# In Search of the Holy Grail: Projects, Proposals and Research Design, But Mostly about Why Writing a Dissertation Proposal is So Difficult

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*[[T]here is too little emphasis [in the US academy] ... on what it means to do independent research.*

-William Bowen and Neil Rudenstein

In Pursuit of the Ph.D. 1992

## Introduction

The art of writing a research proposal is a curious, even paradoxical exercise. It is indisputably foundational for graduate student training and professional formation: it is arguably one of the most difficult and exacting tasks confronting students. And yet it is one of the great curiosities of academia that proposal writing, and research design and conduct more generally, is so weakly institutionalized within the university and within the life world of graduate social science. This is precisely what Bowen and Rudenstein (1992) gesture to in their rather extraordinary, but nevertheless sadly accurate claim, that universities lay too little emphasis on what it means to do "independent research", what in other words, we would all take to be the bedrock of the academic enterprise. The same claim might be made of fieldwork, so often a constituent part of the research design upon which any good proposal rests. Whether "the field" is a village in northern Uganda, a barrio in Los Angeles, a legal firm in Paris, or an archive in Pittsburgh, the actual conduct of independent research in the field is an exemplary case of what Bruno Latour (1987) calls "black boxing". Fieldwork, which typically has a central place in the social scientific study of the developing world, has the aura (and anxiety) of any rite of passage. But with a difference. It is a learning-by-doing ordeal for which there is presumed to be no body of preparatory training (i.e. coursework), for which the measure of success is Darwinian in nature. Those who succeed return; those who don't presumably were failures, bound for academic extinction.

It is perhaps for all of these reasons that Bowen and Rudenstein (1992), in their important book, In Pursuit of the Ph.D, characterize the period between the end of graduate coursework

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[1] A version of this essay was delivered at the IPFP Annual Fellows Conference in October 2002. I am grateful to the participants, to Ellen Perecman in particular, and to the army of IPFP fellows who have struggled with the sorts of questions I try to raise here. More critically, much of what I have to say here has emerged from fifteen years of running Dissertation Workshops at the University of California, Berkeley, a system focused on proposal writing that emerged in the careful hands of David Szanton while at the SSRC (see http//:globetrotter.berkeley.edu/DissProp/).

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 encouraged to engage with their dissertation projects in the first and second years of study at the graduate level. In practice, they rarely are so encouraged in the social sciences or in the humanities, and the transition from course work to dissertation project, from writing nifty critical literature reviews on organization theory or post-structuralism to "the research question" – another rite of passage – is often paralyzing. There is no obvious road map to facilitate this transition or the discovery of a robust, credible, interesting and original project. And the fact that students have been swimming along merrily in a sea of high theory – hegemony practices, disciplinary discourses, or transaction costs – does not necessarily help much either. ("How exactly am I going to 'operationalize' my Foucauldian study of the micro-physics of political power in San Francisco's credit unions"?) The discovery and articulation of a compelling research project and robust research design is so often a source of radical bewilderment and anxiety. It is after all about making fundamental choices: Brazilian social movements and not schooling in Oregon; an engagement with neo-Weberian theory rather than rational choice and so on. And not least, it necessitates decisions that engage the intellectual with the personal: can I take my companion and child to Tierra del Fuego for a year and a half? Can I really hack-it in Calgary for two winters? All of this talk of emotion and reason may sound terribly West Coast-ish in tenor but it is always worth recalling the old adage that in its most demanding form, research and writing require 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 the research enterprise, and yet it is surprising how many classic monographs cover their tracks and obfuscate the mistakes, errors and panic of how the book emerged, ignoring the lived realities of working in the 'field.' It is interesting to reflect on why the research proposal or research design has become a sort of "public secret" on campuses and indeed why, in my view, it has all too often not been an object of serious scrutiny in the last few decades. Perhaps the post-structural skepticism 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.

I want to introduce a number of issues pertaining to research design and proposal writing, and to lay out in broad terms the kinds of concerns and knotty problems that enter into the long and complicated process of framing, designing, conducting, and obtaining funding for a researchable project.

## The Funding Regime

Before I turn to the nuts and bolts of the proposal writing process, let me say a few words about the political economy of funding and proposal writing. Some readers may wish to consult some interesting work by the anthropologist Don Brenneis who has conducted ethnographic work on the proposal review process (see Brenneis 1994), posing the question: what actually transpires in the course of peer group assessment? What sort of normalization transpires as multidisciplinary or theoretically contrary academics "review" a proposal? What indeed is the canon that defines the "good proposal"? Students depend on a variety of funding sources: federal agencies, such as the National Science Foundation and the Fulbright Program of the Institute of International Education; private foundations, such as the Ford Foundation, the Carnegie Foundation, and the MacArthur Foundation; small donors such as the American Association of University Women (AAUA), non-governmental organizations, such as the Aspen Institute and the Social Science Research Council (SSRC). These organizations have quite different interests, forms

of governance and review, and may require markedly different sorts of proposals (a 2 - page precis versus an elaborate 20 - page research proposal). Such variability in the interests and foci of funding programs speak directly to the need to consider and reflect upon the institution (the funder), the program (the substance) and the process (review and evaluation). Let me begin with several self-evident, but nonetheless important, starting points for thinking about the relation between the intellectual project and the mundane need to bankroll it:

- ☐ Identify the panoply of organizations that might consider funding your project; the Foundations Register and university research offices are obvious places to begin.

- ☐ Be creative and flexible in reading the rubric of a funder and of the specific grant program in question, and consider the ways in which you might package your interests (take note: not compromise) in order to be eligible for funding through a particular program. A funder may have a program on "peace and security" or "technological innovation" that precisely rest on the desire to think expansively and in cross-disciplinary ways about such issues and may readily encompass what you do.

- ☐ Excavate background information on the funding agency to learn, for example, about the sorts of projects they have funded in the past, about members of the selection committee, and whether they have privileged or encouraged certain sorts of approaches or problems.

- ☐ Take careful note of the deadlines and requirements of each application to give yourself time to prepare a proposal; *two weeks or even two months will not do*. The process is long, iterative and time consuming. Any proposal that leaves the author's desk without having been read by a large number of people and subjected to rigorous critical feedback is, almost by definition, vulnerable.

- ☐ Recall that all research competitions are competitions! Getting support is competitive and is becoming more so. The consequences are rather obvious. You have to submit a proposal that represents your best effort; you cannot submit a wildly disorganized and incomplete project proposal just because the deadline comes around, in the hope that something is better than nothing. Experts in your field will be judging what you write.

- ☐ Your project will be (for better or worse) assessed relative to others. Research monies are tight, and competition is intense. A reviewer/screener might be reading thirty proposals from which he/she has to eliminate twenty. To stand a chance, your proposal must not simply be solid; it must be a stand-out. It must, in other words, excite the reader. There are several ways in which a proposal can achieve this distinctiveness. One is to have three typos in the first line. Another is to bury the hypothesis in the trivial details of a footnote on page 8. I would not recommend either. Your proposal must 'grab' the reader: a tight, compelling, well-written and clever opening paragraph does wonders[2] A meandering "fishing expedition" or a poorly articulated alleged association between modernity and protest will ensure that your proposal is heading for the wastebasket. Crude and harsh, perhaps, but these are the conditions under which your project is reviewed and, at the very least, they demand some serious reflection.

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[2] I speak from the bitter experience of having read at least 100 proposals a year throughout the 1980's and 1990's for the Social Science Research Council (SSRC), National Science Foundation (NSF), and other funders.

- ☐ You only have one chance for success, as most, but not all, programs have one deadline per year. Again, this speaks to allowing yourself time to think, write, and plan for the deadline. You simply cannot begin too early. I would suggest the first day of graduate school…..

**Primary Objectives and Parameters**

For the purposes of this essay, I am making a number of assumptions about the construction of a research proposal as an exemplar of research design. I do this because I am assuming that most readers are in the process of drafting a research proposal or are thinking about dissertation projects and that they intend to conduct something like fieldwork, namely, a process in which they have to generate evidence. There is, in my view, something like a canon of what a good proposal looks like and what are the properties and qualities that reviewers of proposals, or dissertation committee members for that matter, look for and privilege. I propose to walk through the process of designing a research proposal as a way of flagging issues that we all need to think about because the process is so difficult, so demanding and so drawn out. In passing I shall draw upon my own experiences conducting research in West Africa (Nigeria/Senegambia), in South India (Kerala) and in California (the Sacramento Valley), which like most everyone else's, reflects a combination of systematic, contingent, accidental and occasionally ridiculous human practices. My interests have focused, in particular, on peasants, rural transformation, social movements, and a variety of agrarian issues including household dynamics and gender questions. These interests shape the sorts of examples I provide here, but the principles I seek to emphasize are as valid in the humanities and social science professions (for example Law and Public Health).

Let me immediately say that this essay will not be a primer on the problems of research design. 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 (other chapters in this volume explicitly address such questions). Neither should this essay be construed as a treatise challenging or even questioning the theoretical or disciplinary approaches one might adopt as an economist, anthropologist or historian. Of course, I have my own biases and, in the interests of full disclosure, I will try and make them explicit now. First, I take seriously the notion that one should consider a variety of methodological approaches to a research problem and look into multiple methods (an exemplary case would be Paul Lubeck's book Islam and Labor, 1987). Second, I will emphasize some 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 one needs to be as clear, as self conscious, and as explicit as possible in explaining HOW one will conduct a project (i.e., you've arrived in rural Idaho with your US made pick-up truck and gun rack to study the militias. Now what are you going to do?). A good research design makes research life in the barrios of Los Angeles or Bogota much easier. Given all of the unknowns associated with doing fieldwork and collecting data, a research proposal is a sort of security blanket. By definition, a proposal pushes you to construct something more deliberate than a fishing expedition, i.e., "I'll go and poke around and see what is there". A well thought out research proposal provides you with an identifiable problem, a tentative hypothesis, claim or proposition, a road map of necessary evidences, and at least some ideas about how and where that evidence can be located and/or 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– the state most people are in when they start thinking about a dissertation topic. We can all do with less of this. 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 or proposition at all.

Let me briefly define a research proposal as a text that links- in a more or less formal way- theory, method and evidence (Burawoy et al 1991). A more elaborate definition would be that a research proposal presents a question or problem theorized in such a way that it generates a claim or argument (a hypothesis, if you wish) attached to which are 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 or linked –for example through a comparative study of three country cases using a large-n sample–represents what I would call the research design. As I have already implied, different funders impose different requirements and organizational templates, and the institutional culture of disciplines may vary with regard to how formal (let's say how amenable evidence should be to statistical interpretation) such proposals should be. In some disciplines, the language of hypothesis testing may seem remote 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, and that some methods may not be appropriate for some questions or the genesis of certain sorts of information. Researchers are in the business of writing narratives of differing sorts. Some sustain arguments and propositions; others provide different explanations and understandings of social life.

Put in this way, independent research seems straightforward and, perhaps, pedestrian. But, of course, it isn't. It's the most difficult thing you will do, even more difficult than writing the dissertation. There are very good reasons why Bowen and Rudenstein (1992) emphasize 'anxiety' and 'paralysis' in their account of the genesis of a research project. The process is loosely institutionalized; it is often compounded by bad advising and poor training; and it is certainly made no easier by the profound arbitrariness of arriving at a topic. How can something predicated on logic and reason so frequently be so contingent or accidental? Why on earth did I choose beer-brewing co-operatives in Burundi and not national dental organizations in Des Moines? Is beer or teeth more or less likely to get me a job? There really is no avoiding this reality. 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 fact that students come to it relatively late in their graduate careers. Because of highly structured coursework and the impending nightmare of qualifying exams, when a student steps into the field, the planning horizon for dissertation research is typically the immediate and the short term rather than three years down the way. Graduate training can sometimes appear like permanent crisis management (perhaps not unlike Trotsky's account of capitalism). But it is, in fact, impossible to start thinking about a research project too early, for several reasons. First of all, the identification of a place and problem–e.g., household dynamics and commercialization in northern Kenya–carries with it enormous implications as regards the skill-set required to carry out the research: fluency in the local language, an area studies background, and training in large scale social survey design, to say nothing of training in theory, which is the bread and butter of graduate formation. Starting as early as you can is key to building into graduate formation a series of integrated needs for conducting a project. (It's rather difficult to pick up Chinese language late in the game.) Second, the process of writing a research proposal is deeply and profoundly recursive. The proposal can change radically between the sixth and the tenth drafts, which take

into account feedback from your committee, friends and peers. To expect this process to take anything less than six months is myopic. Proposals undergo radical change from the first to the tenth draft; if you look at the website I referred to previously (http//:globetrotter.berkeley.edu/DissProp/) you will see examples of precisely what is entailed in this recursive process of framing, reframing, refining, sharpening, and so on. Third, the practical "start-up" demands of a project are time consuming, particularly when the project is to be conducted in a non-native environment. 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, there is 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 requires time and flexibility.

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

- ☐ Spanish language training, and perhaps training in a local vernacular as well, should the Ecuadorian NGOs be representative of indigenous peoples
- ☐ Theoretical work on transnational organizations and transnational networking
- ☐ Methodological training in interviewing, ethnography 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

What makes for a good and compelling research proposal? A 10-15 page research prospectus that includes sections on theory, method, design, and plan of work will typically be required by the funding organization, and this will provide the template for my discussion below. There is no one way, one narrative structure, or one 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 respond to, namely:

- 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. This implies two things. The reader must be able to understand the design of your project and why you have chosen this particular approach to your problem or question. 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 since 1985, then it must be clear how you are going to measure differentiation (what criteria, how many people), what means you will use to collect data appropriate to the measures you have chosen, and how you plan to 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 validity of your results. The implications of transparency are that a reader must be able, without effort, to be clear on reading your proposal how you link a theorized problem with a claim, with evidence, and with method.

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" a reader through a proposal in such a way that he/she fully and easily grasps the internal logic or architecture of the study. The demand for clarity does not imply a linguistic or expressive dilution (a dumbing down), but rather highlights the dangers of obfuscation (what exactly is this proposal suggesting?), ambiguity (exactly how many people is she interviewing?) and a lack of sufficient information (what precisely is the author proposing to do in the name of ethnographic fieldwork or "hanging out" in the village?). The clarity question can quite easily be tested by asking two or three people read your proposal and explain to you, in a few sentences, the central problem and central claim. Let me assure you, this is always a rather humbling experience.

Methodological precision asserts the importance of focusing on the "how" question. It is typically the part of the proposal that addresses the knotty problems of evidence and its genesis that funders scrutinize with particular care, yet that students fudge or gloss over: (How large must the sample be? How will the sample be selected especially in view of the total absence of any reliable survey data? Is representativeness an issue? How can one confidently assume that data on peasant credit relations 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 which generate evidentiary demands *that cannot be met* (i.e., they might presuppose that we have access to the internal records of large transnational oil companies, or require taking up arms and fighting for a liberation movement in a small African country). 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.

The demand that the proposal meet certain expectations derived from theory is perhaps counter-intuitive and somewhat controversial. It reflects the idea that the ways in which you couch your problem – the theoretical tradition in which you have chosen to operate – provides something more than a "context" for the research to which you gesture in a "literature review". It is theoretical precisely because it leads us to expect certain outcomes or generate specific hypotheses. One can argue over the extent to which theory is predictive or overdetermines the research process. But theory must be useful. As Gilles Deleuze put it, 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, what she expects to find. 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 that your proposition is plausible and worth exploring. A compelling proposal cannot stop at the point where you pose a question: you must have something to say about it (a claim, an argument, a hypothesis) and this is what theory does for you (see Pryke, Rose and Whatmore 2003).

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 complete the analysis in several months. Typically driven by a sort of data insecurity, most proposals vastly overestimate both the quantity of evidence that they need and their capacity to generate or collect it. That's why one of the most common responses to the first draft of a research proposal is: "interesting but this is a lifetime's work!" I remember vividly one of the first proposals I read in 1980 for the Dissertation Fellowship Competition of the SSRC Africa Committee. It was a project on peasant differentiation in Kenya with a sample of 4000 households that the applicant hoped to interview in 3 months. Another applicant hoped to explore the problem of runaway children in Nigeria using police records. Comparisons often confer great explanatory power, but the costs in terms of time, skills and practicality are accordingly enhanced. The do-ability question is both practical and epistemological in nature.

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 the findings are valid?

All of these questions are in some measure shaped by field and by discipline. For example, validity may be rather different for a rational choice analysis of collective action than for an ethnographic analysis of a social movement. But you must always keep them in mind because they represent an important set of criteria by which your project will be assessed and evaluated. It's always a useful exercise to put yourself in the position of someone reading and evaluating a proposal. For this reason, a Dissertation Proposal Workshop on the SSRC model, which turns everyone into a "reviewer", can be a powerful learning experience.

At this point let me say a word about the construction of a proposal in relation to the reader -- or more precisely, reviewers, screeners, and selection committees-- and in relation to 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. Nevertheless, there are a number of narrative devices, "tricks of the trade", and obvious "dos and don'ts" that bear reiteration.

- ☐ 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 what this project is about in a few sharp, snappy sentences.

- ☐ 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 for showiness, fashion, or superficial cleverness. Nonetheless, one way of giving a project some panache is to construct your study around a puzzle, a paradox or a conundrum. Consider the following illustration. The rise of political Islam has been associated with a particular social basis to recruitment and a rejection of certain liberal ideals; case X in Jordan, however, stands as a striking contrast. Why? My theory would lead you to expect that people would vote in one way, but in practice did the opposite. Why does Y movement in Nigeria that attacks ethnic politics as a stain on the Federation have ethnic identification as its basis for political mobilization? Paradoxes, exceptions, crises, and comparisons are powerful ways of making a scholarly statement.

- ☐ Never bury ignorance or sensitivity: Even the best plans and early proposal writing can come up short, and the best plans are confounded by unexpected crises and risks. A student preparing to conduct fieldwork in Chiapas in 1994 in lower Manhattan in mid September 2001 obviously had to confront unexpected political and practical difficulties. The point is that there will always be absences and deficiencies in one's training and knotty practical and ethical difficulties to be confronted. Never bury these 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 your life or the lives of others. If you are collecting large-n data of a social survey sort but have no training in survey design, explain how you intend to acquire these skills. You might consider a summer intensive course at the University of Michigan's Institute for Survey Research, in which case you should say so and include it in the budget! A reviewer of a proposal will fully understand that one cannot know everything in advance and that all questions cannot be answered at the time of writing; but this is no excuse for sloppiness or inexactitude. What the reviewer wants is evidence that at the very least you have thought about these things and have something sensible to say about them.

- ☐ Security in ambition: conducting a project is always anxiety provoking, given the 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, e.g., adding questions, expanding the theme (do I have enough?), and adding more data (a sample of 800 not 80). Insecurity breeds ambition. But this can work against 'do-ability'. One of the most common refrains of the dissertation advisor or the screener is: "it's just too big".

- ☐ Self-promotion: never be reticent about making clear why you are the person to do this project. You are fluent in the necessary language(s), have work experience and personal connections in the region, and have made several pre-dissertation trips. Explaining why you are the person to do the project is imperative.

- ☐ 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 judgment on these issues ("I'll figure it out when I get there"). There are good reasons, perhaps, for improvisation in fieldwork. Sometimes things don't work out or local contingencies shape outcomes and choices. But such logic can breed either complacency or a 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. Convince a reader that you have thought about these questions in the context of not knowing all the relevant details and that you have plausible and credible answers.

- ☐ Shopping: A research design cannot be a 'look-see' or a shopping expedition. A huge shopping-list of generally unstructured questions does not make a proposal.

- ☐ You can rarely 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-waving.

## 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). I have taken this language and the discussion that follows from an, alas, unpublished book project on Social Science Epistemology (1985) by Professors Paul Lubeck and Bob Alford of the University of California, Santa Cruz who kindly sharing their work with me. A similar approach is outlined in Andrew Sayer, Method in Social Science (1980). Often we start with 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. Just how and why we get to these entry points 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 entry points are all important, but they do not address critical middle level questions and specifics: what are the local 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 to identify this or that variable?

Entry points then usually take the form of a particular sort of question or query, and their goal is to identify the "right" research question. Often this process is treated as one of individual choice or as a curious process of osmosis in which the field of knowledge is transmitted to the researcher or emerges inexorably "from the data". In practice there is of course a complex tacking back and forth between theory, question and data. One cannot overemphasize 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 key ways: to mastering literatures, to identifying with a theory, and to plowing through sources of data. 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 stage 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). Each entry point typically generates a different sort of question and may provide the groundwork for the elaboration of a research program. Practical questions might emerge from a student's experience working in a non-profit, the Peace Corps or a government agency. How can an Indian NGO better deliver family planning advice to south Indian women in deeply patriarchal male dominated households? How might organic grape growers in Napa Valley improve their market share? Why the hell do the farmers who are at the tail end of the irrigation system never get water? One's practical concerns must be located with respect to a theoretical framework and within a logic of inquiry if they are to qualify as action-research. By action research, I mean 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 Catholics more involved than Muslims in the genocidal activities in Rwanda in 1994?"), and some are historical ("How did the discourse of the 1946 rebellion in X differ from the rebellion in 1978?"). And finally some questions are theoretical: "does bureaucratic authoritarianism reduce the legitimacy of rule?" "Under what historical conditions does social integration increase?" "How do members of militant movements construct beliefs about the meaning of life which justify suicidal acts?"

The question then becomes how does one push this question forward, develop and refine it, and convert a hunch into a research proposal. There are several immediate 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 to identify the sort of evidence that is appropriate to the questions and the means by which valid evidence can be collected. A third is to try to understand how a particular approach to linking evidence and theory is shaped by practical considerations: limited time, energy and resources. In quality research institutions much time is rightly spent on teaching students a road map of theory appropriate to the discipline and to the selection of concepts 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 and it is often weak because it is under-specified–e.g., "I shall engage in participant observation". But it may also be weak because the connections – the rules – by which evidence is linked to theory or theorized claims are often opaque.

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 different

actors. One might categorize these different sorts of questions, which, incidentally, could be approached in Marxist, Foucauldian or Weberian terms, into one of three logics of inquiry: phenomenological, historical and causal. Such logics provide ways of linking theory and evidence. They do not help you answer your questions, but rather highlight the choices that have to be made given the fact that working with one set of tools limits what can be pursued as well as the manner in which it can be pursued. Logics of inquiry offer the opportunity to formulate and reformulate a question within different approaches and to see the choices available to you regardless of the content of the question.

Let me examine each of these logics, drawing on Lubeck and Alford and Sayer as a way of showing how the logics of the different rules linking theory and evidence confer different 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 neo-classical), it must be transformed into "variables". This approach is frequently grounded in, and typically draws strength from, positivism. The model for this logic is the natural (or experimental) sciences, which assume that the world is knowable, real, and divisible into autonomous parts; that observations can be replicated; and that bias can be controlled. The most important variables cannot be manipulated by the investigator. Rather, she must assume that classification into subgroups substitutes for experimental manipulation and 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. A survey is appropriate when some degree of independence of the independent variables can be defined. [See Park's essay in this volume.] Objectivity requires the careful specification of variables 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 Emile Durkheim's (1997) 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 methods, and secondarily (though often importantly) on the interpretation of key texts. Within this logic there is a sort of causal connection between categories in the actor's mind and his actions; between the roles being played and the rules of the game. But, as Lubeck and Alford note, 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 typically be qualitative data. Participant observation is the method that links phenomenology to interpretive theory and to qualitative field notes as the form of evidence (see Burawoy 1991 and Burawoy et al 2000). 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 in the discursive political struggles of a particular expression of political Islam are problematic sources of conflict and differing interpretations? -- are associated with cultural theory, and the humanities, 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 Michael Gilsenan's Recognizing Islam (1983) or in a different register Clifford Geertz's account of thick description in Interpretation of Cultures (2000).

*Historical-Dialectical*: This approach is 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, but not wholly, 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. According to Lubeck and Alford (1985), 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. Blok's (1974) account of the genesis of the Mafia in nineteenth century Sicily is a very powerful case in point. The sequences of events are contingent outcomes that cannot be attributed to separable causes; it is, in other words, 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 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 Karl Marx's The 18th Brumaire of Louis Bonaparte(1960)

The descriptions of these logics that I have provided above are abbreviated and stylized, 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 a 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. They 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 involve 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. 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 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 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 the sorts of choices that are available once a question has been formulated.

Once you have made your choices, e.g., a Marxian analysis of the culture of work in south Indian textile factories or an institutionalist analysis of the ejido reforms in Mexico, you can begin to seriously explore the sorts of evidences you need and the knotty questions of validity and reliability and so on. This is not the place to work through these complex issues. But I would in passing take note of a number of issues that are typically lost sight of in many projects falling within the international studies arena.

- ☐ 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 Malian staple food output can be, and often are, enormous. Clearly, 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 historical sources deployed by the social sciences, such as colonial archives, missionary archives, and business archives has also become an almost standard part of field research in many parts of the world. But 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! Simply because you are interested in prostitution in colonial Nairobi or communal violence in colonial South India, does not mean that the archive is laid out in a fashion that will expedite the discovery (a colonial file entitled 'Prostitution' waiting for your arrival), or indeed the interpretation, of the information you need. To invoke the archive as a source of evidence is simply a beginning, not an end. As Luise White discovered in her book on prostitution in Kenya – The Comforts of Home (1990) - one needs in some way to understand the social and epistemological organization of the archive– to somehow endeavor to get inside the "colonial mind"–in order to figure out where certain sources of information might be located.

- ☐ The Assistant: many dissertation projects have quite limited budgets, but the use of assistants in conducting surveys or as translators is ubiquitous. Much has been made in Anthropology of the deployment of the assistant (or relatedly as the "key informant" who may be in effect an assistant). I simply want to raise here the practical dimensions of using enumerators and assistants, the dull details of employment, and the hermeneutic complexities of a sort of intellectual intermediation, of obtaining information twice removed. What kind of person does one recruit as an assistant? From where? With what background? With what local understanding and connection? How are they to be trained? And what is their contractual or

other relation to you, the Principal Investigator? What salaries and benefits should they receive? Whether all of this needs to be documented in a research proposal is an open question. But to simply indicate in a methods section that you will make use of 'interviewers' can only raise flags if these issues are not addressed.

- ☐ The Survey: 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 the PI has the training to undertake such a project. 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 suffice! Finally, some sections of the social sciences and the humanities shy away from using survey data ("I do not collect that sort of data", "I prefer ethnography" and so on). Given the value of multiple methods in research design, I would encourage the use of surveys in research even if it is not a central data collection device because it 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 and synergistic effects and insights derived from the collection of data in 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 for which the need for such data was not anticipated. Being prepared for such eventualities then has a particular payoff.

## Violations, Pathologies and Eyes on the Prize

It is precisely because these principles of proposal writing are so intractable and demanding that one can better understand certain 'pathologies' or violations that attend the crafting of a research proposal: the flight into high theory (to avoid the demands of 'operationalization') or the descent into data and empiricism (to circumvent the demands of theorizing a problem). All of this in turn feeds the necessary and inevitable panic and self-doubt associated with a first cut at thinking about a dissertation project. One would have thought that opportunities–formal or informal–to share these anxieties and to benefit from collective experiences of others would have been institutionalized in some way in every department. And yet, they rarely are. It all seems to be *ad hoc* and word of mouth. On the Berkeley campus, for example, it is almost impossible to find a course on fieldwork, ethnography, or writing a proposal. The moral of the story is clear: create such opportunities, seminars and courses in your own program! Organize! Organize!

Let me turn to one final 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. Second, 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 constitute the necessary and inevitable risks and uncertainties of doing research. They drive home the point that no matter how theoretically brilliant and methodologically sound the proposal is, it may -- and often does -- confront a real world and lived experience, including your abilities to do what you think you can do. When it does, it will demand 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 us can be prepared for such eventualities. But even the best-laid research plans cannot -- and should never -- 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, and the ups and downs of data collection, as well as the need 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 forest 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 and of research in action takes me 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 field notes, surveys, and interviews; and, not least, of writing the dissertation (Zerubavel 1999). Here is not the time or place to reflect upon how we organize our field notes, how to prepare for our return to the University after a spell in Africa or France, or how to begin the arduous process of writing. But they are all part and parcel of this complex thing called "doing research". Writing a research proposal is, of course, a foundational moment in this process, and this essay is by no means intended to generate panic or massive depression. But I would be the first to acknowledge that intellectual and academic work must necessarily engage with the emotions. It is perhaps inevitable that making explicit the silences and absences in graduate training programs, and in our intellectual formation –of actually talking about and taking seriously the business of doing independent research–raises the bar and, by definition, makes clear the challenges that we all face.

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