A scholar of comparative politics need not leave her office to have at her fingertips an astounding array of information about most countries. In addition to compilations of economic data, readily available information includes recent proclamations by many insurgent groups, internal debate within some social organizations via email lists and online fora, public opinion data from most poor as well as rich countries, texts of parliamentary proceedings, local electoral results, and many national, regional, and local newspapers. Even if a source is not available online, networking among libraries increasingly makes the resources of the best research libraries available to scholars working elsewhere.

This unprecedented access to information sharpens the focus on the question this chapter addresses: Why ever leave one’s office? Given the declining cost of non-field methods, should researchers not invest more in those methods? What does field research contribute to scholarly understanding? Are there particular questions or settings for which field research is particularly suited? Or particularly not suited? How might we practice it better? What are the challenges typically confronted in field research?

By “field research,” I mean research based on personal interaction with research subjects in their own setting.¹ Field research includes research with residents of one’s own neighborhood or organization; one need not go abroad to be in “the...
Field research methods include carrying out surveys, interviews both informal and structured, field experiments, and the activities known as participant observation (which often includes living in a community alongside residents, ongoing observation of community life or organization meetings, and working alongside other workers). Archival research is excluded, although some similar practical issues arise where archives are not located in universities or other easily accessible sites. Also excluded is the analysis of survey data gathered by others, newspapers or other compiled sources, or other types of databases. Some “natural experiments,” the interpretation of exogenous sources of variation as a quasi-experimental method, draw on field research; where there is no personal interaction between the researcher and the participants they do not count as field research as discussed here. “Narratives” may be based on field research, but need not be as they may be based on sources such as newspaper articles or databases not compiled through field research.

The methodological challenges confronting field researchers are well known (King, Keohane, and Verba 1994). Data gathered in interviews with political actors are often difficult to interpret and verify, particularly their stated reasons for doing what they did. Without careful attention to research design, field research may merely confirm preconceptions with which the researcher went to the field. Causal inference is often difficult from data gathered in the field and generalization to other settings often problematic. To randomize an experimental “treatment” or to draw a randomized sample is difficult in societies where demographic characteristics are not well defined due to inadequate statistical services or to population movements not captured by those statistics due to poverty, war, or fear of state authorities.

Despite these challenges, field research can and often does make contributions to social science that could not occur via other methods of analysis and data collection. Field research is often the only source of adequate description of social, economic, or political processes that are not evident in documents or data already gathered. In this case, close familiarity with a well-chosen case may not only identify what appear to be key processes but also central concepts and relevant actors. Field researchers often develop better measures of key concepts (Adcock and Collier 2001) or new analyses of rare events (Mahoney and Goertz 2006).

Field research contributes to causal inference in several ways (George and Bennett 2005; Brady and Collier 2004). Works that focus on a single case often take the preliminary form of theory generation, the positing of a causal relationship that appears to hold in the case investigated, with the suggestion that it may hold for others as well. Field research on a single case may also disconfirm a theory or model.

---

2 In anthropology the “field” in “field research” refers to research in communities “in the field,” that is, in communities other than one’s own. Historically, anthropological field sites were usually isolated culturally to some degree from markets and the state, and were often in another country. Beginning in the late 19th century, field research was increasingly also carried out in indigenous communities in researchers’ own countries. Sociologists of the Chicago school of urban ethnography extended “the field” to include domestic urban sites defined by neighborhood or occupation. Today field “sites” include institutions, organizations, bars, occupations, and street corners.

3 On archival research, see the chapter by Steven Pincus in this volume.
when it shows that the sequence of events does not match that predicted by theory, when the salient events were driven by actors other than those identified in the theory, or when an alternative mechanism appears to be doing more causal “work” than the postulated one. In particular, the analysis of contingent events and path dependency often depend on “process tracing” impossible without data gathered through field research (Bennett and Elman 2006). Field researchers often identify causal mechanisms and the conditions for their occurrence through comparison of cases chosen for that purposes. Some field researchers working with comparative research designs suggest that distinct “paths” lead to similar outcomes and thus that social science need recognize the pervasiveness of “multiple, conjunctural causation” in either deterministic or probabilistic versions (Ragin 1987, 2000; Mahoney and Goertz 2006).

In part to address the tradeoff between depth of knowledge of a few cases and the stronger causal inference possible with more, research designs in comparative politics are increasingly sophisticated, as scholars attempt to widen their work’s descriptive or inferential power. Scholars who do field research increasingly combine field methods with one another and also with non-field methods such as formal modeling and statistical analysis. Field researchers may identify potential causal mechanisms that underlie a statistical regularity, or explore alternative mechanisms that may account for outliers in the statistical analysis (Lieberman 2005). And the identification of the mechanism may contribute to the subsequent development of a formal model. The relationship between field research and a formal model may be reversed: field research may confirm (for a particular case) the presence of a causal mechanism developed via a formal model. If the causal process unfolded as posited by the model in the sequence prescribed, via the mechanisms proposed, by the actors claimed, and with appropriate perceptions on their part, field research adds to the plausibility of the model.

One indication of the abiding contributions of field research comes from the surprises that field researchers often report. Such surprises often come from the realization that research findings contradict the presuppositions with which they went to the field: for example, the crucial question turns out to be different from that originally posed, the actors not those identified earlier, or the relevant constraints or opportunities other than understood. Thus experience in the field may correct initial bias on the part of the researcher and lead to the recognition that existing theory is inadequate and the recognition of new empirical patterns or causal mechanisms. In particular, many field researchers report learning enormously from their subjects, ranging from the surprise expressed by respondents at a particular question to themes that subsequently become central to the researcher’s analysis. Sometimes such insight comes from self-reported data in interviews, sometimes from the data conspicuously missing in interviews.

An important category of data unavailable except through observation or face-to-face interaction with subjects is the preferences and beliefs of political actors as varied as union members (Lipset 1956), local elites (Dahl 1963), parliamentarians (Fenno 1978), and lobbyists and activists (Graetz and Shapiro 2005). How actors understand their identities and interests is often best approached through interviews, observation, and
surveys designed specifically for that purpose. General public opinion surveys may be useful for some questions but often do not ask the right questions or do not ask them such that the results are relevant for scholarly analysis. In particular, how political actors perceive their strategic interactions with other actors in real settings—what choices they confronted, their beliefs concerning the likely consequences of different choices, their analysis of paths not taken—is often unavailable except through face-to-face interaction with the actors themselves, that is, through field research.

This is especially the case in four circumstances. The first is when the political actor is at a permanent disadvantage as a result of repression or domination or lack of education severe enough that its access to global media is severely limited. Although insurgent and social movement organizations increasingly publish material on the internet, it is rarely sufficient for scholarly investigation. The second is when the scholar seeks to disaggregate actors such as organizations into their constituent factions or individuals whose beliefs and preferences may be quite distinct from the official line available in publications. The third is when the scholar seeks to understand the internal processes of a group, which may become available only through participant observation and interviews. The fourth is when the actors have reasons to obscure their preferences and beliefs from public view, as when they are engaged in strategic interactions with other actors and thus stand to lose advantage should they reveal their preferences and beliefs, as in nearly all political and economic negotiation processes. Surprisingly often, such actors are nonetheless willing to talk with academic researchers.

To understand why political actors make the choices they do the researcher often must evaluate the relevance of their own reports of why they made those choices. Self-reported motivations are best evaluated in face-to-face interactions, that is via field research, so that the strategic context can be discussed, inconsistencies probed, and alternative motivations raised. Of course those interviewed, surveyed, or observed may well interact strategically with the field researcher. The reasons may vary from a desire to obscure a belief that is believed to be illegitimate, to exaggerate or minimize one’s own role, to mislead the researcher concerning rivals, and so on. And first-hand reports may be subject to various other kinds of distortion. A good field researcher does not take reports and observations as true per se nor as a complete report of the causal forces at work, but as data reported within a particular context. In order to develop an interpretation of self-reported data, the field researcher typically seeks to interview, observe, or survey other people concerning the same subjects to explore whether some pattern seems general or particular to specific people or setting. Where available, the researcher may also “triangulate” with other sources, by comparing reported data to judicial, newspapers, human rights reports, and other records of relevant events. The purpose may be to prod the memory of those interviewed or to assure them that the interlocutor already knows quite a lot and thus a superficial answer is not desired and a fallacious one unsustainable.

The alternative to considering self-reported reasons relevant is the assumption that observed behavior reveals the actor’s reasons. But even in markets settings it is
difficult to construct economic preference orderings, for example, to construct demand functions by observing economic choices as relative prices vary. And even in the controlled laboratory settings of experimental economics, the interpretation of motives is highly contested as in the current debate about social preferences. It is difficult to see how in complex political settings motivations can be imputed without consideration (perhaps to identify subterfuge or contradiction) of self-reported reasons.

In addition to subjects’ preferences and beliefs, researchers in the field often identify, locate, or generate other kinds of data that would not be available without personal interaction. It is frequently the case that face-to-face interaction plays a role in persuading informants to pass on data judged sensitive. For example, persons interviewed a second or third or tenth time often provide previously withheld relevant names, documents, photographs, archives, maps, or budgets. Similarly, personal interaction may persuade an actor to generate new data for the researcher in the form of hitherto unexplored statistical analysis of databases unavailable to the researcher, newly drawn maps, or the inclusion of new questions in their own survey.

In this essay, I address these “why leave the office” questions principally through discussion of exemplary works that draw on field research. I begin by considering a classic work of field research in comparative politics, James Scott’s Weapons of the Weak, and then turn to some recent works that explore related topics using some combination of interviews, surveys, and participant observation. I then discuss other field research methods, illustrating with selected recent works the trends toward natural and field experiments and combinations of field methods with non-field methods. I discuss the challenges that field researchers confront irrespective of their particular method, including some ethical and practical dilemmas. In concluding, I recommend that graduate training for students in comparative politics include more in-depth training in field research than usually occurs.

1. James Scott’s Weapons of the Weak

James Scott’s Weapons of the Weak: Everyday Forms of Peasant Resistance draws on extensive field research to discredit the theory that in class societies ruling groups ideologically dominate subordinate groups in the sense that subordinate groups believe that social structures that favor superordinate groups are immutable, inevitable, natural, legitimate, and perhaps just.

---

4 I do not restrict the discussion to works by political scientists but describe works based on exemplary field research by other types of social scientists as well. I do not attempt to summarize practical advice for the variety of field methods discussed here but provide some references for the interested reader.
Scott spent two years (1978–80) living in a Malaysian village in the Muda region of north-western Malaysia. In 1971, several years before Scott’s fieldwork, the village began harvesting two crops of paddy rice a year rather than one, thanks to a recent irrigation scheme and other “green revolution” policies. As a result, most residents of the region were better off than before (Scott 1985: 64). The site was a crucial case for the exploration of class relations: if the dominant ideology was contested even in such a propitious setting, it was all the more likely to be contested in settings where average income was declining. Among Scott’s explicit criteria for the selection of a particular village was that it had been studied before the advent of double-cropping.

Scott gathered a wide range of data concerning the perceptions of class relations through participant observation, including passing time at the gathering places of different village factions, threshing paddy fields, and attending ritual gatherings such as weddings and funerals. Through more structured interviews he also gathered data on land tenure, farm size, land use, production costs, income, and political affiliations (party affiliation and whether or not the household head belonged to the farmers’ association) for each of the seventy-four households in the village. And he collected a variety of relevant documents and quantitative data from government and other sources. Comparing his data with the (far less detailed) data gathered by a development economist a dozen years before, Scott found that for the first six or seven years after double-cropping began, nearly all households increased their income substantially (essentially double the work was available for those reliant on wage labor and two harvests for those with access to land). For a brief period, even poor households held ritual feasts, a significant rise in their local status. However, by 1978 or so poor families began to experience several negative consequences of the increased productivity of the land. Because large farmers began farming more land themselves and because outsiders also began to farm land in the village, less land was available for rent and contracts increasingly required a large lump sum up front (sometimes for several years’ rent). Many of the owners and larger tenants began harvesting their crop with combines and some began broadcasting paddy seed rather than transplanting seedlings, resulting in a drastically reduced demand for local labor. The result was a sharp increase in inequality in the village and a severing of the traditional economic interdependence between rich and poor families that undergirded traditional social relations.

Scott documented in great detail the very different interpretations of these changes by distinct social groups, the claims made by each, and the strategies they employed. The village poor sought to persuade the rich to continue traditional patterns of employment and charity through various strategies, the “weapons of the weak.” While deferential in direct interactions with the rich, in private settings some poor men attacked the reputations of rich individuals as stingy, condemning their abandonment of ethnic and religious notions of fellowship. Some teams of women laborers delayed transplanting seedlings for farmers who had harvested with combines (but did not announce a boycott as that would have been to run the risk of direct retaliation). While Scott did not hear of attacks on combines in the village,
such attacks were rumored to have occurred elsewhere in the region. Several poor heads of household refused to join the ruling party despite the material benefits they would have received. In contrast, rich individuals generally sought to legitimize their new practices by identifying and holding up for emulation “good” workers and scorning others as lazy and incapable. And in public they maintained that due to the high costs they confronted in fact they themselves were poor despite their access to abundant land.

Scott concludes that the poor actively resisted the ideological domination of the rich, despite their deference in public interactions with their well-off neighbors. The poor contested the claim of the rich to have to do things in new ways and asserted the moral superiority of the old ways through indirect channels that nonetheless circumscribed (to a limited extent and for a limited time) the behavior of the rich. Thus Scott argues that the theory of ideological hegemony does not generally hold. The ideas of the dominant class do not persuade subordinate groups that class relations are inevitable, much less legitimate and just. Rather, subordinate classes actively resist the ideology of the dominant, often through appeal to traditional ideologies of reciprocity or to millenarian interpretations of religious tradition. Scott asserts a sweeping claim: resistance by peasants, slaves, and workers pervades history; class relations are contested even where no overt rebellious activity is observed.

Thus the work challenged a theory with meticulous ethnographic field research and suggested an alternative theory of subordinate resistance. The persuasiveness of the work also comes from its descriptive depth, its accumulation of information about social dynamics in the village as experienced by various factions. Importantly, the combination of quantitative and qualitative methods lends it a particular authority: it is hard to imagine Scott being misled by his informants given the detailed knowledge of local conditions he commanded. The carrying out of the survey, in particular the gathering of sensitive quantitative data (such as household income), depended on the trust and acceptance developed through participant observation methods (1985, 202–3). That Scott is explicit about the limits to his research (he found he could not gather data on thefts or violence and had no way to evaluate the chilling effect of repression on discontent in the village) strengthens the reader’s confidence in the data Scott did gather.

Thus the book’s persuasive power rests in large part on the internal validity of Scott’s ethnographic data, supported by relevant statistics and documents. I consider below a variety of strategies to increase the external validity and replicability of participant observation research, include randomized surveys to test findings, choosing field sites on more explicitly theoretical or statistical grounds, and conducting experiments in the field.5

2. Comparison of Cases in Field Research

Scott’s research design is a classic one in comparative politics: a single case chosen as a test of a theory is analyzed in great depth against a background of theory and other cases using a variety of field methods; the theory is shown to be false; and a new theory suggested. A single case can of course only refute a theory that asserts a pattern should hold deterministically rather than as a tendency, and can only suggest an alternative theory. Thus Scott’s book set a theoretical and comparative agenda: whether consent to class hierarchy occurs in other cases and the conditions for covert resistance to become overt collective action.

Susan Stokes (1995) addresses the first question, whether subordinate classes ever consent to domination. Stokes combined in-depth participant observation with a survey in a single community, a shanty-town in Lima, Peru. After a year of participant observation in the community during which she developed relationships with local leaders, Stokes carried out extended interviews with two dozen leaders. She identified two broad patterns of political culture, one that replicated traditional clientelistic relations and a new one that challenged such traditional relations. She then carried out a survey of nearly a thousand residents of the shanty-town (randomly chosen from the list of voters) and confirmed that ordinary people also tend to have one of these two political cultures. Contrary to the elite interviews, the survey found that women were in general less mobilized and more conservative than men. Stokes argues that, contra Scott, the clientelistic pattern was founded on consent to core aspects of the dominant ideology. This finding together with Scott’s suggest that in some settings the rich ideologically dominate the poor while in others they do not. But neither work articulates in any depth the conditions under which elites will or will not ideologically dominate the poor; nor is any inference of the frequency of domination possible from their work. Stokes’s analysis of non-clientelist politics does explore the sources of ideological change among the poor, namely, unions, new religious teachings, and schooling under Peru’s reformist military regime.

The literature on collective action frequently addresses the second question raised by Scott (and others before him), the conditions for mobilization in hierarchical societies. Drawing on a comparative research design in the tradition of Barrington Moore, Jr., Deborah Yashar (2005) explains the emergence of indigenous social movements in the late twentieth century in some Latin American countries but not others. She argues that increasing challenges to local autonomy, combined with the opportunity in indigenous communities to organize and their capacity for collective action, led to the emergence of powerful indigenous movements in Bolivia, Ecuador, and to a lesser extent in Mexico and Guatemala. Despite similar challenges to indigenous autonomy, a movement did not arise in Peru as the ongoing civil war closed local associational space and destroyed local capacity. In classic Millian fashion, Yashar compares and contrasts her cases (including in some cases comparison within a
case) to show that her theory explains the observed levels of indigenous mobilization whereas rival theories do not. Her analysis drew on field research in all five countries (with a particular focus on the core cases of Ecuador, Bolivia, and Peru), including interviews with 150 movement leaders, observation of dozens of meetings, and the collection of a wide array of documents. Developing networks of contacts in social movements in a single country is a challenge; the scope of Yashar's work testifies to her skill in persuading activists to talk with her.

In my own work (Wood 2003), I explored why some peasants supported insurgent groups during El Salvador's civil war despite the high risks they thereby ran. In this setting where few police records were kept and the data compiled by human rights organizations were quite incomplete, I relied on extended, often repeated, interviews with local residents (both those who supported the insurgency and those who did not) of five case study areas. The interview took the form of oral histories of their family and the local community. (I also interviewed field-level insurgent commanders and military officers, government and UN officials, and landlords.)

I found that support for the hitherto small insurgency dramatically escalated in the wake of extreme violence by state agents against a non-violent social movement whose legitimacy was deeply rooted in liberation theology, a Catholic movement that taught that social justice was God's will. In areas where the grip of the state was relatively weak (rural peripheries and near insurgent bases), this support evolved from covert individual activities such as the provision of military intelligence to overt collective action, including a wave of land occupations that spread across three of the case study areas. I documented the land occupations by a distinct method: I asked insurgent activists from four of the case study areas to draw maps showing property rights and land use in their neighborhoods before and at the close of the war. Drawn collaboratively by at least two and usually several members in a process interspersed with much discussion of the history of the area as well as gossip, jokes, and teasing of one another (and of me), the resulting maps document how cooperatives of insurgent supporters literally redrew the boundaries of class relationships through their collective action. Drawing maps was not a familiar task; only a few said they had ever seen a map (insurgent commanders had a few, well-worn maps held together with tape). One non-literate elderly leader traced property lines with his forefinger while his grandson drew the line in its wake. Each pair took two days to draw, an unpaid sacrifice of time that I understood as an indication of their enthusiasm that the history of the war in their area be documented.

In accounting for why about a third of the residents of four of the case study areas actively supported the insurgency, I took seriously the reasons for their activism evident in the oral histories and supported by details they included in the maps. I argued that reasons for this high-risk collective action were threefold: the meaningfulness of

---

6 The accuracy of the claims by these cooperative leaders to occupy extensive areas of land in 1992 was confirmed by my own travel and observation in the case study areas and by examination of the land claims data held by the insurgent group, the government, and the United Nations during the post-war land transfer process.
participation in the building of God’s Kingdom, defiance of unjust authority in the wake of state violence, and pleasure in historical agency—the remaking of class relations, culture, and history through one’s own efforts. I cleared the way for this argument by showing that alternative explanations did not account for the observed pattern of mobilization across the case study areas: mobilization occurred in areas of tenancy and large commercial estates; class position did not predict which individuals participated; nor did pre-war community structure account for the pattern. And while acknowledging the difficulties in interpreting self-reported reasons for collective action (Wood 2003, ch. 2), I also argued that the particular elements of this insurgent political culture required explanation and that my interpretation of those elements was more parsimonious than alternatives.

Both Yashar and I base our findings on analysis of five cases, sacrificing ethnographic depth of analysis for analytical traction through comparison of cases that vary in the extent of mobilization observed. Comparison of subnational units has some advantages over comparison of national units, allowing a narrowing of likely explanatory variables as those shared across subunits cannot account for the different outcome observed. There are often practical advantages as well: subnational comparison often requires skills in only one language, one local affiliation, and one research approval. And of course travel between sites is usually less demanding. However, there are two distinct problems with subnational comparison (in addition to a frequent pattern of inadequate variation within a single state). As Richard Snyder (2001) pointed out, distinct subunits may not be independent cases, undermining the supposed power of the observed pattern of similarity and differences. And the assumption of national-level similarity may not hold, particularly in just the types of settings that at first glance might appear ideal for this design such as large federal states with great internal diversity whose constituent states may in fact differ significantly in the factors often supposed constant across the federation.

3. Natural and Field Experiments

The findings of such comparative research designs rest on the assumption that the cases vary in the causally relevant variables in ways well understood by the researcher. Yet whether that is in fact the case is often asserted rather than demonstrated. An alternative design is that of the so-called “natural experiment,” in which cases that were once indistinguishable (except in trivial ways) diverged because of an exogenous event whose consequences are analyzed.

Daniel Posner (2004) used the arbitrary nature of colonial boundaries in Africa as the setting for a complex natural experiment. Posner compared the cultural relationship between two ethnically distinct groups (the Chewa and the Tumbuka) on the Malawian side of the border with their relationship on the Zambian side. Posner
conducted an open-ended questionnaire and focus groups in two pairs of very similar villages located on either side of the border. In one pair, both villages were homogeneously Chewa but one was in Zambia and the other in Malawi; in the other pair, both villages were homogeneously Tumbuka. Posner found that while, in Zambia, relations between the two groups were very cordial (“we are brothers”), in Malawi, the same cultural difference had high political salience. Because he had picked the pairs of villages ingeniously to control for confounding alternative explanations (such as different exposures to markets or levels of modernization), Posner showed that the demographic importance of the ethnic groups within each country is a hitherto-neglected mechanism that explained the difference in the political salience of the same cultural difference. In Malawi, both the Chewas and the Tumbukas comprised relatively large fractions of the population (28 and 12 percent, respectively), while in Zambia together the two groups comprise only 11 percent of the population. The two groups compete for political resources in Malawi, aligning with different political parties, but join the same coalition in Zambia.

Raghabendra Chattopadhyay and Ester Duflo (2004) analyze a very different type of natural experiment that explores the effect of a particular institutional innovation, mandated female representation, on the provision of local public goods in India. Beginning in the 1990s, a third of the seats on the village council and a third of the chiefs of the councils in each village-level election were reserved for women. The councils that were reserved were chosen randomly, which should control for differences other than reservation. Chattopadhyay and Duflo carried out a survey of public investment in a sample of both reserved and unreserved village councils in two districts, one in West Bengal and the other in Rajasthan, in order to analyze whether the type of local public good depended on whether the council headship had been reserved for a woman. They first confirmed that reserved villages had indeed been chosen randomly and that the villages did not in fact differ significantly other than in being reserved or not. They found that mandated representation made a difference in the provision of public goods: in villages reserved for women heads, public investment was significantly more likely to target the concerns of women such as provision of drinking water (as evident in their analysis of complaints to the council by gender).

Both works test the observable implications of social science theory, in the first case the theory of political identity developed by Posner in his dissertation (and later book) and in the second the classic claims of representative democracy. The analytical power of the natural experiment method relies on exogenous events (the drawing of a colonial boundary across an ethnic group or the requirement that women hold particular offices in randomly chosen sites) to generate distinct “treatments” across groups who are supposed otherwise similar in all relevant ways. Thus the exogenous event effectively randomizes the subsequent “treatment” (which polity a subset of the previously identical population joins or the application of a policy to a particular

---

7 Posner 2005 further explores the relationship between political institutions, group size, and ethnic identity in Zambia using another natural experiment, the switch from a multiparty regime to one-party rule and back again.
site or not). The inferential power of the method is of course undermined when the exogenous event was not in fact entirely exogenous (as when the population varies with the treatment in some way) or when subsequent processes in addition to the hypothesized one occur with different causal force on distinct sides of the border.

In contrast to the reliance on exogenous events to randomize treatment in natural experiments, randomized variation for the purpose of experimental control in field experiments is achieved through the research design itself. In the most common design, groups are randomly selected to receive particular treatments or none (the control group); the randomness of the selection allows the presumption that the groups are otherwise indistinguishable (the differences “cancel out”).

Leonard Wantchekon carried out a field experiment in Benin to explore the determinants and consequences of clientelistic appeals to voters. With the cooperation of the four major political parties running candidates in the 2001 presidential election, Wantchekon compared voting behavior in randomly selected, ethnically homogeneous villages that received clientelistic appeals (“vote for candidate X as he will bring goods Y to this district”) and those that received public goods appeals (“vote for candidate X as he will bring goods Y to all people of Benin”) to that in control villages which received the usual mix of appeals. (In order to avoid affecting the outcome of the election and to secure the parties’ cooperation, the treatment groups were all in districts dominated by one party or another.) Wantchekon’s research team gathered the data on voting behavior via a representative survey carried out in all districts that also gathered data on respondents’ demographic characteristics and the degree of exposure to the campaigns. He found that clientelistic appeals had an effect everywhere, but were significantly more effective in some regions than in others, were more effective for incumbent candidates than for challengers, and that men responded to clientelistic appeals more than women. Thus voters’ response to clientelistic or public goods appeals depends on political and demographic factors, not just ethnicity.

In a research project that compared the results of experimental games across radically different field settings, an interdisciplinary group of anthropologists and economists carried out a series of experiments in fifteen “small-scale” societies including hunter-gatherer groups in Latin America and Africa and pastoral groups in African and Mongolia (Henrich et al 2003, 2005). The project grew out of the puzzling findings by experimental economists that many students played economic games in controlled laboratory settings in ways not predicted by standard economic theory: they did not play selfishly. For example, consider an interaction called “the ultimatum game,” in which one randomly chosen player is given a stake, say ten

---

8 Green and Gerber 2002 and Harrison and List 2004 review the role of field experiments in political science and economics respectively. Not all field experiments fall within “field research” as defined in this essay.

9 These experiments might be understood as laboratory experiments in that the researchers attempted to some extent to establish laboratory conditions in the cultural settings of each society. However, the games are better understood as involving interactions between members of a small-scale society in their own cultural setting and hence I discuss them here.
dollars, and makes an offer to a second student of some part of the stake, say two dollars; the second player may accept the offer, in which case they divide the stakes accordingly, or reject it, in which case neither gets anything. In the laboratory, respondents frequently rejected low offers, apparently punishing (at a sacrifice to themselves) unfair offers, and proposers typically made offers much higher than the minimum (perhaps anticipating the rejection of low offers). Experiments involving many variations in university laboratories across the world had confirmed this (and related findings).

Realizing that these findings were nonetheless limited to university student populations in market societies, the group decided to radically vary the type of society and see if members replicate the way university students play the games. Ethnographers with long experience in different hunter-gather, pastoral, and other small societies were recruited to ask residents in their field sites to play the ultimatum and other games, suitably adjusting the stakes appropriately, e.g. tobacco rather than cash in some societies. As in the university settings, the roles were assigned randomly.

Compared to the limited variation in the play of students across diverse university settings, the findings across these populations were extremely diverse. In some societies, respondents accepted even low offers; in others, proposers made hyper-fair offers (more than half of the initial stakes), which were sometimes rejected; and so on. Because fifteen societies were included in the study the patterns of play could be analyzed statistically both by individuals and by small-scale society—a key innovation of the project. Individual characteristics such as gender or wealth did not predict how individuals played; rather, the different patterns of play corresponded to group-level characteristics. The group’s findings draw on its collective ethnographic knowledge as well as the game results. In particular, the group found that the average ultimatum game offer in each society was significantly and positively correlated both with the degree of market integration and the degree of cooperation in everyday life.

To be persuasive, field experiments face similar challenges to natural experiments and laboratory experiments, including whether the treatment was truly random, whether subjects differed in relevant ways other than the treatment, and whether the interpretation has external validity. In the case of the fifteen societies, whether the experiment was understood as the same game in the different societies is problematic: the researchers used a variety of methods to operationalize the game (Henrich et al. 2005, 805–6). Whether anonymity is possible in such intimate societies is also a question; if not, the comparability with the university-based findings is lessened. And a variety of other interpretations of the findings have also been proposed.11

10 The project is now in its second phase with ethnographers asking residents of a wider range of field sites to play a wider variety of games (with stricter controls for divergent cultural interpretations) and measuring more precisely apparently important factors such as market integration and everyday cooperation.

11 See the various commentaries on Henrich et al. 2005 in the succeeding pages of the journal.
4. Combining Methods

In comparative politics, researchers are increasingly combining a variety of field methods with non-field methods in order to more powerfully confirm apparent factual findings, to establish the relative causal force of distinct mechanisms, and to parry alternative explanations. For example, case studies are sometimes identified in tandem with statistical analysis of many cases (Lieberman 2005). An anomalous case “off the regression line” may be chosen to explore why the observed statistical regularity does not hold in that case, perhaps identifying a new mechanism. Or a crucial case “on the regression line” is chosen to verify whether the observed regularity appears to occur via a posited mechanism.

In his analysis of communal violence in India, Steven Wilkinson (2004) combined statistical analysis of a large dataset of Hindu–Muslim riots since 1900 that he and Ashutosh Varshney collected based on a close reading of Indian newspapers and police reports with field research in a case study area chosen to explore the relationship between elections, mobilization, and policing. Wilkinson tests with these data not only his own hypotheses but several others. Such explicit hypothesis testing is unusual in the field of ethnicity and politics, which usually relies on only narrative analysis. Wilkinson shows that elections comprise an incentive to violence in a divided society under democratic government when local elections are close and when the level of government that controls police forces (in India, the state government) values the votes of the aggressor group more than minority votes and thus does not order police forces to prevent and stop ethnic violence. Thus the work circumscribes the frequent claims that in a divided society politicians will “outbid” each other and that ethnic polarization and mobilization are thus unavoidable. Wilkinson shows that this claim is sometimes, but not always, true.

Scholars in comparative politics are increasingly explicit about the extent to which field research gathers data to test the observable implications of a previously developed theory, as opposed to which the data gathered largely inform the theory generated by the research project. For example, Jeremy Weinstein (2006) gathered data to test the observable implications of his informal model of the organizational challenges that insurgent groups confront. His model suggested that groups tend to recruit distinct types of members (opportunistic or committed) depending on whether their endowment is economic or social, and that the type of member shapes a distinct pattern of rebel organization and violence. Nascent groups with access to significant economic endowments will attract opportunistic members and will thus exhibit a lack of discipline evident in excessive violence, looting of civilian resources, and an inability to punish abuses by those within their ranks. In contrast, only individuals highly committed to the goals of the organization will join organizations with only social endowments. The idea is that committed individuals find credible the promised long-term gains of the insurgency; their interaction with one another supports this as a common belief. Such long time horizons and common expectations
support cooperative relations with civilians and a high level of discipline within the organization, making possible selective use of violence and a degree of co-governance with civilian leaders.

Weinstein tests this theory by tracing its observable implications in the trajectories of four rebel organizations, the National Resistance Army in Uganda, Renamo in Mozambique, Sendero Luminoso in Peru, and a distinct faction of the latter in the Upper Huaullaga Valley. Weinstein carried out several months of field research on each case, interviewing former guerrillas, military leaders, and civilians in conflicted areas, gathering documents from a wide range of archives (some hitherto unavailable), and compiling data from newspaper articles for statistical analysis. Weinstein shows that the groups varied significantly in the form and extent of the violence they deployed and that initial patterns of endowment shaped subsequent rebel choices concerning recruitment, governance, and the use of force. Weinstein argues that his theory illuminates as well two out-of-sample cases, rebel groups in Colombia and Angola. He also carried out multivariate analysis of patterns of violence in all civil wars since 1945 with an explicit assessment of four cases not lying on the regression line.

In a work that in its methodological and theoretical contribution sets a high standard for research in comparative politics, Stathis Kalyvas (2006) presents a new theory of violence in civil war. His theory begins from the insight that civil wars are nearly always fought as irregular wars in which control of territory depends not on the preferences of civilians living there but rather on their behavior, namely, whether or not they give information to the opposing force. The degree of control an armed group exercises compared to its rival determines its interaction with civilians and therefore its use of violence. Thus patterns of violence follow from patterns of control, rather than vice versa. Kalyvas develops this relationship between control and violence in a formal model that predicts a counterintuitive pattern of selective violence, namely that it will be low in areas where the parties exercise approximately equal control.\footnote{Kalyvas also analyzes the frequent (and counter-productive) use of indiscriminate violence: actors kill indiscriminately when they do not have access to local information to identify combatants and supporters of the other side.}

Kalyvas shows the general plausibility of the argument through anecdotal evidence from dozens of civil wars. He then tests the predictions with data on patterns of violence during the Greek civil war. During his field research in the Argolid, an area in eastern Peloponnesse, he compiled data on approximately 750 civilian deaths in the sixty-one villages of the case study area for the year beginning in September 1943, drawing on archives of the Greek Army, the Greek Communist Party, the regional criminal courts, memoirs and autobiographies, and a variety of European archives. This diversity of archival sources enabled Kalyvas to prod the memories of the 200 residents of the area in interviews. He compiled a database for most of these civilian deaths, recording the identity of the victim and perpetrator, the ties between them, and whether it occurred as a result of selective or indiscriminate violence. He also constructed measures of incumbent and insurgent control, which were clearly
independent of his observations of violence. His multivariate analysis of the resulting database largely confirmed his theory: the model’s predictions prove significant and the predictions of alternative explanations for civil war violence are not borne out. Kalyvas then carried out a number of out-of-sample tests, including multivariate analyses of a second, ethnically divided area and of a dataset of civil war deaths from 136 villages across Greece based on various types of local histories.

An additional strength of the work is Kalyvas’s explicitness about the way in which his initial research on the Greek case (interviews but not in the Argolid, which he later chose as his field site) informed the development of his theory and model (2006, 14–15; see also the methodological appendix). The data gathered in his subsequent research in the field site then served as a test of the theory. Thus the reader is able to trace the dialogue between theory and data and evaluate the overall claims of the work.

5. SOME CHALLENGES OF FIELD RESEARCH

The field researcher confronts some particular challenges in the course of her work due to the personal interaction with research subjects. In this section I discuss some of the challenges involved in face-to-face research but do not address the many practical challenges of field research or the methodological challenges discussed above.13

Because field research depends on personal interaction, field researchers may gather data that suffer from unrecognized selection bias by cultivating ongoing contact with individuals with whom they are more comfortable, for example, individuals who are more educated or hold similar values to those of the researcher. Even if the researcher starts out in the field with such dangers in mind, an element of selection may creep into the sample without the researcher’s being aware of it. For example, the enthusiasm of some of those interviewed for the project may render their views or ongoing participation more acceptable, more accessible, or simply more fun to gather. Their enthusiasm is itself data: why they see the research project as interesting rather than as some alien chore is important to understand. But the lack of enthusiasm on the part of others is also of course data, but data more difficult to gather. Purely personal likes and dislikes may mean that the views of some individuals weigh more heavily than others, thereby shaping the field researcher’s evolving understanding of the topic in ways not always recognized.

Thus the field researcher must manage her own subjectivity, attempting to be aware of how her own proclivities shape her interactions with her research subjects.

13 On field research methods, see Lofland et al. 2006; Emerson, Fretz, and Shaw 1995; Bernard 2002; Rubin and Rubin 2005; PS Symposium 2002; Qualitative Methods Symposium 2004. On the practical challenges of field research, see Barrett and Cason 1997; Devereux and Hoddinott 1993.
The keeping of field diaries and logs, as well as more formal notes of interviews and observations, is one way not only to manage one’s subjectivity but also to comprehend one’s own evolving understanding of interactions and findings as field research data (Emerson, Fretz, and Shaw 1995; Bernard 2002, ch. 14).

Field researchers also face ethical challenges in the field. At a minimum, for field research to be ethical, research subjects must consent to their participation in full understanding of the potential risks and benefits (Kelman 1972; Belmont Report 1979). In the procedures for ensuring informed consent, field researchers must state the purpose of the project in understandable language, inform the subjects of any potential risks (and benefits) of the research, and assure the subjects that their participation is entirely voluntary and that their privacy will be respected. Consent procedures usually include information intended to ensure that the researcher is accountable to his subjects, such as contact information for the institutional review board responsible for the project. Yet whether the research subjects understand the purpose and risks to the extent needed for informed consent is often difficult to determine.

Ensuring the security of field data (important both for practical and ethical reasons) may be difficult, particularly if the research design requires either extended stays in one community, where private spaces may not be respected, or frequent travel between field sites, which increases the risk of theft, inadvertent loss, or confiscation by authorities or others. Field researchers increasingly protect electronic data with encryptions or passwords, but some types of field data (artifacts or maps, for example) are less amenable to such safeguards (at least in the field). A related dilemma concerns the inclusion of sensitive field materials in publications. Some decisions are dictated by the conditions given by individual respondents, who may specify some material as not for publication. But sometimes the field researcher should decide not to use some material even if given permission, because it may in the researcher’s own judgement nonetheless seem too sensitive to publish, for example, likely to identify or implicate the interviewee.

Scholars based in the USA carrying out research involving human subjects must submit their research protocols (including detailed procedures for obtaining informed consent and securing confidential data) to a mandatory review by their local institutional review board, which must either approve project procedures or rule the project exempt. Researchers in comparative politics are increasingly often required also to submit their protocols for review in the country where they will carry out the research. Review of protocols is of course particularly important in contexts

---

14 See National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research 1979, known as the Belmont Report.


16 Review board standards evolve in response to the emergence of new research issues and also the development of new legal standards and research norms. Issues currently under discussion include whether review boards should specialize in particular kinds of research (e.g. social science or medical research), whether informed consent protocols have become too formal and legalistic (providing institutional cover but obscuring comprehension by the research subject), the risks and opportunities provided by new information technologies, and the interpretation of a key phrase in the process, “minimal risk.” See National Research Council 2003 and National Science Foundation n.d.
(such as conflict zones) where the degree of risk to the research subject needs particularly careful evaluation. An unintended consequence of the requirement that changes to research protocols (such as interviewing a new group of people or addressing subjects not anticipated before going to the field) must also be approved by the review board is that the process constrains innovation in the field, as approval may take several weeks.

But approved protocols are rarely sufficient to meet the ethical challenges of field research. The researcher may well confront ethical dilemmas whose resolution depends on her judgement of issues in a particular context.

Lesser dilemmas also arise in the course of field research. For example, field researchers often have to decide whether or not to challenge lies that they are told in the course of their work. This is a practical dilemma but one that may have ethical implications (for example, challenging lies may lead to greater hostility toward the project and perhaps toward research subjects). My personal resolution of this dilemma was not to challenge lies but to invite elaboration in a bland and naive way, which often leads to extremely useful material reflecting the speaker’s ideology, values, analysis of events, and so on.

A challenge many field researchers confront is how to thank those individuals or groups whose cooperation has made the research possible. For many field researchers, this is a source of ongoing discomfort as there may be no acceptable way to do so. Anthropologists and other ethnographers endorse a particular form of reciprocity: materials gathered in the field should be returned to the community of origin. Some years ago this was understood as making publications available to the academic communities of the countries where field research was conducted. Increasingly, however, many ethnographers hold that the field researcher’s obligations extend far beyond the dissemination of publications to include the return of the field materials themselves. But the norm says both too much (not all material should be returned; for example, confidential material should not be) and too little (returned to whom and when?).

Some field researchers may find that sustaining their research role is difficult for emotional reasons. In the field, researchers often go through predictable periods of depression during which they question the meaning and feasibility of their project and whether they are adequate to the task. Such field research “blues” typically occur a few months after entry to the field site, often after the first excitement has worn off, and again after exit (whether or not “the field” is near one’s home institution). These periods often reflect the stress and loneliness of making a transition between cultural settings and of leaving family members or friends behind. For example, the absence of privacy is particularly stressful in many field settings. For many researchers the necessary balance between engagement and observation is another source of stress as

17 For a more extended discussion of the ethical dilemmas confronted in conflict zones, see Smyth 2001; Bell 2001; Wood 2006; for field research generally, see Wilson 1993.

18 See the essays collected in Jaarsma 2002 for extended discussion of the difficulties in implementing this norm.
they come to feel there is an element of deception in the scholarly reserve necessary to observe as well as participate.

Those carrying out research in conflict zones, who may experience intense emotions of fear, anger, outrage, grief, and pity, often wonder why research is worth pursuing over purely humanitarian relief work. And in sharply polarized settings where research objectives require the researcher to work with all parties, researchers may find it stressful to “manage” information from both sides and to engage empathetically with everyone, especially when one side is clearly responsible for much of the violence. The challenges of doing so in some settings may preclude the attempt, leading the researcher to focus her work on only one party to the conflict, with the resultant narrowing of perspective and possible bias.

I mention these emotional dynamics because I am persuaded that inadequate attention to them may lead field researchers to make errors in judgement that may have significant consequences for their research subjects as well as for their research and possibly for themselves personally (Wood 2006).19 In the emotionally challenging circumstances of field research, most people endure extended periods of doubt about their project and their ability to complete the anticipated work on what often comes to seem an impossible timetable. As a result, the researcher may decide, on the one hand, to curtail the original research design in ways that undermine its scholarly value, or on the other hand, persist in an overly ambitious design that results in superficial understanding. Or the researcher may be susceptible to flattering invitations to share their experiences (and inevitably their data) or to take on policy jobs or consultations that offer the comfort of an immediate work product. On a more personal level, the researcher may find it enjoyable to entertain new friends with stories (and data) from their field site (which may compromise the confidentiality of the research subjects) or to embark on relationships that someone may see as compromising the project (and perhaps lead to their no longer sharing information with the researcher). Or the researcher may become frustrated with the research role and decide to “make a difference” by passing on field data “confidentially” to some (supposedly responsible) organization.

Good field researchers find ways to manage these challenges and take care to shelter their ongoing research from their own emotional vagaries, sometimes in the form of close friendships with those similarly positioned as “outsiders” and sometimes through interim visits to their own academic institution for consultations but also to reaffirm a sense of engagement with the academic community. In contrast to the usual immersion for a year or two in field research that is typical in comparative politics, some research designs themselves provide for periods away from the field, as when the researcher carries out a sequence of visits to different field sites that allows for a return to her home university along the way.

19 Which is not to say that the emotional vagaries of field research should be inflicted on the reader. See Ellis 1995 for her analysis of how her personality shaped her fieldwork in a fishing village on Chesapeake Bay. See also Clark 1975 and Gans 1986.
6. Conclusion

It is hard to imagine how the data and the findings of these works could have been gathered and supported without field research.

But given the declining cost of non-field methods, should researchers not invest more in those methods? Because data and analysis that depend on field research and those that depend on other types of research are usually complements in the production of useful knowledge, not substitutes, the declining price and growing availability of the latter raises, rather than lowers, the marginal productivity of investment in the former. Whether or not researchers should shift relative investment toward non-field methods cannot be decided in the abstract, as it depends entirely on the strength of this complementarity for the particular topic. Where this complementarity is strong, the case for increasing the share of scholarly resources going to field research is compelling.

But the obstacles to making valid causal inferences based on field data are of course formidable. In particular, research based in large part on participant observation is not easily amenable to replication in part because so much follows from the quality of the relationship between the researcher and the subjects. For example, an ethnographer less skilled than Scott might well have missed the significance of the “weapons of the weak” entirely and argued that class relations in the villagearmed the theory of ideological hegemony. Studies of the same ethnographic setting by different ethnographers within a relatively short period of time are not common, however.20 Much more common are studies of the same community after a lapse of a few decades (by the same ethnographer or a different one); differences between the resulting ethnographies may reflect either different skills and experiences or cultural change in the interim.21

While the subjectivist turn in recent anthropological writing often obscures the research subjects from view, the tendency for ethnographers to provide more details of their engagement with the research subjects does allow the reader to judge the quality of the ethnography, a process often difficult for ethnographies written as though the observer were a fly on the wall whose presence had no effect.

If the scholarly community is to realize the promise of field research, better training in field research methods should become part of graduate training in social science. For all too many students, the first encounter with the challenges of field research is in their field site where the consequences of practical, ethical, or

20 One example of an ethnographic “restudy” was carried out in the central Mexican village of Tepotztlán. Oscar Lewis, whose field research occurred seventeen years after that of Robert Redfield, confirmed some of Redfield’s findings, but argued that his work did not report adequately the degree of conflict and violence in the village, perhaps because of Redfield’s theoretical and normative proclivity toward understanding rural culture as idealized “folk culture” (Lewis 1970).

21 Some examples that are often cited as restudies were not based on significant new field research within a reasonable period of time. Derek Freeman argued that Margaret Mead had misrepresented key aspects of Samoan culture, in particular, the degree of sexual freedom of adolescent girls (see Orans 1996 for a review of the debate). However, Freeman’s work was published more than half a century after Mead’s, raising the possibility that the discrepancy in findings was due to the changes in the culture. For examples and discussion, see Kemper and Peterson Royce 2002 and Carmack 1988.
methodological mistakes may not only undermine the project but may undermine or endanger the research subjects. Where resources are not sufficient to offer courses focused on field methods, provision should be made for students to attend the course offered every January by the Consortium on Qualitative Research Methods (www.asu.edu/clas/polisci/cqrm).

However, training should go beyond the standard seminar discussion of research design to include the hands-on trying out of various methods. Instructors should consider having students practice fieldwork techniques through the carrying out of a local research project over the course of a semester. Many field researchers hold that field techniques can only be learned by doing them but that is no reason such learning need take place in the dissertation field site. Participants in the class would think through issues of entry to the field site, the identification of key respondents and sample frames, and the minimization of selection bias. They would practice particular skills, including participant observation, formal and informal interviews, and oral histories. The class would together brainstorm over the problems and opportunities encountered, in part by reviewing various types of field notes. And class discussion might include explicit consideration of the emotional dynamics of research in the field.

Urban sociologists often teach such practice-based courses. The challenge for the comparative politics student is to define a project within reach of her university that is nonetheless engaging and that hopefully has some relation to her core interests. For example, a student who intends to analyze the evolution of policing policy in Rio de Janeiro could study policing policy in her university town. While such semester-long projects are unlikely to serve as comparative cases, the student will at least have carried out a field research project and will carry that experience to the “real” field site.

References


For an example of such a course, see the syllabus for my Qualitative Field Research course, available on my website, pantheon.yale.edu/~ejw33.


