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ARTICLE

'Getting Out' in Ethnography
A Seldom-told Story

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ABSTRACT

In ethnography and related qualitative research that relies on naturalistic observation or fieldwork, 'getting in', or accessing a research population or site, receives considerable attention, as do data collection and analysis and writing-up. Yet despite the recent increase in ethnographic publications and methodological sophistication across the globe, scant attention is paid to 'getting out', or leaving the field. The exploration in this article stems from unexpected challenges to 'getting out' that a team of nine researchers experienced during a five-year, five-city ethnographic research project in the USA. Given the growing emphasis on reflexivity in ethnography, the expanding mandates of institutional review boards, and the vigorous theoretical and method-ological debates taking place in many countries, increased attention to 'getting out', from multiple theoretical and epistemological perspectives and locations, could enrich the ethnographic research enterprise.

KEY WORDS:

ethnography

'getting out'

leaving the field

postmodern perspectives

reflexivity

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THE 'GETTING OUT' STORY: PROLOGUE

In ethnography, access to a research population or site receives understandable attention, as the research could not be conducted without 'getting in'. As such, authors of ethnographic monographs who describe their research procedures and methods texts focus intensively on access, data collection and analysis, and writing-up. Yet despite the global increase in ethnographic publications and methodological sophistication over the past couple of decades, little attention has been paid to 'getting out', which is also called leaving the field or disengagement.

For example, over 25 years ago Snow (1980: 100) noted the 'scattered references to disengagement in the ethnographic literature', attributing the in-attention to the separation of personal/private from professional concerns: 'Since such constraints and pressure are generally thought of as "personal" or "private" or extraneous to the research process, they are typically glossed over, if discussed at all, in scientific and professional journals and monographs' (Snow, 1980: 118). More recently, Lofland and Lofland (1995: 62–3) argue, as we do, that there is still insufficient attention to disengagement: 'The handling of these voluntary departures probably deserves more careful thought and pre-planning than field-workers have traditionally given to it'. Further, 'for those investigators (known or unknown) in more stable settings who – because of close personal ties with the researched or because of future research agendas – want to leave open the possibility of a return, the issue is a live one indeed'.

Whatever the reasons for limited attention, three aspects of the contemporary research landscape suggest the need to revisit 'getting out'. First, the growing, albeit contested, emphasis on reflexivity (Bourdieu, 1977; Bourgois, 1995; Burawoy, 2003; Marcus, 1998), which signifies that the researcher is conscious of her/his relation to those she/he studies and to a body of theory she/he shares with other scholars, redirects attention to all aspects of social research. Second, the ethical mandates of institutional review boards (IRBs) are increasingly far-reaching, although of greater concern to researchers in the USA than in Europe at this point (Kusenbach, 2005). Third, vigorous theoretical and methodological debates about ethnography and related qualitative research are taking place across the globe: in the UK (Henwood and Lang, 2005), Germany (Mruck and Mey, 2000), Italy (Bruni and Gobo, 2005), Japan (Suzuki, 2000), Israel (Weil, 2005), Slovenia (Adam and Podmenik, 2005), Mexico (Cisneros Puebla, 2000) and elsewhere. These interchanges offer scholars the opportunity to further refine ethnographic theories and practices.

Contributing to this discourse, this article focuses on 'getting out' from two standpoints: research experience and the ethnographic literature. First and centrally, the issues raised here stem from researchers' experiences in a national ethnographic research project that I headed as principal investigator (PI). The research was designed from the outset to have clear parameters (boundaries) and

a fixed termination or end point. The research team members initially conceptualized 'getting out' accordingly, as field notes from the Seattle researcher to the PI midway through the original one-year study reflect: 'I think people might need another note in late September before you visit – saying that you'll have some final/or near final questions for them when you visit'. In response, my field notes read: 'Call families to "exit" – Remind *all* families to contact their Seattle case manager if they need anything/questions, etc.'.

A different reality, however, surfaced in an email from one of the Philadelphia ethnographers in late November 2006 – more than three years after the official end of the research – that revealed continued contact with one of the families: 'I have kept up with [study participant]. Her story is not a happy one, but I would like to bring you up to date'. This surprise led me to revisit the empirical material to examine more fully how the other researchers conceptualized and experienced disengagement.[1] Second, the researcher's email also led me to re-examine how ethnographic monographs and methods texts address getting out. As Burawoy et al. (1991: 9) note, 'When our [pre-entry] expectations are violated – when we discover what we didn't anticipate (for us, the variation in "getting out") – we then turn to existing bodies of academic theory [or literature] that might cast light on our anomaly'.

Accordingly, this article first describes the economic mobility ethnography in which the researchers' experiences with disengagement were embedded. Their experiences are then illustrated by the ethnographic material, with reference to the scholarly literature. Briefly, the varied disengagement processes we discovered, both between researchers and even in the actions of a single researcher, seem related to four ethnographic issues: the researcher's disciplinary and epistemological orientation; researcher and participant characteristics; researcher and participant role perceptions; and, the pattern of research funding. While the first three issues are not completely absent from ethnographic literature, they are seldom discussed in relation to leaving the field. The fourth issue, the pattern of research funding, has heretofore received virtually no attention.[2] In addition, these issues seem to be intersecting and multiplicative, although they are discussed singly here to highlight their particularities. Final thoughts and possible directions for the ethnographic research enterprise form the conclusion.

THE ECONOMIC MOBILITY ETHNOGRAPHY

As background, classical or traditional ethnography is rooted in the field of cultural anthropology. Characteristically, the ethnographer is immersed in the culture of the 'other', often for years, expecting thereby to understand the culture as an 'insider' through rapport built over time with the society's members. From the more recent perspectives of urban sociology and critical anthropology,

ethnography is interactive and collaborative rather than unitarily and 'objectively' imposed (Burawoy et al., 1991; Holmes and Marcus, 2005), and increasingly is action oriented as well (Denzin and Lincoln, 2005). The economic mobility ethnography draws from these more recent paradigms; as such, it is oriented reflexively toward both theory building and action.

Research Design

Between 1998 and mid-2003 in five US cities – Philadelphia, New Orleans, Milwaukee, St. Louis, and Seattle – the research team of nine, including myself as PI, followed 25 low-income families and about 1000 related auxiliary contacts to learn about contemporary economic mobility in the USA (Iversen, 2002; Iversen and Armstrong, 2006). The cities were added sequentially over a two-year period. We focused specifically on how low-income families, their work-places and their communities experience the parents' efforts to move up through work. 'Low income' was defined as earnings from employment below 200 percent of the federal poverty threshold. 'Moving up' meant that the key parent in the families attended a local job training program that was affiliated with a national workforce development demonstration, obtained a 'good' job in terms of wages, non-wage benefits, and advancement opportunity, and was eligible for emotional and/or instrumental post-program support for a period.

Although the field study and methods characteristic of ethnography are increasingly varied (Denzin and Lincoln, 2005), most involve interpersonal engagement in natural settings. In this research, life-history and informal interviews, observation and participant observation, shadowing and informal interviewing in the families' worksites, training programs, community organizations and neighborhoods, document review, program administrative data, transcripts, field notes (including emails), census data, responses to repeated administration of a community-focused depression scale, and media accounts constituted the full set of empirical material. The discussion here is drawn primarily from the audiotaped and transcribed interview material and research field notes.

For the study as a whole, we organized the interview and observational material around the key parent and his or her family and used a qualitative software program for initial categorical coding and retrieval of broad themes. The core analytic strategy was mining the empirical material to develop 'family stories' through a diachronic narrative approach (telling a story through time). The local researcher and I met with each family to review the written narrative before publication to check for both fact and interpretation – a process variably called 'member checking' (Padgett, 1998), 'valedictory revisiting' (Burawoy, 2003), 'checking in' (Duneier, 1999), or the 'avoidance of errors of attribution' (Becker, 1996). Changes resulted through a process of discussion and resolution. Where disagreement remained, we identified it as such in the

narrative. As postscript, and extending the notion of 'checking in' to researchers as well as participants, I sent the near-final draft of this article to the researchers quoted herein for their thoughts and comments, some of which were then added to the final manuscript.

'Getting Out' Design

The story of 'getting out' in the economic mobility ethnography underscores that the disengagement aspect of the research process is embedded epistemologically and methodologically in the prior phases: purpose of the research,[3] as described earlier; selection of local researchers for the team; getting in and starting the research; forming relationships; extending contact beyond the families' walls; and, reflecting throughout on interactions and processes.

As such, the PI selected experienced researchers who lived in or at the periphery of the five research cities. In Milwaukee, New Orleans and Philadelphia, two local researchers split the family inquiry. Getting in and starting the research was the PI's responsibility, facilitated by the demonstration's intermediary in the cities. In most cases, the PI and local researcher conducted the initial family interview meeting together. The local researcher subsequently spent intensive time with the families and the PI spent a week in each city at three-month intervals. Thus the fieldwork consisted of what Burawoy (2003) calls a 'rolling revisit', whereby each visit is a conversation that is connected to earlier and portends subsequent ones. Both researchers identified and engaged relevant auxiliary contacts throughout the study period.

The customary repertoire in my discipline of sociologically-based social welfare, which is also informed by social work principles and practices,[4] includes definitive ending practices which are based on boundaries and endpoints that are specified from the onset of research contact, frequent reminders of the timetable during fieldwork, and referrals, if needed, upon disengagement. As such, the application to my university's Institutional Review Board (IRB) described a 12-month project that the participant consent form reflected and that the local researchers and I discussed with key parents in the first meetings. In those meetings we gave each family a study information sheet that included the research parameters and procedures and how to contact us. We also invited questions about the study several times during the first meeting.

It was particularly important to include everyone in the family in the discussion of research procedures, especially those present who may be critical gatekeepers, as my field notes from the first meeting with a 24-year old Milwaukee parent describe:

> While Tasha was reading over the family information and consent forms,[5] she asked her father to come over and talk with me and see the forms. I explained the study to him also. (PI field notes)

The importance of including Tasha's father was borne out in the family story review at the final meeting:

> Tasha: [Re family story] I liked it and my Dad liked it. [I said how glad I was that her parents had read it]. There's some negative stuff in there, but it's true. (PI field notes)

The research team also reviewed the procedures in subsequent meetings, which provided an index of the participant's understanding of the research, and periodically by letter. In addition, we created ending rituals that included plaques, books or gift certificates at the 'final' research contact at twelve months. About six months later, the local researcher and I met with each family to review their written narrative and subsequently mailed each family its revised and updated 'Family Story'.

That said, many of the potential plusses of ethnographic field methods, such as intensivity, extensivity and relational embeddedness, can make getting out more complex. We look now at the four issues that seem related to variation in 'getting out' in the economic mobility ethnography identified earlier: disciplinary and epistemological orientation of the researcher, researcher and participant characteristics and role perceptions, and research funding.

ISSUES IN THE ECONOMIC MOBILITY ETHNOGRAPHY THAT INFLUENCED 'GETTING OUT'

Disciplinary and Epistemological Orientation

In examining the empirical material from this ethnography, the first thought was that different 'getting out' processes might stem from disciplinary tradition and/or epistemological position, as Denzin and Lincoln (2005) also suggest. Briefly, the general anthropological tradition is to stay in the field for a year or more to capture a complete annual cycle of events among the population under study (Padgett, 1998: 69) or to re-visit the culture after time away. Because the culture or population is often geographically distant, disengagement may be rather complete (see also Note 3). In contrast, some ethnographers 'stay in'. Urban sociologist Elijah Anderson's research (1990, 1999) is ongoing by design, likely facilitated by geographic proximity. Other researchers disengage but leave the door open for future inquiry. For example, sociologist Kai Erikson (1976: 248) felt that the legal financial settlement that coincided with his post-research trip back to the study site provided a natural closure to the research: in his words, 'All of us felt that we had come to the end of a very important episode in our lives and were about to move on to other personal and professional concerns'. Even so, he left the door open, adding that 'all of us managed to leave a piece of unfinished business behind in order to have a reason for return', and in fact Erikson returned several times thereafter.

These and similar reports from other ethnographies suggest that disengagement differences are not necessarily uniform by discipline, although full assessment of this is difficult because few scholars articulate how they left the field. As such, and especially because IRBs now require researchers to specify and report end of contact information, we revisit the possibility of disciplinary influence on disengagement in this research.

I had hired a diverse team of researchers, believing that triangulation (Padgett, 1998), which implies a fixed point of reference that can be illuminated by varied perspectives, or 'crystallization' (Richardson and St. Pierre, 2005), which 'reflects and refracts, creating ever-changing images and pictures of reality' (Denzin and Lincoln, 2005: 912), would enrich the empirical material and team conversations without substantially altering the design parameters. The disciplinary backgrounds of the multi-ethnic team included cultural anthropology, applied anthropology and urban studies, urban anthropology, social welfare, sociology and social welfare, public administration, and professional workforce development. As Massey et al. (2006) underscore, it is important to identify the lenses from the outset. In addition, Cicourel (Witzel and Mey, 2004: 2–3) cautions that researcher bias is expected because of 'variations in the way different research analysts *use* methods' (emphasis in original). Tape-recorded interviews, team and PI-researcher meetings facilitated the transparency that Cicourel and others (Comaroff, 2004; Padgett, 1998) now call for.

Consistent with earlier research, disengagement differences in the economic mobility ethnography were not systematic by discipline. Some team members wished they could maintain contact, but hewing to the study design, did not. For example, as the St. Louis researcher reported several years later, 'I think of the people in the study often, and have thought about contacting them, but then thought better of it'. Other team members volunteered or agreed to 'check in' with the families post-research, but these contacts seldom came to fruition, and several were engaged in varied levels of post-research contact. That said, other post-research contact may have taken place that was not officially reported.

The Philadelphia anthropologist mentioned at the beginning of this article has engaged in continuing post-research contact, initiated by a participant she characterizes as 'the most vulnerable', and reciprocally from the researcher's moral, ethical, and religious 'commitment to social action and performing acts of social justice' (Philadelphia Researcher #1, personal communication, 1 July 2007). The Seattle public administration researcher encountered some of her research families in other community venues, thereby learning about post-research happenings, but over time such contacts and revelations lapsed. By study design, I (the PI, with a sociology and social welfare background) was the only official post-research contact, as I reinforced verbally and through business cards and follow-up letters that the families could contact

me with any post-research questions. A few families called or wrote period-ically to update me on their lives, several called at tax time with questions about the study participation honoraria, and I recontacted as many as I could locate when the book from the research (Iversen and Armstrong, 2006) was published three years later. In all, the lack of obvious differences in disengagement by discipline may be due partly to the small sample, and partly to the fact that the researchers are simultaneously parents, practitioners, and persons with per-spectives and relational preferences constructed out of their epistemological worldviews, as both the ethnographic literature discussed earlier and the next section suggest.

Researcher and Participant Characteristics

Obvious differences in disengagement practices did not appear to be related to the researcher's gender, age, family status, or racial or ethnic heritage. However, disengagement did seem particularly variable when the researcher had forged strong connections with the family's children. Spending time in their schools, attending their sports events, graduations and religious rites, taking them out for food, and just 'being there' as an interested ear made for many close associ-ations. Substantive concerns about the children's environments of poverty, under-resourced schools, dangerous neighborhoods, and insufficient social and cultural capital often made disengagement more difficult. For example, although the St. Louis researcher generally 'got out' according to the research design, the situations he described led to protracted disengagement. As context, Lynn, the key parent, was laid off from her promising post-training job at the same time as her mother sold the family home, leaving Lynn and her four children, aged 2 to 13, homeless. The researcher's email reads:

> Please wait to read this if you're having a good day! I can't ever remember crying over a client or participant's situation before (and I've heard and seen some bad stuff) but Lynn and kids' days in St. Louis before going to [a smaller city two hours away] were unacceptable by anyone's standards. I cried as I typed her and her kids' last days here [not able to find shelter in St. Louis; stayed in a bus station for three days, rather than with their abusive father; severe asthma; no financial reserves]. No one should experience this, especially children! I really hope you send a copy of this to those 'professionals' at the training program. They should be ashamed and held accountable for their part in this. No ifs, ands, or buts. Sorry, but I was really disturbed by this. So much for objectivity! (St. Louis Researcher email to the PI)

In contrast, when the children were very young, or the researcher–family relationship was strained, getting out more closely followed the original design, perhaps experienced as a sense of relief by both parties. For example, one of

the Milwaukee researchers found the Jackson family quite challenging, as her comments at the 18-month follow-up suggest:

> Also, five times I've been to Randy's and every time he either isn't there or is there but leaves shortly thereafter. (Milwaukee Researcher #1, field notes)

By chance or intent, the local researcher did not join the PI for the Jackson family story review and final research meeting. Role perceptions also seemed to be a factor in this situation, as illustrated in the next section.

Researcher and Participant Role Perceptions

It is commonly held that extended time in the field minimizes reactivity. On this view, over time the researcher becomes part of the landscape, although customarily 'does not interfere with the people or activities under observation' (Angrosino, 2005: 730). As noted earlier, more contemporary accounts characterize time in the field as a continually negotiated, collaborative enterprise, which may or may not facilitate role clarity or disengagement. In family research in particular, parents often come to perceive the researcher as a friend or trusted resource (Alverson et al., 2006), welcoming and wishing to continue the 'voice' and reciprocity the research process offers. For example, one Milwaukee parent describes what study participation meant to her in sentiments that suggest some ambiguity about the research role, including an open door for post-research contact:

> Tasha: Glad to help out. It was very nice talking with [Milwaukee Researcher #2] – she's like a mentor to me. I really benefited from having conversations with her. She was like a friend. I hope I'll talk with her after the study even. I'd probably have felt the same about you (the PI) if I'd gotten to know you better. I liked your letter too. (Transcript)

In the final meeting some months later, the researcher also left the door open, as her field notes describe:

> Here is my card (I reach into my wallet and pull out my 'research director – women in poverty' card and hand it to Tasha). If you need anything that I can do, please do not hesitate to let me know. (Milwaukee Researcher #2 field notes)

In other instances, and despite researchers' educative attempts, a participant may not really understand the research role in ethnography, as it is markedly different from more common forms of research, such as surveys. For example, in the Jackson family mentioned earlier, Milwaukee researcher #1 ended up spending more time with the key parent's wife than with the target parent. The

wife was surprised and unhappy to read her comments in the family story, which her husband identified in the story review meeting as her confusion about the research role:

> Randy said that the context for many of Shawn's [his wife's] comments was that she thought she was having 'girl talk' with the researchers – she didn't realize it was part of the research. Randy realized that everything they said was part of the research. He particularly liked the recommendations and also said that most of the report [family story] was accurate. (PI field notes)

As described earlier, the local researcher's passive disengagement from the family seems to have been influenced accordingly.

Alternatively, participants' needs and desires may override their perception of the research role, which then can influence disengagement. The initial meeting with one of the New Orleans families portends this situation, as well as the ways in which a researcher's disciplinary background can intersect with the researcher–participant relationship and role perceptions:

> When we stopped during the meeting for Rachel's questions, she wondered, with some concern, how 'personal' our conversations will get. We emphasized that whatever *she* considers 'too personal' will guide us. She can tell us at any time that she does not want to answer or discuss something. She then asked us what we'd consider 'too personal' in our lives, which each of us answered in terms of family relations. (PI field notes)

In the next interview a few days later, Rachel continued to discuss personal matters, despite the researcher's attempts to focus on Rachel's training and work:

> She said that when we had left the last time, she questioned what she was getting into and reiterated her fears about having to talk about anything personal. So I suggested we talk more about her job, the pre-employment training and strategizing about how I could speak with her kids. (New Orleans Researcher #1 field notes)

Rachel: I'm a little bit crazy. I just don't want to be alone, that's all, and I don't know what the problem is, why I keep getting left over like I do. And all I do is stay home, go to work, go to school, come home. And when I come home I got to hear drama. Then they don't want to hear drama from their mom.

N.O. Researcher: Teenagers.

Rachel: No, it's not my teenagers. It's my friend [boyfriend] this time. I'm just fed up with it. (Transcript)

Shortly thereafter, Rachel revealed deeply personal information to the New Orleans researcher, which caused the researcher to question her role, as my field notes from our subsequent phone conversation describe:

> Given Rachel's deep, sobbing tears, the researcher couldn't figure out how to get back to the 'research' without sounding crass and insensitive. Basically, the researcher felt she was being perceived as a counselor but does not have training for such a role. The researcher and I then reviewed ways that her background in applied anthropology and urban studies, which included activism, could be drawn upon in this research, such as exploring what work supports the participant felt he or she needed, and then deciding with the participant whether the support should come from the program, public policy, or some other source. This was a more comfortable role for the researcher. (PI field notes)

Field notes from my subsequent phone conversation with Rachel, which I shared with the New Orleans researcher, indicate that Rachel was also confused about the research role: 'I thought you were a counselor. I know I'm not happy. I gotta learn how to control my anger'. At the end of our conversation, Rachel mentioned that 'She [the N.O. researcher] always asks me how I feel'. She said a little more here, but the gist was that form of questioning partly led Rachel to the counselor perception. Thereafter, the researcher adjusted her mode of inquiry, although Rachel periodically continued to try to engage with the researcher as a counselor.

The final 'chapter' in disengaging from Rachel and her family began over a year later, according to the New Orleans researcher's field notes: 'Rachel talked about how we (you and I) had "gotten all up in their business" and how she couldn't believe we were just going to be gone from their lives when the study ended. She didn't understand how we could do this. I told her it was hard for us too'. The final meeting about six months later included the family story review and these reflections on disengagement from my field notes:

> Rachel asked that we return to a favored restaurant we'd taken the family to earlier, which the N.O. researcher and I felt was perceived by Rachel as a form of reciprocity, and thus we readily agreed. At parting, Rachel asked that I write to her every now and then. She didn't promise to write back, but seems to want the continued contact. For someone like her, who has had such trouble maintaining loyal friends, this outreach on my part may be important . . . a way to continue to thank her, respectfully, for her significant participation in the study, and not be yet another source of exploitation. It also freed the New Orleans researcher from any perceived responsibility to offer post-research contact. (PI field notes)

In contrast, and demonstrating that a single researcher's disengagement actions may differ, the same New Orleans ethnographer navigated a slightly

protracted ending with another key parent who better understood the role parameters of the research, as the final transcript shows:

PI: You know, Elizabeth, this is our last official visit with you.

Elizabeth: I know. [N.O. researcher] was telling me.

PI: But I will send you a copy of the story when it's finished. I'm always available to you. You can e-mail me, you can call me collect.

Elizabeth: Okay.

N.O. Researcher: I'll sort of check on you every now and then, and make sure where you are and all that, not for the research, but just because.

Elizabeth: That's nice. (Transcript)

The participants' perceptions of the research role also seemed related to the extent that the researcher engaged in minor 'interventions'. For example, the Seattle researcher whose field of expertise was child welfare, the St. Louis researcher who also worked in compensatory programs for children and youth, the workforce development professional, and the Philadelphia anthropologist who was personally and professionally invested in enriching children's futures engaged more frequently than did the others in small-scale direct interventions. Such interventions included providing lists of summer, pre-school and after-school programs, accompanying families to court, researching housing alternatives, offering cultural outings, and locating free or low-cost legal or therapeutic services. Although the team reported all interventions in this research as empirical material and anonymously shared emergent needs with training program staff or community organizations accordingly (Ostrander, 1995), the intervention role complicated disengagement practices for some, especially when the research role was misperceived.

On the other hand, the PI, the Philadelphia-based social welfare researcher, the two New Orleans researchers, who were also activists, and the Milwaukee anthropologist perceived their roles as 'connectors': that is, they acted as a liaison between the families and relevant community organizations rather than provide direct intervention, however minor. The final family story review with Nasir in New Orleans is an example of the liaison role:

The best outcome is that through persistence on the part of both the research team and the job training program, Nasir was once again fully connected to the post-program services and supports available from its main support administrators. (PI field notes)

Nasir:	[Support administrator] is going with me [to court].
PI:	How's your relationship with her?
Nasir:	Hm, hm, real cool . . . She's real good. (Transcript)

For these ethnographers, disengagement generally proceeded as initially planned, even if it was emotionally hard or complicated by the challenges associated with research funding that we discuss next.

Pattern of Research Funding

The pattern of research funding was a particularly vexing influence on the 'getting out' process in this research. Yet of all the methodological issues in ethnography, funding is rarely discussed in depth. The exceptions are the extent to which a funding source influences the content and direction of the research (Fetterman, 1998; Witkin and Iversen, 2008) or the constraints of limited funding (Alverson et al., 2006). In this ethnography, however, the process of research funding distinctly influenced disengagement.

I was initially awarded a one-year grant to examine family economic mobility in two research cities, Seattle and Milwaukee. The research design specified an initial six-month period of intensive contact with the family and related auxiliary contacts, followed by rolling revisits during the second six months. Given the detailed preparation for disengagement described earlier, I thought it would be easy to navigate a considerate but clear break with the families, but the funding process altered that outcome.

At the end of the original 12-month period, the foundation generously offered additional funding to extend the ethnography to St. Louis and Philadelphia. This also meant that the research in Seattle and Milwaukee could be continued, even though the formal ending in both cities had already taken place. About a year later, a third grant extended the research to New Orleans and the final grant enabled additional follow-up in all five cities. Inopportunely, the 'final' endings with the families often occurred before the next cycle of grant funding was known. Thus unpredicted extensions of the research meant that the notion of a defined ending became a blurry, ever-changing uncertainty throughout the research period, which seemed to make the families, and even some of the researchers, less clear about what 'ending' entailed. This excerpt from a letter I sent to the St. Louis research families with their family story drafts, in which I also told them that we were writing a book and, as such, we would like to visit with them and observe their children's schools once more after the story review meeting, illustrates the disengagement blur:

> I will be back in St. Louis in fall or winter 2002–03 and will contact you then in hopes you'll be willing to let me talk to you once again. I'll try to stay in

> touch in the meantime, and *please*, if you should happen to move, let me or the St. Louis researcher know. (PI letter excerpt)

As such, some families responded with considerable surprise when the field research actually ended in summer 2003, which made it more difficult for some researchers to disengage. For example, the St. Louis researcher's field notes from his last meeting with Loretta's family show that the cumulative weight of her struggles (short job cycles; likely depression; mother in hospital with a stroke and cancer; over-populated housing conditions; son's multiple school switches) led him to offer to augment the training program's supports with those of his social service agency:

> I tried to be encouraging, explained that she would receive her honorarium check in about a month, and that this would probably be the last time I would interview her. She seemed startled and hurt by this. I quickly gave her a card and told her to call me if she needed anything. I explained that the agency was mainly for people with developmental disabilities, but that I was extending services to all who had been in this [research] project. (St. Louis Researcher's field notes)

Multiple study extensions also contributed to researcher uncertainty about the study parameters; this query was typical:

> By the way, when is this study scheduled to end? I was just curious, as I've kind of lost track! (email from St. Louis Researcher to PI)

In fact, the study did not end for nearly two more years in that site.

As the examples in this section also show, the uncertainties and complexities of 'getting out' due to ever-changing funding intersected with and seemed to compound those related to the researchers' disciplines or to the characteristics and role perceptions of the researchers and the families.

CONCLUDING THOUGHTS AND NEXT DIRECTIONS

Although the research team members in the economic mobility ethnography initially shared a 'common conceptualization of basic and key concepts' (Massey et al., 2006: 146), such as about leaving the field, their disengagement practices differed. This article explored how the disciplinary and epistemological orientation of the researcher, researcher and participant characteristics, researcher and participant role perceptions, and research funding influenced the different 'getting out' practices. Two thoughts from examination of the empirical material seem particularly salient. First, the four issues just described seem to affect leaving the field singly and together. None is determinant but each matters more to

some ethnographers than to others, in different combinations and intensities. Second, the pattern of research funding can strongly influence researchers' and participants' experiences with disengagement, although this aspect is little mentioned in ethnographic monographs and methods texts. A further conclusion from these varied practices and experiences is that the topic of 'getting out' in ethnography still receives insufficient attention. We located little guidance for ethnographic projects such as ours on how to navigate disengagement, especially when it does not go according to plan, or on how to most sensitively leave a field of vulnerable children or families struggling to move out of poverty.[6] For the future, a richer body of literature on ethnographic disengagement, especially, as New Orleans Researcher #1 underscored after reading this article draft, pertaining to 'such under-resourced, and in two of my cases, also emotionally and psychologically compromised individuals', and more explicit and ongoing conversation about 'getting out' among the research team as a whole, particularly as the design parameters changed, are two directions that emerged in hindsight from this research team.[7]

That said, this article is neither exhaustive nor final in relation to 'getting out' in ethnography. Rather, it aims to stimulate a greater level of reflexive, collaborative attention to ethnographic disengagement. Such attention is not only ethically responsible but also epistemologically sound. The discursive evolution of the 'researched' from 'subject' to 'informant' to 'respondent', and for some to 'collaborator' and other forms of relational mutuality, necessitates full attention not only to how ethnographic research begins and is conducted, but also to how contact ends such that disengagement is more mutual as well.

Furthermore, a number of epistemological and methodological ideas in contemporary ethnography that are not discussed here might enhance processes of 'getting out' or leaving the field. Postmodern thoughts about ethnography, such as the need to interrogate varied notions of reflexivity (Marcus, 1998) or 'inquiry' itself (Richardson and St. Pierre, 2005), and new modes of research engagement, such as autoethnography (Ellis and Bochner, 2006), critical design ethnography (Barab et al., 2004), imaginary participant observation (Bruni and Gobo, 2005), and creative analytical processes (Richardson and St. Pierre, 2005), are just a few new directions that might usefully extend the story that this article has begun. All seek to decenter the positionality of the research actors, engage multiple alternatives, and acknowledge the situatedness of knowledge claims toward richer, more mutually constructed stories of life journeys that can enhance both theory development and action.

Finally, there may also be different modes of and attention to leaving the field related to researcher country of origin. Kusenbach (2005) characterizes ethnography in the USA as a unique scholarly identity, while she characterizes ethnography in Europe as simply a description of one technique of qualitative inquiry, participant observation. Cicourel (Witzel and Mey, 2004)

notes similarly that there are cultural differences about what field research is and how it should be conducted. From any of these perspectives and directions, further attention to 'getting out' in ethnography could enrich the research enterprise.

Notes

1　Following Denzin and Lincoln (2005), this article uses *empirical material* for what is commonly called *data*, except when describing traditional methods.

2　One anonymous reviewer valuably identified that more discussion of field exiting may take place within dissertation research, where one is expected to provide very detailed and comprehensive discussions of methodology.

3　I am grateful to Milwaukee Researcher #2 for stressing, in her post article review, that the purpose of ethnographic research in one's own social system often differs from that in another social system, particularly along lines of power relations and locational influence on post-research relationships, which can also make disengagement processes more varied, even among anthropologists.

4　I am grateful to Philadelphia Researcher #2 for this insight. After reading the article draft, she reflected that, 'social work training hones the ability to foster intimacy and maintain boundaries at the same time'.

5　All person names are pseudonyms chosen by the families.

6　I am grateful to Philadelphia Researcher #1 for the observation, after reviewing this article, for underscoring the 'interconnection between the topic of the study of vulnerable individuals' attempts to move out of poverty and the issue of ethnographer disengagement'.

7　I am grateful to New Orleans Researcher #1 for suggesting (after reviewing the article draft) that these reflections on future directions be added to the text.

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