small a studies that apply large n tools. Yanow helpfully points out that the essence of interpretive methods is ontological constructivism and epistemological interpretivism, and not necessarily small n (e.g., 1994)." See Helper (2004) for an overview of the use of what he refers to as "qualitative" methods in economics. See also Theis-Moor, Friedman, and Sneath (2003) for an example of M&M social science supplemented by interpretative strategies for establishing the meaningfulness of the categories of social experience represented by mathematical constructs. The former is the case with theory by Sneath (2003). See also Plume (1979) for an interesting anecdotal account.


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Introduction
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Field research remains common in comparative politics and other branches of political science. While fieldwork is more appropriate for some projects than others, leaving home base and striking out into unfamiliar surroundings is an essential way in which researchers gather new data, acquire fresh perspectives, subject seminar-room musings to cruel but necessary reality checks, and establish credibility. Yet for all our discipline’s attention to methods and research design, it does relatively little to train graduate students (or the practical and conceptual challenges of managing a field research project.

For the past three years, Melanie Carmett, Lauren Morris MacLean, and I co-taught a APSA short course entitled “Strategic Field Work for Field Researchers in Comparative Politics and International Relations.” A common denominator in our own research projects has been working in settings (the Middle East, sub-Saharan Africa, and China, respectively) and on topics (political and economic work, local organizations, norms, and political perceptions) that require particular care, sensitivity, and judgment in the process of gathering data. In such circumstances particularly, neither what is being studied nor the relationship between researcher and subject is always straightforward, transparent, and easily established. Up-close and direct interaction in the ethnographic tradition – trying to get as near as possible to the people from and about whom you are learning – is the promise of particularly high validity. But what exactly does it take to achieve this result? How much time is it necessary to spend at one’s site or sites? How should a researcher present herself, what kinds of relationships should she build with her subjects? Is there any sense in which we should “partner with” or “give back” to the informants and communities that we study?

Social scientists have been grappling with these problems and questions for generations, of course. And to some extent everyone who does fieldwork comes up with his or her own unique solutions, through trial and error, judgment, intuition, and serendipity. Yet there are many general issues and approaches to be discussed and assessed. Some can be posed as trade-offs, and each of the three pieces below explores consequences of a particular face. My essay considers two prominent but also contrasting examples of field research, one based on much “thick” ethnography at a single site, the other on less deeply embedded participant-observation at multiple sites. It calls for a collective effort in the discipline that would enhance compuls and promote both modes of research within a shared framework of “site-intensive methods.” Morris MacLean’s piece challenges political scientists to follow the lead of other disciplines in thinking about our subjects’ interests in ways that go beyond merely filling out IRB forms, by adopting more collaborative models of research. Carmett’s article focuses on research in deeply divided political settings, where gaining challenges to data validity can be acute. Many of her observations also apply to research on sensitive questions in less complex settings (though they certainly also apply to conducting research in politically repressive places).

Note
1. This continued a course first developed by Marc Morjé Howard,
Site-Intensive Methods: Fenn and Scott in Search of a Coalition

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The methods that generally go under the labels ethnography and participant-observation occupy a somewhat awkward place in political science (Bayart de Volotra and Scott 2004). Our discipline lays claim to prominent, if rather isolated, examples of scholarly work based on these methods—with perhaps the two most widely read being Fenn's The Body Politic (1975) and Scott's Weapons of the Weak (1985). A subset of empirical researchers has always been drawn to them, going back at least as far as the immediate post-WWII generation and presumably earlier, although they are discussed on the occasional conference panel. They are actively employed in much exciting research today, by themselves or in conjunction with other methods (examples include Adhe and Parmentier 2004, Bayart and Dagne 2005 and 2007, Chen 2006, Galvan 2004, Morris, MacLean 2004, Roitman 2004, Schwartz 2004, Strauss 2006, and Tsai 2007).

And yet, these methods remain marginal. It is fairly safe to say that they have not significantly caught on in mainstream political science curricula. Even within the walls of the APSA organized section on qualitative methods and the stimulating ferment that it has fostered in the “third” timeframe, they have shown up so far as distant cousins. Very little in this vein appears on the 2006 Institute for Qualitative Research Methods (IQRM) syllabus, for example.1 Two of the most important recent books on qualitative methods are only marginally useful to ethnographers and participant-observers, also seem to have been written without these approaches particularly in mind (George and Bennett 2004, Brady and Collier 2004).

What are the reasons for this marginality? Let me first mention a few obvious ones. Acquiring the skills needed to use these methods and then applying them is time consuming and costly, especially when research is conducted in foreign languages, periods, and regions. It is also recommended to good conscience the average graduate student who cares about minimizing time to completion. And, of course, other methods skills are much more in demand within the discipline as a whole.

But clearly there are other reasons, as well. Practitioners are split by apparent differences in epistemology, such as a perceived schism between the “positivistic” and “interpretivist,” and sometimes even seem to highlight these divides rather than bridge them (Yano 2003, Scott 2006). There is also a reluctance on the part of many to go beyond describing what they did in their own research and prescribe sets of general procedures for others to follow. Efforts within political science to spell out explicitly (and promote) the benefits of these methods for the building and testing of theories have thus far been limited, and perhaps most importantly, there has been no real push to build a coalition behind the critical appreciation, application, and teaching of these methods.

Does marginality matter? Some people might not think so, but it seems clear that real costs to practitioners and to the discipline as a whole. Researchers of our work lack a clear framework within which to understand, evaluate, and criticize it. It is perceived as alien, something that deviates in some obvious and often characteristic ways from mainstream political science, but is not suitable for our own. In the minds of many of our peers, it tends to be equated with a "quibbling," barefoot empiricism that blunders about hap hazardly collecting anecdotes. Students not only aren't trained in it, but don't even get a sense of what it is, or that it's available to them as a legitimate way to pursue an answer to questions they wish to ask. Finally, all this means that there are two basic, if somewhat interrelated, problems—their general ignorance of the field is neither the case, and consequently our collective understanding of politics is much the poorer.

I believe an appropriate step forward would be for researchers who use these methods—either exclusively or in combination with other methods—to work toward building a coalition of researchers or groups within the discipline and within the broader qualitative methods section. This enterprise would probably not be achieved by a pre-determined set of approaches to the gathering of sources, evidence, and data. This would, one hopes, cross-cut and set aside underlying epistemological divides.

It often feels to me to propose new terminology, but it strikes me that an umbrella term to bring together a set of related methods might be useful. One such term would be "site-intensive methods" (SIM), referring to the collection of evidence from human subjects within their own contexts, their interaction with which informs the study just as the researcher's own observations do. This implies the need for a deeper engagement with a site, context, locality, or set of institutions than is obtained in, for instance, telephone surveys or some types of one-time interviews—though other kinds of surveys and interviews in fact require considerable stage-setting and trust-building. This term would subsume most of what is referred to as "ethnography" and "participant observation," and perhaps some forms of other practices, like focus groups. It would also highlight the diverse forms that this kind of research can take—hence a high degree of depth in a single locale, as well as those that also aim for breadth as well, and projects in which SIM is the main course, so to speak, as well as others where it is more of a side dish.

I do not suggest some vague "SIM" in particular, for example, because I'm not convinced that other categories are up to the task of bringing together the most useful and productive group of researchers within political science. "Ethnography," "participant observation," and "participant-observations" is a holistic approach to the social sciences and a voluminous methodological literature, especially in anthropology. But it may have drawbacks as well. Some versions of the anthropological method may set the bar too high in implying that months or years of immersion are required in order to obtain "true" participant-observes. It may imply a holistic orientation according to which the entirety of a community or locale must be comprehended in order to make sense of any one part. It should be pointed out that the meaning and practice of participant-observation is today undergoing evolution and stark debate; by no means do all practitioners see ethnography as limited to these forms. Nonetheless, the term seems to me problematic as applied to projects employing shorter stints of fieldwork and less encompassing modes of information gathering.

"Participant observation" would also seem to be a possible umbrella term. Yet here, too, the boundaries implied are unclear and terminus are with what it is that we want to bring together. On the one hand, the researcher "participates" in other kinds of research, such as straight-up interviews. Conversely, to some it may imply only that through a long-term process in which the researcher becomes a part of community life can full or meaningful participation be achieved. Some may feel that this covers only a subset of the practices comprised by ethnography, while others see the two concepts as interchangeable.

Finally, there are those who favor the term "narrative methods" (Laatin 2002, 2003), but I see this as both too broad and too narrow for present purposes. As Laatin uses the term, it includes almost any kind of case studies, interviews, and other qualitative work. Moreover, even if unintentionally, it seems to reinforce the belittling notion that this type of research is "just a collection of stories." I would drop or even to deeper forms of thick knowledge that might be achieved through ethnography, or to the fact that site-based field research can be used to acquire "thick" data points as well as narratives and other forms of evidence.

At the outset, I noted that two studies by Fenn and Scott might be the most widely read examples of site-intensive methods in political science. Reflecting on these books in particular, I wanted to consider what it might take to promote an initiative within the discipline that incorporates and promotes both types of research. Viewed from one perspective, they can be seen as strongly contrasting, perhaps even polar opposites in their approach. Scott describes his project as a "close-to-the-ground, fine-grained account of class relations" (p. 41) in a Malaysian village, population 360, that goes under the pseudonym of Sodaka. Situat- ing his own "search for the primordial tension of anthropolo- gists" (p. viii), Scott states that he spent at least 14 months in Sodaka, interviewing, observing, and taking part in village life.

Fenn's work was motivated by questions concerning the relationship between representatives and those they claim to represent: "What does an elected representative see when he or she is enjoying a cup of tea, as a natural follow-up? What experiences do these perceptions have for or his behavior?" (p. xlii). His approach was to spend time in the company of the members of the U.S. Congress in their home districts. He famously characterizes his research method as "largely one of soaking and poking—or just hanging around," and situates it explicitly in the context of participant observa- tion as practiced by sociologists and other political scientists: ethnography is not mentioned, as far as I can tell (pp. xiv, 295). In the text of the book and its long methodological ap- pendix, Fenn explains: his modus operandi of accompanying politicians wherever they would let him tag along, building rapport, recording their remarks, and asking questions when possible. Clearly this is a research approach much closer to Scott's village study. Relative to a single-site project, Fenn traded depth for breadth, studying eighteen different representatives and then obtaining substantial variation on change over time (pp. 253-254).

The total time Fenn spent with each representative ranged from three working days to eleven, averaging six (pp. xxv, 256), and some of those days the research subject was available only part of the time.3

Despite their differences, these books can readily be seen as belonging to a common category. Both scholars were prol- elenged into the field by motivations of historical and theoretical interest in one case, and theories of representation in the other. In both instances, the researchers identified an empirical subject of key importance where at least as great weight was attached with assumptions or interpretations that ought to be tested or fleshed out through on-the-ground study. Whether "site-intensive methods" are a label that other practitioners might adopt, we do not yet know. It is certainly possible that further work in this area and its immediate surroundings of overlapping, closely related methods of observation might do more to talk to one another.—within the political science context. We in this field are fortunate to benefit from a wide selection of examples and methodological texts from other disciplines too numerous to cite properly here. We should absolutely make the most of this material. But the development and communication of these methods within our discipline ought to be with an eye towards evidence and joint ownership in our own right.

The purpose would not be to try to impose a consensus on a diverse set of researchers with varying ideas and commitments concerning the philosophy of science behind what is being done. Rather the idea would be to push some of the following goals:

Encourage the creation of clear statements of what is meant by this research and the ways in which it can be said to address theoretical works in our own discipline. Researchers would differ on the meaning or status of this work. Within the field, there can be no doubt that this is the landscape by which work is judged in political science today. The full range of research takes place on the scope of this essay, but they include collecting data that can be quantified and subjected to numerical analy- sis, gathering qualitative insights into a broader, more complex process, and deriving observable phenomena from the systematic and causal processes, checking the plausibility and validity of theoretical claims, and refining analytic categories for use with other methods. Fenn, of course, has explained in straightforward terms the various occasions under which SIM is particularly valuable—indeed, occasions where it is not.
necessary—for political research. One way to think about this is that such methods are of particular value when what we're studying is subtle (for example, relationships, networks, identities, styles, beliefs, or modes), and when what we're studying is sensitive, hidden or otherwise kept behind barriers that require building trust or otherwise unlocking access. Scott's and Feneo's projects serve again as examples. In both books, the fundamental subject of study was individuals' perceptions (peasants') and the particular narratives and perspectives of their communities. These perceptions are both subtle and, most of the time, hidden. This fact made forms of research like surveys and short interviews unworkable and necessitated strategies involving trust-building and over-time observation.

Discuss how best to teach these methods. To some, ethnography and the like may appear virtually unteachable. It may seem that each individual can learn how to do these things only by doing it him- or herself, and that the practical problems faced by students in their specific research sites will be virtually unique and impossible to prepare for in advance. But I believe this position is far too pedagogically prescriptive. It also understates the extent to which there is a set of coherent principles and techniques that underlie (even if sometimes implicitly) what ethnographers and participant observers do. This, in turn, creates a space for variability. Certainly, my colleagues and I who have co-taught the APSA short course on field research methods feel that reflecting on these methods has made us better and more efficient at using them ourselves.
The internal validity of the data collected.

Some scholars have moved in the direction of this more collaborative approach without recognizing it when they conduct open-ended interviews or focus groups at the very beginning. It has helped to define the research questions to be examined. I did some of these preliminary investigations on a pre-dissertation fieldwork to Ghana and Côte d’Ivoire in 1996. By listening to villagers and local government officials, I was able to narrow my topic and express economic liberalization to a more targeted set of issues around the reification of public provision services in health and education. As a result of these initial contacts and refinement of my question to a priority interest in the community, my respondents were eager to participate in lengthy interviews when I returned. In addition to a 97.5% response rate, I had more confidence in the content of the data.

In my second project, working with federal, state, and American Indian tribal government officials, my consultations with the various subject populations took place even earlier and were even more systematic. Instead of using feedback to narrow my own question, I sought input before any question had been formulated. In according my subject more power to identify what was substantively important to them, I have been able to enter into multiple communities that are historically hostile to outsiders researchers and to each other. I cannot count the number of times that respondents have expressed their desire to have their voices heard because of the account they have to offer. Also, despite the fact that the majority of my respondents were relatively underpaid and overweighted civilian servants for their respective government agencies, they almost all have spent considerably more time than I initially requested providing detailed, in-depth responses to my questions. This project and my work were not atheoretical; it was simply that the communities helped identify the problem in need of theorizing.

While the more interpretive notion of informed consent, we are still confronted with the concrete requirements of formal human subjects committees or Institutional Review Boards (IRBs). The rest of this symposium continues to be less convincing. Below, I will explore how this alternative conceptualization of informed consent might shape our everyday actions in the field. Before we continue, it is critical to understand that universities are not the only ones with IRBs. Many organizations, communities, and nations have their own institutions that review and approve research studies. For instance, my study on the politics of women and medicine in India required IRB approval from one university, one federal agency, two regional areas of the same federal agency, and ten tribal nations. Each of these IRBs has its own rules and procedures. Our training has led us to observe that our studies are being reviewed by each of the necessary “official” IRBs in the most appropriate order. This latter point involves recognizing the potential polities involved in the sequencing of different IRB qualifications. While admitting, my experience has been that the feedback I have received from the multiple IRBs processes has only strengthened the quality of my study.

Through these ongoing IRB experiences, I have built trust with future respondents; factual inaccuracies have been highlighted before they were presented publicly; and alternative interpretations of my data have been shared.

The form of the recent writing on informed consent, but not the content, focuses on whether subjects truly understand the information given to them about the research project. While many IRBs stress the need to convey information at a relatively low and thus opaque level to the informed consent process, which emphasizes the sheer quantity of information given to subjects. He finds that most informed consent forms give too much detailed information, creating memory overload and the possibility of truly informed consent. Cuming et al. (2006) emphasize how the power relationship must be shifted to a two-way flow of information to ensure participant understanding rather than a top-down delivery of information. This expands the power of the individual involved in the study from being a passive and subservient “subject” to an active participant in the process. Gold (2002) similarly emphasizes how study participants may not understand how their data may be used or owned by others if it is part of a larger cross-national comparative project.

All of the above suggests that individuals might need more time than is typically granted to read and digest the information provided in the informed consent form. Rather than being seen as a perfunctory signature or nod of the head prior to beginning the “real work,” researchers might give potential participants more information. Others have suggested that workers be trained to take the time to explain the research to potential participants or by sending via mail, e-mail, or fax. We might also incorporate active learning strategies from our teaching, and use active questions to inquire whether and how the participant has processed the information. Many researchers tend to rush through the actual process of informed consent because they fear the consequences of participant refusal or rejection. But rather than jeopardizing the study, allowing time for discussion and questions can aid in the collection of the data collection process by building trust early on.

Another strategy that can help individuals understand the process of informed consent more meaningfully is the provision of an “on demand” form. In many developing countries, no individual-level interactions can proceed until a village- or neighborhood-wide public meeting has been held where the researcher openly interacts with the research team and presents objectives. Even after such a public meeting was followed by the village gong beating passing “the news” at 5 a.m., my research team and I still had the problem of maximizing the reach to date as we went door-to-door greeting and introducing ourselves (and mapping the village to do a sample). While this kind of face-to-face meeting may not always be logistically feasible in larger or advanced industrialized countries, there are often large public meetings organized for other purposes where a researcher may request the opportunity to make a brief introduction.

The discussion of informed consent often focuses on the risks of participation in a study, there is less talk about the benefits. I have argued that a more collaborative approach to research would help promote research that would be of greater salience to participating individuals and communities. This is a way of fostering long-term benefits. But what about benefits to individual participants in the short-term? IRBs will often ask about the risks of participation by the participants, but less often, mentioned in the methodological literature in political science. Compensation is one of the most obvious ways that we, as researchers, have more power over our subjects and may create new inequalities through our actions. Even if we offer a small sack of sugar or soap in return for the time spent in an interview, jealousies and tensions might be aroused between those individuals or communities selected to participate and those not chosen. Several researchers have described their success in taking photographs of participants as a gesture of appreciation (Chacko 2004, Price 2001) or even as an object of discussion and component of the research (Deutsch 2004). In her more anthropological work, Gottlieb (1994) discusses how she gave back to the village communities where she has conducted long-term fieldwork by donating her book royalties to a community development project.

Many researchers fine the issue of compensation and simply do not consider it, arguing that it is not financially possible due to their limited resources. But there are non-monetary means of thanking or giving back to our respondents that are important to examine. First, researchers often have expertise in their subject area and beyond that may be of value to the participants. Some researchers have tutored participants’ children or helped others with government paperwork. Others have given formal or informal training on government policies and regulations that is free and publicly available but often unknown in rural or remote areas. For example, I was able to tell many Ghanaian farmers about the availability of free social services, particularly for the elderly or indigent at the hospital. Many researchers have provided information about both the opportunities and constraints for traveling and studying abroad in the U.S.

Our work could also make available information about ourselves. One way of leveling the playing field with participants is by allowing them to ask questions of us. Again, political scientists need to be more conscious of our power in the process of compensation. Sharing power by asking what participants need and responding when appropriate to personal, or even (how dare they?) sensitive questions can not only improve our human experience of research but also again improve the ultimate quality of our data collection and analysis but also invigorate our everyday lives with a new level of democracy.

References


Political Ethnography in Deeply Divided Societies

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Many projects based on field research or "site-intensive methods" in political science incorporate sensitive compo-

nents that can hinder the researcher's capacity to gain and maintain access to sources and informants. But doing field research arguably presents particularly acute challenges in deeply divided societies, where religious, ethnic, regional, or
other types of cleavages are highly politicized and may even constitute the ostensible cause of political violence. I encountered these challenges firsthand while carrying out field research on the relationship between faith-based social service provision and the formation of sectarian identities in Lebanon. A country that has been torn by civil war, Lebanon has officially recognized religious groups, and the political system institutionalizes divisions within and across the Muslim and Christian communities by allocating top leadership positions to religious leaders (institutionalization) according to sect. This contribution to this symposium on field research describes some problems that may arise when working in a divided political context and suggests strategies for addressing them. I focus on several practical issues, including the presentation of the project and the researcher to potential informants and institutional "gatekeepers," gaining access to interviewees, working with local translators and research assistants, affiliating with local institutions, and collaborating with local scholars. This is by no means an exhaustive list of potential challenges nor are the solutions suggested meant to exclude alternative approaches. Many of my observations also apply to research on politically sensitive issues in less divided political settings and to conducting research in places where the enterprise of scholarly research is suspect.

In any context, the initial presentation of both the research project and the researcher is critical. In divided societies, introductions are all the more crucial because potential informants and research subjects may situate their group as a "we" in a highly contested political space. Presentation of the research often begins well before face-to-face contact with informants or (institutional) representatives in email messages or letters sent from home institutions. At this stage, establishing the relative impartiality of the researcher and the research goals is key, whenever possible. In divided societies, informants may view members of other groups, especially those who are foreign or non-partisans, as well as foreign researchers, in extreme terms: either as opponents or potential converts. Framing the project in neutral language helps to dispel expectations of hostility or sympathy, and researchers may wish to preface the validity of interview data and the rapport needed to carry out interviews. A technical issue is whether political description of the research goals can bolster efforts to establish neutrality. For example, presenting oneself as a research assistant in a clinic, school, or social assistance rather than as a supporter of a sectarian organization emphasizes a more neutral yet nonetheless central role for the project. Still, an image of neutrality is not always convincing. This is all the more true in divided societies in which foreign actors—such as the US, UN, EU or NATO—have played direct roles in conflict (in particular, the initiation) processes. Researchers from some countries, for example, may face the added challenge of presenting themselves as independent of their governments. In most Middle Eastern countries, for example, it is critical for US-based researchers to stress their non-affiliation with governmental or quasi-governmental agencies because of widespread antipathy towards US policy in the region.

In divided societies, informants may view an interview as an opportunity not only to tell their side of the story but also to convince the interviewer of their perspective or to disparage other researchers. This researcher may bring his or her assumptions (or even unconscious) empathy or support to induce interviewees to share more information or facilitate access to data sources. Showing open support for an informant's views, however, can be a serious breach of his or her anonymity. The researcher may not want to reveal his or her perceptions, responses, and, second, it can backfire: words spread—especially across what appear to be rigid communal boundaries—and, therefore, it is important to be consistent in self-presentation and avoid direct or indirect confirmation or denial.

Gaining access to interviewees is often a function of who you know, even for seemingly innocuous research questions, and the challenges are all the more acute in deeply divided societies. In political science, we often hear that "snowball sampling" (Biemacki and Waldorf 1981), or referral chains, limit generalizability and bias because access to interviewees is dependent on existing networks with potentially limited variance. For research on sensitive issues or in places with deep, politicized cleavages, personal referrals often provide the only entrance to respondents. Indeed, some organizations and communities may be so sensitized that lower-level representatives refuse to meet with foreign researchers without approval from above.

Researchers can initiate and pursue referral chains by asking interviewees and contacts to refer researchers to those with whom they are familiar and to introduce interviewers to interviewees. Referral chains are used to tap into networks in social research. The degree and nature of the referral chain(s) is as important as the number of people. Referral networks are often limited, with few people willing to "introduce" someone to a researcher. Researchers may not trust potential interviewees even if they know and respect them. Researchers may ask for referrals from interviewees or from others who have expertise in the community. Researchers may present themselves as outsiders to gain access (or as insiders who are "outside"

The pluses and minuses of recording or taking notes during the interview should be considered carefully. While audio recording can provide more complete records, my research has shown that despite the risk of reduced control over the data collection process, contracting is often essential. The sheer scope of the task at hand in terms of the number and length of interviews to be conducted as well as the geographic coverage of the survey may necessitate hiring a firm to collect the data. In addition, some local research firms can match enumerators to specific segments of the population. While this might entail higher costs, it can allow the researcher to have more control over the questioning and to reduce the possibility of non-response.

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The division of state and societal power and resources occurs on a continuum, with a mix of state and societal power in most cases. With a mix of state and societal power, the distribution of power is not clear cut; it is often difficult to determine how much power is held by the state, how much by the community, and how much by both. In such cases, it is important to understand how power is distributed and how power is exercised. In Lebanon, there were twoOTQS_WV14531001_0001.jpg

Notes

1. See Braden's contribution to this symposium.

2. I emphasize "worst case" scenarios: that ethnic, religious or other apparently identity-based political violence is hardly inevitable and involves some combination of political entrepreneurs and fragmentation.

3. By institutional gatekeepers, I am referring to individuals with high-level positions who control access to the research-related resources and institutions and organizations, including documents, quantitative data, appointments with interviewers, entries into commu-
The Hermeneutic Foundations of Qualitative Research

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This article is the result of reflection that emerged while conducting qualitative field research on nationalism and exclusion in Portugal. The problem I confronted was when to stop interviewing. Stated more precisely, I was seeking an answer to the question of when one has collected enough empirical data to support or reject one’s hypotheses. This initial problem led me to a rather old discussion on the difference between natural and human sciences that has characterized German academic life for many years—in fact, since the early 19th century—producing some more heated phases of academic dispute, known as the Postimmediatist in the 1930s and the 1960s.

Hans Georg Gadamer’s Truth and Method, first published in German in 1960, stands out in this dispute as one of the major works to advance the discussion about the epistemology of human and social sciences. Gadamer makes an important contribution to the endeavor of providing a scientifically solid ground on which to make valid and reliable claims about the subject of their analysis: human action, transmitted or not in the form of written text. His main argument is that any human action and any utterance is meaningful and requires interpretation. The interpretation of a speech or a text must necessarily start from the concrete historical and cultural context of the researcher and on the cognitive and historical contexts of the reader, or not to the reader at all. These, in turn, are determined by one’s own cultural, linguistic, and historical context and background. Understanding others therefore first requires understanding yourself. To do this, you have to understand that the more the social context becomes aware of his or her own situation, the more the social context is a question of understanding others and initiating a process of systematic understanding via comparison and selective overlapping of two different cognitive systems.

This process of understanding others is potentially endless, as one can expand the horizon of meaning further and further. Gadamer’s work does not offer a direct answer to the problem of when to stop collecting data; it does, however, allow for a deduction drawn from the more general arguments presented. I argue that one of the implications that flow from Gadamer’s hermeneutics is that integrating a clearly defined research design and a concrete research question into the interpretative process controls the potentially endless process of interpretation. In other words, although a full and complete understanding of others is impossible, the answering of a research question about a well-defined aspect of their life world is possible.

In order to elaborate these arguments, I will first lay out the intellectual and historical contours of Truth and Method. Secondly, I will explain Gadamer’s theory of the hermeneutic circle, and thirdly, I will apply it to the question posed above, namely, when to stop interviewing.

Gadamer’s Truth and Method

Georg Gadamer’s Truth and Method, first published in 1960, was written in order to provide a solution to the problem of interpreting historical texts and to the broader problem of understanding historical utterances, a problem solved by understanding the interpreter’s work and life. Gadamer’s work succeeds in providing an epistemological grounding for the interpretation of historical texts and speech. As pointed out by Hans Albert during the German Postimmediatist debate, the major weakness of the interpretive work that dwelled on the Schleiermacher tradition was that understanding of meaning expressed in speech or text required empathy. But empathy does not provide a solid enough grounding for reaching reliable and inter-subjectively verifiable generalizations. How then, can we reach a reliable interpretation of historic texts? This is the question Gadamer sets out to answer. By elaborating on this problem, Gadamer presents a theoretical system of related problems, such as how to understand and interpret the utterances of others in the first place and how to proceed methodologically in order to reach a reliable interpretation of these utterances. I will first elaborate on these themes.

How to Understand and Interpret the Utterances of Others

We are born into institutional settings that predetermine our entry. As human beings, we constantly have to learn and interpret the world, and more specifically the institutions that surround us, in order to understand and interpret correctly the meaning of actions and utterances with which we are confronted. In the case of my research on nationalism and minority rights in Portugal, it became clear that contemporary reality is the product of historical forces and as such the prejudices and the human interaction and struggle. Past relations of domination and control influence the ways individuals with different ethnic and religious backgrounds interact in contemporary Portugal. Portugal’s ambivalent situation in Europe is equally influenced by the century-old struggle for recognition and respect from the much richer and influential northern European states. Historical continuities also emerged when analyzing the prevalent nationalist discourses in Portugal. An ongoing threat of early 20th century scientific positivism that had tried to demonstrate the "naturalness" of belonging of certain people to certain places, justified with reference to their essentialized cultural characteristics, lurked through many of the contemporary statements and discourses I collected about nationalism and the position of minorities. During my initial time in Portugal, I was unable to "place" the statements and interviews I recorded into their contexts of relevance. Although I understood all the words that my informants told me, their statements didn’t make much sense to me, because I had yet to become familiar with their locutions.

To complicate matters even further, cognitive systems are closed systems (Luhmann 1985: 404f), and it is by no means self-evident how communication across different consciousnesses is even possible. Social interaction relies most exclusively on language as a means to communicate, but language communication is inherently threatened by misunderstanding. This means that this is that utterances are made to convey meaning, and hence require interpretation. Chances for misinterpretation are accordingly high, first because language does not always overlap 100 percent with the intended meaning of an utterance and second because misunderstanding is mediated by the cognitive constitution or consciousness, of the receiver, i.e., that any information is filtered, sorted out, and changed by the receiver. Some information might not be recognized as relevant by the receiver, or it might be processed, and some information will be changed in the process of perception in order to fit the available frame of reference and to "make sense." In my case, I had to first become familiar with Portugal’s colonial history before I was able to correctly interpret the statements of most of my informants. I also needed to become familiar with ways the Portuguese interact on a daily basis and with the takes-for-granted and normalized ways they regarded and their everyday reality in order to be able to reach a more adequate interpretation of the statements and behaviors I observed and recorded.

All these complications have led many social scientists to propose and develop different theories of communication, such as motivation, meaning, intention, etc., and focus their efforts on observable and measurable outcomes on the one hand, and on one strong motivational pattern on the other, namely rational self-interest. This endeavor has produced some very strong and extremely useful theories able to explain human behavior, but their very elegance is the cause for their heuristic limitation, as they do not allow for an assessment of human behavior if and when it is irrational. Research on identification tends to be predominantly, motivated by the urge to maximize profits. In addition, a true understanding of meaningful human interaction cannot be achieved through these methods. Luhmann, through an analysis of causality and validity necessarily follow because we are left in the dark about such important factors in human action as motivation. Merely measuring actions and observable outcomes of social interaction therefore provides an "under-socialized" (to use the term coined by Mark Granovetter, 1985) view of meaningful social interaction. Furthermore, as Gadamer notes, "the individual case does not serve only to confirm a law from which practical predications can be made. Its ideal is rather to understand the phenomenon itself in its unique and historical concreteness." (Gadamer 1994:5).

The Problem of Objectivity

According to the widely accepted research guidelines provided by King, Keohane, and Verba (1994), all social inquiry must be reliable and replicable, and therefore must be subjected to the criteria of a systematic and transparent methodology. Such procedures should ensure that findings are verifiable or falsifiable by others. Although this seems a self-evident requirement, it bypasses a whole series of problems and complications, the most obvious being the assumed neutrality of the researcher that is required in such an approach. When addressing this problem,
Gadamer dwells on Edmund Husserl, who argued that “The objectivity of ‘objectivism’ which completely ignores experiencing, knowing subjectivity, subjectivity which performs real, concrete achievements, the scientist of the objectivist kind is blind to the fact that all the truths that he acquires as objective, and the objective world itself is that the substratum in his formulas in his own life construct [emphasis in the original] that has grown within him, he has not the possibility of making this transparent, while life [emphasis in the original] comes on the scene” (quoted in Gadamer 1994: 249).

Gadamer himself argues that “the theme and object of research are actually determined by the motivation of the inquiry. Hence historical research is carried along by historical movement of life itself and cannot be understood teleologically in terms of the object into which it is inquiring. Such an ‘object in itself’ clearly does not exist at all. This is precisely what distinguishes the human sciences from the natural sciences” (Gadamer 1984: 285). Others, like Jürgen Habermas, have followed in this tradition, arguing that “everyday experience is a task for a new structured, intersubjective and more adequate to be understood” (Habermas 1984: 110).

The criticism raised by these authors is that the researcher cannot escape the conditions of his own time, that he or she is analyzing being part of a socially structured world that implies one is not free in one’s relationship to this world and that the very perception of the world around us is influenced and structured by the beliefs we hold and even the facts we perceive have been handed down to us by tradition and we cannot escape from its influence.

Thus, per se, is not a problem, according to Gadamer, as long as we are aware of our own cultural, linguistic, and historical, in addition to our racialized and gendered, background: “All that is asked is that we remain open to the meaning of the other person or text. But this openness always includes situating the other meaning in relation to the whole of our own meanings or ourselves in relation to it” (Gadamer 1994: 268). The worst would be to not be aware of one’s own background, not making it explicit in one’s account of a given analysis. But that would not change the overall observed reality, analyzing it “neutralily.” Such an attitude must necessarily lead to unreflected and undisclosed bias and distortion, and it leaves the reader without a means of taking the position and situation and the researcher into account.

The Positioning of the Researcher and the Fusion of Horizons

Several theoretical influences inform our ways of perceiving, understanding, and explaining reality. At the minimum, any researcher must assume a critical and self-reflective posture towards himself or herself as a researcher. There is no escape from the fact that the researcher shares some of the categories and some patterns of perception, interpretation, and explanation with the subjects of her/his study and it is important to define one’s own critical identity necessarily part of the ongoing process of reconstructing the world through meaningful interaction and potentially influences the very outcomes one wants to observe and understand. The only way to achieve more reliable findings about any given social reality is to include one’s own historical, cultural, gendered, and racialized background and situatedness into the interpretation, which is such an explicit inclusion of the chance. This positioning of the researcher within the object of his or her research is driven by an understanding of the world as socially constructed through meaningful interaction.

Gadamer’s main question is why the inquiring subject becomes self-aware of his or her traditions, backgrounds and institutionalized ways of seeing and interpreting the world. According to Gadamer, this means realizing that the interpreter, who is self-aware of his or her cultural background, is self-aware of his or her culturally and historically institutionalized way of understanding the text. In this way, the interpreter’s own horizon is productive, not as a personal standpoint that he maintains or enhances, but as an opinion and a possibility that one brings into play and plays at risk, and that helps one truly to make one’s own what the text says. I have described this above as a “fusion of horizons” (Gadamer 1994: 388).

Ultimately, then, understanding others requires self-understanding; it is necessary to seek a “fusion of horizons,” a rethinking, or at least not being less, understanding others and their verbal or textual utterances. In the words of Gadamer: “All such understanding is ultimately self-understanding [emphasis in the original]. Even understanding an expression means, ultimately, not only immediately grasping what lies in the expression, but disclosing what is enclosed in it, so that one knows one’s own understanding. This means to understand, when a person understands, understands himself, projecting himself upon his possibilities” (Gadamer 1994: 260).

Accordingly, the part of the task of understanding others is self-analysis. Concretely, the researcher must analyze his or her interpretative patterns and prejudices in an effort to become aware of them so that they can be considered and addressed, or at least integrated into the analysis as limiting factors. The researcher’s task is “not to make the reader understand, to make the reader his own interpreter, not to approach the text directly, relying solely on the fore-meaning already available to him, but rather explicitly to examine the legitimacy, i.e., the origin and validity-of the line of things dwelling within him [my emphasis]” (Gadamer 1994: 267).

In the case of trying to reach an adequate interpretation of the statements made by my informants in Portugal, I soon realized that being a white male of German nationality helped me gain initial insights from white Portuguese citizens who sometimes counted on my benevolent understanding when making racist statements about African, Asians, and blacks (“presto”). Such unstructured talk allowed for important initial insights into the social and racial hierarchies of Portuguese society because it demonstrated how far everyday language had incorporated stereotypic and preconceived ideas. This is something that I am also limited in my access to the lifeworlds of excluded groups and my understanding of historical processes. It took me months to grasp the oftentimes very subtle references and values that were not easily recognizable to an anticolonialist observer, and even after a year of sharing the lifeworld of Lisbon, I still confronted situations where I did not know how to interpret correctly what I saw or heard. Countershaling my status as an outsider was the fact that I speak Portuguese with a strong Brazilian accent, which goes back to several years spent in Brazil. As a result, I was perceived by most Portuguese as a Brazilian, and this idea it explicit to the chance of the typically "immigrant experiences," such as being banned from eating certain bars and being attitude badly in several shops, restaurants, libraries, and even by university personnel.

Given the importance of this question from the perspective of research, if understanding is possible in the first place, and how correct and reliable interpretations can be achieved without falling into the highly speculative and voluntaristic state of research, it has been essential to engage Gadamer’s hermeneutics. This is the question that Gadamer sets out to answer with his hermeneutics.

Hermeneutics

When analyzing a text or speech, we make use of the frames of reference that we have at our disposal, anticipating the meaning of the whole by reading or listening to a part. We first, for example, seek to understand another person’s story, even though not sufficient, condition to understanding others and their verbal or textual utterances. In the words of Gadamer: “All such understanding is ultimately self-understanding [emphasis in the original]. Even understanding an expression means, ultimately, not only immediately grasping what lies in the expression, but disclosing what is enclosed in it, so that one knows one’s own understanding. This means to understand, when a person understands, understands himself, projecting himself upon his possibilities” (Gadamer 1994: 260).

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Method

As explained above, Gadamer’s basic insight is that any utterance is historically situated and cannot be understood without also understanding the historical context in which it is produced. One of my key texts is Dwelling on the work of Edmund Husserl and Martin Heidegger, Gadamer argues that we approach any utterance, be it in the form of speech or text, by anticipating its meaning according to our current understanding of or in relation to it. Gadamer 1994: 268).

The only limitation to this claim is that any interpretation is historically situated as the text sets out to understand and any interpretation is therefore open to different by different interpreters and traditions. This is where Gadamer’s hermeneutic of scientific work as defined by Kuhn and Popper. In his hermeneutics, he instead lays out a way to reach reliable interpretations of the text.

During my own research, my interpretation of the meaning of a statement changed as I became more familiar with the background and context of the speaker, allowing me to reach a better and more accurate interpretation of the data. A statement such as “I am a Portuguese citizen but at the same time I am not a Portuguese citizen,” made by a black Portuguese, initially did not make much sense to me, but once I became aware of the anti-colonial history, I was able to understand what this interviewee tried to convey to me.
their utterances into our existing framework of sedimented patterns and stereotypes. In order to reach an understanding of the meaning as it was conveyed by the speaker, we must therefore start a process of not only understanding a single utterance, but also understanding the overall presence of the speaker, her way of making sense, and her categorizations and stereotypes. We must, in the words of Gadamer, understand a person’s lifeworld so that we can understand where a single utterance is coming from. In this way, we come to realize that the analysis becomes relevant, by including one’s own horizon into the analysis, one can reach a sufficient understanding of the text or speech in question that is grounded in one’s own historical and cultural background. Thus, it is important to understand how one’s own understanding is constructed within one’s own lifeworld by including into one’s own framework of reference and way of making sense of the world. Life-world, as I use it here, is best defined as “represented by a culturally transmitted and historically developed organized stock of interpretive patterns” (Habermas 1989: 124).

Implication: When to Stop Interpreting

The hermeneutic circle provides a answer to the question of when to stop researching, and, as a corollary, when to stop interviewing. The premise for accepting this answer lies in recognizing that any scientific research must start with theory, as theory provides the foundation for the negotiation of realms for systematic inquiry. First, we have to define what we are interested in; then our theory will tell us what variables we have to look for and how we suppose they relate to the phenomenon in question. In other words, the hypotheses we formulate allow us to determine what is relevant to our inquiry. It is important to remember that theories cannot be true or false, but only useful. They can generate heuristic models that allow us to pose useful questions and formulate useful hypotheses about reality, by artificially separating one realm of reality from the totality of the world. Theories reduce complexity in order to be able to make fruitful causal statements about reality. As Kenneth Waltz (1959: 5) reminded us, theory is not equal to reality but a heuristic construct operating on a higher ontological level. Theory allows us to separate a realm of relevance and substructure of the problem we want to consider.

Once we have separated a realm for our inquiry, we can start the process of gathering data, where speech acts are considered part of the data to be collected. In addition to collecting speech acts, we must contextualize this data with other information relevant to the speaker(s) so that we can reach an understanding of her lifeworld and situate her speech. This is achieved by going forward and back through hypotheses and the general, concrete and specific act and the political, historical, psychological, and, in general, institutional context in which the speaker and the speech are embedded. This conception leads us to gather empirical data up to the point when each single piece of information makes sense, i.e., it complements the logical structure of the lifeworld we are exploring. Each new interview must relate to and ultimately confirm what we already have found out, in a positive or negative way. We cannot rely only on the contextualized interpretation of the single speech act in question, which allows us to interpret each new piece of information and locate them within the logical structure of the lifeworld.

Conclusion

One of the central insights flowing out of Gadamer’s work is that understanding requires that the researcher be part of the social reality he or she is trying to understand. Gadamer demonstrates that understanding cannot be obtained in an objective position toward social facts. As human beings we are already born into a world that has been structured and invested with meaning and interpretive perspectives constantly influence the world around us. Max Horkheimer explains, “the world which is given to the individual and which he must ac-

If we come across information that “does not make sense,” i.e., utterances or data which can be explained by the frame of reference we have already elaborated, we are forced to revise this framework by gathering more information and amplify our perception of the lifeworld in question until each single piece of information fits into and is explained by our revised framework. The hermeneutic circle closes when all the gathered pieces of information complement each other, forming a closed whole that is coherent and self-consistent, a world in the sense of Gadamer; when the specific and the general complement each other, a world in which the stage is achieved, each new utterance and new piece of information “makes sense” and fits into the already achieved understanding and interpretation of available data and its relationship to the lifeworld in question. Any new information can be explained and understood from within its context, even apparently deviant information. Once this point is reached, the process of interviewing is finished, as at this point in time any new information would only confirm what has already been found and merely add unnecessary layers of validity to the findings.

In practical terms, interviewers will reach a point where all the new information they receive confirms the insights and explanations already achieved. One of the main understandings I gained when researching the ethnic bias and the exclu- sive construction of Portuguese nationhood was that Portu- guese citizens of African decent were treated as foreigners in their everyday interactions with white Portuguese citizens. After some exploratory research, I was also able to formulate the hypothesis that Portuguese leaders supported and disseminated the conception of nonwhite Portu- guese citizens as foreigners by strategically financing those studies that dealt with foreigners and immigrants, while at the same time trying to distance their own children from those minorities. The formulation of these hypotheses led me to collect data, in the form of interviews, to confirm or refute my arguments. I was then able to interpret correctly what an interviewee meant when he or she said “I am a Portuguese citizen, but at the same time I am not a Portuguese citizen.” Once I had reached this level of familiarity with the Portuguese way of understanding group identity and the nationhood, all the new information I was able to collect started to make sense and “fell into place,” i.e., it complemented the overall and more general explanation I had reached. At this point, any research could stop, because all the new evidence I collected only additional layers of validity to my argument. The hermeneutic circle had closed.

Language

Language is the medium through which understanding can be reached: “For you understand a language by living in it—a statement that is true, as we know, not only of living but if dead languages as well. Thus the hermeneutical problem of understanding the language of those not able to express this very proper understanding about a subject—matter, which takes place in the medium of language...Thus we do not relate the other’s
cept and take into account is, in its present and continuing form, a product of the activity of society as a whole. The objects we perceive in our surroundings—cities, villages, fields, and woods—bear the mark of having been worked on by man. It is not only in clothing and appearance, in outward form and emotional make-up that men are the product of history. Even the way they see and hear is inseparable from the social life process as it has evolved over millennia. The facts which our senses present to us are socially preformed in two ways: through the historical character of the object perceived and through the historical character of the perceiving organ. Both are not simply natural; they are shaped by human activity, and yet the individual perceives himself as receptive and passive in the act of perception” (Horkheimer 1972: 200).

Parting from this insight, Gadamer constructs a philosophical grounding of understanding that is rooted in interpretation. He then develops the rules which allow for a systematic and method-driven interpretation, namely, through following the ins and outs of a “hermeneutic circle.” I suggest in this article that the logic that drives this methodology of interpretation can be expanded and applied to the very practical question of when to stop interpreting. Accordingly, the short answer is “when the hermeneutic circle is closed and all the new and partial information obtained fits into the broader explanatory context, forming one coherent whole.”


In Memoriam: Alexander L. George, 1920—2006

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Alexander L. George died of a stroke on August 16 in Seattle. George was a towering figure in the international relations field, and he made pioneering and enduring contributions to case study methodology, foreign policy decision-making, political psychology, and the study of deterrence, coercive diplomacy, and crisis management. George was a former president of the International Studies Association, a key figure in the distinguished international relations faculty at Stanford University for over three decades, and a valued teacher and mentor of countless students at Stanford and throughout the discipline.

George was born in Chicago on May 31, 1920. He did his undergraduate and graduate work at the University of Chicago and received his PhD in Political Science in 1958. He served as a research analyst for the Federal Communications Commission during World War II and then as a civilian affairs officer in Germany after the war. George was an analyst at the RAND Corporation from 1948 to 1968, and became director of its social science department. He moved to Stanford University in 1968, where he taught until he retired in 1990. He was emeritus at Stanford until 2006, when he moved to Seattle. George was professionally active until the very end. His short book On Foreign Policy: Unfinished Business was published a month before his death.

George’s first book, Woodrow Wilson and Colonel House (1956), written with his wife Juliette, had a major impact. It is widely regarded as one of the very best psychobiographies ever written, and is still in print after fifty years. George and George were concerned as much with the methodology of psychohistory as with the substance. They looked for pat-

Notes
1 Reiter 2005.
3 E.g., Gilroy 2000: 281 f.
4 As pointed out, e.g., by Sandra Harding (1993).
5 Interview conducted on 10 June 2003 in Lisbon. My translation.
6 The German nachvollziehen captures this process with more accuracy, as it implies a temporal dimension of “after acting,” different from imitation, but also more precise than “understanding.”

References


