is the literature. Carefully read empirically-based, published research articles to see from where the data came and consider the possibility that the data might be able to answer the questions you are posing or provide evidence not yet analyzed by anyone. In all cases of data collection, the great effort that goes into collecting data usually yields a gold mine of under utilized evidence that is just waiting for a graduate student to dig up and make use of in creative ways!


**International Labor Office**

http://laborsta.ilo.org/cgi-bin/brokerv8.exe?_debug=2&_service=appsrv1v8&_program=pgm.applpgm.start.scl

**United Nations**


**World Bank**


**Some Recent Surveys Available Online**

Demographic and Health Surveys: http://www.measuredhs.com/

World Fertility Surveys: http://opr.princeton.edu/archive/wfs/

Family Life Surveys from Indonesia, Malaysia, Bangladesh, and Guatemala: http://www.rand.org/labor/FLS/
Mexican Migration Project: http://www.pop.upenn.edu/mexmig/

Nang Rong Surveys: http://www.cpc.unc.edu/projects/nangrong/


African Ideational Diffusion Project: http://lexis.pop.upenn.edu/networks/
10) ESSENTIALS


*Fieldwork in Developing Countries*, edited by J. Hoddinot and S. Devereux (Harvester Wheatsheaf, UK and Lynne Reinner, USA), 1992.


Tony Larry Whitehead and Mary Ellen Conaway, eds. *Self, Sex and Gender in Cross-Cultural Fieldwork*, University of Illinois Press (1986).

Recommendations by:

Bradford Barham (BB)
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Anthropology, Brown University

David William Cohen (DWC)
History/Anthropology, University of Michigan

David Collier (DC)
Political Science, UC Berkeley

Sara Curran (SC)
Sociology, Princeton University

Albert Fishlow (AF),
Council on Foreign Relations, New York

Barbara Geddes (BG)
Political Science, UCLA

Bryna Goodman (BrG)
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Gail Henderson (GH)
Sociology, University of North Carolina School of Medicine

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Demography, Brown University

John Knodel (JK)
Sociology, University of Michigan

Larissa Adler Lomnitz
Anthropology, Universidad Nacional Autonoma de Mexico

Andrew Orta (AO)
Anthropology, University of Illinois

Alberto Palloni (AP)
Demography, University of Wisconsin

Joseph Potter (JP)
Demography, University of Texas

Lakshmi K. Raut (LR)
Economics, University of Hawaii

Michele Shedlin (MS)
Sociomedical sciences, Sociomedical Resource Associates

Thomas Spear (TS)
History, University of Wisconsin

Robert Vitalis (RV)
Political Science, Clark University

Luise White (LW)
History, University of Florida