

Trends in Research Cooperation: Gaining Access for Long-term Fieldwork in Japan

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Research in the social sciences requires the cooperation of the people studied. The types of cooperation vary, of course, given a particular research design. Of particular concern to me as an anthropologist are the needs for cooperation that accompany long-term field studies of communities and institutions, in particular the need for permission to gain access. After adumbrating the importance of long-term fieldwork studies to the social science of Japan, I will explain the reasons for my concern about access and suggest some measures for improving the prospects for this form of cooperation in social science research in Japan.

The Contributions of Long-Term Study

Sociocultural anthropologists claim expertise in long-term, intensive studies of subject groups, using the terms *ethnography* and *participant observation*. The former refers to a detailed and comprehensive study of a group's lifeways; the latter, to a research strategy involving development of rapport between researcher and subjects through continued association, sometimes involving shared activities. (Bernard 1994 provides an excellent discussion of the procedures involved.) In adjacent social science fields, references to "longitudinal," "qualitative" or "case-study" approaches subsume the same issues. Common to all is the requirement of far more contact with subjects than in survey studies.

There are no short-cuts to high-validity, fine-grained data: we can obtain them only through extensive, intimate association. Long-term fieldwork also confers tremendous flexibility to the researcher in the field, through opportunities to follow up on unanticipated observations and connect them in a holistic web. Thus, it is practically a truism — among anthropologists, at least — that groups undergoing rapid change and/or about which little is known must be studied on an intimate, long-term basis.

Long-term approaches need not pose mutually exclusive alternatives to survey studies: indeed, they should complement and inform each other (See Hammersley 1992: 186 for a perceptive view of their relationship; Coleman 1995 provides a recent example of survey methods used in a participant observation context.) Nor are anthropologists the only academic specialists conversant with the merits of long-term fieldwork; sociologists have had an abiding interest in fieldwork, and numerous senior scholars in management have called for studies with longitudinal and qualitative components, particularly when different cultures are

involved. (Hammersley 1992; Coleman and Preston 1994 provides extensive bibliographic cites for such recommendations in management research.)

A Rich Literature in English Based on Fieldwork

In the United States and UK, the social science of Japan has developed a rich literature informed by studies requiring months or years of observation and involvement with the subject community. A separate Appendix provides a partial listing of the most well-known of such studies, which I assembled in a modest “mini-survey” for this paper. I emphasize that this sample is incomplete. It reflects the studies that one anthropologist (this author) found most noticeable. Suggestions for additions are welcome. The mini-survey does not include authors from mainland Europe, which perhaps reflects a bias of perceptions or information availability in the United States. The omission may also reflect a tendency of European Japanologists to work with secondary sources rather than conduct fieldwork (Inoue 1993: 1,2).

Most of these are classics that have combined first-hand, detailed observation with national-level statistical analyses. This approach has ignored the unproductive distinction between sociologist and anthropologist, in which the former is supposed to deal only in numbers, and the latter, his or her own subject community. Such sociologists as Robert Cole, Ronald Dore, and Ezra Vogel have conducted participant observation studies that they have melded with aggregate data. (Note the extent to which William Kelly cited these and other sociologists’ works in his 1991 “anthropology of contemporary Japan” review.) Anthropologists Thomas Rohlen and Robert J. Smith, for example, have not shied away from using national-level statistics in their analyses; Rohlen described it as “working on a broader plain” (1983: x). Even Liza Dalby’s study of the geisha world made use of aggregate figures in a discussion of professional life cycles (1983: 197).

This melding of first-hand observation with statistics delivers the best of both worlds: a sense of the human condition and ‘what’s actually happening on the ground’ through qualitatively rich description, plus reliable generalization that can come only from critical use of bird’s-eye aggregate figures. The result is at once perceptive and analytical. This body of literature has surmounted — or sidestepped — disputes between advocates of “qualitative” and “quantitative” approaches, which have intensified in more recent years with the rise of interpretive and purely phenomenological approaches.

The Growing Obstacles to Access

The liabilities to our subjects in long-term research can discourage *any* community from permitting access. The longer the time of stay originally agreed upon, the greater the discomfort if the visiting researcher becomes an irritant (the proverbial “camel in the tent”). The potential for uncovering sensitive facts also increases with length of time. Ethnographies in non-industrial village societies, for example, are more likely to detect witchcraft

accusations and other sensitive issues with researchers' stays of a year or more (Naroll 1962: 87-89, cited in Bernard 1988: 140). The more inductive and holistic the study, the greater the potential for apprehension in the subject group: since the scope of the inquiry is comprehensive and the researcher hopes for serendipity, it is hard to provide a statement of reassuringly precise research topics and goals.

It would be hard to provide a precise description of anthropologists' difficulties in negotiating permission for long-term stays because, despite the importance of access, no one is keeping a count of successes and failures. The lack of inquiry may reflect a high overall success level, in which everyone is simply taking access for granted. On the other hand, silence on the access question might result from professional embarrassment among individual researchers who have experienced rejections. (Some years ago the editors of a volume on fieldwork proposed a book entitled *Failed Fieldwork*, but they could not get anyone to contribute chapters; Lawless, Sutlive and Zamora 1983: xv).

I believe that access to research sites in Japan for long-term fieldwork could become more difficult for American researchers over the coming years, for several reasons. One is a decline in American economic power and domestic standard of living. If the Golden Age of American prestige in Japan is not past, it is at least waning rapidly. For some years now, popular perceptions of the United States have been ambivalent (Powell and Martin 1990); Japanese commentators and intellectuals have been discussing America's decline in a variety of domains, sometimes with open disgust (Helms 1991; Itoh 1993). American academic Japan specialists reminisce about fieldwork before exchange rates became unfavorable for visiting Americans; today, Japanese foundations assist "poor" American scholars," and kindly local residents take pity on "Americans and their sadly devalued dollars" (S. Vogel 1991: 285).

These developments must inevitably influence the responses of members of prospective subject groups in Japan to our requests for access and other cooperation. The scales have begun to tip unfavorably, if ever so subtly, in subjects' diffuse and subliminal calculations of benefits and disadvantages. More tangible prospects of payment to informants have disappeared as socially inappropriate and/or a prohibitive budget item. These issues have not been discussed to any extent by anthropologists in general, accustomed as we are to "studying down" by focussing on peasants and other relatively powerless groups in Third World countries. Fieldwork methodology texts still dwell on the practical and ethical problems of how much to pay our informants, a practice based entirely on the assumption that they are less affluent than we.

A second trend that will probably intensify the problem of access is the need for more studies devoted to highly specialized organizations, as opposed to regional (or geographically based) communities and less formal organizations. Japan, like every other modern industrialized culture, has created numerous government agencies, corporations, hospitals, research laboratories and educational institutions that provide specialized services for clientele beyond their walls. These organizations have proliferated in recent decades, due to the development of ever more specialized technologies and the growth of service industries.

These organizations have become, in turn, major influences on the nature of life in the present-day hyperindustrialized societies of Europe, Japan and the United States (Harris 1987, 1991).

The importance of studying these highly specialized (or “modern”) organizations in Japan was noted by Thomas Rohlen some twenty years ago (1974: 1) and, more recently, by L. Keith Brown (1988: 122-124), both anthropologists who are also prominent Japan specialists. U.S.-Japan economic competition has brought calls for organizational studies of institutions that propel the Japanese economy. Chalmers Johnson identified Japanese R and D and manufacturing as important research issues in political economy, and stated succinctly: “None of the answers will be supplied by more ‘catwalk’ tours of Japanese factories, looking only at the hardware but ignoring the software and the organization” (1988: 105). There is another reason for the importance of these institutions to anthropologists: for some years now, half or more of all Anthropology Ph.D.s have taken jobs with such organizations in business and government, as avenues to academic employment have closed (AAA 1991).

Highly specialized, professional organizations are harder to penetrate than regional communities and less formal aggregations. They have gatekeepers, figurative and literal: members are carefully recruited on the basis of particular skills and advanced education; strangers are not afforded casual entry to the physical premises. These features result from the fact that highly specialized organizations compete, in varying degrees, with other organizations having the same functions. (If they are specialized government bureaucracies, they have constituent groups to whom they must respond). Everyone has to be there “on business.” The members are busy (or at least appear busy) particularly when they occupy the upper ranks (Thomas 1993).

Two other factors, noted by Rohlen, also help create “a special degree of inaccessibility”: members’ geographically dispersed residences, and sensitivity to publicity (1974: 258). The latter concern, in the case of Japan, may go beyond “public relations” worries to include issues of organizational competitiveness or international politics, particularly in the industrial sector.

The Case of Japanese Biomedical Laboratories

These trends call for an informed response lest a vital area of research constrict or close off entirely. The best starting point is, perhaps, to turn the problem on its head and ask a question that has been too long unaddressed: why would they (the gatekeepers of highly specialized organizations) let us in? The question assumed vital importance to me a few years ago as I prepared to conduct an ethnographic study of biomedical laboratory organization and researchers’ careers in Japan. My proposal called for access to laboratories at three separate locations in Japan for periods of four months each. The story had a very happy ending; I received, in all, extremely good cooperation, some of it well beyond my expectations. Although I have no “control group” in Japan of several laboratories that did not permit my

study, I have assembled some relevant facts to argue that two factors were crucial to my acceptance: first, I had meshed my research interests with issues of keen interest to the gatekeepers; second (with apologies for the triteness of the observation), I was blessed with good professional network connections.

The organizational weaknesses of Japanese basic laboratory science are well-known in professional circles, their solution frequently debated. Most prominently discussed are: a centralized, bureaucratic administration; scientific careers lacking interinstitutional mobility and performance-based rewards due to lifetime employment; a dearth of opportunity for aspiring young researchers and women scientists; and, cultural insularity. I highlighted those interests in my research proposal, which I presented to the heads of four prospective institutions.

Two of those institutions, the Osaka Bioscience Institute (OBI) and the neighboring Protein Engineering Research Institute (PERI), originated in large part as a response to organizational and funding inadequacies in the Japanese university system. Both featured term employment systems and institutional mechanisms for academic-industry cooperation. Both had recruited large numbers of female researchers, and both aspired to international prominence through cooperation with researchers from other countries. Their concerns, in short, were my concerns. On several occasions they expressed interest in learning the results of my work — a move that may have been inspired partly by defensive concerns about image, but more likely came from a genuine desire to know, since they welcomed my suggestion that I present some tentative results to the staff before leaving the field.

I must have also benefitted, indirectly, from my home country's excellent track record in biomedical research, a counterpoint to the general decline in American prestige I mentioned earlier. My informants at all levels had some familiarity with American organizational forms and styles, and were typically interested in comparing them with Japanese arrangements. Their intellectual curiosity closely resembled the responses that Sharon Traweek elicited in her study of Japanese physicists (1988).

The other two institutions I approached were medical school research laboratories, one at a national university, the other in a private medical college, both also in the Osaka area. Neither was experimenting with new organizational forms, which is one reason for my interest in including them in my study. The dean of the former rejected my request, citing a wholesale move into new facilities that coincided with the time of my study. The president of the latter accepted me, but his and his colleagues' repeated questions about my research, plus their last-minute arrangements for office space for me (in a distant and disadvantageous location at that) led me to believe that the president had accepted me largely as a function of his professional relationship with the network contact who had introduced me to him.

Introductions through my existing networks were an undeniable factor in the formula for acceptance. My initial contact, the Science and Technology Agency's Genya Chiba, had originated the innovative and successful Exploratory Research for Advanced Technologies (ERATO) projects in 1981, bringing together academic, government and industry researchers

(Anderson 1992). Chiba, in turn, introduced me to OBI's Director, Dr. Osamu Hayaishi, a former ERATO project leader, and the then Director of PERI, Dr. Morio Ikehara. These men were well known and highly placed, but their efforts to create alternative organizational structures bespoke their dissatisfaction with the organizational status quo in Japanese science.

My attempts to gain entry may have also benefitted from my very explicit statements to the effect that I would take special care not to interrupt the flow of work in my host laboratories. I also offered my services as an English editor, which sweetened the deal but did not prove decisive. Although my continual offers of assistance had a very positive effect on rapport after I had entered each of my research locations, I did not encounter a systematic attempt to make use of my services. (My rather rough and very liberal estimate of time spent in English editing would be four hours per week on average at the PERI and OBI locations; I had no requests from researchers elsewhere. I myself benefitted from providing editing work, which gave me insights on authorship, the phrasing of scientific arguments, and the problems of nuance that exist even in scientific writing.)

Access in the Literature

My mini-survey of major fieldwork studies in the Japanese social science literature is far from definitive, but does afford a few tentative generalizations on which I hope others will build more analysis. First, most authors provided some discussion of the process, but it was typically a cursory treatment tucked away in the Preface or Acknowledgments. Rarely did their accounts dedicate a separate section to methods discussions, much less a separate section on access or introduction to the subject community. One tendency was clear: the more specialized the group under study, the more the discussion of access and other forms of cooperation.

Although the literature yielded little discussion of why subject communities agreed to let in the researcher, the available comments are suggestive. The Preface of David Bayley's book on police devoted some space to explaining a very high degree of accessibility and cooperation, in an attempt to assess the veracity of the data (1991: xiii-xv). Bayley wrote that Japanese police were proud of their accomplishments and, like most countries' police officers, wanted to convey their own point of view to outsiders, particularly those from the United States (the country with the perfect combination: prestige and a high crime rate). Glenda Roberts' attempt to study an apparel factory also succeeded in part because of the president's pride in his company and its success (1994: 5).

Bayley also believed that the Japanese police admired his expertise in police-related matters, and were favorably impressed by his desire to see their daily lives rather than just the highly publicized aspects of police work (1991: xiii-xiv). Not all administrators in Japanese organizations appreciate the study of the routine and commonplace; Rohlen found that most of the executives in his bank study were uninterested in the "mundane activities" he studied, even though workers' experience of life in the company centered on the daily demands of

office work (1974: 256,93). My literature mini-study also produced a case contrary to Bayley's experience of nearly impeccable police cooperation. Walter Ames reported that prefectural police officials denied his request to conduct a questionnaire survey; they evidently feared that it would reveal problems of policy at the line officer level that the higher levels of administration would not want to acknowledge (1981: xiii; Ames had explained his own access for observation as a result of introductions from highly placed officials). A suggestive note: unlike Bayley, Ames had a working command of the Japanese language and was in more constant association with his subjects.

The literature did afford a few other thoughtful treatments of the question of how methods affected outcomes, particularly Cole's *Blue Collar* (1971: 40-51) and Rohlen's *For Harmony and Strength* (1974: 255-259). There was only one discussion, however, of the relationship between access and the formulation of the research topic. Matthews Hamabata's study of marriage and kinship in prominent business families resulted from his judicious leverage of networks of personal obligation as a last resort, after he found that he could not penetrate the ranks of males in upper management (1990: 2-5). What may be unusual here is Hamabata's candor, and the last-minute nature of his change in research subject, rather than the occurrence of an access-related obstacle.

Access as a Collegial Issue

Robert Cole experienced difficulty in finding a location for his 1960s participant observation study of factory workers, and attributed it to "the failure of Japanese scholars to do these kinds of studies" (1971: 42). The landscape of Japanese scholarly fieldwork studies in Japanese society still looks sparse today — at least in terms of studies that someone in the United States could become aware of. The only one of my acquaintance is a 1991 study of *yobikō* by Tsukada Mamoru, which he conducted with the academic guidance of the American sociologist Patricia Steinhoff. (Professor Steinhoff recently drew to my attention a study of detectives by Miyazawa Setsuo published in English in 1992 by SUNY Press under the title *Policing in Japan*.)

The prospects for getting access to highly specialized organizations for long-term fieldwork would be brighter if Japanese colleagues themselves were involved extensively in such studies; prospective communities would have more familiarity with the process (as well as a sense of its scholarly legitimacy), and Japanese colleagues could provide local collaboration ranging from introductions to team-type studies. Introductions from Japanese academic colleagues for long-term fieldwork are certainly not unknown, but the practice evidently withered as long-term fieldwork projects shifted focus from rural community studies to highly specialized organizations. In the monographs in my mini-survey, references to Japanese scholarly connections for introductions and placements rarely appeared in the studies of highly specialized institutions.

Nevertheless, proof of the assertion that foreign scholars' problems of access result from

the shortcomings of our Japanese colleagues hinges on some important but as yet unanswered questions. We need to know whether or not Japanese scholars have attempted to conduct field work and were stopped by uncooperative gatekeepers. Several Japanese colleagues in Science and Technology Studies told me they wished that they, too, could conduct laboratory ethnographies like mine, but they would never get permission. I should have asked if they had tried and were rebuffed.

Another question: *is* it easier to obtain access for fieldwork studies in the United States? I myself experienced more ease in securing laboratory ethnography fieldwork sites in Japan than at my home institution in the United States (Coleman 1995b). In his article on access to top management in large American corporations, MIT sociologist Robert J. Thomas observed:

Few executives know what sociologists or anthropologists do in general and much less about what they might do in a particular study. Thus they have little reason to treat you as something other than an unimportant journalist (1993: 93 n.4).

Scholars versus Journalists?

The issue calls for a comparative study of institutional permeability in both Japan and other industrial societies, using a cross-culturally valid typology of organizations. We could also learn from a comparison of social scientists' and journalists' roles. Robert Cole highlighted the absence of scholarly participant observation study in Japan by crediting a "social-minded journalist" with "the classic study in Japanese on the life and working conditions of Japanese blue-collar workers," published in 1898 (1971: 42 n.2). There was a precedent for the 1898 study; according to Ronald Dore, an investigative reporter wrote a series of articles based on his experiences in the mines on the island of Takashima, into which he had sneaked (Dore 1973: 379). The tradition has lived on in Kamata Satoshi's study of Toyota factory workers, published in English in 1982. A NACSIS researcher's recent interviews with five elite sociologists and anthropologists of Japan (at Harvard, Columbia and Stanford Universities) found them "more interested in seeing Japanese journalists and other non-academic writers rather than the Japanese scholars of the same academic fields" (Inoue 1993: 5).

Have Japan's social scientists been preempted by journalists who imparted a sensationalistic dimension to the participant observer enterprise in the process, or were the journalists stepping into a void left by indifferent ivory tower types? Perhaps, as a third possibility, *both* journalists and academics have adopted a leftist orientation that has cost them cooperation. The creators of the STS intellectual tradition in Japan included left-leaning natural scientists and other intellectuals frustrated with the status quo (Nakayama 1974), and I believe that perspective persists. An example of Japanese subjects' unfavorable perceptions of their own social scientists' political stances comes from Bayley's study; the Japanese police, he wrote, felt that native scholars did not appreciate them, approaching them instead with "strong ideological biases" (1991: xiii).

American journalists do not conduct fieldwork studies in Japan, but neither have we. Japan social scientists addressed the issue of how we deal with power, as have our colleagues who do domestic field research (Hertz and Imber 1993 provides a recent example). American journalists have also overshadowed our role as field researchers because we have neglected our lay audiences. The most influential, high-profile statements on Japanese society in the United States in recent years have come from journalists, not from social scientists. I note, in particular, Christopher's *Japanese Mind* (1983) and van Wolferen's *Enigma of Japanese Power* (1989). The list could include James Fallows and Jonathan Rauch — or even Dave Barry, for that matter, in light of his popular 1992 book.

Policy Suggestions

I have no sure answers for our respective quandaries, but I do believe that the best solutions in both the United States and Japan will come from a re-examination of our constituencies and their needs, resulting in new roles for social scientists. I have three specific suggestions that may not open all doors to us, but should nonetheless reaffirm our special role as academics and set us apart from journalists without confining us to ivory towers.

1) Choose topics with policy relevance. Respect for academics has fallen in both Japan and the United States, and one major reason, according to educator Ernest Boyer, lies in our failure to grapple with important problems of our day such as poverty, health care and public education (Jordan 1994). Policymakers in the United States are showing a renewed interest in the social dimensions of topics of global importance, such as pollution, population, and ethnic strife, with new potential for interdisciplinary research teams (Holden 1993). Social scientists in both Japan and the United States would benefit greatly from joining such efforts.

2) Reclaim scientific authority, and earn it. Several trends have obscured the unique role of academic social scientists. The media have blurred the line between fact and fiction with “docudrama” format reenactments and productions. (The Japanese public have long been familiar with the problem of *yarase*, and Americans have seen major television networks’ news time creations as well, such as alleged spy transactions and defective truck bodies. Even technology has conspired through new computer-aided techniques of sound- and image-doctoring.) The most notorious example for us, as Japan specialists, is that best-selling pastiche of novel and scholarly bibliography, *Rising Sun*. The antidote to this dismaying trend lies in our educating the broader public about our unique responsibility to analyze the world logically and empirically, with information on how we generate knowledge.

3) Write for laypeople. The realization of the first two suggestions hinges on effective communication with those outside our fields. Not only have the subjects of social science become too abstruse, so has the language with which they are conveyed, to the dismay of our lay constituencies (Kantrowitz 1992).

4) Provide academic professional rewards accordingly. There have been calls within the

anthropological profession for all of the suggestions I have made, including publicizing to laypeople our work. (Not long ago the *Anthropology Newsletter* featured the front-page exhortation to “Go Public Or Perish.”) I can make one specific suggestion for academics in the United States: reward good popular media coverage and popular articles in Japanese written by fellow social scientists.

APPENDIX

A “mini-survey” selection of published studies of Japanese communities and organizations by foreign researchers, listed alphabetically by author. (Bibliography provides complete citations.)

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