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eFIELDNOTES

The Makings of Anthropology in the Digital World

Edited by

Roger Sanjek and Susan W. Tratner

PENN

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Chapter 2

Digital Technologies, Virtual Communities, Electronic Fieldwork: The Slow Social Science Adapts to High-Tech Japan

William W. Kelly

The first edition of *Fieldnotes* (1990) is that rare collection that has been consistently instructive and thought provoking throughout the decades since its publication. It remains relevant and enlightening to me, both in guiding my own field research practices and for introducing my students to the central significance of fieldnotes for our research and writing. Even this year (2014), it is required summer reading for students entering our Ph.D. program in sociocultural anthropology here at Yale.

The volume was not the first to discuss fieldnotes, but it was the first to bring into the discipline's awareness their manifold forms and functions and the disparate practices of note-taking, from the early ethnographers like Malinowski, Boas, and Mead to the volume contributors' generation. In particular, it demonstrated three crucial qualities of fieldnotes and note-taking over the history of our discipline. First, the collective lesson of the sixteen chapters was that anthropologists' use of the broad term "fieldnotes" actually covers a wide mental and material life cycle, from the chrysalis of headnotes and scratchnotes, jotted quickly in the midst of daily activities, to the full bloom of notebooks, cards, and files, written out in nightly sessions or less frequent binges. The actual practices of note-taking—our forms of notation; our preferences for techniques of writing down, typing up, speaking into, or keyboarding; and our complex feelings

about notes—these and other features of fieldnotes have always varied widely.

However, and this is the second and perhaps most crucial lesson of the volume, whatever the forms of or feelings about fieldnotes, they generally play a more central role in the field research process than we frequently realize. In the midst of fieldwork, we must often gird ourselves to sit down at the moment in the day—late at night or first thing in the morning—to remember and record what we have done since the last time we "wrote up" our notes. However, what *Fieldnotes* brought out was the crucial significance of what we might call the "three R's" of note-taking—recording, reviewing, and reflecting. Over and over, the chapters document how the routine (a fourth R!) of remembering and recording continually prompts reflections on lines of inquiry, inspires connections among who and what is happening, exposes gaps, and generally heightens one's sensitivity to the research setting. I will return to this later in the chapter because it remains germane to the argument I will make.

Finally, the contributors to the original volume also drew attention to the multiple and freighted "afterlives" of our fieldnotes. We may return from the field, but the notes and other materials that we return with keep that experience present and palpable for years and often decades. They have material substance; they sit on our shelves or rest in file cabinets (or now in hard drives and cloud storage). They have intellectual force, as contemplative stimulus and reference for our writing. And they exert a powerful emotional charge as sources of pride and anxiety, possession and obsession.

Thus it is an intriguing challenge to consider what a quarter of a century of technological change and disciplinary transformation has meant to our field research practices since the publication of *Fieldnotes*. This is especially true given the digital revolution we have experienced over that period. In 1989, as Sanjek and his authors were finishing their chapters, the World Wide Web did not exist. I was just replacing 5¼ inch floppy disks with the new hard-cased 3½ inch disks; my brand-new IBM desktop computer took up my entire desk top. Amazon was a culture area, not a corporate behemoth; "social media" was an oxymoron; institutional research boards did not exist; George Marcus had not yet uttered his fateful phrase, "multi-sited research"! It is bracing to think about what the past quarter of a century has brought to our methods and analytical priorities and theoretical orientations. Memory is a form of time-space compression.

My approach to this challenge has been to examine the large body of English-language ethnographic research publications about Japan that have appeared since the original *Fieldnotes*. I chose this point of entry not (just) from lazy self-interest—I am a Japan anthropologist—but also because I believe that Japan anthropology is a useful diagnostic for other regional anthropologies and for other topical zones of the discipline. There are several reasons for this.

Japan anthropology has produced quite a rich corpus of regional ethnography. Since our first Ph.D. dissertation, filed by John Embree at the University of Chicago in 1937 (“Suye Mura: A Changing Economic Order”), there have been more than 350 English-language dissertations in anthropology on Japan, largely done at U.S. universities but with others filed at British, Canadian, European, Australian, Singaporean, and, recently, Japanese universities. The growth of Japan anthropology has been particularly explosive since the publication of *Fieldnotes*, with just over 200 of the 350 dissertations filed after 1989. To this, we can add some 140 ethnographic monographs and many hundreds of articles and volume chapters. The anthropology of Japan may well be the largest single-country corpus of field research in the discipline, historically deep and topically broad.

Equally important, Japan anthropology pioneered many topics that have since become popular and productive in the broader discipline. From the 1960s, for instance, often well in advance of the discipline elsewhere, Japan anthropologists started producing studies of business corporations (white- and pink-collar offices and blue-collar assembly lines), government bureaucracies, high-rise apartment complexes, old-age homes, urban day laborer exchanges, formal schooling from kindergarten to college, television studios, urban train stations, and more. Since 1990, as Table 2.1 indicates, the scope of field research in and of Japan has expanded exponentially, well beyond even this earlier range to topics distant, digital, and distributed.

So I undertook as broad a review as I could manage from this outpouring of dissertations and monographs since the original *Fieldnotes*, paying special attention to the accounts of methods given by the authors themselves, in sometimes full and frank detail. I have not had access to actual fieldnotes beyond my own and those of several of my students, but I have talked with some Japan colleagues about their fieldnote practices, and I believe that a review of 200 or so of the dissertations and most of the 140 monographs does offer an instructive perspective on field methodology.

Table 2.1. Field Research in and About Japan, 1990–2013: A Sample of People and Places from 200 Ph.D. Dissertations and 140 Ethnographic Monographs

Women’s production cooperatives	Elderly resident Koreans
Deaf political organizations	Widows and single mothers
Anime production studios	Overseas housewife circles
Manga publishing houses	Whaling hamlets and fishing ports
Broadcast television newsrooms	Female abalone divers
Pop idol agencies	Organic agriculture communes
Robotics laboratories	Department store sales floors
Second Life sites	Financial derivative trading groups
Gamers and cosplayers	Japanese corporate subsidiaries in France
Iranian migrants in Tokyo	Japanese avant-garde fashion designers
Osaka day laborers	Alcoholic spouse support groups
Urban homeless camps	HIV/AIDS clinics and activists
Osaka Burakumin neighborhood	Right-wing political groups
Japanese-Filipina marriages	Shinto shrines
Japanese-Brazilian workers	Mountain pilgrimages
Karaoke boxes and bars	Volunteer social organizations
Rap music clubs and recording studios	Nonprofit organizations (NPOs) and nongovernmental organizations (NGOs)
Reggae clubs	Overseas tour groups
“Noise” performance spaces	Child protection agencies
Funeral companies	Maid cafes and otaku spaces
Commercial wedding halls	Domestic violence shelters
Childbirth “back home”	High school sex education classes
Psychiatric clinics	Working-class high schools
Addict support groups	“Freeter” bars
Feminist activist circles	Sumo wrestling stables
Waste treatment facilities	Company sports teams
Antipollution social movements	Fitness clubs
Earthquake research laboratory	Recreational marathon clubs
Exam preparatory schools	Professional baseball clubs
Advertising agencies	Martial arts dojos
Universities and junior colleges	Tokyo Disneyland and other theme parks
Host clubs and hostess bars	Japanese supermarket chain in Hong Kong
Convenience stores	
Okinawan women’s war memories	
Deep-rural elderly	

What did I discover? Is a radical revision of *Fieldnotes* required to reflect a radical change in our research practices? The short answer—which shocked me—is that there seems to have been far less change in fieldnote taking and wider fieldwork practices than I was led to expect from the

digital revolution and the expansion of research topics and sites. To put it more precisely, *what* we now study has indeed expanded immensely—our topics, our questions, and our sites—and the *concepts* we use to analyze what we study have broadened impressively; the palette of theories that color our work runs the full spectrum of the social sciences and humanities. However, for many more cases than not, my argument in this chapter is that our actual research practices bear a striking resemblance to the practices that *Fieldnotes* V-1 examined so thoroughly and so discomfortingly. By practices, I mean the thick nexus of project design, fieldwork (in its deceptively multiple forms and among those forms, fieldnotes), and the subsequent process that formulates our field research through reasoning and writing to final representations as dissertations, books, and articles. In other words, fieldnotes, from head to foot.

I was left puzzled by this general methodological continuity and much disappointed (I had wanted to bring readers a message of dramatic change!). To be sure, the mobilities and modalities of communication have multiplied beyond what any of us could have imagined in 1990. The quality, density, and duration of field contact have proliferated. Fieldnote-taking itself has changed for many anthropologists, who are now armed with smartphones, digital pens, voice recognition software, mapping programs, and other recording techniques.

Much has been made of multisited research (Marcus 1995), but equally importantly, we have become multisited *researchers*. Whatever distance between home and fields existed in the past has collapsed. When I first went to rural northeast Japan for my dissertation research in 1976, I would write a thin, one-page aerogramme every couple of weeks to my parents and friends back in the United States, but in two years, I never made a single international phone call. Now, from the same rural location, with 24/7 Internet, smartphones, and social media, I have daily Skype conversations with my daughter and others back home, instantaneous communications with my students and colleagues in my home department, and, if I choose, every university library journal, streaming Netflix movie, and online newspaper I want from home. The long-standing immiscibility of home and field has been thoroughly disrupted.

We are still inquisitively observing—be it homeless tent shelters, national fisheries bureaucracies, global financial services firms, shellfish diver routines, early morning sumo wrestler practices, or anime script conferences. We are still constantly interviewing and incessantly talking and

furiously scribbling and obsessively recording. We are still, more occasionally, participating—as assembly line workers in auto factories, as clerks in convenience stores, as feminist circle archivists, as disaster relief volunteers, as Rakugo performers and teen idol judges, and in many other roles.

The following is quite typical of the accounts of field methods that still characterize almost all of the dissertations. It is from a study of several religious and ethics-training associations in Tokyo:

My fieldwork consisted of: 1) attendance at daily and weekly meetings and social gatherings, and at monthly and annual ceremonies; 2) participation in study sessions, training retreats, workshops and conferences; and 3) formal and informal interviews with individual members. In order to gain a broader understanding of individual members in and beyond their organizations I also met with informants outside of official institutional contexts. In addition to participant-observation, among the 46 individuals I conducted interviews with I followed up with 25 informants for more personalized in-depth interviews at their homes and at my apartment. I also stayed overnight with their families to join morning meetings and followed informants throughout a typical day. When I returned for follow-up fieldwork in late 2011, I visited these informants to follow-up on the continuous changes in their lives, especially after the March 11, 2011 disaster. (Gagné 2013: 17)

Because I had the opportunity to work with Gagné through his dissertation project, I know well how thorough was his note-taking and how integral that practice was to the evolving analysis. It might be argued that small-scale religious movements are an anthropological staple and reliance on such a conventional field strategy is hardly surprising. To be sure, strikingly “contemporary” research sites and topics have provoked experimentation and innovation, but even where we might surely expect radical methodological shifts, methodological continuities still prevail.

For instance, in the aftermath of the earthquake, tsunami, and nuclear plant disasters that befell Japan on March 11, 2011, anthropologist Theodore Bestor, historian Andrew Gordon, and their colleagues at Harvard’s Reischauer Institute of Japanese Studies began assembling what has become perhaps the largest digital archive of a human/natural disasterscape, data-vacuuming the digital world for material and links, maintaining open

access, and inviting all users, including anthropologists, to explore, research, and curate this “big-data” base, facilitating a kind of post hoc, distributed digital fieldwork.¹ The possibilities for creative research and analysis are immense, and there are some fascinating projects at the database website that scholars, students, and others have done with its primary documents, interviews, photographs, and other resources.

Nonetheless, for all the big data–mining possibilities of the Digital Archive of Japan’s 2011 Disasters, the anthropologists now doing active field research in the Tohoku region on postdisaster issues have almost all eschewed big data for small worlds. A striking example of ongoing research is the recent volume *Japan Copes with Calamity* (Gill, Steger, and Slater 2013), five of whose contributors are anthropologists developing separate post-3.11 ethnographic monographs: Tom Gill, with angry farmers in the Fukushima nuclear zone; Brigitte Steger, with the dispirited elderly in the temporary shelters; Alyne Delaney, with the coastal fisherfolk whose livelihood was devastated; Tuukka Toivonen, with the surges of youthful volunteers from Tokyo; and David Slater, with the resolute survivors in the mud-clogged houses along the Sanriku coast. Slater’s account is quite representative of their common fieldwork pattern:

The data for this paper comes from more than 50 volunteer trips I made during the 18 months after the disaster in March, to places ranging from Otsuchi in Iwate to Chiba City and even Tokyo. My duties ranged from handing out food and blankets in emergency shelters in March and April to winterizing the temporary housing units when it got cold again. Most of the work was manual labour, with digging mud out of houses being the most common form, but this gradually gave way to beautification of the grounds around the units and then to more complex community care, sometimes referred to as “care of the heart.” I also led groups of students and business executives, both Japanese and foreign, in an effort to increase CSR (Corporate Social Responsibility) giving among larger companies. We are now engaged in an oral narrative project on the construction of local story repositories, referred to as “Archives of Hope,” at 8 different sites in Tohoku. (Slater 2013: 267–268)

We may draw another example from the very different but equally “contemporary” topic of robotics. Japan has the largest production and

largest population of robots in the world, and Hirofumi Katsuno, among a half dozen other anthropologists, has been exploring the “techno-animism” among humans and robots as they enter into shared worlds of intimate sociality. How? By joining a robot-building circle, crafting a robot, and competing with it, all the while pondering this mixed sociality of human-human, human-robot, and robot-robot through interviews and note-taking.

These amateur robot-builders form local groups to have weekly meetings for exchanging information in such large cities as Tokyo, Nagoya, Osaka, and Fukuoka. I conducted fieldwork in two such groups from September 2005 to August 2007: one based in Akihabara, Tokyo, the center of Japanese consumer technology, and the other at a robot museum in Fukuoka called Robosquare. I participated as a neophyte robot-builder and experienced the processes of learning robot-making, joining the robot-builders’ community, building a robot, and taking part in the competitions held at public venues such as industrial expositions, theme parks, local festivals, and hospitals. . . . Along with other local robot-builders, including some from my program, I met amateur robot-builders from all over Japan. Based on this common experience, six of us formed a group to develop our robots collaboratively and started to meet at Robosquare on a weekly basis. (Katsuno 2010: 37–38)

Another recent dissertation, by Chiho Inoue (2012), explored how the digital space of linked Japanese households over transnational distance (between Texas and Tokyo) was sustained by regular domestic camcording—using the recursive method of camcording the camcording, eventually enlisting the families in their own autorecording and analysis of the sessions on both sides of the Texas-Tokyo divide—but he found that the process of analytical discovery and methodological refinement still required the mediation of fieldnotes.

The focus of my dissertation is to investigate the intersection of two domestic spaces that diasporic families overlap by way of webcam. . . . I conducted interviews and made videotapes of webcam interactions in participants’ homes [in Texas] . . . I traveled to Japan four times . . . I interviewed families and videotaped webcam interactions

based in Japan. . . . Once the family became accustomed to the recording device, I invited the family to participate in the data collection process. I left the video recorder at the house, taught the family when and how to press the record button, and how to change a cassette. Every week I visited the family, collected the recorded tapes, and gave them a new set of blank tapes. As soon as I obtained the tapes, I watched them, took notes, and made a list of questions that I would bring to the family the following week. Sometimes a webcam conversation started while I was visiting the families. In that case, I joined the conversation. . . . While webcam conversation data allowed for direct analysis of interactional coordination in webcam-mediated spaces, interviews provided me with ethnographic data on participants' habitual ways of using a webcam and other tools to interact with their extended family members. (Inoue 2012: 24, 27, 30, 37)

Ian Condry's recent monograph on the production of anime was also an ingenious analysis of a diffuse world of material, economic, aesthetic, and social dimensions and global reach that was grounded in interactions, interviews, and immersion that crossed the real and digital divide:

My research centers on ethnographic fieldwork, primarily in several anime studios in Tokyo . . . attending script meetings, voice recordings, and editing sessions, and I conducted interviews with dozens of creators . . . I attended anime conventions . . . and I follow many aspects of online anime fandom in both Japan and the United States. (Condry 2013: 5)

What these and many other (almost all other!) examples from the corpus I have examined demonstrate is that our methods, including the mundane acts of note-taking, remain, by and large, *recognizable* to any of the contributors to the 1990 *Fieldnotes*. In dissertation after dissertation, in monograph after monograph, there is a strong family resemblance in the inevitable "methods" section about an ensemble of activities and practices that provide the evidentiary base of the analysis. A thousand analytical flowers may be blooming in riotous color, but they still seem to be growing from familiar methodological soil.

Why Is This?

If indeed this is a fair assessment, why is it so? Perhaps there are some readers who are thinking (even smugly!), well, that is just Japan anthropology for you, isn't it? It confirms their lurking suspicion that Japan anthropology is a hopelessly anachronistic backwater of the discipline, and if we would only turn to other ethnographic regions, we would find far more evidence of new methods, innovative strategies, transformational field logics, and creative appropriations to the new worlds (digital and real) around us. Maybe so, but I will leave that to others to demonstrate—while holding to my own suspicion that such readers will not reach a different conclusion.²

Indeed, in many respects, my finding here about the contemporary corpus of Japan anthropology is consonant with arguments and experience of four of our most accomplished ethnographers of virtual life worlds. Tom Boellstorff, Bonnie Nardi, Celia Pearce, and T. L. Taylor recently produced a most stimulating "handbook of method" for *Ethnography and Virtual Worlds* (2012). One of their strongest conclusions was about the continuing salience of our regulative fiction, "participant observation," as *reflective* method as well as reflexive experience in digital lifeworlds that were unimaginable back in 1990. Their fieldwork in virtual domains and social gaming is still rooted in participant observation; indeed, participation, as gamer or as digital-double avatar, may be even more necessary than real-world fieldwork, and they assert forcefully that the technologies of the virtual multiply rather than replace the demands of note-taking:

Participant observation is the embodied emplacement of the researching self in a fieldsite as a consequential social actor. . . . Through participant observation, ethnographers step into the social frame in which activity takes place. . . .

In addition to fieldnotes, virtual worlds provide unique data collection possibilities based on the software that underpins them. . . . These include chatlogs, screenshots, and audio and video recording. None of these data sources should be construed as a substitute for fieldnotes, but fieldnotes need to take them into account. (Boellstorff et al. 2012: 65, 84)

The lesson of their work, together with those following similar Japan-centered virtual worlds, is that, more often than not, our move into new

dimensions of experience, with new technological means, allows—perhaps demands—that we do *more* of what we have been doing, rather than *less* of what we were doing.

Beyond this, however, I think there are at least three other reasons why actual research practices have been changing noticeably more slowly than the questions that drive the research, the sites of research, the theories that inspire that research, and the analyses that we fashion from that research. The first is that “fieldwork” has always been a forgiving cover term for an adaptive *mélange* of methods (and I mean “cover” in the double sense of an umbrella term and an effective disguise). As the cases cited above illustrate, even the most recent projects still rely upon the serendipity of encounters, the nimbleness and resourcefulness of the researcher, and this supple mix of methods. Direct engagement, sustained encounter, an intellectual and emotional openness—these remain the rather underspecified research qualities that we expect in ourselves, our colleagues, and our students.

To put it in other terms, the discipline still assesses our work much more at the level of “doing” fieldwork rather than in the details of what precisely was done. Situated analytical reasoning is generally valued over demonstrated statistical significance. We do not demand publication (even citation) of fieldnotes or other field materials, and we do not require ethnographic writing and reasoning to conform to a standard set of protocols that will measure results against replication, as in other social sciences.

Second, it remains true of our modes of inquiry that we may begin our projects with a place or begin with a topic or a question or even a hypothesis, but field research by most of us is open-ended in the literal sense of the term. It is iterative, reflexive, generative—we incant a litany of descriptors for the emergent quality of ethnographic analysis that continues to put a premium on grounded discovery.

Journalism may be history’s first draft; surely, fieldnotes are ethnography’s first draft. But they have a more profoundly immediate place. Wherever we go, before we ask about local lifeways, we must “learn to ask” in Charles Briggs’s apposite phrase (Briggs 1986). Our best questions are *following* questions, not *leading* questions. And those questions get sharper, our engagement deepens, and our conversations become more focused through the routine of note-taking, which, we all know, is as essential to the *reasoning process* as to the *recording project*. It is not the variable forms of notation but the regular practice of notation that moves our inquiries

into focus and toward understanding, and this helps to explain the persistence of the practice.

A third reason why most of us remain bound to a methodological conservatism is the inertia of our location in the academy. It is our home work as much as our fieldwork that keeps us cautious. By this, I mean that although our research fields and questions are expanding continuously, even exponentially, our writing conventions and publishing platforms change much more slowly. The fieldwork-based, solo dissertation for the doctoral degree; the “dissertation book” based on that dissertation for tenure; reliance on ranked disciplinary journals for promotion and advancement still constitute the demands and standards of our university workplaces, promotion committees, and academic colleagues. Despite the casualization of academic employment, the nervous experiments with digital publishing, the insidious infiltration of corporate audit logics into university assessment, and more, the academy’s standards and practices, enunciated and implicit, continue to dominate the discipline, and these have been slow to change.

And I do not think they will change significantly until the center of disciplinary gravity shifts from the academy or at least until we are more broadly distributed across locations beyond the academy. There have always been important practitioners of anthropology outside universities and colleges, but they remain underacknowledged, and they often must emulate academic forms of research and writing rather than pioneer new styles. Perhaps the recent expansion of anthropologists in corporate settings will precipitate experimentation. Consider, for example, the example that Dawn Nafus and Ken Anderson give of corporate ethnographic research groups that literally write on the walls of the project rooms:

Project rooms [provide] a physical, three dimensional space to write, display artifacts and media, and draw. Though used differently in different places, the practice of writing on the walls has become an everyday part of life as an anthropologist in industrial contexts. Walls have become materials to think with, think through, and perform what it is researchers are thinking about. Our main claim is that these are not just simple mnemonic devices, a record of what happened while doing fieldwork or while thinking through business problems, but that these materials make a certain social configuration possible. Social relations happen in the process of

people moving between text, visual material, and orality. (Nafus and anderson 2009: 138)

Such methods are still, however, on the horizon of possibility, and I have used the very large archive of Japan anthropology, 1990–2013, to argue that the methodological currents run slowly in the mainstream.

If so, the skeptical reader might ask, do we really need this new version of *Fieldnotes* after all? Of course we do—without a doubt, as one can see when reading through the other chapters of this book that offer a new generation of best practices, exemplary cases, and cautionary tales with new technologies of field research. Here, though, I have tried to draw attention not to the novel but to what remains conventional and why such conventions remain so. As long as our research problem orientation is toward grounded inquiry and emergent understanding, our research practices, whatever new technologies and socialities they may employ, will require an ongoing routine of recording, reviewing, and reflecting.

This is a case, then, in which a V-2 does not replace its V-1 but rather augments and extends it. The 1990 volume will not be rendered obsolete. It remains relevant for enlightening us about issues of form and function, of privacy and responsibility, of sentiment and surveillance, of diligence and negligence that still surround our core methodological practices. We are indeed the slow social science, but we have also long been the self-contentious rather than the self-confident social science. This new edition will only serve to sustain our healthy level of constructive anxiety and self-improvement.

Notes

1. The portal for the “Digital Archive of Japan’s 2011 Disasters” is <http://www.jdarchive.org/en/home>.
2. For instance, no recent and thoughtful reviews of regional ethnography have remarked on field research and recording innovations. See, for instance, Albera 2006; Chernela 2005; Lederman 2005, 2008.

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