

Across the Atlantic: Current Issues and Debates in US Ethnography

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Abstract: This overview of selected issues in US ethnography covers the last five to ten years. The paper is divided into three major sections. The first one describes the institutional standing of ethnography in four separate contexts: organizations and conferences, publications, research funding, and careers and centers. Second, the paper reviews recent debates regarding the roles of politics and theory in US ethnographic research. A third focus is placed on methodological issues, such as the increasing diversity of ethnographic research methods and designs, as well as a new set of research requirements administered by ethical review boards at all US universities.

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1. Introduction

Qualitative research and more specifically ethnography have played an important role in the history of US sociology. Many excellent overviews exist of the roots, development, and legacy of such work (e.g. FINE 1995; VIDICH & LYMAN 2000; EMERSON 2001a; DEAGAN 2001; MUSOLF 2003; KATOVICH, MILLER & STEWART 2003).¹ Instead of adding another historical treatment of ethnographic research in US sociology, this paper pursues a different goal: providing a

¹ Consistent with the overview style of the paper, references throughout are listed chronologically and not alphabetically, beginning with earlier work.

snapshot of some current issues in US ethnography without grounding them in a historical context. Some of my observations reach back to the 1990s, but the bulk of the essay deals with more recent developments in US ethnography, roughly since the watershed year of 2000. [1]

Who am I to address recent trends in US ethnography? I began studying sociology at the University of Cologne in 1987 and received my master's degree from the University of Constance in 1993, for a genre analysis of personal ads in magazines (guided by Thomas LUCKMANN). Seeking challenge and adventure, I applied for Ph.D. programs in the US and entered the University of California, Los Angeles (UCLA) as an international student in the fall of 1993. I soon gravitated towards the ethnographers in the department, especially Jack KATZ and Robert EMERSON, who provided a second round of training in qualitative and ethnographic research methods. In 2002, I earned a Ph.D. degree for a study of neighboring in two urban neighborhoods in Hollywood. The following year, I was hired as assistant professor at the University of South Florida in Tampa, where I still live and work. Over the last dozen years, I have been an enthusiastic participant observer of ethnographic research in the US. I maintain close relationships with junior and senior colleagues, read, teach, conduct research, and publish in this area—all in full pursuit of "going native." [2]

The following observations are based on a variety of sources. First, I make use of the general knowledge of US ethnography I have acquired over the last dozen years. Second, I rely on a focused reading of a good number of papers, monographs, edited volumes, and handbooks (especially DENZIN & LINCOLN 2000; ATKINSON, COFFEY, DELAMONT, LOFLAND & LOFLAND 2001; REYNOLDS & HERMAN-KINNEY 2003).² Third, I spent some time fact-finding, on-line and in libraries. After an attempt at surveying the experts did not produce the expected results, I did not undertake additional efforts to systematically consult my colleagues. Instead, I decided to rely solely on my own knowledge, findings, and hunches. Thus, while hoping that this paper resonates with colleagues elsewhere, I need to emphasize that the selection and treatment of topics is entirely my own. [3]

1.1 Defining ethnography

Classifications such as "ethnography" and "qualitative sociology" adopt different meanings in different national contexts. It is thus important to define how these concepts are used in this text. Being an ethnographer—as opposed to, say, an interviewer—is a widely understood and accepted scholarly identity for US sociologists, indicating much more than the correct mix of participation and observation in one's research. In the US, being an ethnographer means utilizing certain methods of research, engaging certain analytic strategies and perspectives, and seeking certain audiences for one's work. In contrast, it seems that, among Europeans, ethnography equals participant observation, thus describing a technique of qualitative inquiry, equivalent in scope to focus groups

2 The third edition of DENZIN and LINCOLN's handbook (DENZIN & LINCOLN 2005) was not yet available by the submission deadline for this paper.

and content analysis, rather than a scholarly identity. In turn, the European custom of identifying as a "qualitative," "interpretive," or "micro" scholar tends to sound excessively self-limiting to American ethnographers, who prefer not to restrain the scope of their findings, nor limit the utility of other than qualitative data sets. [4]

Consistent with the use of the term in the US, I here consider ethnography to be a sub-category of sociological research, not of qualitative methods. While observing and participating are clearly the most traditional and still the most widely used techniques of ethnographic inquiry, contemporary ethnographers employ a much greater variety of research methods, such as interviews, life histories, autobiographies, visual, and even experimental methods in their work. In fact, many ethnographers today assemble a variety of data sets, collected through a variety of techniques, with the goal of amplifying their field access and analytic scope. [5]

What makes a study ethnographic is less apparent in the kinds of tools used than in the grounding of the research in multiple contexts of social reality, ranging from bodies, physical and built environments, personal biographies, historical processes, relationships and identities to political and other structural contexts. It would be wrong to conclude that because ethnographers often use individual-level data and emphasize situational contexts (what GOFFMAN 1983 called the interaction order), their work is by definition "micro" in scope and does not interface with social structures such as relationships, inequalities, and institutions. [6]

In this essay, I discuss recent studies by US sociologists (as well as several anthropologists) that exhibit the qualities I have described, whether intended or unintended. I also consider research by US sociologists who self-identify as ethnographers. Since in the US being an ethnographer includes a reflexive statement of intent regarding the academic contexts and audiences of one's work, it would be foolish not to honor these members' self-identifications. [7]

1.2 Some substantive trends

One somewhat objective indicator for the significance of a sociological monograph is the organization of an author-meets-critic session at the yearly meeting of the national organization. These events tend to honor work that has attracted the attention of a wide circle of colleagues. Several ethnographic studies have been featured in such sessions over the last few years, among them DUNEIER (1999), ANDERSON (1999), HONDAGNEU-SOTELO (2001), and LARAU (2003). These are doubtlessly among the most important ethnographies of the last years, yet the list of omissions is long. [8]

My casual and eventually incomplete survey of ten senior and ten junior colleagues in 2004 regarding the most important ethnographies of recent years indicates that sociologists of various substantive specializations can easily identify such studies in their fields, yet it also indicates how difficult it is to discover any general trends. There simply *is* no consensus among US

ethnographers regarding the best ethnographies of recent years. Of the roughly sixty books listed in the nine responses I received from colleagues, only five were mentioned more than once: DE VAULT 1991; THORNE 1993; BOURGEOIS 1995; DIAMOND 1995; DUNEIER 1999. This inconclusive result is a good "unobtrusive measure" of the overall fragmentation of the field. [9]

If anything, what can be noted about substantive trends in US ethnography? Two of the repeatedly mentioned books (DE VAULT 1991 & THORNE 1993) deal with gender and the life course; one (DIAMOND 1995) intersects with medical sociology, work, and the life course; and the last two (BOURGEOIS 1995 & DUNEIER 1999) count as urban ethnographies yet can also be categorized under deviance/crime. These six substantive areas—gender, life-course, medical, work, urban, and deviance/crime—are among the traditional strongholds of ethnographic inquiry, and they still occupy a large market share today. Separate chapters on four of these areas can be found in the 2001 *Handbook of Ethnography*, whereas five of the six are reviewed thoroughly in the 2000 *Handbook of Symbolic Interaction*. The following paragraphs provide more examples of recent ethnographic studies in these and other areas. [10]

Gender: Aside from the mentioned publications, important recent ethnographies on gender-related issues include: GARDNER 1995; PIERCE 1995; HOCHSCHILD 2001; FERGUSON 2001; CASSELL 2000 (cf. STEWART 2003 for an overview). *Life Course:* This topic groups together ethnographic research conducted on various formative stages in personal lives, especially childhood/young adulthood and old age. More examples include VINITZKY-SEROUSSI 1998; BEST 2000; CHIN 2001; LAREU 2003; ROSENFELD 2003 (cf. JAMES 2001; HOLSTEIN & GUBRIUM 2003 and CAHILL 2003 for overviews). *Medical/Health:* For additional ethnographic work in this area, see CHAMBLISS 1996; GUBRIUM 1997; TIMMERMANS 1999; CASSELL 2000 (cf. BLOOR 2001 for an overview). [11]

Work: For additional ethnographic research on work-related issues, see FINE 1996; NIPPERT-ENG 1996; VAUGHAN 1996; HONDAGNEU-SOTELO 2001; ADLER and ADLER 2004 (cf. SMITH 2001 and SHAFFIR & PAWLICH 2003 for overviews). *Urban:* For important new urban ethnographies aside from those mentioned, see SNOW and ANDERSON 1993; DORDICK 1997; PATTILLO-MCCOY 1999; NEWMAN 1999; MCROBERTS 2003; SMALL 2004; WACQUANT 2004; LOW 2004, FALK 2004 (cf. LOFLAND 2003 for an overview). *Deviance/Crime:* For additional ethnographic studies in this area, see DORDICK 1997; ANDERSON 1999; FERGUSON 2001; MITCHELL 2001; NEWMAN 2004 (cf. HOBBS 2001 and HERMAN-KINNEY 2003 for overviews). [12]

Emotions: The sociology of emotions is a significant area of ethnographic inquiry that has not received adequate attention in the past. Important recent studies include KARP 1996; KATZ 1999; CLARKE 1997 (cf. FRANKS 2003 for an overview). *Other:* The mentioned handbooks offer specialized reviews of qualitative, symbolic interactionist, and ethnographic research in the areas of race and ethnic relations (WILLIAMS & CORREA 2003), science and technology

(HESS 2001), material culture (TILLEY 2001), communication (KEATING 2001), sports and leisure (STEBBINS 2000), social movements (SNOW 2000), educational settings (GORDON, HOLLAND & LAHELMA 2001), and collective behavior (MCPHAIL & TUCKER 2003). Overall, readers should keep in mind that these lists are by no means complete. [13]

The rest of the paper is devoted to discussing three other topics. First, I provide an overview of the institutional contexts of US ethnography; second, I review some political and theoretical disagreements in contemporary US ethnography; and third, I discuss recent methodological issues, such as the increasing diversity of ethnographic methods and research designs, as well as changes in the academic research process. I conclude with a brief summary of the paper. [14]

2. Institutional Contexts of US Ethnography

This section investigates the institutional standing of US ethnography in four separate contexts: organizations and conferences, publications, research funding, and careers and centers. [15]

2.1 Organizations and conferences

American Sociological Association. The year 2005 marks the centennial anniversary of the American Sociological Association (ASA), which currently has over 13,000 members. This year's meeting in Philadelphia, entitled "Comparative Perspectives, Competing Explanations: Accounting for the Rising and Declining Significance of Sociology," called for a reflexive inventory of the entire discipline. Undoubtedly, ethnographic research has had its moments in the overall history of US sociology. A good measure of the success of any specialization within a discipline is the degree of visibility it has managed to achieve within its national association. [16]

As is true for many other countries, the US national association of sociologists does not have separate "ethnography" or "qualitative methods" sections among its current forty-three sections. There is a general "methodology" section which, at least theoretically, should accommodate qualitative research issues. However practically, the section is dominated entirely by quantitative sociologists, and has been for many years. "Comparative and historical sociology," "mathematical sociology," and the newest section "ethnomethodology and conversation analysis" are other sections that prioritize particular methodologies, yet none of them serves as an outlet for ethnographic work. [17]

Nonetheless, qualitative research, and to some extent ethnography, dominate a number of substantive ASA sections, among them "sex and gender" and "culture"—to name two of the largest—besides "social psychology," "emotions," "children and youth," "sexualities" and the second newest section, "animals and society." Ethnography and qualitative research also play a role in a majority of the other substantive sections, yet without taking a leading role. [18]

While sections do assume a good amount of control over the content of the annual meetings, proponents of special interests are given plenty of opportunity for encounter and exchange. The topics "ethnography," "qualitative methods," and "symbolic interaction" are fixtures in the list of regular sessions soliciting open submissions at virtually every meeting. Further, more specialized topics that intersect with ethnography, for instance "feminist research methods" in 2005, are frequently included in the list of conference sessions. Many other panels, for instance thematic sessions (addressing the overall conference theme), or the author-meets-critics sessions mentioned above, regularly incorporate the work and views of ethnographers. [19]

Over the last several years, aside from a rising presence in the program, there have been other institutional signs of increased visibility for ethnographers and their work in the ASA. Most notably, Michael BURAWOY, a prominent ethnographer, was the ASA president for the year 2003-2004 during which he presided over the 2004 meeting in San Francisco. The 2004 conference theme, "Public Sociologies," allowed for particular emphasis on the work of qualitative sociologists and ethnographers. Aside from Eli Anderson's vice presidency in 2001-2002, this was the first time an ethnographer had led the organization since the 1980s—a decade marked by the presidency of four influential ethnographers (William Foote WHYTE 1981; Erving GOFFMAN 1982; Kai ERIKSON 1985, and Herbert GANS 1988). [20]

Numerous ASA awards have been given to ethnographers in recent years. The prestigious ASA award for "Distinguished Scholarship," which is something like a life-time achievement award, was given to Dorothy SMITH in 1999 and to William Foote WHYTE in 2001. The ASA award for "Public Understanding of Sociology" went to Howard BECKER in 1998, Herbert GANS in 1999, and Arlie HOCHSCHILD in 2000, indicating that ethnographers are well-situated to bridge academic and public discourses. Finally, the "Jesse Bernard" award (for enhancing the understanding of the role women play in society) was given to Barrie THORNE in 2002. All in all, there seems to be an increasing awareness and acknowledgment of ethnographic work in the larger field of sociology, as represented by recent events in the US national association. [21]

Other Organizations. There are at least three other sociology organizations in the US that are more or less dominated by qualitative sociologists, including many ethnographers. These are, first, the "Society for the Study of Symbolic Interaction" (SSSI), second, the "Society for the Study of Social Problems" (SSSP), and third, the "International Visual Sociology Association" (IVSA) which has its home base in North America but also maintains a strong European membership, especially of Italians. These organizations widely open their arms to ethnographers who share the special interests pursued by their members. For the SSSI, active membership requires engaging the symbolic interactionist paradigm; for the SSSP, it means putting a substantive focus on social problems; and for the IVSA, it implies the production and/or analysis of visual images. [22]

All three organizations publish their own journals: *Symbolic Interaction* (SSSI), *Social Problems* (SSSP), and *Visual Studies* (IVSA; which was called *Visual Sociology* until 2002). To maximize attendance and visibility, the SSSP and SSSI hold annual meetings that coincide with the date and location of the ASA conferences. The IVSA coordinated its 2004 meeting with the ASA for the first time yet has committed to holding every other meeting in Europe. [23]

Special Conferences. Over the last few years, there have been three meetings bringing together US ethnographers not as members of specific organizations but based on the initiatives of individuals. The first such conference was held in May 2002 at the University of California, Los Angeles; it was entitled "Fieldwork in Contemporary Society." This meeting gathered approximately seventy participants and guests and was organized by Jack KATZ, Robert EMERSON, and David HALLE. [24]

Another two-day meeting followed in November 2003 at the University of Pennsylvania in Philadelphia, entitled "Being Here and Being There: Fieldwork Encounters and Ethnographic Discoveries." This conference was organized by Eli ANDERSON and several of his graduate students. It brought together close to two hundred participants and guests. Some of the conference papers and discussions have been published in a special volume (ANDERSON, BROOKS, GUNN & JONES 2004). [25]

A third two-day meeting entitled "2005 Ethnography Workshop" took place in April 2005, also at the University of Pennsylvania, again based on the initiative of Eli ANDERSON. This last meeting gathered roughly sixty presenters and guests. Most of the time was devoted to discussing the research of graduate students and young professors. Future meetings of this sort are being planned even though financing them is becoming increasingly difficult. [26]

Each of these three meetings was highly successful in bringing together novice and experienced fieldworkers from different parts of the country to share their work, discuss new directions, and to generally strengthen collegial relationships, thus building community. These meetings have also forged and strengthened connections with ethnographers from Europe, especially the UK and France. Nevertheless, it would be naive to believe that these extended family reunions dissolve disagreements existing within this group, or that they build bridges to the other groups of US ethnographers which are not part of this particularly active network due to methodological, theoretical, political, or even personal differences. [27]

2.2 Publications

In the US, there are numerous outlets for the publication of ethnographic research and methodology. Several well-known journals specialize in the publication of qualitative research, which means that ethnographic work can be found here regularly as well. Aside from those mentioned, the most accommodating periodicals are the *Journal of Contemporary Ethnography* (formerly *Urban Life*) and *Qualitative Sociology*. I should also mention the annual

publication of *Studies in Symbolic Interaction*, a peer-reviewed volume specializing in qualitative research and methodology. [28]

Further, the new journal *Ethnography*, jointly edited by a US ethnographer (Loic WACQUANT) and a UK ethnographer (Paul WILLIS), has quickly gained in reputation since its first appearance in 2000. Besides publishing work from both sides of the Atlantic, *Ethnography* also attempts to narrow the gap between sociological and anthropological ethnography, which is far from closing in the US. Other journals that regularly publish qualitative and ethnographic research are *Social Psychology Quarterly* and *Gender & Society*, two of the largest and most respected journals in US sociology. [29]

Ethnographic research and debate can also be found in the two most prestigious US sociological journals, the *American Journal of Sociology* (AJS) and the *American Sociological Review* (ASR). In 2002, the AJS was the arena for an important dispute within urban ethnography which I discuss in more detail below (WACQUANT 2002; DUNEIER 2002; ANDERSON 2002; NEWMAN 2002). And in 2003, the ASR published a high-profile paper on "reflexive ethnography" by BURAWOY (2003). Generally speaking, it is rare for these journals to publish ethnographic research by newcomers. Nevertheless, it seems that if established ethnographers have something very new or very important to say, the ASR and the AJS are available as outlets. [30]

Ethnographic writing often requires more space than is available in journals. This is one of the reasons why more ethnographic studies than quantitative ones are published as monographs. To maximize visibility, it is particularly desirable to publish one's work in the context of a specialized book series. Of the three currently active book series for ethnographic research I am aware of, two specialize in alternative genres. Both these series are published by Alta Mira Press. [31]

One of these series, called [Crossroads in Qualitative Inquiry](#) has issued six volumes until now. The other series, [Ethnographic Alternatives](#), has thus far produced seventeen titles. [32]

The third and newest series is called "Fieldwork Encounters and Discoveries." It is published by the University of Chicago Press and targets more traditional naturalistic ethnographies. It was launched in 2003 with the second edition of ANDERSON's classic "A Place on the Corner" (ANDERSON 2003). Two more titles (BEARMAN 2005; DE LA PRADELLE 2005) are forthcoming later this year. There are of course many topic focused book series which I cannot review in detail. Some tend to prioritize qualitative and ethnographic research, for instance Temple's [Animals, Culture and Society](#) series. [33]

Generally speaking, university presses and trade presses like to publish ethnographies because they attract wider audiences than other kinds of sociological prose. Over the years, the University of Chicago Press has accumulated an impressive record of ethnographies, and other presses, such as

Alta Mira, Routledge, Temple, Waveland, Rutgers, and the University of California Press, have also published many excellent ones. [34]

2.3 Research funding

For US sociologists, the two most important federal funding agencies are the National Science Foundation (NSF) and the National Institute of Health (NIH). Several other federal agencies, such as the National Institute of Justice (NIJ) and the US Institute of Peace, also provide funding for sociological research. Of these agencies, only NSF has a mandate to support basic research, irrespective of practical outcomes. All other agencies request that funded research operates within identified areas of need which may or may not be compatible with qualitative and ethnographic research designs. [35]

Even though NSF has funded a number of qualitative and ethnographic research projects in the past, this support was, by the organization's own account, disproportionate to the increasing numbers of qualitative and ethnographic proposals it has received over the last several years. In 2003, the NSF *sociology and methodology, measurements and statistics* programs convened a workshop of experts to remedy this imbalance. The workshop produced guidelines for reviewers and investigators regarding the characteristics of strong qualitative research proposals, and it formulated recommendations on how to strengthen qualitative methods in sociology (see CHEEK 2000). [36]

Last year, the NSF *sociology* program launched a new funding initiative entitled "Strengthening Qualitative Research through Methodological Innovations and Integration." The first deadline for grant proposals passed in May 2005. Depending on the availability of funds, two additional cycles are planned for 2006 and 2007. The program description explicitly seeks proposals that utilize and develop qualitative, including ethnographic, methodology. This is an excellent opportunity for US ethnographers to secure federal funding for their current and future research. Such opportunities did not exist in the past, as evinced in the collective frustration of ethnographers who have sought and not received federal funding. [37]

It is difficult to generalize about the willingness of private and non-profit funding agencies to support qualitative and ethnographic research. Once in a while, ethnographers get lucky and manage to secure private or non-profit support for their work (for instance HSU 2004). This is more likely when the planned research has strong policy implications and addresses social issues that are currently *en vogue*, such as school violence, terrorism, or natural disasters. Overall, trying to fund qualitative and ethnographic research through non-federal organizations is a needle-in-the-haystack kind of affair. Often, private agencies do not follow a rigorous review process, nor are they accountable to outsiders (as long as they operate within the law). One of the ironies is that many non-profit and private agencies promise funding for the remedy of social problems that strongly warrant qualitative and ethnographic research but are unwilling to fund such projects due to their image as being less scientific or rigorous. [38]

I cannot resist including anecdotal evidence gathered from personal experience: in 2003, I submitted an ethnographic research proposal to the Robert Wood Johnson Foundation to explore the uses and meanings of public parks and recreation facilities, consistent with the larger goal of the organization's program to explore public health deficits of Americans, especially obesity. I was told my proposal was rejected because it did not include numeric measures of physical activity as a dependent variable. Upon asking, one officer of the organization suggested that I use heart-rate monitors instead of observations and interviews to objectively measure the utility of public parks. In this case, the mismatch between the organization's research questions and the kinds of studies it funded was particularly obvious, yet this is not an uncommon problem. [39]

2.4 Careers and centers

The academic system in the US features several mechanisms that, to a degree, undermine the formation of intellectual "schools" at individual departments. Even when something like a school develops, its traditions and resources cannot easily be passed on to younger generations. School members have to interact with people at and from other universities if they want to fill the ranks of retiring scholars with like-minded ones. In the US, teaching and mentoring are taken very seriously and take up substantial amounts of a professor's time. This emphasis and the substantial length of Ph.D. programs (ideally five to six years, but students conducting ethnographic research often spend seven or more years in graduate school) foster close bonds between mentors and mentees. Yet there is a firm rule against hiring new Ph.D. recipients at their home departments, and it is very rare for scholars to ever return to the institution at which they were trained. On average, American scholars are more mobile than Europeans which further inhibits the development of schools. Two, three, or more moves during one's academic career *after* the Ph.D. are quite normal in the US. [40]

This system requires US ethnographers who want to find jobs to be marketable to non-local and more mainstream audiences; it also requires the members of any school to maintain close relationships with scholars at other institutions. Ethnographers everywhere seem to be at a special risk of not making it in the academic world early in their careers, because their research and writing takes longer, making them look less competitive in a tight academic market (also view WELLIN & FINE 2001). [41]

Of course, there have been schools in US ethnography in the past, and there are currently some centers at which ethnographic research flourishes and to which one would direct interested students. The strength of a center depends on the number, productivity, and renown of ethnographers among the faculty of a department, and occasionally also on institutionalized resources, such as long-term grants or funds that provide support for ethnographic research and teaching. Several of the largest and highest-ranked sociology departments in the US, such as the University of California, Berkeley (UCB) and the University of California, Los Angeles (UCLA), are powerhouses for the teaching and practice of ethnographic research. Some smaller University of California campuses, such as

Davis (UCD), Irvine (UCI), and Santa Barbara (UCSB) also have solid and/or growing reputations as centers for qualitative and ethnographic research. Several other universities around the country that can be considered centers for ethnographic research are the University of Colorado in Boulder, Syracuse University in upstate New York, and the University of Pennsylvania in Philadelphia. Any department with two or three prolific ethnographers on the faculty stands a good chance at attracting like-minded junior colleagues and graduate students, thus forming niches in which ethnographic research can thrive. Over time, these large and small centers produce scholars who are sent to departments all over the US, thus continuously shifting the ethnographic landscape. [42]

3. Recent Debates

It is not the goal of this section to walk readers systematically through theoretical approaches and perspectives within contemporary US ethnography. In the conference presentation on which this paper is based, I paid attention to two kinds of boundary work, one concerning the *outside* borders of ethnography with neighboring research strategies, and the other concerning ethnography's *inside* separations and debates. [43]

Yet I will only pursue the latter issue in the following paragraphs, the reason being that the (shifting) outside borders of ethnography have been repeatedly discussed in handbook articles and other places (regarding ethnography and ethnomethodology see POLLNER & EMERSON 2001 and MAYNARD & CLAYMAN 2003; regarding ethnography and phenomenology see MASO 2001; regarding ethnography and ethnodrama see MC CALL 2000 and MIENCZA-KOWSKI 2001; regarding auto-ethnography see ELLIS & BOCHNER 2000 and REED-DANAHAY 2001). [44]

To my knowledge, no summary exists of the recent scholarly debates concerning the proper roles of politics and theory in ethnographic research. These ongoing discussions take place in a variety of forums, such as journal articles, methodological appendices, book reviews, replies to book reviews, classrooms, conference rooms, and personal conversations. The spatial dispersion of the debates and the number of people involved make it difficult for occasional witnesses to piece together the arguments. I here offer nothing more than a basic sketch of some major fault lines. A more detailed treatment of the issues, especially of "false consciousness" as a catalyst for both the theoretical and political disagreements, is in progress. [45]

Generally speaking, the US debate over politics and theory in ethnography has become quite heated in recent years, and it might strike Europeans as being overly ideological. It is, nevertheless, an important discussion because it addresses some of the key motives, strategies, and mandates of ethnographic research, besides putting personal and political reputations on the line. It should be noted in advance that these disagreements typically do not affect younger

generations as much as they do more seasoned and established ethnographers who have made larger investments of time and personal identity in their work. [46]

3.1 Political disagreements

In a lengthy 2002 book review published in the *AJS*, Loic WACQUANT (WACQUANT 2002) offered a fierce critique of books written by three of his colleagues: "Sidewalk" (DUNEIER 1999) on homeless street vendors, "No Shame in my Game" (NEWMAN 1999) on the working poor, and "Code of the Street" (ANDERSON 2001) on the struggles of inner city residents. The title of WACQUANT's review—"Scrutinizing the Street: Poverty, Morality, and the Pitfalls of Urban Ethnography"—indicates his dissatisfaction with what many consider to be the best of contemporary urban ethnography. WACQUANT's questionable attacks on the authors' personal integrity and the quality of their scholarship are ignored here in favor of the more interesting accusations he makes regarding the political and theoretical shortcomings of these studies. [47]

WACQUANT's political critique goes as follows. In his view, all three authors obsess over making their subjects look like moral actors in pursuit of middle-class values, "for which it is the only guise under which this subproletariat is deemed 'presentable'" (WACQUANT 2002, p.1470). He identifies the moralizing and romanticizing in which the authors (in his view) engage as part of the repressive culture of individualism in US society that "keeps devising novel ways of making its poor shoulder the weight of their own predicament" (WACQUANT 2002, p.1486). To put it differently, he accuses DUNEIER, ANDERSON, and NEWMAN of "blaming the victim," thereby increasing, instead of relieving, their victimization and suffering. By concentrating on how people at the bottom manage under utterly indignifying circumstances, the authors legitimate the oppressive structures that determine their subjects' fate. In short, DUNEIER, ANDERSON, and NEWMAN fail to become political advocates for their subjects, and they distort the real problems and actions of the poor to make them look more likeable. [48]

WACQUANT's second and connected political point is that DUNEIER, ANDERSON, and NEWMAN shy away from exploring the material constraints and the structural forces that massively mold their subjects' lives. Because of their "neo-liberal" ideology, they virtually banish the role of the state from their descriptions and analyses. Further, he accuses them of blindness regarding the impact of power, especially with regards to race and class relations. These are serious accusations, given that all three authors have devoted themselves to investigating racism and poverty in the US through the eyes of people at the bottom. Aside from his personal animosities, WACQUANT's accusations show the strong bearing of Marxism, critical theory, and BOURDIEU on some US ethnographers. [49]

The three authors offered individual replies to WACQUANT's critique that were published in the same volume of the journal (DUNEIER 2002; ANDERSON 2002; NEWMAN 2002). They responded to the accusations by pointing to some, in their eyes, fundamental misunderstandings and mistakes. For one, WACQUANT

confused the messenger with the message. The emphasis on morality and decency is not introduced by the ethnographers but emanates from almost all of their subjects' actions and beliefs. Even though the authors do *not* always share their subjects' values, the ethnographer's ethics and skills require that conflicting and uncomfortable views are respected and represented truthfully. The last thing DUNEIER, ANDERSON, and NEWMAN want is to dismiss their subjects as "victims of false consciousness," which WACQUANT's theory-heavy approach to ethnography is prone to do upon encountering inconsistencies in informants' actions and beliefs (ANDERSON 2002, p.1534). [50]

Another of WACQUANT's misunderstandings lies in the fact that the authors are not making statements about the fundamental character of their subjects when emphasizing the primary role of decency and morality in their lives. Instead, they are examining the logic of their practices which are fluid and can change from one moment to the next. [51]

DUNEIER, NEWMAN and ANDERSON agree that wanting to help people overcome oppression is a noble goal yet they do not believe that it can be done very easily. In their scepticism towards political change through ethnographic research, the three authors are less extreme than other ethnographers, such as Howard Becker, who tends to be, in his own words, "a complete cynic about the value of sociology for influencing anything" (BECKER, GANS, NEWMAN & VAUGHAN 2004, p.272). BECKER points out that his popular 1960s research on marihuana users did not lead to any significant changes towards the better, even though it clearly showed that government policies were "foolish and counterproductive." Worse, today's laws concerning the use and possession of drugs are more punitive than ever. In BECKER's view, the main obstacles to the kinds of solutions ethnographers may be able to offer to policy makers (if they are willing to listen in the first place), is that they are too expensive and too unpopular because they usually call for additional resources and structural change. [52]

Can ethnographic research really make a difference? Despite slim evidence overall, at least one recent case deserves mention. In retrospect, one of the most successful ethnographies in terms of public recognition and political impact was Diane VAUGHAN's study "The Challenger Launch Decision" (VAUGHAN 1996). This ethnography of the organizational structures and processes at NASA tried seriously to figure out what went wrong in the first space shuttle accident, the loss of "Discovery" in 1986. When the second space shuttle, "Columbia," exploded in 2003, VAUGHAN received *instant* attention for her work from the press as well as from the highest-ranking government and NASA officials (BECKER et al. 2004). [53]

Since then, VAUGHAN has been consulted intensely in a desperate effort by NASA to avoid further catastrophes. She has revisited her former site and found that little has changed since she left in the early 1990s. VAUGHAN is sure that she has something to offer that might lead to improvements at NASA, yet warns that it is very difficult to put ideas into practice, even though the people with whom she talks seem to have the best intentions. Organizational structures, not individuals, are the problem at NASA, and "systems are hard to change"

(BECKER et al. 2004, p.273). In sum, the political hopes and agendas of contemporary US ethnographers vary widely, ranging from complete resignation to fully fledged campaigns. [54]

3.2 Theoretical disagreements

WACQUANT's political accusations are closely intertwined with the theoretical issues he takes with his colleagues' work. He bemoans that DUNEIER, ANDERSON, and NEWMAN do not utilize sociological theories to bring the impact of race, class, and the state more strongly into focus. In his view, the problem is that they do not theorize much at all.

"The remedy here is to recognize that there is no such thing as ethnography that is not guided by theory (albeit vague and lay) and to draw the implications, that is, to work self-consciously to integrate them actively *at every step into the construction of the object* rather than to pretend to discover theory 'grounded' in the field, import it wholesale postbellum, or to borrow it ready-made in the form of clichés from policy debates." (WACQUANT 2002, p.1523, emphasis in original). [55]

The authors' weak use of theory is apparent in their flawed sociological analyses. In WACQUANT's view, DUNEIER is guilty of "reducing sociological analysis to the collection and assembly of folk notions and vocabularies of motives" (WACQUANT 2002, p.1523) while ANDERSON "conflates folk with analytic concepts" (WACQUANT 2002, p.1500). Instead of seriously engaging in theorizing and analysis, the three studies tell the "fairy tale of grounded theory" and celebrate the kind of "raw empiricism" that has been symptomatic of US urban ethnography for many years. [56]

WACQUANT's critique goes along, in spirit and in substance, with what Michael BURAWOY (his colleague at Berkeley) has voiced in various places about new Chicago-style ethnography. In a milder version, WACQUANT's review also reflects some of the concerns and priorities of other critical and feminist ethnographers. [57]

How did the three authors respond to WACQUANT's theoretical accusations? By strongly refuting the idea that ethnographic research should start with an elaborated theory, no matter of what kind. DUNEIER concedes that everyone comes to the field with questions, maybe even theoretical ones. The important issue is not to let these theoretical notions get in the way of the actual research. Ethnographers always need to be willing to change their theories if they do not fit the facts. ANDERSON further points out that the folk concepts dismissed by WACQUANT are exactly what ethnographers are after in their work. The three authors' replies thus resemble Harold GARFINKEL's "unique adequacy" requirement for work in the ethnomethodological tradition, and SCHUTZ's emphasis on "first order constructs" in the phenomenological perspective. [58]

A recent paper by Jack KATZ (2004) can be read as an attempt to bring more order into these kinds of exchanges. KATZ suggests a model of three different

genres of contemporary ethnographic research—provocatively called worker, bourgeois-professional, and aristocratic—which fundamentally differ in their research designs and ultimate goals. [59]

Worker ethnographies, such as "Sidewalk" (DUNEIER 1999) or "A Place on the Corner" (ANDERSON 2003) are primarily single-sited studies whose authors collect huge amounts of relatively unspecified data. Their major goals include the debunking of stereotypes that apply to certain groups of people, and the investigation of new life-worlds. The *bourgeois-professional* style of ethnography, exemplified by VAUGHAN's study of divorce (VAUGHAN 1986), prefers a more comparative kind of design which requires researchers to amass a series of data sets. Its major goal is the construction of models and hypotheses, following the logic of analytic induction. [60]

The last genre of ethnography, which characterizes the work of WACQUANT and BURAWOY, is called *aristocratic* because its proponents use a privileged point of view. From KATZ's perspective, the somewhat shallow research designs that are typical of this type of work (KATZ 2004, p.305)—even though WACQUANT's latest book (WACQUANT 2004) seems to be an exception—do not meet the established standards of ethnographic methodology. It is the goal of these ethnographers to verify previously developed master theories, and to expose and correct the "false consciousness" of their subjects. Needless to say, this is not the kind of ethnography KATZ encourages his students to do. [61]

The current political and theoretical debates in US ethnography have helped to clarify viewpoints and disagreements that were often left implicit in the past. While there is still a good deal of rhetoric and "class warfare" behind the various statements, it is important to figure out exactly what the substantive disagreements among various branches of US ethnography consist of, and it will be important to continue the discussion of these issues in the future. [62]

4. Methodological Issues

The previous section, as well as the following one, shows that political and theoretical aspects of research are difficult to disentangle from methodological ones. I here address two salient issues concerning the recent practice of ethnographic research in the US: first, the increasing diversity of research methods and designs, and second, the now mandatory approval of all research at US universities by so-called Institutional Review Boards (IRBs). [63]

4.1 Diversity of research methods and designs

A flood of new and revised manuals and textbooks on qualitative and ethnographic research has hit the US market over the last several years (FETTERMAN 1998; ROSSMAN & RALLIS 1998; JANESICK 1998; BERG 2004; MC CURDY, SPRADLEY & SHANDY 2005; LOFLAND, SNOW, ANDERSON & LOFLAND 2005; WARREN & KARNER 2005; ANGROSINO 2005; plus the seven-volume series edited by [SCHENSUL & LE COMPTE](#)). Without a doubt,

many of these publications offer innovative angles and approaches. Yet there is only so much that has not been said (much of it many times) in previous volumes on doing ethnographic research. I believe that two of the last noticeable gaps on the methods shelf of ethnographic researchers were closed with "Writing Ethnographic Fieldnotes" by EMERSON, FRETZ and SHAW (1995) and "Tricks of the Trade" by BECKER (1998). [64]

Aside from practical guides and the three handbooks, a good number of recent publications (not limited to the US context) reflect on methodological aspects of ethnographic research (ELLIS & BOCHNER 1996; WOLF 1996; PRUS 1996; DENZIN 1997; GUBRIUM & HOLSTEIN 1997; DE VAULT 1999; COFFEY 1999; EMERSON 2001b; BOCHNER & ELLIS 2002). Rather than reviewing the above sources, I prefer to highlight the quickly growing diversity of ethnographic research techniques and designs. [65]

The increasing use of *visual images* in US ethnography is a notable phenomenon that has been commented and reflected upon before (HARPER 2000 and BALL & SMITH 2001; general sources include EMMISON & SMITH 2000; BANKS 2001; VAN LEEUWEN & JEWITT 2001; ROSE 2001). The number of ethnographers who include images in their books, ranging from a few documentary photographs to theory-generating visual analyses, is growing quickly. For examples, see BOURGOIS 1995; DUNEIER 1999; KATZ 2001; CHIN 2001; LOW 2003; NEWMAN 2004; WACQUANT 2004; ADLER and ADLER 2004. [66]

On the one hand, ethnographers and qualitative researchers have developed a vivid interest in analyzing the role of naturally produced images or visual artifacts in people's lives, ranging from personal snapshots (CHALFEN 1991) to billboards (GUDIS 2004). A better understanding of how we consume visual images is crucial for the analysis of popular culture, as well as many other arenas of everyday life. [67]

On the other hand, ethnographers are developing new ways of creating and then using original images. One new method consists of providing cameras to subjects who are then asked to visually record their own environments and experiences. The produced images provide insights into aspects of a person's experience that are otherwise unavailable, or at least difficult to access. This year's Academy Award winning documentary film "Born into Brothels" (BRISKI & KAUFFMAN 2004) has drawn popular attention to this increasingly widespread method of ethnographic research, especially for studies involving children. Another interesting way in which photographs become a resource in interactions with research participants is the "photo-elicitation interview," expertly used and described by Douglas HARPER (2001, 2002), and adopted by many others in recent years. [68]

To briefly highlight a second trend, I noticed that more and more ethnographers begin to recreate, and then observe, real-live situations with their study participants. For instance, CHIN (2001) reports taking African-American children on a \$20 shopping trip to better understand their views and practices surrounding

consumption. While conducting neighborhood research in Hollywood, I asked many participants to give me a tour of their homes and neighborhoods, and to take me along on routine walks and errands in the area, thereby exploring a technique I later called "go along" (KUSENBACH 2003). Several other examples could be cited here. The implications of such techniques, which could be called "experimental" to distinguish them from more naturally occurring events, are not yet fully explored. [69]

The last several years have also demonstrated the employment of increasingly complex and sophisticated research designs by ethnographers. (This said, street-corner studies that rely on "hanging out" as the primary research strategy are not threatened with extinction.) Several trends can be observed: single-sited studies are often enhanced through investigations of other sites that are both similar and different than the original one. Compared with earlier decades (e.g. ZORBOUGH 1929 and KORNBLUM 1974), more recent ethnographies (such as ANDERSON 1990; THORNE 1993; DUNEIER 1999; LOW 2003) pursue multi-site comparison as a systematic, analytic strategy in their work (see MARCUS 1998 for a detailed discussion of multi-sited ethnography). [70]

Another strategy leading to the exploration of multiple sites has less to do with contrast and comparison and more with understanding how what happens elsewhere influences the original site, by tracing the off-site origins of personal, institutional, and political realities. This is an approach DUNEIER has called "extended place method" (DUNEIER 1999, p.344), as an alternative to BURAWOY's "extended case method" (BURAWOY et al. 1991). BURAWOY's approach characterizes a theoretical stance more than it outlines a research design, yet it encourages the inclusion of historical and geographical contexts in one's research. In a more recent book (BURAWOY et al. 2000), BURAWOY continues the development of what he now calls a "global ethnography." Likewise, Dorothy SMITH's feminist model of an "institutional ethnography" (SMITH 2005), aimed at dissecting the "relations of ruling" that shape social sites, also leads scholars to explore places other than those focused on at the beginning of their work. [71]

Increasingly, multi-site ethnographies are conducted by research teams which combine scholars of different ranks, ages, genders, races, and ethnicities. Barrie THORNE's ongoing team ethnography of childhoods in Oakland is a good example of this trend. Other examples include an ongoing investigation of housing project relocation headed by Susan GREENBAUM in South Florida, or the Hollywood research project headed by Jack KATZ in which I participated. [72]

Lastly, there are now more examples of the sort of study that has been practiced and called "revisits" by BURAWOY (1979 and 2003). A good recent example is Diane VAUGHAN's mentioned re-study of NASA (which has not yet been published). Many of the benefits revisits provide can also be found in long-term research conducted at the same site(s), for instance in the work of Eli ANDERSON. All in all, it becomes obvious that ethnographic research methods and designs in the US have become increasingly diverse over the last decade.

This development invigorates the methodological discourse, yet it also contributes to further fragmentation and division of the field. [73]

4.2 Dealing with Institutional Review Boards

The ethics of conducting ethnographic research is an important and widely discussed topic in the US and international academic contexts (cf. VAN MAANEN 1983; CHRISTIANS 2000; MURPHY & DINGWALL 2001; BOSK & DE VRIES 2004). As MURPHY and DINGWALL point out:

"The ethics and the politics of ethnography are not clearly separable. Questions about the right way to treat each other as human beings, within a research relationship, are not wholly distinct from questions about the values which should prevail in a society, and the responsibility of social scientists to make, or refrain from, judgments about these. [...] The lack of consensus about methodology, which marks contemporary debates in and about ethnography, is reflected in discussions about its ethics." (MURPHY & DINGWALL 2001, p.339) [74]

Even though debates concerning fieldwork ethics are clearly open ended, something more definite can be said regarding recent changes in the US academic landscape towards stricter enforcement of certain ethical guidelines. For the first time in history, all research undertaken at universities needs to demonstrate compliance with a set of official rules designed to protect human research subjects from various possible abuses. After briefly outlining the history of this requirement, I discuss some of the practical concerns ethnographers have about its implementation. [75]

The US history of protecting human subjects in scientific research began with the "Nuremberg Code," a set of rules developed for the post-World War II Military Tribunals in Nuremberg to guide the judges in their decision-making regarding the atrocious scientific experiments conducted on Jews in Nazi Germany (http://cnri.edu/IRB_History.htm). These outlined principles include, most notably, the freely given consent of all individuals involved in the research, the minimization of risk and damages, a favorable harms/benefits ratio, and the highest attainable quality of research design and research personnel. [76]

Despite the long-term existence of these guidelines, US medical researchers in the 1950s, 1960s, and 1970s regularly conducted experiments with patients without their consent, and without fully disclosing the dangers of such trials. The most infamous case is probably the so-called Tuskegee Syphilis Experiment. As recently as 1973, researchers intentionally withheld penicillin treatment from Black male subjects who were unaware of their positive diagnosis of syphilis. As a direct result of these experiments, twenty-eight research subjects died, hundreds suffered severe disabilities, and an unknown number of sexual partners contracted the disease. Another equally scandalous case involved the prescription of the experimental drug *thalidomide* (better known in Europe under the name *contergan*) to pregnant women in the United States, Canada, and Europe to control nausea and insomnia, which resulted in high infant mortality

and severe limb deformations in approximately 12,000 newborns during the late 1950s and early 1960s. [77]

As a direct result of the Tuskegee Syphilis Experiment scandal, the first Institutional Review Boards (IRBs) were established in 1974 to improve the general oversight of all federally funded research, and to ensure that it complies with the outlined ethical principles. Many universities volunteered to subject all science research involving humans, not only government funded projects, to IRB procedures and guidelines. Special clauses protecting pregnant women, prisoners, and children were added to the existing regulations throughout the 1980s and 1990s. [78]

Initially, the review procedures were aimed at medical research, where flawed research designs can mean the difference between life and death for participants. This does not mean that social science research cannot be unethical and potentially damaging, as the famous MILGRAM torture experiments at Yale from 1963 or ZIMBARDO's prison study at Stanford from 1971 show. Considering ethnographic studies, ethical controversy surrounds Laud HUMPHREYS' research of anonymous sex in public places (HUMPHREYS 1975) and Richard LEO's investigation of police interrogations (LEO 1995a and 1995b), to give only two examples. Yet until the late 1990s, researchers at US social science departments did not seek approval for their work from the IRBs of their institutions, and they were not pressured to do so, whereas medical researchers complied on a routine basis. [79]

Nevertheless, federally supported medical research suffered a deep crisis in the late 1990s which increased the pressure on *all* researchers, now including social and behavioral scholars, to seek IRB approval for their work. The death of a young man in a gene-therapy trial at the University of Pennsylvania in 1999 created a sort of hysteria at universities. This trial was only one of several cases in which the government terminated its financial support for a large research project because of a lack of compliance with IRB rules. Facing or imagining the cost of malpractice lawsuits accusing them of oversight lapses at their institutions, university administrators panicked and began more rigorously to enforce IRB compliance. Administrative pressure from the government and from universities, combined with political pressure from conservative organizations in Washington who were concerned about research involving children, lead to a climatic change that fundamentally altered the research process of ethnographers and other sociologists (BRAINARD 2001). [80]

Initially, sociological research proposals, and especially qualitative and ethnographic studies, faced resistance from IRB members who were largely unfamiliar with these kinds of research. The first few years of required IRB compliance for sociological research were especially difficult and fraught with delays, misunderstandings, and conflicts. The main problem was not that quantitatively trained reviewers outright rejected qualitative and ethnographic research, but that they encumbered it with time-consuming and sometimes inappropriate requests for specifications and modifications, causing immense

frustration on the side of the applicants. Generally speaking, it took several years to work the major kinks out of an approval system that seemed systematically to disadvantage social scientists in general, and qualitative/ethnographic scholars specifically. [81]

Today, virtually all large and most mid-size colleges and universities have Institutional Review Boards that are exclusively geared towards the approval of social science research. On average, qualitative researchers are still underrepresented on these boards. Expedited procedures and even instant exemptions are available for projects that do not seem to pose any dangers to subjects, such as most survey research, for which approval seems to be a formality. Yet whether a project qualifies for quick approval or exemption still has to be decided by the members of an IRB committee and not by the researchers themselves, which, almost paradoxically, results in yet another round of review. [82]

After the first approval is received, any major change in the research design has to be re-submitted and re-approved by the IRB, posing a particular challenge for ethnographic research that routinely evolves in unexpected directions. A glance at the [various forms](#) that need to be filled out and submitted together with a detailed research proposal and consent forms reveals the high level of complexity of the procedure. [83]

A special problem is posed by the extension of mandatory IRB compliance to the work of graduate and undergraduate students. It does not apply to research exercises completed within the context of a course, yet without prior (not retrospective!) IRB approval, students are prohibited from using any originally collected data for theses, dissertations, conference papers or publications. It is difficult to underestimate the extent of anxiety and procrastination this causes among students, and the extra amount of work it entails for their advisors who often end up taking it upon themselves to gain approval for their students' research. Imagine the headaches this causes instructors of ethnographic and qualitative methods classes in which students cannot simply begin doing research but need first to submit comprehensive research proposals, consent forms, and IRB forms (which often takes up half or more of the semester) before they can start their fieldwork projects. [84]

Many ethnographers are not fundamentally opposed to the goals of IRBs and follow the procedures, even though they might feel burdened by them. A streamlined process and more knowledgeable reviewers will make a big difference to this group of scholars in the future. One source of consolation consists in the fact that IRBs have little means of controlling and even knowing what scientists are doing *after* receiving approval for their proposals. In this view, IRB compliance simply becomes one more hoop to jump through before the real work can begin. [85]

Yet a good number of ethnographers express more than annoyance with these external regulations, which did not exist as recently as five or six years ago. Still today, a good number of (mostly senior) ethnographers simply do not apply for

IRB approval of their work, or seek permission for partial research projects only, as a deliberate challenge to the system. Several say that they are ready to go to court over this issue, and a few seem to look forward to the opportunity to defend their views in public. Clearly, confrontational strategies cannot be recommended to, and are rarely adopted by, students, untenured faculty, and large grant recipients because of the tremendous career costs that might follow from such opposition. [86]

For the hardcore opponents of IRBs, their resistance is not rooted in procedures but in principles. They have two primary concerns: first, they claim that it is impossible for ethnographic research to meet IRB requirements and at the same time preserve the methodological flexibility, analytic creativity, and human investment that are essential ingredients of good ethnographic work.

"For ethnographers, the primary data gathering tool consists of the relationships that we forge with those whose lifeworld we are trying to understand. Few of us start with specific hypotheses that we will later test in any systematic way. Furthermore, to the degree that we can restate our disciplinary curiosity as a set of testable propositions, these hypotheses are likely to be trivial." (BOSK & DE VRIES 2004, p.253) [87]

In this view, it is impossible to clearly state the risks and benefits of participating in an ethnographic study because they are inseparable from the risks and benefits of forming relationships in everyday life. [88]

Second, opponents claim that IRB regulations violate the constitutional "First Amendment Right" to free speech. They frequently emphasize that professional journalists, whose methods of inquiry resemble those of ethnographers, are protected by this amendment, and that this protection should therefore apply to academics as well. One difference is that journalists are not employed by the state, nor do they receive federal funding for their work. This argument seems to overlook structural differences between journalism as a "commercial enterprise" (BOSK & DE VRIES 2004, p.255) and academia. Another general argument that has attracted the attention of lawyers claims that it is unconstitutional to require approval *before* issuing the right to research, as opposed to implementing ethical reviews, and penalties for unethical conduct, after the fact. [89]

Aside from the general process, critics have concerns about almost all of the IRBs' specific guidelines. Yet if one needed to identify the two most controversial ones, it could well be the requirements of informed consent and anonymity of research subjects. [90]

The requirement of consent means that virtually all covert ethnographic research is impermissible. For ethnographers studying illegal, immoral, or deviant life worlds, it seems almost impossible to seek consent from research subjects without destroying the very access to such social worlds. Therefore, not all ethnographers are willing to give up the opportunity to operate under cover, at least part of the time, in parts of their sites. Even when scholars are willing to seek full consent, and subjects are willing to give it, it is sometimes simply

impossible to do so, for instance when studying semi-public settings with a high turnover of people. [91]

Another problem consists in that the mandatory consent forms are long and complicated, and can thus be intimidating to subjects of limited literacy skills. Furthermore, the signatures on consent forms create a record of the subjects' participation in the research which many might be hesitant to leave behind. It seems ultimately impossible to preserve documentation of both the subjects' consent *and* their anonymity at the same time. [92]

Regarding anonymity, a recent case of non-compliance might illustrate some of the problems this requirement poses. DUNEIER (1999) did not anonymize any of the subjects in his ethnography of New York City street vendors which began years before IRB compliance was considered mandatory, and for which he did not receive financial support. Instead, he used his informants' real (first) names with their explicit approval, along with the full names of city officials and other experts who he consulted in their professional capacities. Furthermore, DUNEIER included personal profiles and numerous photographs of most of his informants in the book. An hour-long documentary film on the lives of street vendors, featuring many of the original subjects, has just been completed. Lastly, DUNEIER regularly invites some of his informants to visit his classrooms and to collaborate with him in other ways. [93]

What are his reasons for so bluntly disregarding the golden rule of anonymity which many ethnographers endorse, independent of what IRBs might require? One of the reasons, he explains, is that his subjects did not want to have their identities disguised, and falsifying their names would have thus been disrespectful of their preferences (DUNEIER 1999, p.349f). Many ethnographers discover that while they are ready to grant full anonymity to their subjects, this kind of protection is not important to them and might even be viewed as dishonest. [94]

DUNEIER also points out that by not anonymizing the location of his study and the identities of his subjects, he can be held to "a higher standard of evidence" (DUNEIER 1999, p.348). Because others can easily "check out" the quality of his work, he has increased, and not decreased, his accountability by putting his professional and personal reputation on the line. This forceful argument is mirrored in other arrangements that go far beyond what is usually done in ethnographic research. For instance, DUNEIER shares the royalties of his successful book with the persons depicted in it, he sought explicit consent from his participants for every book passage in which they are described, and he allowed his key informant to write an afterword which is published in the book. It would thus be unfair to accuse DUNEIER of ethical misconduct without seriously considering the choices he makes, and his reasons for making them. [95]

In sum, we might need to reconsider who benefits more from the anonymity requirement, the *subjects* or the *researchers* (by shielding them from scrutiny and accountability), and with which other ethical principles the outlined requirements

might conflict. A more comprehensive discussion of the ongoing debates concerning IRBs would take up much more space than is available here. Time will tell whether the concerns many ethnographers now have can curb or even diminish the practical control IRBs have managed to seize over the last several years. [96]

5. Conclusion

In summary, my selective overview of the state of ethnography in US sociology has shown that it is doing well overall. In institutional perspective, US ethnographers have successfully capitalized on their (comparatively) large numbers. Ethnography is firmly established and recognized as an important genre of research in the national sociological association (ASA). Over the last decade, ethnographers have continued to organize themselves through organizations and journals, and began holding their own conferences. Recent efforts towards community building have united, yet also more clearly divided, the large field of practitioners. One positive trend lies in the openness of contemporary US ethnographers to invite future generations and international colleagues into their meetings and debates. Ethnographic research continues to be popular among US students; it often recruits them into sociology. Even though ethnographers manage to find jobs at US universities, fieldwork as a career is not the fastest and surest way to success. Some of the structural disadvantages ethnographers currently face are unlikely to disappear in the future. [97]

Among themselves, US ethnographers are currently engaging in lively debates concerning the proper roles of politics and theory in their work. These discussions should be informative for all sociologists because they are a version of the fundamental struggle of how to reconcile "micro" and "macro" perspectives, how to merge theory and research in one's work, and how to best combine research and advocacy. Past trends towards diversification and specialization of ethnographic research methods and designs have intensified over the last decade. Ethical questions also loom large. While mandatory compliance with institutionalized guidelines now affects everyone in academia, it poses a unique challenge to ethnographers by fundamentally altering the process and strategies of their research. Imagination, flexibility, and personal skills are key ingredients of all field research, and ethnographers of all convictions tend to resist the increasing bureaucratization and oversight of their work. [98]

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