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
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Introduction

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

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

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 is 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 ale 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".

- *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

[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 it self 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!!