

and revise not only the assumptions animating existing theories, but more important, to reevaluate the accepted wisdom on how and why specific outcomes are determined. In the process, we are restoring a sense of richness and depth that comes from the close study of specific people in particular circumstances. Integrating the "extraordinary diversity that makes up the Chinese political experience" (Perry, 1994b: 712) into scholarly debates promises to fuel increased exchange and expansion of knowledge in the social sciences.

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More Than an Interview, Less Than Sedaka Studying Subtle and Hidden Politics with Site-Intensive Methods

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The field of Sinology has benefited greatly in the past twenty years from a rich array of studies based on ethnography and participant observation.¹ Anthropologists, not surprisingly, have led the way (Bruun, 1993; Chen, 2003; Fong, 2004; Friedman, 2006; Gladney, 1996; Hertz, 1998; Jacka, 2004; Jankowiak, 1993; Jing, 1996; Judd, 1994; Kipnis, 1997; Litinger, 2000; Liu, 2000; Murphy, 2002; Notar, 2006; Perkins, 2002; Pun, 2003; Rofel, 1999; Schein, 2000; Watson and Watson, 2004; Yan, 1996, 2003; Yang, 1994; Zhang 2001). A number of sociologists also embrace these methods (Calhoun, 1994; Chan, Madsen, and Unger, 1992, 2009; Farrer, 2002; Lee, 1998).² Whatever its disciplinary origin, and whether the specific topic is interpersonal relationships or village governance or migrant entrepreneurs, much of this work is strongly *political* in orientation. It hardly seems possible to come away from such books without acquiring immensely valuable knowledge about the workings of power in China, particularly at the local level.

Several political scientists have applied versions of these research techniques in China as well.³ But these methods have a more problematic relationship with the discipline of political science. Though members of our field have long employed ethnography and participant observation (which I group together below under the term "site-intensive methods"), such approaches have never been seen as mainstream. The discipline is now going through a process of energetic debate over what constitutes important knowledge and what methods usefully contribute to such knowledge. Practitioners of other qualitative methods began some time ago to weigh in with articulate statements explaining

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¹ This is to say nothing of earlier work by Fei Xiaotong, Sidney Gamble, and others. Also, this chapter deals only with English-language studies.

² I owe thanks to Elizabeth J. Perry, Li Zhang, and Ethan Michelson for bringing to my attention some of the items mentioned here.

³ In addition to the works discussed later, see Blecher and Shue (1996), Friedman, Pickowicz, and Selden (2005), Hurst (2009), Steinfeld (1998), K. Tsai (2002), (2007).

how what they do fits into the process of building and testing political science theory. A new generation of ethnographers and participant observers has recently emerged as well (Schatz, 2009b). As Schatz notes, members of this group differ among themselves, with those identified as interpretivists challenging multiple aspects of the ontology and epistemology of mainstream political science. Others, he writes, carry out ethnographic work in a qualified neopositivist mode, one that "uses attention to detail to generate middle-range theories, that considers cumulative knowledge a possibility worth pursuing, and that is optimistic about the scholar's potential to offer contributions" (Schatz, 2009a: 14). While I applaud the idea that politics should be studied from many perspectives, not merely those fitting within scientific paradigms, what I have to say here falls within the latter category. I argue that political scientists of many stripes can and should appreciate the value of the fieldwork techniques under discussion.

This chapter draws on examples from research on China as well as other locations. Most of what it has to say is not specific to any one country. It makes an argument that site-intensive methods form a particularly appropriate and often necessary part of the apparatus used to study China, though the argument pertains just as well to other non-Western and other politically repressive settings. I wish to develop the theme that those who use ethnography and related techniques need to build bridges with one another rather than digging moats as is sometimes the case. This means cultivating greater awareness of what specialists of other regions are doing, among other things.

The revitalization of ethnographic work in political science that I am advocating – building on existing studies – has two components to it. The first concerns how we, the practitioners, conceptualize our work, what we do in the field, and how we write up and package our findings. We should strive to come together on accounts of this methodology that can guide what we do and help make the research process as efficient and productive as possible. The second concerns the discipline more generally. To some extent, bringing site-intensive methods to full fruition in political science requires efforts to shape the discipline itself. It is necessary to make more clear to other political scientists how these methods fit into the methodological world with which they are familiar. Only then will this type of work, whether on China or anywhere else, achieve its maximum potential.

This chapter particularly concerns *trade-offs* that are inherent in site-based research, notably the balancing of breadth (studying more units, maximizing variation among them) and depth (getting the most validity, richness, and understanding out of each unit). Although by no means uniquely so in the social sciences, political scientists are obliged to confront issues of generalizability. Our work is expected to speak in abstract terms to conceptual questions of broad interest to our peers, and the explaining of variation is prized. Sometimes the most appropriate research strategy lies at one or the other end of the spectrum: training microscopic scrutiny on a single locale, as in James Scott's (1985) famous study of the Malaysian village he called "Sedaka," or applying thin

measures to a large number of sites or respondents, as in much survey research. Often, though, there are reasons for pursuing a compromise approach in which a small or medium number of units is observed in ways that obtain some fraction of the depth that can be achieved in traditional ethnography. Although there can be no formula that resolves this dilemma neatly for all projects, this chapter attempts to conceptualize and clarify some of the factors involved.

Finally, I happen to be a committed proponent of applying multiple or "mixed methods" to attacking empirical questions in political science. In my own work on neighborhood organizations in China and Taiwan, for example, I have adopted an approach that pairs the deeper insights from participant observation and interviews with thinner but more extensive survey data. Some of the other research considered in the pages that follow is also multimethod, and the benefits of exploiting more than one type of information will be evident in the discussion. However, ethnographic and participant-observation methods need to be understood and justified on their own terms as well as in combination with other approaches.

SITE-INTENSIVE METHODS

The methods that generally go under the labels *ethnography* and *participant observation* occupy a somewhat awkward place in political science (Bayard de Volo and Schatz, 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 Fenno's *Home Style* (1978) 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-World War II generation and presumably earlier (Banfield, 1958). 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 (non-China examples include Adams, 2003; Allina-Pisano, 2004; Bayard de Volo, 2001; Cammett, 2005, 2007; Galvan, 2004; MacLean, 2004, 2010; Roitman, 2004; Schatz, 2004; Straus, 2006).

And yet, these methods remain marginal. I think it is fair to say that they only occasionally crop up in methodology curricula. Even within the world of the American Political Science Association's organized sections on qualitative and mixed methods and the stimulating ferment that has been fostered in recent years, they have shown up so far as a distant cousin. Very little in this vein appeared on the 2006 Institute for Qualitative Research Methods (IQRM) syllabus, for example. Two of the most important recent books on qualitative methods, although immensely 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, particularly when research is conducted

in foreign-language settings. They are thus rather difficult to recommend in good conscience to the average graduate student who is under pressure to minimize time to completion. Also, 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 the previously mentioned divide between "interpretivists" and "positivists," which looms large in some recent accounts (Burawoy, 1998; Schatz, 2006; Yanow, 2003). There is also a hesitance on the part of many to go beyond describing what they did in their own research to suggesting sets of general procedures for others to follow. Efforts within political science to spell out the benefits of these methods for building and testing theory have so far been limited. Finally, and perhaps most important, there has been no real push to build a coalition behind the critical appreciation, application, and teaching of ethnography and participant observation.

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 or users group within the discipline. This enterprise would start by identifying common ground in a related set of *approaches to the gathering of sources, evidence, and data*. This would, one hopes, cross-cut and set aside underlying epistemological divides.

An umbrella term bringing together a set of related methods might be useful. One such term would be *site-intensive methods* (SIMS), referring to the collection of evidence from human subjects within their ordinary settings, where their interaction with the surroundings informs the study just as the researcher's own questioning does. This implies the need for a deeper engagement with a site, context, locality, or set of informants than is obtained from, for instance, telephone surveys or some types of one-time interviews.⁴ This term would subsume most of what is referred to as "ethnography" and "participant observation," and perhaps some forms of other practices, such as focus groups. It would also highlight the diverse forms that this research takes, including studies that strive for a high degree of depth in a single locale along with those that 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 somewhat reluctantly suggest some new term like SIMS in part because I am not convinced that other categories are up to the task of bringing together the most useful and productive coalition of researchers within political science. "Ethnography" on its own has the advantage of a long pedigree within the social sciences and a voluminous methodological literature, especially in anthropology. But it may have drawbacks as well. Some versions of the anthropological model may set the bar too high in implying that months or years of immersion are required to obtain insights. It may connote a holistic orientation according to which the entirety of a community or locale must be

⁴ In fact, some kinds of surveys and interviews require considerable stage-setting and trust-building; see Posner (2004).

comprehended to make sense of any one part. It should be pointed out that the meaning and practices of ethnography today are undergoing evolution and sharp debate; by no means do all practitioners see ethnography as limited to these forms.⁵ 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 by this concept may not be coterminous with what it is 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 that only 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 whereas others see the two concepts as interchangeable.

As noted earlier in this chapter, two studies, by Fenno (1978) and Scott (1985), may be the most widely read examples of site-intensive methods in political science. Reflecting on these books in particular provides an opportunity 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 almost polar opposites in their approach. Scott describes his project as a "close-to-the-ground, fine-grained account of class relations" (Scott, 1985: 41) in a Malaysian village, population 360, which goes under the pseudonym of Sedaka. Situating his methods in the ethnographic tradition of anthropologists (Scott, 1985: xviii, 46), Scott states that he spent at least fourteen months in Sedaka, interviewing, observing, and taking part in village life.

Fenno's (1978) work was motivated by questions concerning the relationship between politicians and those they claim to represent. "What does an elected representative see when he or she sees a constituency? And, as a natural follow-up: What consequences do these perceptions have for his or her behavior?" (Fenno, 1978: xiii). His approach was to spend time in the company of members of the U.S. House of Representatives 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 within the tradition of participant observation as practiced by sociologists and other political scientists; ethnography is not mentioned, as far as I can tell (Fenno, 1978: xiv, 249, 295). In the text of the book and its long methodological appendix, Fenno candidly and rather self-deprecatingly explains his modus operandi of accompanying politicians wherever they would let him tag along, building rapport, recording their remarks, and asking questions when possible.⁶ Clearly this was

⁵ Marcus (1998), for example, provides arguments in support of multisited studies, albeit with reservations.

⁶ He also discusses his methods in a 1986 *APSR* article and other essays, all reprinted in Fenno (1990).

a far "thinner" form of engagement with a research milieu than was Scott's village study. Relative to a single-site project, Fenno traded depth for breadth, studying eighteen different representatives and thus obtaining substantial variation on characteristics such as party affiliation and seniority (Fenno, 1978: 253–254). The total time he spent with each representative ranged from three working days to eleven, averaging six (Fenno, 1978: xiv, 256), and on some of those days the research subject was available only part of the time.⁷

Despite their differences, these books can readily be seen as belonging to a common category. Both scholars were propelled into the field by strongly theoretical motivations – theories of hegemony and false consciousness 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 they portrayed it) existing accounts relied on assumptions that needed to be tested or fleshed out through on-the-ground study.

WHEN SHOULD RESEARCHERS USE SITE-INTENSIVE METHODS?

What are the circumstances under which these approaches are particularly valuable – sometimes even necessary – for political research? One way to view it is that such methods are especially valuable when what we are studying is *subtle* (for example, relationships, networks, identities, styles, beliefs, or modes of action),⁸ and when what we are studying is *hidden*, sensitive, or otherwise kept behind barriers that require building trust, waiting to observe unguarded moments, or otherwise unlocking access.⁹ Scott's and Fenno's projects serve again as examples. In both books, to simplify somewhat, the fundamental subject of study was individuals' perceptions (peasants' views of class relations and politicians' views of their constituents). 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.

Subtlety, as considered here, is in part a relative concept. The ways in which site-intensive methods are best applied to a problem may change significantly depending on the existing state of theory. The success of a project like Scott's, in the way that he framed it, hinged on the scholarly readership accepting false consciousness as a viable explanation or foil. How a specific application of site-intensive methods is pitched depends in part on the sophistication of existing hypotheses and data. If everyone agrees that X_4 has a positive relationship with Y when X_1 , X_2 , and X_3 are controlled for, and the debate is over the

⁷ One could go on about the contrasts. Scott is, of course, particularly attuned to the voices and experiences of the subaltern, whereas Fenno does not conceal an often admiring sympathy for the elites whom he studies.

⁸ In fact, most concepts I can think of in the realm of politics contain subtleties that probably deserve up-close scrutiny. At the same time, this is not at all to deny that less-intensive methods can also shed light on these topics.

⁹ On hidden aspects of politics, see also Scott (1990) and Kuran (1995).

magnitude of the coefficient, whether the relationship is linear or quadratic, and whether an interaction term with X_5 is required, then merely observing that X_4 and Y appear linked does not provide as much added value as it would have earlier in the research cycle. But by no means are site-intensive methods appropriate only for little-studied topics or when existing theories are crude. To continue the above analogy, up-close fieldwork could still usefully point out that X_4 is more complex than we thought it was and requires new measures; that, in real life, the causality seems in fact to run from Y to X_4 ; or that the relationship is conditioned by other factors that previously had been ignored entirely.

This leads to one answer – in the affirmative – to the question of whether such methods have special applicability in a country like China. It would be a mistake to argue that sociopolitical phenomena in China are somehow "more subtle" than those elsewhere. But one might point out that China has long produced events and things that, at least initially, either confounded or else fit only problematically within the social science categories created in North America and Western Europe. A list would include the CCP's rise to power; the Cultural Revolution; local associations; patterns of interest representation; modes of contention and grievance articulation; rapid economic development under Communist Party rule; the transition from socialism; and even the PRC's current regime type. The question, "What is this phenomenon I am looking at in China a case of?" is not always easily answered.¹⁰ This has created stumbling blocks but also opportunities for innovative contributions. Thus, as in the study of other non-Western systems, empirical phenomena in China need to be analyzed carefully before determining their relationship with existing concepts, and site-intensive methods are well suited for this.

The other part of this answer concerns "hidden" subjects. It should be emphasized again that even in open, liberal political systems, certain important topics remain hidden, such as a politician's true perceptions of his or her constituents. In China, barriers created by state controls lead, in some circumstances, to obstacles to access and concerns about the validity of the data that are obtained. China today is not as open as, say, Taiwan, nor as closed as, say, North Korea or Myanmar. Particular institutions or individuals vary greatly in how available they are. Occasionally, one can obtain what is needed from a government agency just by walking through the front door. For some topics, a short phone or questionnaire survey conducted by a stranger may be adequate. But a common configuration is one of *formal barriers and informal porosity*. In these circumstances, valid or usefully detailed information cannot be acquired in a single visit or conversation but can be obtained through a more patient approach. The proportion of settings in which relatively intensive strategies are required to obtain any valid information is large in China compared to countries with more open political institutions and stronger norms

¹⁰ I thank Laura Stoker of the University of California, Berkeley, for insisting that students ask themselves the "What is this a case of?" question.

of transparency and disclosure. Conversely, the rewards to approaches that involve trust-building and over-time information gathering are correspondingly large there and in other closed or semiclosed political systems.

For these reasons, certain kinds of projects require extensive cultivation of research sites whether or not they involve traditional aspects of ethnography, such as direct observation of human behavior. For example, Melani Cammett, in her project on Hezbollah in Lebanon, went through an elaborate process of establishing links with the organization in order to lay the groundwork for a series of interviews with its members.¹¹ Relatedly, studies like Whiting (2001) and Remick (2004), although not ostensibly ethnographic in nature, clearly relied on building rapport and networks within the two or three localities upon which they were based.

In my own work, I have used site-based methods as one way to study the state-fostered neighborhood organizations of China and Taiwan. These are known as Residents' Committees (RC) or Community Residents' Committees (社区居民委员会) in the former and neighborhood heads (里长) and block captains (邻长) in the latter. My research in China took place in seven different cities but focused on the city of Beijing. My goal there was to study several neighborhoods in different parts of the city and in a variety of socioeconomic settings. I wanted to choose my sites more or less at random. I also wished to avoid the "model" neighborhoods that each district establishes; in major cities, local and international visitors alike are routinely shepherded to such showcases. Equipped with a letter of introduction from my "host unit," the Chinese Academy of Social Sciences, but otherwise without special permission, I knocked on the doors of RC offices, identified myself, described my research, and asked to come in and chat.¹² In cases where the committees were receptive, I made repeated follow-up visits and eventually established a set of ten neighborhood sites in this fashion.¹³ I spent about thirty half-days (morning or afternoon) with two of the committees, and made twelve to fifteen trips to most of the others over fourteen months of dissertation fieldwork (1999–2000) and during three shorter follow-up stays (2003–2007).¹⁴ On each visit, I sat in the offices, chatted with the staff, and watched and listened as they went about their work.

The most fundamental purpose of my research was to understand the power dynamics of neighborhood communities and the kinds of relationships that

¹¹ As of June 2009, papers from this project were under review but not yet published.

¹² In two cases, initial contact was made informally through acquaintances, and in two other cases I took a more official route involving a personal contact at a Street Office but also obtaining formal permission via the city government and my host unit's foreign affairs office.

¹³ In five neighborhoods I was turned away, either by the RC itself or by government officials. In two of these cases I was accompanied by a police officer to the local police station (派出所) and told not to return to the neighborhood. In two of the ten sites, after six successful visits the committees were told by the authorities to discontinue contact with me.

¹⁴ In three of the ongoing sites I offered to teach English to staff members or their children, and for several months I ran a weekly language class in my apartment.

state-sponsored organizations at this ultra-local level have with the residents they notionally represent. These topics are subtle in that they feature complexity and nuance, and in that they vary across places, people, and situations. They are hidden, or at least potentially hidden, in that RCs or constituents might not speak frankly to strangers about these relationships, and in that interaction between them generally leaves no publicly available record. Direct observation does not automatically overcome these challenges. But by building familiarity and trust, by probing gently, and by happening to be present as the committees dealt with all manner of situations and problems, I could learn much about this point of contact between state power and urban society. The RCs thus exemplify the pattern described above: there are formal impediments to studying them, but it is often (though not always) possible to obtain entrée, especially via informal channels.

HOW DO SITE-INTENSIVE METHODS CONTRIBUTE TO THEORY?

In some circumstances, as the previous discussion indicates, gathering any valid data at all requires building ties of trust and familiarity with one's research subjects and the institutions in which they are embedded. This alone is a powerful rationale for site-intensive methods. But apart from this, it may be constructive to spell out at least four ways in which ethnographic work contributes to the building of social science concepts and theory.

First, ethnographic work can *inductively generate new hypotheses*. For Fenno (1978), the choice to conduct his field research by "soaking and poking" was in part driven by his conceptualizing the project as breaking trail through wholly uncharted territory. He argues that political scientists had previously all but ignored representatives' understandings of their districts and how they behaved there (Fenno, 1978: xiii). Given this theoretical tabula rasa, a "totally open-ended and exploratory" approach was needed:

I tried to observe and inquire into anything and everything these members did. I worried about whatever they worried about. Rather than assume that I already knew what was interesting, I remained prepared to find interesting questions emerging in the course of the experience. (Fenno, 1978: xiv)

Whether it was absolutely necessary for Fenno to cast the project in this way seems open to question. In the course of acknowledging previous research, he states that "political science studies conducted in congressional constituencies have been few and far between," but goes on to cite no fewer than eight books and articles on this topic as just the ones that have been "the most helpful to me" (Fenno, 1978: xvi, n. 9). Moreover, I fear that Fenno's heavy emphasis on the unstructured nature of his project may have helped lead many members of the discipline to believe that participant observation is necessarily speculative and free-form, useful, at best, *only* for the earliest and most preliminary phases of a research cycle.

Nonetheless, this open-ended approach convincingly results in insights that can be taken as new hypotheses concerning politicians and voters' behavior. Though Fenno soft-pedals them somewhat, the hypotheses that he puts forward include these: House members in the early phases of their careers are most attentive to their districts; members focus most on constituents who are most organized and thus accessible in groups; and constituents care about personal attention, respect, and the assurance of access at least as much as they care about the representative's congruence with their own issue positions (Fenno, 1978: 215, 235, 240–242). His book thus provides a good example of the value of participant observation for inductively generating new theoretical claims.

Some of Calvin Chen's findings from his extensive ethnographic studies of Zhejiang enterprises can also be understood as falling into this category. As his chapter in this volume reports, among other things he discovered that Communist Party institutions were being established in these firms. As Chen indicates, without the benefit of the "worm's-eye view," it would be easy to misinterpret this as the authorities imposing their organizational hegemony, or else as the companies going through the motions of compliance in order to get a political pass. Instead, a surprising third possibility emerges: that the firms actively encourage this development in order to enlist the party's assistance in resolving social tensions among the workforce (C. Chen, 2006, 2008).

Second, site-intensive methods can also be used to test hypotheses by gathering qualitative measures of their observable implications. This formulation derives from King, Keohane, and Verba's *Designing Social Inquiry* (1994: 28–31). The notion that qualitative researchers can maximize the leverage they apply to the testing of hypotheses by identifying and evaluating as many as possible of their "observable implications," of which even a single case may have many stands, as one of the more constructive and less controversial messages of this book. Naturally, this echoes long-standing practices of case-based researchers.

The fine-grained accounts of villagers' narratives, rituals, insults, and struggles in Scott's *Weapons of the Weak* (1985) can be read as a series of tests of the "false consciousness" hypothesis. The village ne'er-do-well, Razak, provides one: "As a beneficiary of local patronage and charity, however reluctantly given, one might expect Razak to entertain a favorable opinion of his 'social betters' in the village. He did not.... 'They call us to catch their (run-away) water buffalo or to help move their houses, but they don't call us for their feasts ... the rich are arrogant'" (Scott, 1985: 12). Others appear in perceptions of local misers (Scott, 1985: 13–22), the celebration that results when a job-displacing combine bogs down in the mud (Scott, 1985: 163), and the conflict over the village gate that had protected villagers' paddy-hauling jobs (Scott, 1985: 212–220).

In my Beijing research, I used evidence gathered from the neighborhood sites to test several hypotheses about relations between constituents and their Residents' Committees. One such hypothesis concerned clientelism. In China

and around the world, relationships between ordinary citizens and power-holders can take the form of patron-client ties, typically involving exchanges in which subordinates provide political support while superiors provide material goods and opportunities such as jobs. But my RC office visits showed that the committees' activists and supporters obtained few or no tangible rewards for helping to keep watch over the neighborhood and conveying information to and from the committees. They often received only token gifts such as towels and bars of soap at annual parties. The committees, closely managed by the Street Offices, had little latitude to channel state benefits toward them, and certainly not career opportunities. Instead, the activists' motivations proved to be similar to those of volunteers everywhere: a sense of pride and importance, and the pleasures of sociability. Private interviews with urban residents and quantitative surveys helped to reinforce this conclusion, but participant observation was crucial. Only by observing the activists day after day, by listening to them converse with the RC staff, by talking about these relationships with committee members who were willing to speak frankly about their work, and by acquiring a firsthand sense of just what material resources the committees control (and how limited these resources are) did I become convinced that the clientelism hypothesis is not the key to understanding this aspect of the state-society relationship.

Although the words, attitudes, and behavior of local interlocutors constitute one form of evidence – essentially, data points – that can be gathered to test hypotheses, this evidence can also take a different form, that of *causal process observations*. Seawright and Collier define such an observation as "an insight or piece of data that provides information about context, process, or mechanism, and that contributes distinctive leverage in causal inference" (Brady and Collier, 2004: 277). The search for such observations is essentially the same as what is meant by "process tracing." In political science, some of the most prominent methodological discussions of process-related evidence have taken place in the context of case-based research using historical sources.¹⁵ The logic is the same for ethnography and participant observation. Indeed, site-intensive methods may be *even better* suited for gathering causal process observations, as they allow for active probing by the researcher rather than relying on the passive analysis of extant sources.

Lily L. Tsai's (2002, 2007a) multimethod research on conditions influencing public goods provision in Chinese villages provides outstanding examples of causal process observations derived through participant observation. In this project, much of the analytic power comes through quantitative evidence, in the form of a survey of 316 villages in four provinces, as well as a survey of villagers. Regression models estimated on these data offer evidence that, *ceteris paribus*, villages that possessed certain kinds of social institutions – notably,

¹⁵ Chapter 10 of George and Bennett (2004) gives an overview of process-tracing with an eye toward research based on case studies drawing on historical documents. See also Sidney Tarrow's chapter in Brady and Collier (2004).

temple associations and village-wide lineage groups – also tended to provide good roads and schools. Tsai further employs a series of focused qualitative comparisons among at least nine villages to elucidate the causal link between these social institutions, termed “solidary groups,” and the outcomes of interest. Each village case study is derived from visits of between two and twenty days and is presented in the space of several pages. Together they offer vivid illustrations of the causal processes that are in play. In Li Settlement, for example, leaders rallied their constituents on the basis of a common lineage and community spirit in collecting donations to pave a road; in Pan Settlement, by contrast, long-standing conflict among the sublineages impeded similar efforts.

A fourth way in which site-intensive methods make contributions is by *creating, correcting, and refining theoretical concepts* and the categories within which they are understood, measured, and analyzed. As Henry E. Brady points out in a defense of qualitative methods in political science more generally:

Concept formation, measurement, and measurement validity are important in almost all research and possibly of paramount importance in qualitative research. Certainly notions such as “civil society,” “deterrence,” “democracy,” “nationalism,” “material capacity,” “corporatism,” “group-think,” and “credibility” pose extraordinary conceptual problems just as “heat,” “motion,” and “matter” did for the ancients. (Brady, 2004: 62)

Within the China field, the work of Kevin J. O’Brien and Lianjiang Li on “rightful resistance” springs to mind as embodying the importance of protracted, up-close study for concept formation (O’Brien, 1996; O’Brien and Li, 2006). The authors note that their work to date leaves a number of avenues open for later exploration, such as explaining when governments react negatively rather than positively to these particular calls for justice and exploring regional variations (O’Brien and Li, 2006: 114). The project’s major éclat stems from its pioneering elaboration of an important category of contentious collective action, with wide application both in China and elsewhere, along with detailing the causes behind it and the dynamics through which it plays out in interaction with successive levels of government. Although a variety of sources are exploited, not all of which are ethnographic, the authors make clear that a relatively small number of village sites in which extensive access and deep background were available played a crucial role in launching and guiding the study (O’Brien and Li, 2006: xi–xvii, 131–133, 139–141, and personal communication with O’Brien).

TRADE-OFFS: DEPTH VERSUS BREADTH

Would *Weapons of the Weak* (Scott, 1985) have been more compelling (or less) if it had involved multiple village sites? What if Fenno (1978) had tracked just four politicians, but spent months with each instead of days?

All research involves trade-offs. In ethnographic and participant-observation studies, by their very nature, the researcher is choosing to invest time in observing and developing relationships with particular sites and informants. Given a finite amount of time and other resources for the project, there is thus an inherent trade-off between depth and breadth: between working more intensively at a smaller number of sites (or a single one) and developing a larger number of sites but spending less time at each. How many sites, then, should be developed in a given project?¹⁶

Site-Level Variation

The answer to this question depends in part on the number of site-level independent variables and the nature of variation within them. Scott discusses reasons he chose Sedaka: its apparent typicality as well as the fact that previous studies had established a baseline from which to assess change (Scott, 1985: 90). He does not seem to justify in an explicit way his decision to focus on just one village.¹⁷ Implicitly, the reasons seem to be that long-term immersion in a single place was required to obtain access to “hidden transcripts,” and that variation within the village itself (between elites and poor, and among different informants) was a more important focus than, say, interregional variation between Kedah and Johore. The overall framing of the research also seems to discourage the exploring of different sites: the denizens of Sedaka are intended to speak for peasants everywhere.

In the case of *Home Style*, Fenno (1978) adopts a casual tone toward the choice of his eighteen congressional representatives (“I make no pretense at having a group that can be called representative, much less a sample” [Fenno, 1978: xiv]). But it is a sample, of course, and he makes clear that he selected it in such a way as to observe members and districts of different parties, regions, races, ages, levels of seniority, and electoral competitiveness. Thus, he pursued the familiar small-n strategy of obtaining variation on a number of potentially important independent variables. The problem of small sample size remained, but Fenno’s logic is clear.

Up-Front Costs of Site Development

Each site involves what might be called start-up or up-front costs, referring to expenditures of time and effort as well as other costs. What is the minimum, or optimal, degree of access required to obtain valid information for a

¹⁶ An essay by Maria Heimer (2006) contains a useful discussion of the choices she made in her work on local cadres, traveling to twelve counties and pursuing a tightly focused set of questions in each. More generally, the volume in which this appears, *Doing Fieldwork in China*, will be of interest to many readers of this book.

¹⁷ To be more precise: the study is hardly limited just to Sedaka; it brings to bear evidence concerning the region as a whole, and Scott mentions excursions to nearby villages. Still, it is framed as a single-village study.

particular research effort? In my encounters with the Residents' Committees of Beijing and other mainland cities, in most cases it appeared that at least three or four visits were required to begin to cross a relaxation threshold. During the initial visits, the atmosphere was at least a little tense and conversation stilted. Obtaining answers to straightforward factual questions – what's the population of the neighborhood? how many low-income residents are there? and so forth – posed little problem, but open-ended questions typically resulted in guarded, bland responses. In most although not all of the sites, this wariness gradually gave way to a much more accepting and, in some cases, even welcoming atmosphere. Thus the early time investment paid dividends by allowing me to observe something closer to the committees' ordinary day-to-day work and interaction with constituents. For present purposes, the point is that the greater the costs and risks of establishing each site, the fewer sites a researcher can afford to develop given his or her limited resources.

The Nature of the Data Sought

This is, of course, typical for ethnographic research, but what is particularly important to point out is that the amount of time required for building trust and rapport (in this and other settings) depends on the type of information needed. If the point were merely to ascertain basic facts of the sort that the RCs themselves regularly collect and often post on their office walls, such as the number of women of child-bearing age within the jurisdiction, this could probably have been accomplished in a single visit, depending on the credentials one could present. To understand something partially hidden but limited in scope – for instance, the actual and highly state-managed process through which RC elections are run – would have required just a few visits.

On the other end of the scale, to aspire to write a comprehensive "neighborhood study" on the model of the best urban anthropological work (e.g., Bestor, 1989) would have required drastically reducing the number of cases. It would have meant applying much more comprehensive and sustained efforts than I brought to bear at even my most intensive sites. It also would have required a strategy of regularly going out within the same neighborhood and talking to a wide sample of residents, in private and independent of their interactions with the RC itself. This would have been possible in my project, but might have threatened the premises of my tacit understandings with the committees. Instead, I chose to do my citizen interviews and questionnaire surveys (designed to get at residents' opinions in a private way, removed from the RC members' presence) in neighborhoods other than those where I was conducting participant observation.

Optimizing Validity

One question to consider in choosing a research strategy is whether the time and effort involved in ethnographic work are justified relative to what can

be obtained through one-time interviews. For many purposes and in many settings, the essential things that the researcher needs to understand can be ascertained in a single session. By minimizing the time spent with each subject, the number of interviewees can be maximized, thus potentially obtaining high degrees of variation and some or all of the benefits of large-*n* research. Even to many qualitative researchers, the idea of returning to an informant for a second or a tenth encounter may seem like a puzzling waste of time.

Some of the reasons one might want to return to a site multiple times have already been made clear: because trust and familiarity are required to obtain information. But there are other reasons as well. First, even the most cooperative and forthcoming of informants may "change their stories" over the course of a single interview and between interviews. It is well established in the world of survey research that different question wordings, question orderings, and contexts can lead to quite dissimilar responses. Similarly, interviewees can reveal different and possibly conflicting sides to their beliefs and experiences on different occasions. Moreover, if the research site is such that multiple informants are present at any given time, their relationships with one another may strongly condition what is revealed. If the point of the in-depth research is to obtain the truest possible measure of an individual's perspective, the extra validity that can be gained through multiple sessions must be considered.

This is all the more evident when the project relies on critical informants, particularly when their memories must be plumbed for recollections of events that took place years or decades earlier. Peter Seybolt's *Throwing the Emperor from His Horse* (1996) serves as an example. Seybolt constructed a political history of an impoverished Henan village through a series of long interviews with Wang Fucheng, who served as its party secretary from 1954 to 1984. The interviews were clustered in four visits, with the first in 1987 and the last in 1994. Several other villagers also provided information, but Wang was central. As the author describes it, on each visit the relationship between Seybolt and Wang (and between their families) deepened, and further layers of memory and nuance were added to the account. This book simply could not have been written on the basis of the author's first visit alone.

A final circumstance requiring multiple or protracted encounters occurs when the project relies significantly on watching the subjects interact with their surroundings – for instance, by observing events that crop up in the course of the visit. Fenno's (1978) research drew heavily on this practice. He points out that his project would not have been successful had it taken the more conventional form of forty-five-minute audiences in the congressional representatives' Capitol Hill offices. He depended on being at his subjects' side as they careened from one home-district event to the next, on noting their actual behavior and demeanor at these events, and on catching them in reflective moments as they unwound after meetings with one or another part of their constituencies.

Residents' Committee offices present broadly similar opportunities in that they are sites of frequent and highly varied interactions among state

intermediaries (the committee staff), residents, and higher state officials. Some days and locales are busier than others, but often a great deal of activity goes on in these offices, and much of it is unpredictable from the RC members' perspective and thus impossible for them to plan or script in advance. Residents and transients drop in for an astonishingly vast assortment of reasons: complaining about noisy neighbors; requesting help in applying for state benefits or documents; paying small fees; negotiating the use of sheds, spare rooms, or open space for commercial purposes; demanding that the committee resolve whatever pressing problem they face, whether concerning housing maintenance, part-time employment, match-making, or any number of other things. At the same time, officials from the Street Office regularly stop by to explain the latest tasks assigned to the RC and check on its performance. Police officers, particularly the "beat cop" (片儿警) assigned to the neighborhood, regularly pull up a chair, sip tea, and have a cigarette while conveying or receiving information about burglaries, parolees, recidivists, or dissidents.

Firsthand observation provides an opportunity to watch how residents and committee members handle all these situations. Naturally, it is always possible that the observer's presence affects the behavior under observation, and thus careful judgment and honest evaluation in one's field notes are required. This also underscores the importance of establishing trust and familiarity, as mentioned earlier. By taking note of body language (imperious or supplicating, for instance) and listening not just to what is said but also to the tone of voice in these interactions, one can infer a great deal about power relations between citizens of many kinds and local agents of the state. Also, such observation allows for follow-up questioning after the fact on the basis of details gleaned from the conversation, which makes it possible to probe deeply into specific, concrete cases rather than settling for general or impressionistic answers. For instance, after a resident came in to demand the committee's help in resolving a dispute with a neighbor, I would inquire about the history of that dispute, the parties involved, which government offices or courts had played a part in dealing with it, and the like. If one were interviewing an RC member outside the context of the office without such cues, she might well never happen to mention that particular dispute and thus the information gained about mediation behavior could be much blurrier.

CONCLUSION

A great deal of political information that matters to researchers, from people's unvarnished opinions to the workings of closed institutions, is *hidden*. Also, much in the political world is *subtle*, in that understanding or coding it is no trivial matter, or in that its relationship to established social science concepts is uncertain. When researchers are confronted by any of the above three situations, or all at once, methods derived from ethnography and participant observation, or "site-intensive methods," may be called for. For the reasons explained in this chapter, these conditions can obtain in any country, but they

are especially prevalent in politically restrictive and non-Western contexts like China. The strategy discussed here is not the only way to approach hidden and subtle politics, of course: careful analysis of public texts, unearthing and interpreting written documents, and drawing inferences from published statistics can also bear fruit.

The purpose of site-intensive methods is to gain access to information that would otherwise remain inaccessible and to obtain data of high validity. Space constraints here have not permitted much discussion of the nuts and bolts of how one goes about using these methods. But in essence, the researcher spends time to form relationships with human subjects that are deeper than those possible in a single interview. Rapport and trust are built up to the extent possible. Through prolonged interactions, the researcher aims to get below the surface of things, the "party line," and the vague or evasive or unconsidered answer. Watching people in the context of their natural habitat makes available data that could never be captured through a questionnaire or an off-site conversation, useful though those may be in other ways.

By definition, these methods involve an investment of time and effort that can be greater than other methods on a per-informant basis. But these costs may not be as steep as some think them to be. Full-blown immersion for many months at a single site is not the only form that these methods can take. Political scientists may have good reason to split their time among multiple sites, or to use SIMS in limited ways to augment other methods.

This chapter has shown (or has provided a reminder) that contrary to the beliefs of those who associate these methods with theory-free, barefoot empiricism, they are well suited for critical tasks in the building of theory: generating hypotheses, testing them in at least two ways, and refining concepts. Classic and current examples of this work, both in the China field and in the broader discipline, illustrate its value. To more fully capitalize on the potential of this approach, it will be necessary to promote dialogue among practitioners within political science about how to employ these methods well, efficiently, and with maximum theoretical payoffs. It also means creating coherent, persuasive explanations of the utility of ethnography and participant observation to our colleagues in the discipline so that it will be better understood and appreciated.