Crisis of Organizational “Ethnography” in the Age of Journal-driven Research

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I What’s in a Name?

“In the meantime, I reject all attempts at being labeled an ‘ethnographer,’ calling myself, as I always have, a sociologist whose primary research method has been PO [participant observation]” (Gans 1999: 544). So said, Herbert Gans some twenty years ago. Gans is a renowned American sociologist who is known for his excellent research monographs based on intensive fieldwork, including Urban Villagers (1965), The Levittowners (1967), and Deciding What’s News (1979). It is, therefore, quite ironic that he firmly rejected being called an “ethnographer.” Another twist is added in the irony, when one finds that the above statement is included in an essay published in a journal whose title is Journal of Contemporary Ethnography.

In his essay, Gans pointed out and lamented the popularization (or vulgarization, to use a stronger word) and consequent dilution of the meaning of “ethnography.” He said, “Empirical ethnography is now a synonym for virtually all qualitative research except surveys and polls” (p.541). What Gans called “participant observation,” as a research method distinct from ethnography, is a painstaking effort to immerse oneself in other people’s social life, and observe what people actually do, instead of merely reporting what people say about what they do. In addition, a participant observer can observe that people sometimes do not do what they say they would do, and vice versa. It often takes a few years for a researcher to complete

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1 Some parts of this paper were published in Japanese in Sato (2000) and Sato (2010).
2 The journal was renamed from Urban Life in 1987, and was originally published, starting in 1972, under the title Urban Life and Culture. See Loseke and Cahill (2004) for the trajectory of the journal and personal experiences as its editors.
such thorough field research. It may take another few years to write up and publish the results of the research.

The term “participant observation” (PO) originally referred to such a time-consuming and labor-intensive undertaking. However, this image has been diluted in the process through which “ethnography” instead of PO has come to be used more frequently to refer to fieldwork in social sciences. In fact, one can frequently find instances in which authors of academic articles or books assert that they adopted an ethnographic method, whereas in actual fact, they conducted a few semi-structured interviews. Arguably, one possible reason of the transformation in the image of field work is the growing interest in so-called qualitative research. During the last three decades or so, we have witnessed the copious publication of textbooks and manuals on qualitative methodology. While in former days, researchers who drew primarily on non-numerical data for their description and analysis of social life used to have great difficulty in finding outlets for their work, there is now a variety of journals specializing in the publication of qualitative research.

Whereas the increasing popularity of qualitative research has apparently contributed to the liberation of social science research from the straightjacket of a “nomothetic” natural science model, it has not necessarily led to the enhancement of “quality of qualitative research.” One of Gans’s telling comments in the above-mentioned essay is informative in this regard as well: he asserts, “Nonempirical ethnography is dominated by an endless stream of books about PO methodology, which may now even outnumber book-length PO studies” (p.541). This observation by Gans, which was made some twenty years ago, still holds true today. It appears to be a particularly appropriate characterization of the state of qualitative research in Japan. In fact, while a great many numbers of textbooks and manuals on “qualitative methods” or “ethnography” have been published during the last two decades, we find few book-length monographs that include thick descriptions. The meaning of the words “thick description” (Geertz, 1973), which have been frequently used in combination with ethnography as twin buzzwords, has itself been diluted: we can identify many instances of self-proclaimed thick descriptions, which are in fact nothing but “quick descriptions” (Bate 1997). To use a metaphor of theatrical production then, this situation could be compared to one where one can find few instances of excellent stage productions while there is an abundance of how-to books for playwriting and theatre production. This situation can also be compared to a similar one in Japan where one notices the gap between the wealth of how-to manuals on English conversation, and the paucity of those people having enough proficiency in English.
It appears that the ironical gap between the endless stream of research manuals and the actual quality of qualitative research arises primarily from the essentially craft-like character of PO-type field research. In this regard, the following comment by Gerald Suttles is extremely informative:

Field methods cannot be learned by relying exclusively on written work. It is a craft and like carpentry or bricklaying it must be done to be learned. This places a great deal of emphasis on apprenticeships and trial and error (Suttles 1984).

As Suttles argues, fieldwork includes learning by doing as an indispensable process, and one can learn the craft of field research only by repeating trials and errors. It should also be noted that the “knacks” and/or know-how learned in one area of research are not necessarily applicable in other situations. This is because social science fieldwork, especially that of PO variety, inevitably includes the management of person-to-person relationships. Just as it is almost impossible to provide any definitive answer as to what is the best way to get along with your boy/girl friends, lovers, family members (including your spouse) and other people, it is highly contingent on situational factors whether or not you can establish a meaningful relationship with people in a specific field situation. Moreover, one often has to start again from scratch to build satisfactory relationships when embarking on field research in a new setting. In other words, the only thing that one can learn from fieldwork manuals is the fact that one cannot learn much from such written sources.

There is, however, at least one genre of PO-related publications (or “ethnographic writings”) from which one can occasionally learn important lessons for one’s own field research. That is what John Van Maanen (1988, 2011) called “confessional tales.” A fieldwork confessional is a personal account of what a fieldworker (or participant observer) actually did in a research setting, as well as what she or he experienced and felt during the field research. According to Van Maanen, the distinctive characteristics of fieldwork confessional are their highly personalized styles and their self-absorbed mandates (Van Maanen 1988 : 73).

Ethnographic confessionals, then, often include various episodes related to the trials and errors fieldworkers are involved in during their research endeavors. Each research setting is by its nature unique so that other researchers’ experiences may not easily be generalized and applied to other situations. Yet, personal accounts of field experiences often include important clues to cope with seemingly serious errors that were made in a specific research site. I myself repeatedly consulted ethnographic confessionals (e.g., Malinowski 1967 ; Whyte 1955, 1984 ;
Emerson 1983; Venkatesh 2008) when I felt distressed during fieldwork. The first-person accounts by the authors of such confessionals gave me great emotional relief as well as important clues to help me get over the predicament that had been caused by the mistakes I unwittingly made in the field.

The remainder of this paper also belongs to the genre of ethnographic confessional. I will primarily talk about my own experiences in two research settings, i.e., motorcycle gangs in Kyoto and the theatre world in Tokyo. In both of them, ethnographic fieldwork included participant observation as an integral element in the whole research process, and there were frequent occasions of learning by doing through a repeated process of trial and error. While it may not be possible for my own experiences to be generalized and applied to other field settings, they may provide some clues for readers’ own ethnographic undertakings. In a later section of this paper, I will talk about the predicament that PO-type ethnography now faces in the academic world that has been stricken by the disastrous “audit culture” (Power 1997; Strathern 2000). While Gans also pointed out the difficulty of PO research in American “academic economy,” the situation seems to be getting worse not only in the US, but also in other nations because of the increasing influence of narrow-minded meritocracy coupled with a selective research funding policy. To cope with the crisis of PO-type ethnography, we have to carefully examine its methodological characteristics and distinctive merits.

II The Long and Winding Road to “Ethnography”

Fieldwork at Juvenile Training Schools and Prisons

My forty years of research experience can be divided into five phases:

1. 1977—1980: Correctional institutions
2. 1983—1984: Motorcycle gangs
3. 1986—1999: Theatrical companies
5. 2011—Present: Higher education institutions

Of these five phases, fieldwork on motorcycle gangs in Kyoto and research on theatrical companies in Tokyo can be characterized as participant observation (PO) research. Research at other settings may be more appropriately classified into the umbrella category of “qualitative research.” My choice of PO as a basic methodological approach for the two researches was
not dictated by any preconceived plan but largely urged by emergent circumstances. In other words, I did not deliberately “chose” PO but happened to adopt the research technique. In retrospect, this “encounter” with PO was closely related to my intellectual pilgrimage starting from my college days.

I began my case studies of juvenile delinquents at a juvenile training school shortly after I became a graduate student at Tohoku University (TU) in Sendai, Miyagi Prefecture. I belonged to TU’s psychology department and majored in social psychology. When I completed my undergraduate degree at the University of Tokyo (UT), I chose to matriculate in the graduate program of TU partly because I had been disillusioned by the “scientific” orientation of the psychology department at UT. Most of its faculty members and graduate students were specialists in either experimental psychology or animal psychology and their basic analytical orientation was in line with nomothetic natural science model. Partly because my initial interest in psychology originated from my readings of Sigmund Freud’s works in my high school days, I could find little incentive to condition a mouse wandering in a T-maze.

While the majority of the faculty members and graduate students of the TU’s psychology department specialized in experimental psychology, some were also social psychologists who were engaged in fieldworks in rural areas. Among them was Professor Hideshi Ohashi, my mentor at TU, who had been conducting a series of social psychological and anthropological studies on yuta, or female shamans on the Okinawan islands. There was also a long tradition of field research conducted in correctional institutions in the TU’s psychology department.

During the four-and-half years that I spent at TU, I carried out a series of research studies on the life histories of the inmates of four correctional institutions, including two juvenile training schools, one juvenile prison, and a prison for adult criminals. Every year during the summer, I stayed at the official residence of a correctional institution for a couple of months. Typically, I spent my days in the staff’s office and perused the official records of inmates. I also interviewed inmates with regard to their life histories leading up to the criminal cases for which they were incarcerated.

While my major research concern was initially the careers of “delinquents” and “criminals” incarcerated in correctional institutions, during the course of my research I developed an interest in the organizational structure and processes of such institutions. I began reading the literature written by American sociologists (e.g., Clemmer [1958], Sykes [1964]) on the prison community. I also became interested in sociological literature on organizations in general. It seemed to me that Amitai Etzioni’s (1975) compliance typology was quite useful in making
sense of the clear contrast between the two worlds that I inhabited at that time, i.e., the prison and the university. More general sociological literature, including works by Ralph Dahrendorf and Alvin Gouldner as well as Talcot Parsons and Robert Merton, also attracted my attention. The theoretical frameworks of sociology, rather than those of psychology, appeared to be far more suited to account for what I had witnessed in the correctional institutions. This literature also opened my eyes to the methodological issues of field research. It appeared to me that there were a number of interesting differences as well as some commonalities between the methods of psychology and sociology. The methodological appendix of William Whyte’s *Street Corner Society* (1943) was especially appealing and instructive to me: the appendix is, in fact, one of the masterpieces of ethnographical confessionalists.

My reading of the sociological and methodological literature led to a growing skepticism about the validity and fairness of the research techniques that I had been using for my fieldwork in correctional institutions. I noticed that the information on delinquent careers gained from interviews with incarcerated people had only limited reliability and validity as information to examine certain types of research questions. At the same time, I increasingly felt that it was essentially unfair to interview captive interviewees who had almost no other choice to answer my questions (In retrospect, there were serious gaps between most of my questions in the interviews and research issues). The feelings of guilt arising from my recognition of this unfairness reached their peak when I interviewed a man who had committed a domestic homicide: he killed his son who had been alcoholic and violently abusing his family members. I was shocked when the man, who was my father’s age, called me *sensei*, even though I was only a novice graduate student.

My skepticism about the methodology and my growing interest in sociology were probably the two major reasons that pushed me to decide to study abroad, especially in the US.

*Graduate Study at the University of Chicago*

The University of Chicago (UC) seemed to be a quite natural choice for me because prison community studies and Whyte’s *Street Corner Society* had some affinity with the “Chicago School of Sociology.” (While I knew that the Chicago School was also famous for a series of studies on delinquencies in urban areas, it was much later that I began reading such literature in real earnest.) When I arrived in Chicago in 1980, however, the tradition of the Chicago School of Sociology had long been almost extinct. (I was so ignorant about the historical development of American sociology.) I was disappointed to know that there were only a few UC sociologists doing fieldwork.
However, Professor Gerald Suttles was there. He had been well known for *The Social Order of the Slum* (1970), a superb ethnography of a mixed-ethnic neighborhood called “Adams Area” in Chicago’s New West Side. Gerry had also trained a number of talented fieldworkers since he became a faculty member at UC. When I first met Gerry in the summer of 1980, he gave me an offprint of his review article, “Urban Ethnography: Situational and Normative Accounts” (Suttles 1976). It was through this article that I came to know the word “ethnography,” which I had never heard before then.

I instantly became an avid reader of ethnographic works that were reviewed or quoted in the article. I was especially attracted to the studies on adolescent gangs, which include a lot of references to action-seeking activities of gang members. For example, one can frequently find such observations as the following in the monographs written by Chicago School sociologists:

It may be assumed that Stanley’s initial experience in delinquency was an aspect of the paly activity of his gang and neighborhood (Shaw 1930: 50)

The delinquent careers of the brothers had their origin in the delinquent practices of the play group (Shaw, McKay and McDonald 1938: 354)

How to break the humdrum of routine existence—this is a problem for the boy. It is the problem of life generally and a great deal of human energy is expended in the fight from monotony and the pursuit of a thrill . . .

The quest for new experience seems to be particularly insistent in the adolescent, who finds in the gang the desired escape from, or compensation for, monotony (Thrasher 1927; 82).

It seemed to me that there was much resonance between these observations provided in the 1920s and 1930s and what I heard, in the 1970s, during my interviews with those incarcerated in Japanese correctional institutions: many of them also talked about playlike elements in certain types of deviant behaviors. The vivid description in the Chicago Sociologists’ literature addressing action-seeking orientation of delinquent boys appeared to attest to the possibility that deviant behaviors could be carried out not only out of some social and psychological strains but also from pursuit of fun or “just for the hell of it”.

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3 For a review of the literature addressing playlike or action-type deviant behaviors, see Sato (1988). See also Bordua (1961: 136).
In retrospect, the literature eventually led to my research focus on playlike aspects of motorcycle gang activities in Kyoto. However, the torturous graduate program at UC began in autumn, and I no longer had the time to indulge in ethnographic literature. I later learned that at that time graduate programs at UC were famous (or notorious) for the high attrition rates of the students. In fact, Willam Bowen and Neil Rudenstein’s *In Pursuit of the PhD* (1992) mentions the graduate program at UC, along with those at Universities of Berkeley and Columbia, as one of the typical programs that accepts a lot of students, many of whom opt out because of the demanding coursework and/or limited availability of financial support.

Still, it seemed to me that the overall framework of the graduate curriculum of American universities was far more structured than that of Japanese universities; “coursework” was still an unfamiliar concept in Japan at that time. I was surprised to know that there were so many courses that graduate students had to take in order to satisfy their degree requirements. Among the mandatory courses in UC’s sociology department was a class on organizational sociology that was taught by a renowned professor. His was one of the most highly structured classes that I took at UC, and I received the impression that I could finally have an overview of the organizational literature, which I had read in a quite unsystematic way while I was in Japan. The professor’s teaching style was of an archetypical lecture format and he, as a faithful student of Talcott Parsons, framed the syllabus for this class according to the “AGIL” paradigm. He even spent a whole class hour to explain Parsons’ paradigm: the number of students attending his class diminished considerably when he preached the Parsonsian doctrine. (It seems somewhat ironic that while he used to teach and “preach” Parsons’ theory in his classes, much of his high-quality empirical research contains few Parsonsian ingredients.)

As far as research methodologies are concerned, the whole curriculum of the sociology graduate program at UC was geared toward the quantitative approach. Whereas graduate students were supposed to take a number of statistical courses to fulfill their degree requirements, only a couple of courses concerning qualitative methods were offered and they were not mandatory. I was disappointed by the fact that there were so few opportunities to learn qualitative methodology. Yet, in retrospect, the compulsory learning of quantitative methods was beneficial for me in the long run, because otherwise I would not have learned quantitative approaches by myself.
Trials and Errors in the Initial Stage of Fieldwork

After fulfilling the requirements of the PhD candidacy and writing my dissertation proposal, I returned to Japan in the spring of 1983. While I announced in the proposal that I would be using “ethnographic methods” to conduct my research on motorcycle gangs, it was still not clear to me what I would do in the field, or on the street corner. Another cycle of “trial and error” was about to set in, after I arrived in Kyoto in August.

The choice of Kyoto as a research site was a rather fortuitous one. The only lead on which I could count was provided by the fact that one of the students of my senior friend (senpai) teaching at the Kyoto University of Education had once joined in motorcycle gang activities and knew some gang members. I first attempted to make contact with the gang members through the student. It soon turned out that the student was just an occasional participant in gang activities (high-speed racing on city streets) and did not have any intimate relationship with core gang members. While he introduced a couple of former gang members to me, one of them declined to be interviewed and the other once promised but eventually canceled an interview. After that attempt and a number of similar attempts failed, I decided to adopt an orthodox means that had been employed by many ethnographers — going to the street corner and talking to the people who happen to hang about there.

I started visiting the busiest corners of Kyoto late at night and talking to the youths hanging around: they were called yankii (Yankees). It took me about two months to make my first contact with two female gang members of the Ukyo Rengo (Ukyo Association), which was the largest motorcycle gang in the western ward (Ukyo Ward) of Kyoto. While I could talk to some gang boys through the two female members, it took me another month to get permission to participate in their activities. I regularly attended their “weekly meetings” until the following summer. I also occasionally joined in the more mundane activities of the gang boys or yankii. Experiencing the nightlife of street corner boys was quite an eye-opening experience for me, having been raised in a typical middle-class family where both my parents were schoolteachers.

Through participation in the gang activities and the daily activities of the yankii, I found that the action-seeking activities of the gang members, including rumbles (only once was I involved in a rumble and my nasal bone was nearly fractured), one-night stands, and violent car and motorbike racing on the city streets, could not be understood without taking account
of the pervasive boredom of their mundane lives. I was also impressed by the vividness of the street-corner narratives through which the gang members dramatically represented their previous action-seeking activities. The narratives, which included a lot of banter and comical representations of past behaviors, also appeared to serve to neutralize the negative moral implications of their deviant activities, including theft and sexual exploitations.

My research experience in Kyoto also taught me a lesson about the inherent limitation of the questionnaire survey. In the spirit of the “shameless eclecticism” that Suttles pointed out as a distinctive feature of urban ethnographies in his Urban Ethnography article (Suttles 1976: 3), I employed whichever research techniques I could mobilize (e.g., participant observation, formal and informal interviews, qualitative content analysis of the media treatments of motorcycle gangs, and a questionnaire survey). I conducted a questionnaire survey during the end-of-the-year party of the Ukyo Association. Some of the questionnaire items aimed at collecting information on the youngsters’ lifestyles. Other items aimed at analyzing the “flow” or holistic sensation that motorcycle gang members experienced in high-speed racing (cf. Csikszentmihalyi 1975). While I would have been able to collect a large number of fully completed questionnaire sheets if I had administered a similar survey in correctional institutions, less than forty percent of the questionnaires in this case were completed thoroughly. In order to fill out the incomplete questionnaires, I “chased” gang members for a number of months. I visited them in their hangouts, restaurants, or homes and asked (sometimes cajoled) them to fill in the blank answer columns. It was a very tiresome process that I never experienced when I was doing my research in correctional institutions. Yet, through this process, I could obtain more intimate knowledge about the daily lives of the gang members than I could from the other processes. I also became aware of the discrepancies between the intended meanings of the questionnaire items and the respondents’ actual recognitions of the meanings: they tended to misunderstand what I was asking them in terms of the questions in the questionnaire sheet.

Writing up a Dissertation and Publishing a Monograph

I left Kyoto in August 1984 and returned to Chicago in September. The findings of my research were reported in my doctoral dissertation “Bosozoku and Yankee: Anomy and Parody in the Affluent Society,” which was accepted at UC in 1986. An abridged and heavily edited version of the dissertation was eventually published by the University of Chicago Press in 1991 under the title Kamikaze Biker (Sato 1991). Supports and encouragements by Mr. Douglas Mitchell, senior editor of the University of Chicago Press, were indispensable for the
publication of the monograph. His encouraging letters made it possible for me to persevere through the whole process of manuscript revision over four years. It should be also noted that Doug has been instrumental in reviving and renewing the ethnographic tradition at Chicago through his energetic support to the publication of a series of ethnographic monographs from the University of Chicago Press.

Before the English book was published and even before I finished writing my dissertation, I had already published two books on motorcycle gangs in Japanese (Sato 1984, 1985). (While Mr. Akira Shioura, my editor of a Japanese publishing firm was somewhat reluctant, I insisted to use the word “ethnography” for the title of my first book: the book was eventually published under the title *An Ethnography of Motorcycle Gangs*.) I had almost finished writing the manuscript of the first book when I was still in Kyoto. I found that writing certain parts of ethnography while still in the field helps the researcher firmly ground his or her theoretical arguments on empirical data. I also found this way of simultaneously writing a report and collecting data deters the researchers from making far-fetched arguments based on questionable evidence: you can just go to the field again to collect the necessary data when you notice that your data files lacking the relevant data.

I employed a similar technique of simultaneous writing and data collection when I conducted research on contemporary theater in Japan. However, it took me more than seven years to complete the research. During this period, the major research questions, theoretical frameworks and key hypotheses changed to a considerable extent. I also had to conceive of the ways in which I could adjust my professional and family lives to accommodate my research activities. In other words, I had to start the cycle of trial and error once again.

IV Fieldwork on Contemporary Theatre

*Search for a New Research Theme*

After graduating from UC and coming back to Japan, I got a job as a reader at TU in May 1986. I then moved to Ibaraki University in 1988 as an associate professor. By this time, my major research concerns had become those related to artistic activities. That is, I chose a research theme that was completely different from the one that I had stuck to for about ten years. This change of research concerns was largely based on the advice that Gerry gave me shortly before I left Chicago. Gerry warned me that if I stuck to the same research issues that I addressed in my dissertation, my perspective would be too narrowly focused. He advised me to look for a new research theme and construct new reading lists for future research.
I did a study on an amateur Jazz band while I was in Sendai. After I moved to Mito, where Ibaraki University is located, I participated in the activities of an amateur theatrical company, first, as an actor and later, as a technical staff member. This participation in the theatrical group sparked my interest in the then-burgeoning small theatre movement in Japan, which was driven primarily by the theatre artists around my age. While generational sympathy was certainly an important factor that attracted me to the theatre movement, I was also greatly impressed by the artistic achievements of the new breed of theatre artists. My first plan was to conduct research on the small theatre movement mostly through interviews and documentary sources, and actually did preliminary interview with a number of theatre members in Tokyo. However, it soon turned out that I almost completely lacked the requisite knowledge to make sense of what the theatre people were talking about. Also, I was not quite sure what I should ask them in the first place. I became keenly aware of this lack of basic knowledge when I was told by one of my interviewees, “All your questions sound very misplaced to me. You’d better participate in the activities of a certain company. Only if you experience the whole process of putting on a round of theatre performances, will you be able to see what theatre work is like.”

Participation in a Theatre Company and Changes in Research Focus

In late August 1991, I began to participate in a professional theatrical company in Tokyo as a volunteer staff member. (A graduate of Ibaraki University was an actor in the company.) At least two or three days a week, I commuted to the company’s office in Tokyo from my apartment in Chiba Prefecture, where I lived with my wife whom I married in the previous year. While most of my activities in the company were related to management, I also could have the opportunity to experience miscellaneous stage work, including the construction and dismantling of the stage sets, since the company was very small.

After fifteen-months as a participant-observer in the company, I decided to change my research focus. I was initially interested in issues related to the culture-commerce dilemma in art productions. I considered analyzing these issues through case studies of a number of theatre companies. During the course of my participation in the activities of the company, I became interested in issues related to public and private subsidies for the arts. Accordingly, the major subjects of my research changed focus from specific theatre groups to the theatre world as a whole. I also did some research in the US for making a cross-national comparison of the transformations caused by increased public and private subsidies to the arts.

There were two major reasons behind these changes. One reason was the request of the theatre company’s leader to not write anything about the internal circumstances of his
company. According to him, such information, if disclosed, would disrupt the harmony of the company that was crucial for the group’s ability to express itself creatively. Another factor that led me to change the research topic was the transformation of the societal environment of the artistic production. In the early 1980s, Japan witnessed a phenomenal increase in public subsidies for the arts. According to an estimate, the total amount of state and municipal expenditures for “arts and cultures” almost quadrupled during the ten years from 1983 to 1993. Corporate philanthropy for the arts also increased considerably during the same period. The company whose activities I had participated in benefited from such changes and the leader and manager of the company was keen to collect information on private and public subsidies for the arts. These circumstances led me to expand the scope of my research and engage in the research on the “organizational field” (DiMaggio and Powell 1983) related to the arts production.

New Institutionalism as a Theoretical Framework

It was at this time that sociological literature based on the so-called “production of culture” perspective attracted my attention. Key figures in this stream of theoretical and empirical research were a number of American sociologists, including Richard Peterson, Paul DiMaggio, Diana Crane and Wendy Griswold. I was especially impressed by the works of DiMaggio, whom I first learned about through his book *The Managers of the Arts* (DiMaggio 1988). He was also an editor of a very important book, *The Non-profit Enterprise in the Arts* (DiMaggio 1987). These two volumes included a lot of ideas that would help me examine the transformations caused by the increase in public subsidies to the arts.

Through the readings of DiMaggio’s works, I became interested in the theoretical ideas of neo-institutionalism in organizational analysis. Such ideas as “structuration of the organizational field” and “organizational isomorphism” appeared to be quite useful in making sense of what was happening in Japan’s theatre world. At the same time, I noticed that the landscape of organizational studies had changed considerably since the time I attended the class of on organizational sociology in the early 1980s. While the Meyer and Rowan article (1977) was the only piece representing the institutional perspective in the syllabus for the class, the monumental volume *The New Institutionalism in Organizational Analysis* (Powell and DiMaggio 1991) was published in 1991, the same year when I joined in the theatre company. (The volume was partly intended to be stocktaking the development of empirical and theoretical literature of neo-institutionalism up to the early 1990s.)
Changes in Research Methods

Along with the changes in the scope of my research and theoretical framework, the major research methods also changed. Instead of the field observations and informal interviews, documentary materials and formal interviews became two major data sources. Accordingly, my monograph on the transformation of contemporary theatre in Japan, which I published in 1999, quotes such data profusely. This is probably why Professor Gary Alan Fine was somewhat disappointed with an English paper addressing the transformation of the Japanese art worlds that I sent him. Gary was a visiting professor at UC when I was writing my doctoral dissertation on motorcycle gangs, and his class was one of the few classes on qualitative sociology that I could attend at the university. He also provided me with important suggestions for my dissertation, and later supported me greatly in contributing a paper (Sato 1988) on playlike deviance to *Symbolic Interaction*.

As a proponent and one of the most prolific authors of “peopled ethnography” (Fine 2003), he probably wanted to see more information on real-life interactions among people in the field. However, I could not use direct quotes from my field records in the paper or in my monograph on the contemporary theatre, because I had promised with the company’s leader to not disclose anything about his company. Still, what I had seen and heard during my participation in the theatre group constituted an important backdrop against which I could make sense of the accounts of the theatre people and those who were involved in the public and private supports for the arts. In addition, by that time I could be more confident of the questions that I would ask to theatre people. My experience at the company also appeared to serve the theatre people to make sense of who I was and what I was studying: my experience at the theatre company was instrumental in establishing rapport with the theatre artists and managers, primarily because the chief manager of the company was widely known and well regarded by them.

My data files increasingly included various kinds of data, including clippings or newspapers and magazines, photos of on-stage and backstage scenes, transcripts of informal and formal interviews and fieldnotes. It took me about two years to analyze these voluminous and sundry data sets. While at that time computer programs for qualitative analysis were not multilingual and could not be applied to textual data in Japanese, I could use an outline processor software in segmenting and categorizing the huge body of textual data according to the inductive and analytical codes that I came up with through my fieldwork and readings of theoretical books and magazine articles.
Writing Up a Research Monograph

In writing the manuscript of a research monograph on contemporary theatre, I drew upon the ideas of not only the literature on neo-institutionalism but also the literature on other areas such as the sociology of professions and that of industry (e.g., Abbott 1988; Lieberson 1986; Stebbins 1992). However, it was quite late in the whole process of my research of over seven years to be relatively confident of the theoretical framework in the book on which I intended to base my arguments. In fact, I revised the table of contents of the monograph at least five times, and had to ask Ms. Kazue Itoh, a veteran editor at the University of Tokyo Press to extend the deadline of the manuscript several times; it was not until two-and-half years after I sent her the first synopsis of the book that I finally delivered an almost complete manuscript to her.

Such an inordinate delay can be explained in terms of a number of factors. The foremost critical factor that delayed the completion of my book was an increase in familial obligations. Until my wife quit her job in 1996, I shared most of the household jobs with her, and we had three children during the eight-year period between the beginning of my research in 1991 and the eventual publication of the monograph in 1999. I also had difficulty in adjusting to the new job environment that I encountered as a member of the Faculty of Commerce and Management at Hitotsubashi University, where I got a job in 1995. I had to instruct on drastically different subject matters compared to those that I taught at Ibaraki University, where most of my students majored in psychology. In addition to these occupational and familial matters, I had to assign considerable time and energy to a joint research project that included interview research in the US.

My circumstances turned more favorable around 1997. The joint research project was completed in 1996. In the same year, my wife quit her job after the birth of our second son. She helped me concentrate on writing by undertaking the responsibility of most of the household affairs. By then, I had become more confident of my classes for students majoring in business administration. My family had also moved to a city that is very close to the Hitotsubashi University campus as well as the theatre districts in Tokyo. I could, then, concentrate on writing my monograph on the transformation of the theatre world in Japan.

The (almost) final draft of the monograph was completed in December 1998. On the other hand, much in the same way as I did when writing the monograph on motorcycle gangs, I simultaneously wrote manuscripts and collected data after sending most of the manuscript to the University of Tokyo Press. Once again, I found that simultaneous writing and data collection was beneficial for grounding my theoretical arguments on empirical data. The
monograph was eventually published from the University of Tokyo Press in 1999 under the title *The Making of Contemporary Theatre in Japan* (Sato 1999).

A couple of months before the book came out on the market, I together with two collaborators visited and interviewed a former director of the University of Tokyo Press. The interview was the first step in our research on scholarly publishing in Japan. The findings of the research, which was mostly based on interviews and available data, were published as a monograph (Sato, Haga and Yamada 2011) in 2011. The research was also accompanied with a significant number of experimentations and mistakes. There may be another occasion to talk about the research experience in the future.

V Crisis of Ethnography in the Age of Journal-driven Research

Some readers, especially young academics, may find it strange that I was able to engage (or “indulge”) myself in a series of extremely time-consuming ethnographic undertakings. Since most young academics are now urged to produce research outputs constantly and at relatively short intervals, they may believe that confessinals about long-term field research are virtually useless. In fact, it took me almost eight years to publish, in the form of a research monograph, the results of my research on contemporary theatre. In the case of the research on scholarly publishing, there was a twelve-years interval between the first interview and the eventual publication of a monograph.

In these days, such a research style is increasingly difficult to adopt in many countries due to the overwhelming influence of the audit culture (Power 1994, 1997; Strathern 2000) and selective research funding based on research assessment. Since “accountability” tends to be equated with quantifiability, journal articles, whose qualities are supposed to be easily measured by numerical indicators, are often preferred to books or book chapters. In addition, many of those involved in the research quality assessment whose results are used for allocating public funds, will find that journal articles are far more manageable than lengthy monographs. Higher education institutions, then, often encourage their academics to publish journal articles, preferably in prestigious quality journals with high impact factors, rather than monographs.

In this regard, Roger Goodman, an Oxford scholar, comments as follows:

Academics have also had to reorganize their research agenda and publications strategy in order to maximize the financial returns to their institutions. At the most absurd level, it
has been more rational to publish short four articles in peer-reviewed journals than to publish two large, world-class books (Goodman 2013: 48)

In this quote, Goodman refers to the plight of academics working at universities in the UK, where “research-active staffs” are required to submit four research outputs for the nationwide research assessment that has been carried out every five or six years since the early 1990s. The ever-increasing influence of world university rankings since the early 2000s (cf. Hezelkorn 2015; Yudkevich at al. 2016; See also Wedlin 2006) also adds to the pressures upon academics to publish their research in high-impact journals.

At the most extreme level, this pressure of “publish or perish” is typified in the following advice given to academic members at the UK’s management studies departments:

1. Select your journal.
2. Determine consensus in area of research.
3. Do research.
4. Extract from research bits that fit journal consensus.
5. Write paper and submit to journal (McDonald and Kam 2008: 648).

This advice may look quite strange or even perverse to those who are not familiar with the current state of the academic world in many countries. In fact, the five items in the advice do not appear to be arranged in the proper order: especially the relative order of the first and the third items may seem anomalous. An idealized image of a scientist or researcher portrayed in the popular literature, movies and dramas is that of a person who carries out research out of curiosity, or after being motivated by some social cause. Only after he or she finds some innovative and novel ideas, does s/he look for the most suitable medium (e.g., journal, book, or electronic media) to publish his or her findings. Such approaches have been sometimes called curiosity-driven research and socially-driven research. On the other hand, what can be reproved in the above-mentioned advice may be labeled as “journal-driven research” (cf. Ramasarma 2014: 507): academics are advised by their universities to identify the high-impact and highly ranked journal they will submit their papers to, well before they actually do the research. We can see a sort of means-end reversal here, because what is supposed to be the “means” (publication medium) takes overwhelming precedence over what should be the

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4 There are, of course, very different types and portrayals of scientists and higher education institutions. See for example, Slaughter and Leslie (1997) and Slaughter and Rhoades (2009).
ultimate “end” of academic research (scientific innovation or social cause).

The circumstances for researchers who pursue ethnographic (or PO) fieldwork, then, appear to have worsened during the two decades since Gens’ essay was published in the late 1990s. In his essay, Gans pointed out the predicament of participant observation research under the pressure of the American “academic economy” (Gans 1999: 544-545). He argues that because of its labor-intensive and time-consuming nature, an ethnographer is in a disadvantageous position vis-à-vis a quantitative researcher who would be able to find the outlets for publishing his or her research in a quality journal. He also pointed out that the decreasing public and private funds for social sciences research made it increasingly difficult to carry out intensive fieldwork taking many years to complete. A recent development in higher education around the world adds to this predicament of PO-type ethnography. In many countries, especially where public support for academic research is declining, the rate of student completion within a specific number of years is counted as a crucial weighting metric in determining allocation of research funds.

In such circumstances, time-consuming ethnographic research might end up being a luxury for tenured academics or for those academics whose careers are closing to an end. This is an ironic situation because PO research, in many cases, is most suitable for young aspiring academics, who can persevere in labor-intensive and often tiresome fieldwork. They also have flexibility and sensitivity, which are indispensable for adapting themselves to unfamiliar cultural and social settings so as to provide truly “thick” descriptions.

VI Endnote

It is well known that the retrospective reconstruction of one’s own career tends to give too much structure and order to biographical narratives. Although I have characterized my early career as an ethnographer as a series of trials and errors, this essay may not have evaded the limitations posed by retrospective reconstruction. However, some sort of order or structure, if it is not too restrictive, is a prerequisite for any type of written narrative in order for it to serve as an effective means of communication. In fact, a written narrative is distinct from an oral narrative in that it enables its readers to repeatedly come back to and reflect upon its textual context and thereby attain a deeper understanding of what is “told” (Ong 1982). A certain perspective or viewpoint, in this regard, is indispensable in giving a textual structure to the written narrative. Only when one has a certain viewpoint, one can impart an order to miscellaneous topics and themes that may otherwise be disparate bits and pieces of

A similar thing can be said of ethnographic narratives that are published in the form of journal articles or research monographs. One has to have a certain theoretical or analytical perspective in order to lend a consistent storyline to the otherwise disorderly observational data. While there is no essential difference between ethnographic texts and other types of research reports (e.g., journal article based on quantitative research) in this regard (Gusfield 1981; Hunter 1991), there is at least one distinctive feature of written ethnographic narratives: a perspective often emerges amid research process and during the very process of writing. That is, arguably, one of the major reasons why it often takes so many more years for an ethnographer to write up a research report. It is noteworthy in this regard that “ethnography” refers not only to the characteristics of data collection method, but also to the distinctive features of the writing style and genre. Ethnographic narrative essentially belongs to a mixed genre, and can be (or should be) located somewhere between a scientific report and a literary text (cf. Geertz 1983). As long as an ethnographer does not lose sight of this mixed genre nature of ethnography, PO-type ethnography will survive the overwhelming pressures in an audit society where journal-driven research, instead of curiosity- or socially-driven research predominates.

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