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**Festschrift for  
William Shaffir**

by

Antony J. Puddephatt  
& Steven Kleinknecht

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*William Shaffir*

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# Editorial

## A Festschrift for William Shaffir: Guest Editors' Introduction

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It is a privilege to be guest editors for this special issue of *Qualitative Sociology Review*, a festschrift (intellectual celebration) of Dr. William Shaffir's (Billy's) contributions to symbolic interactionism and ethnographic research. Fittingly, the issue is comprised entirely of contributions from his students, each reflecting on how his mentorship helped teach them different aspects of qualitative field research and scholarship generally, paving the way for their own research projects and careers to evolve. Both of us, as guest editors for this special issue, are proud to be among Billy's students as well. As such, we share some of our own thoughts here and try to convey what makes him so special, especially to the interactionist qualitative research community in Canada. In this brief introduction, we hope to highlight some of the more central themes from the contributors' insightful articles. These include (a) Billy's influence on the development of symbolic interactionist research in Canada; (b) his personal approach to fieldwork; (c) his importance as a mentor and teacher; and, finally, (d) his philosophy of using and developing concepts in the field. Taken together, this collection of papers not only provides insight into the work and teachings of a central Canadian symbolic interactionist, but also has valuable lessons for those who practice and teach qualitative research.

William Shaffir is a central figure in the Canadian tradition of symbolic interactionism, as Jacqueline Low carefully documents in her paper, "The Hughe-sian Legacy: William Shaffir—A Principal Interpreter of the Chicago Diaspora in Canada." Indeed, Billy was the first recipient of a Sociology PhD at McGill University in 1972, where Chicago-styled sociology was brought to Canada. When Billy took a post at McMaster University, he infused the program with the ideas and research approach championed by lu-

minaries such as Everett Hughes and Howard Becker, helping to create a symbolic interactionist hub at McMaster University (Helmes-Hayes and Milne 2018). This became the place for Canadian students of interactionist field research to study, and Billy was known as the key person to study with. His expertise was important, but so too was his natural charisma and warmth as a teacher and mentor to his students. Low makes the case that not only did Billy serve as the main conduit for Chicago-school teachings in Canada, but he was also important in developing the institution of interactionism in Canada through the *Qualitative Analysis Conference*, which has helped nurture interactionist scholarship for almost 40 years. It is no accident that many of the contributors here have put forward the notion that Billy is, in both a personal and professional sense, "our Hughes."

Billy's MA thesis and PhD dissertation, later published as a book (Shaffir 1974) on Chassidic Jews in Montreal, utilized a classic ethnographic field research approach with interviews and a heavy emphasis on spending time observing, building relationships, and participating within the group. He would use this immersive approach fruitfully throughout his career to understand Jews in Canada (e.g., Weinfeld, Shaffir, and Cotler 1981; Levitt and Shaffir 1987; Brym, Weinfeld, and Shaffir 1993; Shaffir 2011), especially their maintenance of ethnic and religious boundaries (e.g., Shaffir 1993; 1998; 2007). Yet his broadly interactionist approach to field research offered him the flexibility to explore a host of topics beyond ethnicity and religion. For example, he studied the professionalization of medical students (Haas and Shaffir 1977; 1982; 1987), the experience of political defeat (Shaffir and Kleinknecht 2005), the claims and counter-claims of racial profiling by the police (Satzewich and Shaffir 2009), as

well as broader themes in social psychology such as identity (Haas and Shaffir 1978) and deviance (Haas and Shaffir 1974). His belief that one had to get out of the library in order to learn about the empirical social world, combined with his love of discovering, meeting, and associating with people, led him to numerous field research projects.

Billy contributed a great deal to the theory and practice of ethnographic field research. He co-edited a number of volumes that would collect reflexive “fieldwork experiences” from well-known ethnographers (e.g., Shaffir, Stebbins, and Turowetz 1980; Shaffir and Stebbins 1991; Dietz, Prus, and Shaffir 1994; Pawluch, Shaffir, and Miall 2005; Puddephatt, Shaffir, and Kleinknecht 2009). In these books, the chapter authors would candidly share their personal, practical, and sometimes social and emotional challenges in the field. This was a refreshing change from stale, recipe styled manuals of qualitative research. Instead of treating the research process as formulaic, Billy’s work emphasized the actualities of how fieldwork is accomplished in practice, as a deeply human and social process. Contributors to these volumes would discuss the various challenges of getting in, achieving rapport, managing emotions, handling conceptual problems, and eventually, exiting the field. Separate from his work in these edited volumes, he would also write personal accounts of his own fieldwork experiences, providing important practical lessons for others (e.g., Prus, Dietz, and Shaffir 1997; Shaffir 1999; 2018). Much of this was reflected in the way that he taught his students about field methods, as many of our contributors will show.

Billy’s methodological lessons would have a major impact on his students, both through his writings and, perhaps even more centrally, through supervision of graduate students, and his famous grad-

uate level qualitative research seminar. Rather than merely absorbing the many methodological lessons provided, however, his students would build on his insights and search for their own methodological solutions to field research. Scott Grills, in his essay “The Virtue of Patience,” argues that Shaffir’s field methods encouraged the importance of “hanging around” for long periods of time instead of trying to rush through participant observation or interviews. Such a slow, patient ethnography is the only way to build genuine relationships with others in the field, take the time to acquire the necessary perspectives, and be able to recognize the meaning of what goes unstated in social settings. Andrew Hathaway, Rory Sommers, and Amir Mostaghim, in their article “Active Interview Tactics Revisited: A Multigenerational Perspective,” show how Billy’s focus on interpersonal relationships in qualitative research can be used to consider when and how researchers can make use of their social positions to challenge participants in an effort to dig beneath the surface. And Arthur McLuhan, in his article titled “Feigning Incompetence in the Field,” demonstrates how in developing relationships with others in the midst of research, a “less than able self” is often important to convey in order to fit in, build trust, get better information from informants, and finally, leave the field. These contributions show the lasting relevance of Shaffir’s practical, reflective understanding of field research as a deeply human, emotional, and relational process that is artfully put together over time.

Another common theme in this festschrift is the deep respect and admiration people have for Billy as a personal role model and mentor. This applies not only to how to conduct field research, but also how to handle oneself in the university, in the field, and with people generally. Efa Etoroma, in his article “Journeying into Academia via Immersion in Qualitative Research:



Professor Shaffir as a Master Guide," discusses how Billy encouraged him to study the Black community in Hamilton, and how his subtle but powerful supervisory style and memorable lessons inspired him to pursue his career in academia. Patient and approachable, Billy would calmly help to deal with research dilemmas as routine problems of the field, which once addressed squarely, could be dealt with. Similarly, in her article "The Gift of a Vocation: Learning, Writing, and Teaching Sociology," Sherryl Kleinman recounts her experiences learning from Billy during her MA studies at McMaster. She explains that Billy was a valuable mentor due to the fact that he would be very happy to treat students as colleagues, breaking down the boundaries of teacher and student to provide a good-natured and friendly source of professional socialization. It was about making students feel at home in contributing to research and demystifying scholarly pursuits as routine processes of work to help reduce anxiety. This would be important to her developing sense of what it meant to be a good student, scholar, and eventually a teacher herself. Billy's influence would also shape her later academic trajectory to learning from Howard Becker and then maintaining her faith in her own tenured appointment in a university context that was at times inhospitable to qualitative research. Billy's incredible warmth and sense of humor towards others, especially his students, cannot be overstated. He has influenced us in our conduct towards our own students in the classroom, colleagues and staff on campus, and research participants in the field. But, perhaps more importantly, Billy has helped to shape our more general character as (relatively more!) good-humored, decent, and compassionate human beings.

According to Billy's teaching philosophy, fieldwork methods, if anything, cannot be taught from a book. As Efa Etoroma points out, one cannot learn medita-

tion by reading about it; rather, it must be practiced. The same goes for field research. Legend has it that a common prop Billy would use in his qualitative methods classes was a yo-yo. He would demonstrate a series of tricks with it, explain how it works, and then ask the students if they could perform the same tricks. Of course not! The students would have to practice, make mistakes, and figure out how to do the tricks by trying out these skills themselves. Because every setting is different and poses its own unique challenges, personalities, and social rules, there is no one set "formula" for how to succeed in the field. Instead, the only way to learn is for the student to become immersed in the setting as soon as possible, learning about social life and their craft by spending as much time as possible with participants, adapting to the field as best they can through trial and error. Julian Torelli, the most recent of Billy's students, recounts in his article "Piecing Together the Meaning of 'Dirty Work,'" Billy's philosophy of learning by doing. He considers how his "traumatic introduction" to fieldwork was daunting, yet critical in shaping his qualitative research skills. Such a teaching philosophy, Torelli points out, assumes a deep respect for the students, having confidence in their ability to master their own chosen field-sites, and be trusted to find a conceptual hook that works. And this respect would have indirect benefits as well. Not only do students have the freedom to conduct inquiries as they see fit, they gain confidence in themselves while doing so, knowing they were empowered to make the choices they did.

Yet to try and position Billy as an instructor who would simply send his students into the field to figure things out for themselves would be a major disservice to him, as many of these contributors also emphasize. Instead of a "hands off approach," Billy conveyed a continual supportive presence in

the classroom and to his students, which instilled a faith that things would work out, a confidence in being able to solve problems, and an eye for what is most important. Reminiscent of Robert Merton's (1973) observation of the positive effects of the socialization of PhD scientists by Nobel laureates, Billy would instill confidence in his students. As a supportive and respected mentor, he would help his students develop an assured "sociological eye" (Hughes 1971), crucial in figuring out what is most important and promising, and what can be safely left behind. There are many stories of Billy taking students aside during difficult times in their research, gently leading them back to productive work. Certainly, both of us have benefitted from his gentle "pep talks" that allowed us to regain the confidence to "get back into it" in times of doubt and uncertainty.

On the topic of his knack for fostering the "sociological eye," Benjamin Kelly and Michael Adorjan reflect on Billy's delightfully ambivalent relationship to theory. In their paper titled "Agnostic Interactionism and Sensitizing Concepts in the 21<sup>st</sup> Century: Developing Shaffirian Theory-Work in Ethnographic Research," Adorjan and Kelly argue that Billy is a very insightful theorist, but often downplays this, and seems to practice what they call "theoretical agnosticism." Rather than allowing theoretical frames or concepts to force data collection unnaturally, concepts should only be employed long after the field is well-understood, and only then, if the concepts appear to really make sense. Like Jacqueline Low, Kelly and Adorjan compare Billy's approach to conceptualization to that of Everett Hughes, and to a lesser extent, Howard Becker. Like these inspirational figures, Billy agreed that one must be extremely careful with, and skeptical of, theoretical concepts if one is to be authentic to the empirical field site. It

is too easy to turn a would-be inductive exploration of a new social world into a sloppy application of an "in vogue" theory that only distorts the reality of that field and the participants' own experiences. Yet when patient enough (Grills, this volume), one can find the conceptual ideas that *do fit*, and are helpful not only in describing the behavior in the local site, but also offering generic comparisons to other realms of activity (Prus 1996).

For example, Jack Haas and William Shaffir (1977; 1982; 1987) applied Robert Edgerton's (1967) notion of the "cloak of competence" to their own study of medical students. While Edgerton used the concept to understand how developmentally disabled people would try to pass as "normal," they would analyze the same process in how medical students would try and stand out from their fellow students and impress their superiors at crucial moments to help hide their weaknesses. This concept ended up having legs, returning with Billy's collaboration with Arthur McLuhan and others to consider the possibility of the opposite notion being important as well: a "cloak of incompetence" (McLuhan et al. 2014).<sup>1</sup> And as discussed, Arthur McLuhan (this volume) builds on this concept again here in considering the importance of feigning incompetence as a path to success in field research.

If Billy did break from full agnosticism and hold some theoretical allegiance, it would surely be with classic pragmatist and interactionist thinkers. This would likely be due to their open-ended nature and flexibility for the field. Indeed, Torelli (this volume) demonstrates Billy's influence in introducing him to the ideas of William I. and Dorothy S. Thomas

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<sup>1</sup> See also Puddephatt, Kelly, and Adorjan (2005) for the various merits of "unveiling" the cloak of competence in graduate school.

(1928), as well as Everett Hughes (1971) and Herbert J. Gans (1972), to frame the problem of “dirty work” for homeless shelter workers. These theoretical influences emphasize attention to the “definition of the situation” put forth by actors on the ground, such that “dirty work” is seen as something that actors themselves define. Hence, this background set of theoretical assumptions operates mainly to guide the researcher and encourage an openness to participants’ viewpoints on the ground.

In 2018, after 46 years of teaching, research, and service, Billy retired from McMaster University. His legacy as a symbolic interactionist scholar and field researcher, as well as a mentor *par excellence* to so many, is well-documented in the reflections that follow. His impact on his students is strong and enduring. While we cannot emulate his unique and specifically charismatic style, we can most certainly continue to impart his lessons. It has been an honor to assemble this festschrift and share some of Billy’s influences with you, the reader. The greater privilege is to have studied with Billy and gotten to know him as a person. It may be a bold move to reach out to the *RateMyProfessors* website as part of concluding this introduction, but the following words from one of Billy’s former undergraduate students convey well what it has meant for so many to know and learn from him:

He is the most pleasant human being on earth. He makes the world a better place for all those who come in contact with him on a day to day basis. He is the best professor in teaching and funniest guy ever. His jokes and stories will make you happy and cheer your mood. He is an incredibly fair person and he is very modest despite his achievements.

With Billy’s retirement, others will continue to learn of his contributions and approach both through his writing and the transmission of his ideas through his past graduate students. This important mobilization of his ideas will continue, but is probably a poor substitute for experiencing Billy’s personal teachings and mentorship first-hand. We hope this festschrift goes some distance to translating his students’ mentorship experiences for others, in ways that convey Billy’s unique contributions to research and teaching. We also hope that this volume provides possible “tricks of the trade” for others to learn from in both honing and teaching the craft of qualitative research for future students. Billy’s ideas certainly continue to infuse our own approaches to teaching and research, and we strive to pass his lessons on to new generations. To be sure, we are better scholars, teachers, and people for having had the chance to study and learn from Billy. We hope this volume conveys some of the central lessons learned, such that others might benefit as we have so richly.

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# The Hughesian Legacy: William Shaffir—A Principal Interpreter of the Chicago School Diaspora in Canada

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**Abstract:** In this paper, I discuss the invaluable role played by William Shaffir, my mentor and doctoral supervisor, who shaped my approach to interpretive fieldwork and deepened my understanding of symbolic interactionist theory. Known affectionately as Billy to his colleagues and students, Shaffir is a gifted educator and one of the finest ethnographic researchers of his generation. My focus is on how the scholarly tradition that flows from Georg Simmel through Robert Park, Herbert Blumer, and Everett C. Hughes, passed from Billy on to me, is illustrative of what Low and Bowden (2013) conceptualize as the Chicago School Diaspora. This concept does not refer to the scattering of a people, but rather to how key ideas and symbolic representations of key figures associated with the Chicago School have been taken up by those who themselves are not directly affiliated with the University of Chicago. In this regard, while not a key figure of the Chicago School himself, Shaffir stands at the boundary between the Chicago School of sociology and scholars with no official relationship to the School. As such he is a principal interpreter of the Chicago School Diaspora in Canadian Sociology.

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*Simmel* (2008, *Symbolic Interaction*); *The Chicago School Diaspora: Epistemology and Substance* (Low and Bowden, 2013, McGill-Queen's University Press); and *A Pragmatic Definition of the Concept of Theoretical Saturation* (2019, *Sociological Focus*).

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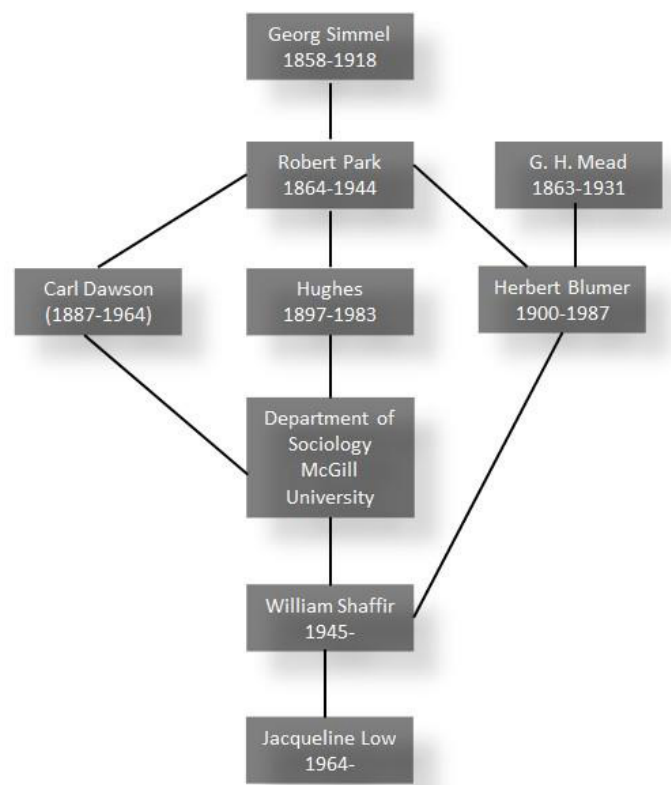
## William Shaffir—A Principal Interpreter of the Chicago School Diaspora in Canada

The construct of the Chicago School Diaspora is meant to conceptualize how key ideas and symbolic representations of key figures that people associate with the Chicago School are taken up by scholars unassociated with the University of Chicago. Moreover, the Chicago School Diaspora does not assume a communally held list of scholars or set of ideas. Rather, a core assumption of the construct is the integration into one’s “scholarly work” and identity “of what individuals selectively see as insights and key figures they identify with the Chicago School” (Low and Bowden 2013:16). This means that, for some, the Chicago School of sociology might mean George Herbert Mead and Herbert Blumer with a focus on interpretive analysis, while, for others, it may mean Georg Simmel, Robert Park, and Everett C. Hughes, with an emphasis on eclecticism in research methods. As Lofland (1983:491) argues, “the ‘Chicago School’ is a kind of projective device; descriptions of it seem to reveal as much about those doing the describing as about the phenomenon itself.” In this sense, the Chicago School is “like a Swiss Army Knife” made up of an assortment of tools that allow individuals to use it in different ways (Low and Bowden 2013:10).

Given the diversity of key ideas and symbolic representations of key figures associated with the Chicago School of sociology, my intent is not to situate Shaffir as an interpreter of the entirety of the Chicago School tradition. Rather, in explaining his role as a principal interpreter of the Chicago School Diaspora in Canada, I follow the intellectual trail of Simmelian symbolic interactionism (Low 2008) that passed through the prism of Park and Burgess’ human ecology perspective (Park 1936) and Hughes’

“lively and reflexive conception of research on society as a collective enterprise” (Chapoulie 2016:39), on through Blumer’s (1969a) insistence on empirical immersion in research and analysis, and so on through the work of Shaffir and from him, finally to me (see: Figure 1).

**Figure 1. William Shaffir: Principal Interpreter of the Chicago School Diaspora.**



Source: Self-elaboration.

In this way, Shaffir’s fieldwork practice, his understanding of symbolic interactionism, and his approach to graduate student supervision have all been fundamentally shaped by the Chicago School

insights of Everett C. Hughes and Herbert Blumer and, as a result, also by both Georg Simmel and Robert Park, in addition to George Herbert Mead. Thus, Shaffir comes by his status as a principal interpreter of the Chicago School Diaspora more than honestly.

### **Shaffir and Chicago School Fieldwork**

In 1922, the Department of Sociology at McGill in Montreal was founded by Carl Dawson who was awarded his PhD from the University of Chicago under the supervision of Robert Park and Ernest Burgess (Hoecker-Drysdale 1996; Drysdale and Hoecker-Drysdale 2013; McGill 2019). Consequently, when he arrived at McGill he was, in Campbell and Hall's (1989:334-335) words, "wedded to the human ecology method" and proceeded "as a missionary for the Chicago School of Sociology" in McGill's Department of Sociology. He was followed in the fledgling department by Everett C. Hughes, who was also principally supervised in his doctoral research by Robert Park (Bulmer 2017; McGill 2019), and brought his ethos of fieldwork as the "paramount" method of social science research to the department (Hughes 1961:vi; Manning 2000). The result was that both Dawson and Hughes imbued sociology at McGill with Park's emphasis on the necessity of first-hand observation in fieldwork. In Park's words,

You have been told to go grubbing in the library... to choose problems wherever you can find musty stacks of routine records...prepared by tired bureaucrats and filled out by reluctant applicants for fussy do-gooders or indifferent clerks. This is called "getting your hands dirty in real research." Those who counsel you are wise and honorable...But, one more thing is needful: first-hand observation. Go and sit in the lounges of the luxury hotels and on the doorsteps

of flophouses; sit on the Gold Coast settees and the slum shakedown; sit in Orchestra Hall and in the Star and Garter burlesk [sic]. In short, gentlemen, go get the seat of your pants dirty in real research. [Park as cited in McKinney 1966:71]

Dawson's training in fieldwork at the University of Chicago led him to conduct "a series of outstanding empirical studies" on issues related to ethnic groups, settlers, and immigration (McGill 2019). His skill as an ethnographic field researcher was such that he was the recipient of a prestigious Rockefeller Foundation award in the amount of \$100,000 (McGill 2019).<sup>1</sup> Hughes brought with him to McGill what Helmes-Hayes (1998:623) characterizes as "interpretive institutional ecology," which he developed through the ethnographic research he conducted for his classic work *French Canada in Transition* (Hughes 1943). Thus, the Department of Sociology at McGill was fundamentally shaped by a Chicago School emphasis on interpretive theory and empiricism in fieldwork (Ostow 1984; Chekki 1987; Shore 1987; Chapoulie 1996).

William Shaffir was awarded the first doctoral degree in sociology from McGill University in 1972<sup>2</sup> for his ethnographic research on the Chassidic Community in Montreal.<sup>3</sup> The oral examination of Shaffir's dissertation was judged such an important event that "Everett and Helen Hughes were invited as guests to celebrate" this inaugural doctoral de-

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<sup>1</sup> This was \$100,000 in the 1930's and was at the time "one of the largest grants ever received in sociology in Canada" (McGill 2019).

<sup>2</sup> Two dates appear on Shaffir's dissertation: a submission date of 1972 and a copyright date of 1973.

<sup>3</sup> The findings from which he published in a series of book chapters and journal articles throughout his career (e.g., Shaffir, Stebbins, and Turowetz 1980; Shaffir 1991; 2000; 2001; 2002; 2004; Brym, Shaffir, and Weinfeld 1993; Shaffir, Dietz, and Stebbins 1994; Shaffir and Bar-Lev 1997).



fense (W. Shaffir, personal communication, May 16, 2018). The fieldwork method used in his doctoral research reflects the strong Chicago School influence provided by Park, Dawson, and Hughes, but also the influence of another key figure of the Chicago School of sociology—Herbert Blumer, also a staunch advocate of naturalistic empirical research. According to Blumer (1969a:44-45):

One goes to the empirical instances of the analytical element, views them in their different concrete settings, looks at them from different positions, asks questions of them with regard to their generic character, goes back and re-examines them, compares them with one another, and in this manner sifts out the nature of the analytical element that the empirical instances represent.

That Shaffir was deeply influenced by the Chicago School tradition is clear from all he has written on fieldwork (e.g., Shaffir, Stebbins, and Turowetz 1980; Shaffir 1991; 2001; Shaffir and Stebbins 1991; 2003; Dietz, Prus, and Shaffir 1994; Shaffir, Dietz, and Stebbins 1994; Pawluch, Shaffir, and Miall 2005) and is most explicitly articulated in an article entitled “Doing Ethnography: Reflections on Finding your Way,” which he wrote for the *Journal of Contemporary Ethnography* (Shaffir 1999). In it he states:

My own position, crystallized over a variety of research, is that the most credible understanding of social phenomena requires the researcher to discover the actor’s definition of the situation—that is, his or her perception and interpretation of reality—and that such discovery and understanding are best accomplished by placing oneself in the other person’s situation. In my estimation, the participant observation approach of the classical Chicago School best meets this objective. [Shaffir 1999:684-685]

And that tradition is one of empirical examination of the social world, best accomplished by deep immersion into the field of endeavor under study. Putting aside the debates between anthropologists and sociologists as to what rightly constitutes ethnographic research, Shaffir (1999:676) argues that “terminological differences aside,” ethnographic fieldwork:

Places researchers in the midst of whatever it is they study. This vantage point is critical as it enables researchers the best opportunity to examine various phenomena as perceived by participants. Primarily a process of attempting to describe, analyze, and interpret social expressions between people and groups, ethnography requires the researcher to enter the natural settings for purposes of understanding the hows, whys, and whats of human behavior.

In other words, ethnographic “fieldwork is carried out by immersing oneself in a collective way of life for the purpose of gaining first-hand knowledge about a major facet of it” (Shaffir and Stebbins 2003:4).

In his long career teaching graduate seminars on qualitative methods and conducting fieldwork, he has published often on critical issues related to qualitative research in general and ethnographic research in particular, such as gaining entrée and leaving the field (Maines, Shaffir, and Turowetz 1980; Shaffir 1991; Shaffir and Stebbins 2003); the importance of relationships with gatekeepers (Haas and Shaffir 1987; Shaffir 1999); the presentation of self and role-taking in research (Shaffir 1969; 1972; 1991; 1999); boundaries between the researcher and the researched (Shaffir et al. 1994; Shaffir 1999); insider/outsider relations in participant observation research (Haas and Shaffir 1987; Shaffir 1999;

Shaffir and Stebbins 2003); relations with research team members (Shaffir et al. 1980; Haas and Shaffir 1987); issues of validity and reliability (Shaffir and Stebbins 2003); emotions in fieldwork (Haas and Shaffir 1987; Shaffir 1991; Shaffir and Stebbins 2003); and ethics in qualitative research (Shaffir and Stebbins 2003), all of which reflect his deep internalization of Chicago School insights. For instance, Shaffir (1999) has always been deeply concerned with the role of the researcher in fieldwork, and his concern reflects a Hughesian influence. To illustrate, Hughes (1961:vi) writes, “fieldwork...is more than other methods of study, itself a practice...in the perceiving and predicting of social roles, both one’s own and those of others” and, likewise, in Shaffir’s (1999:681) words, “by its very nature ethnographic research requires some measure of role playing.”

### **Shaffir and Symbolic Interactionism**

Shaffir has been rightly named by McLaughlin (2017) as one of the foremost sociologists carrying on the Blumerian symbolic interactionist legacy in Canada. From his earliest writings, Shaffir identified himself as a symbolic interactionist in the Blumerian school that derives from the insights of George Herbert Mead, Charles Horton Cooley, and W. I. Thomas “and their students,” not the least of whom was Herbert Blumer (Shaffir 1972:26). Thus, early in his scholarly career, he took on a version of symbolic interactionism that was shaped by key ideas and key figures of the Chicago School of sociology.

Dawson and Hughes not only brought Chicago School style fieldwork to the Department of Sociology at McGill, they also carried with them their theoretical orientation of symbolic interactionism. Principally, they brought the collective insights of

several key figures of the Chicago School, including not only George Herbert Mead, but also George Simmel, Robert Park, and Everett Hughes. These insights combined to create what can be rendered as Blumerian symbolic interactionism with a Hughesian twist (Blumer 1969a; 1969b; Low 2008). These insights are implicit or explicit in all of Shaffir’s research and writing.

This variant of symbolic interactionism understands society as the product of interaction. For instance, Simmel argues that “individuals...form a unity, that is, a society...For unity in the empirical sense is nothing but the interaction of elements” (Simmel as cited in Levine 1988:23). Similarly Park (1915:578), himself a student of Simmel (University of Chicago 2019), writes about the city—the base structure of society—in much the same way:

What we ordinarily regard as the city—its charters, formal organization, buildings, street railways, and so forth—is, or seems to be, mere artifact. However, it is only when and in so far as these things, through use and wont, connect themselves, like a tool in the hand of a man, with the vital forces resident in individuals and in the community that they assume the institutional form.

Likewise Hughes, who was a student of both Park and Simmel, followed a form of symbolic interactionism that Helmes-Hayes (1998:623) characterizes as “interpretive institutional ecology,” a perspective shaped by “classical Chicago sociology...anthropological functionalism and Simmel’s formalism.” Later writing with Simmel, he also expresses that society is the sum total of interaction. In their words, “the impulses and interests, which a man experiences in himself and which push him out toward other men, bring about all the forms of association by

which a mere sum of separate individuals are made into a 'society'" (Simmel and Hughes 1949:254). And for Blumer (1969b:20, 85), who was as much influenced by Simmel, Park, and Hughes as he was by Mead (Rock 1979; Low 2008; Smith 2017), society is "people engaged in living...they are caught up in a vast process of interaction in which they have to fit their developing lines of action to one another... there is no empirically observable activity in human society that does not spring from some acting unit." Even Mead (1962) himself, who is known almost exclusively for his overwhelming focus on mind and self rather than on society, sees the latter as the sum of interaction. He writes "Social institutions, like social selves, are developments within, or particular and formalized manifestations of, the social life-process" (Mead 1962:262). And it is this central understanding of society that is evoked in Shaffir's (1972:22) dissertation when he cites Warren's definition of society as "structures of interaction which endure through time and can be recognized as entities in their own right" (Warren 1963:46). It can also be seen in his selection of the community as the unit of analysis he used in his doctoral research (Shaffir 1972).

Shaffir has always stressed the Meadian emphasis within symbolic interactionist theory. As he writes in his dissertation, "The chief focus of this social psychology, known as symbolic interaction, is that interaction as it occurs among human beings consists in that they do not merely react to each other's actions, but rather interpret or define each other's actions before they act" (Shaffir 1972:25-26). Similarly, how individuals in interaction come to a "definition of the situation" through the meanings they share has been an ongoing concern (Thomas 1923:xv). For instance, Haas and Shaffir (1987:114) write "symbolic interactionism underlies our theoretical anal-

ysis and takes, as a given, that the understanding of human conduct requires a consideration of the meanings and definitions which evoke conduct." And later still, Shaffir concludes that his "own position crystallized over a variety of research is that the most credible understanding of social phenomena is to discover the actor's definition of the situation—that is, his or her perception and interpretation of reality" (Shaffir 1999:677, 684-685). He makes this point more explicitly in writing with Dorothy Pawluch about the use of symbolic interactionism in studies of occupations and professions. In their words:

rather than focusing on the objective characteristics of occupations and their interrelationships and place in the larger social structure, symbolic interactionists view occupations subjectively as groups of workers constructing meanings: deciding who they are and what they are about; what services they should be providing and to whom; dealing with issues that come up with their clients, other occupations, and the society within which they work; and responding to changes in their environment and in the circumstances of their work. [Shaffir and Pawluch 2003:894]

Notwithstanding the above, it would be a mistake to assume that because of this emphasis on how individuals construct meaning Blumerian symbolic interactionism cannot, and that Shaffir does not, address social structural concerns (Maines and Morrione 1991). Rather, Simmel argues that as social forms "crystallize, they attain their own existence and their own laws, and may even confront or oppose spontaneous interaction" (Simmel as cited in Wolff 1964:10). Likewise, Blumer (1980:410) writes, "there is a world of reality 'out there' that stands over against human beings and that is capable of resisting actions toward it." And a distinctly Hughesian, and

therefore Simmelian, strain of Blumerian symbolic interactionism is also evident in Shaffir's analyses in the importance he places on the building blocks of social structure: institutions, roles, "stratification systems...social class and status-group membership" (Haas and Shaffir 1978:20), as well as the necessity of understanding "human action [as] inseparable from its context" (Haas and Shaffir 1987:115). To illustrate, while Chapoulie (1996:20) argues that Hughes' attention to social structure and historical context has been "largely ignored" by later generations of sociologists, Haas and Shaffir (1987) "cite Hughes more than any other scholar" in their classic ethnography *Becoming Doctors* (Low and Bowden 2016:121). And in its methodological appendix they demonstrate their attention to social structural issues when they argue "for an occupation to achieve professional status, it must be granted legitimacy by whatever audience...is crucial to such status passage. The audience may include clients, 'public,' or the state" (Haas and Shaffir 1987:115). In the same way, the importance of institutions, as the large building blocks of social structure, is also demonstrated. In their words: "in modern industrial society, the professional school as part of a scientifically based university becomes the critical legitimating institution" (Haas and Shaffir 1987:4; cf. Shaffir, Rosenberg, and Haas 2004).

### Shaffir and the Transmission of Ideas

The final way in which Shaffir is a principal interpreter of the Chicago School Diaspora in Canada is manifest in his "master status" as a professor (Hughes 1945:357). Shaped by the insights of key figures of the Chicago School that coalesced into a fusion of Blumerian and Hughesian influences, Shaffir developed into a sociologist deeply committed to empirical research and the interpretivist un-

derstanding of everyday life. It was this scholarly tradition that shaped how Shaffir played the roles of professor and supervisor. And it was this same scholarly tradition that he interpreted and transferred to me as a doctoral student at McMaster University. In this regard, I see a resonance between key aspects of Hughes and Shaffir's teaching and supervisory styles.

The first point of resonance I see is in how both Hughes and Shaffir approached the teaching of qualitative methods. For instance, Hughes once had to radically alter a course he was given in order to achieve his goal of teaching fieldwork methods. According to Heath (1984:222), when early in his tenure at the University of Chicago, Hughes was assigned "a first-year introductory course in sociology...[he]... turned it into an introduction to fieldwork. Within the course students were encouraged to conduct small scale empirical exercises." Likewise, Shaffir re-designed the graduate qualitative methods course I took with him at McMaster University to focus on ethnographic fieldwork methods; to the exclusion of other forms of qualitative methodology.<sup>4</sup> As students in that seminar, we were likewise tasked with conducting small scale fieldwork studies. I remember my fellow doctoral student, Joey Moore, saying at the time that the seminar should have been called "Fieldwork Methods," not "Qualitative Methods," which was a disappointment to him because he had been hoping to learn about historical analysis (personal communication, September 1992). It was not a disappointment to me. Having read Blumer's (1969a) *Symbolic Interaction Perspective and Method* as an undergraduate student, I was eager to learn more about Chicago School style field research.

<sup>4</sup> A focus that has remained throughout his teaching of this graduate seminar at McMaster (cf. Shaffir 2017).

For this seminar, I conducted research on how students with disabilities negotiated disabled and non-disabled identities while negotiating the physical environment of the university campus. However, that was not the first topic I chose to research. Because of my mother's many years of struggle with serious chronic illness and disability which necessitated several hospitalizations, my first thought had been to conduct observational research in emergency department waiting rooms. In discussing challenges from his own dissertation research in seminar, Billy demonstrated how difficult it would be for me to gain entrée to such a setting, leading me to change the focus of my study to the topic of students with disabilities. This was an important change in terms of my career progression as I was later able to publish the findings of this field study in an article that has become one of my most cited works (Low 1996).

Also, as part of this seminar, Billy taught us to interview and make verbatim notes without the aid of audio-recording, a discipline I have retained throughout my scholarly career and one that has benefited me in a myriad of ways. In the first instance, it allowed me to have informal conversations with students with disabilities without the distraction of overt note-taking or audio-recording. Moreover, Billy emphasized the importance of the setting in ethnographic research throughout the seminar. This led me to pay attention to the university campus in a way that I otherwise would not have done and which enriched the theoretical depth of my analysis as it enabled me to examine the connections between space, place, and identity (Low 1996).

Later in the seminar, when students brought in field notes for Billy to comment on, he said only one word

to me, "identity," providing me with the conceptual basis for my analysis. He later suggested I read Scott's (1969) *The Making of Blind Men*, which, in turn, led me to Davis (1961) on deviance disavowal. All of this helped me to conceptualize the means used by the students I spoke with to manage the stigma they experienced as people living with a disability. It also led me to choose the area of deviant behavior for one of my comprehensive exams that furthered my on-going interest in stigma and stigma management, and influenced how I teach social problems (Low 2000; 2004; 2005; 2007; 2018).

Billy also taught us the invaluable lesson that "qualitative researchers count"; they count frequencies of observations, events, stages in processes, and so on (Corbin and Strauss 1990; W. Shaffir, personal communication, September 1992). Understanding that numbers matter in qualitative analysis allowed me to address a central question in my doctoral research, namely, why do people use alternative therapies? For instance, in the literature on the lay use of these approaches to health and healthcare, a desire for control over health and healing was commonly cited as a key motivating factor. In contrast, I found that only one out of the 21 informants I interviewed said that a desire for control over their healing process led them to first choose alternative therapies. This enabled me to argue that the generic social process of problem solving, not individual motivating factors, better explained informants' health-seeking behavior (Low 2004). Later in my career, counting frequencies allowed me to explain why, in my research with seniors, informants in the 80-84 age range made more statements of ability than those in younger age ranges. The explanation lay in counting the number of seniors who still drove or had other access to independent means of transportation (Low 2015).

Another point of correspondence between Hughes and Shaffir concerns their teaching styles. More than one contemporary of Hughes has noted that his teaching style was not for all students. For example, Riesman (1983:478) recalls that students “were turned off by Everett. He was never charismatic; his lectures were discursive and wandering, following the question where it led. He did not make it easy for students to take notes nor to predict what they might be asked in an examination.” Similarly Weiss (1996:543) remembers that “Everett’s teaching style required his classes to think along with him. Everett would discuss a topic by providing...stories, anecdotes, and observations, one following another... Often most of a class would miss the underlying thread.” Billy taught in a similarly discursive and anecdotal manner. His lectures were replete with insights and anecdotes from his fieldwork, a practice made plain when he instructed me and his other eleven teaching assistants that he would “teach the course” and we would “teach the text book” (personal communication, September 1992).

A final similarity I see between Hughes and Shaffir regards their relatively light touch as supervisors. Hughes “was not directive with his students” (Helmès-Hayes 1998:632; Vienne 2010; 2016). For example, Becker and Riesman (2017:vii) relate how “When Becker was preparing his first scholarly article, based on his master’s thesis, Everett advised him to...take one idea, attach everything in the thesis to it that would stick and leave the rest out.” Guidance almost identical in essence, if not exactly in substance, to the advice Billy gave me when I asked him how to go about publishing a paper based on the field work report I wrote for his qualitative methods seminar. When we met for coffee to discuss publishing, he began turning over the pages of the report saying, “you take some of this, and you take some of

that, and forget about the rest” (W. Shaffir, personal communication, May 2003).

Thus, despite Hughes’ “discursive” style, Riesman (1983:478) notes that he “sent many gifted individuals off along the many lines of their interests and his own” and likewise, with that one word “identity,” Billy provided me with the key to my analysis and an abiding interest in this concept (Haas and Shaffir 1978; Low 1996). In this way, students of Hughes “took up his ideas via a process more akin to ‘osmosis’ than tutelage” (Helmès-Hayes 1998:632) and the same kind of process of osmosis was operative in how ideas passed through Billy to me. But oh the invaluable things I learned through this process. I owe to him my understanding of fieldwork methods that informs both my research practice and my teaching of graduate level qualitative methods. Echoing Park, as well as Hughes, Billy charged me to “go do,” instead of blinding myself with too deep a reading of the literature before going out into the setting to engage in naturalistic enquiry, as Simmel, Park, Blumer, Dawson, and Hughes did before him. Reflective of Hughes’ methodological eclecticism (Heath 1984:222), Billy used survey questionnaires as part of his participant observation in the Chasidic community, passing on to me an openness to the use of numbers in qualitative analyses.

Finally, but of no less importance, Billy encouraged me to present my work at the *Qualitative Analysis Conference*. The conference, affectionately known as *The Qualitatives*, has been going strong for 36 years, training successive generations of qualitative researchers. Billy was a founding member of the small group of Canadian and US sociologists who began the conference in 1984 to provide a supportive environment where people interested in using a symbolic interactionist perspective and ethnographic

method in analyses of deviant behavior could share their research without positivist criticism. In his characteristically understated way, I remember Billy saying to me, “you should present your paper at this thing, it would be good” (W. Shaffir, personal communication, February 1993), and it was. It was more than good because attending the conference year after year enabled me to hear premier symbolic interactionists and ethnographers give papers, including Shaffir himself, as he demonstrated his own brand of Blumerian interactionism.

It was also good because as a faculty member in the Department of Sociology at the University of New Brunswick, I have had the pleasure of organizing *The Qualitatives* twice, and was deeply honored to host the 25<sup>th</sup> anniversary of the conference, again, demonstrating the passage of ideas from the Chicago School, through Billy to me. To illustrate, Nels Anderson, author of *The Hobo*, the first fieldwork monograph of the Chicago School of sociology, ended his career in sociology at UNB. As such, the decision was made to feature Anderson’s career as an important part of the conference, providing me with the opportunity to read deeply about the history of the Chicago School and to later publish an edited volume, *The Chicago School Diaspora: Epistemology and Substance*, based on papers from the conference that celebrates the Chicago School legacy (Low and Bowden 2013). This had an ongoing

impact on my career because of the strength of both these experiences, Gary Bowden and I were invited to write a chapter on Everett C. Hughes and his influence on Canadian sociology (Low and Bowden 2016) and to other papers on the status and future of symbolic interactionist theory in Canada (Low 2017; 2018). Further, in hosting *The Qualitatives* for the second time this year, we featured a special session in honor of the 50<sup>th</sup> anniversary of the publication of Blumer’s (1969a) *Symbolic Interaction: Perspective and Method* in the program. We will publish a special issue dedicated to this theme in the journal *Symbolic Interaction*, aiding in the perpetuation of the Chicago School tradition.

In the end, it is the Simmelian, Meadian, Hughesian, and Blumerian legacy that connects the Chicago School to me through Billy as my supervisor. This supervisory relationship is therefore central to how key ideas of the Chicago School tradition are interpreted, passed on, and given new life among scholars not directly related to the Chicago School, enhancing our understanding about how ideas in general are transmitted (c.f. Low 2008; 2018; Low and Bowden 2013; 2016). Thus, it has been through his teaching, publishing, and supervision that Shaffir, like Hughes, has trained further generations of interpretivist qualitative researchers who carry on the legacy of Chicago School fieldwork, making him a principal interpreter of the Chicago School Diaspora in Canada.

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# The Virtue of Patience

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**Abstract:** Shaffir (1998:63) writes, “We must learn to reclaim the virtue of patience. When we enhance the pace of doing research, it is often at the expense of acquiring a deep appreciation of the research problem.” This paper engages Shaffir’s claim by examining the importance of undertaking a patient sociology. What is the virtue to be found in prolonged and sustained work? How does this speak to the relationships found in field research and in the identities that inform our work as researchers and theorists? In contrast to recent trends towards various versions of instant or short-term ethnography (e.g., Pink and Morgan 2013) this paper argues for the merits of “slow” ethnography by examining the advantages of relational patience, perspectival patience, and the patience required to fully appreciate omissions, rarities, and secrets of the group.

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## A Personal Introduction

It is a genuine pleasure to contribute this paper to a festschrift marking the contributions of William Shaffir. I began my doctoral studies at McMaster University, and wrote my dissertation under Shaffir’s mentorship and guidance (Grills 1989). The doctoral program at McMaster in the 1980’s sustained a local culture that supported students working within the interactionist tradition. I had a vague generalized notion of what I thought I wanted to do (in my case, I was interested in trying to extend labeling theory by attending more explicitly to the championing of deviance designations) and a methodological commitment to ethnographic research

methods. I was at McMaster during what was the “golden moment” for interactionist work there. In addition to Shaffir, faculty members included Jack Haas (one of Shaffir’s co-authors), Richard (Dick) Brymer, Ralph Matthews, Peter Archibald, Charlene Miall, and Dorothy Pawluch. This combination of interactionist scholars and interactionist-friendly scholars allowed for a unique concentration of interpretive work to occur in one place at one time.<sup>1</sup> This was an engaging time for interactionist scholarship in Canada, and Shaffir was a central part of this and an integral part of my mentorship into this community of scholars.

While McMaster was a center for interactionist thought in Canada, Shaffir was also a part of a network of Canadian interactionist scholars that included rather centrally Robert Prus (University of Waterloo), Mary Lou Dietz (University of Windsor), Robert Stebbins (University of Calgary), and Nancy Mandell (York University). These scholars were instrumental in creating and sustaining what was to become the annual *Qualitative Analysis Conference*, held initially in 1984 at the University of Waterloo and co-sponsored by McMaster University and the *Social Sciences and Humanities Research Council of Canada*. These meetings have run consecutively now for 35 years.

The first published volume to come out of these meetings was 1994’s *Doing Everyday Life* edited by Dietz, Prus, and Shaffir. The second volume based on the conference was my own edited book from 1998, *Doing Ethnographic Research*. Shaffir’s contribution to that volume was his essay “Research in Jewish Orthodox Communities: The Neglected Role

of Sociability.”<sup>2</sup> That essay draws upon Shaffir’s extended body of research with Jewish communities—from his book *Life in a Religious Community* (1974) to his essay *Still Separated from the Mainstream: Life in a Hassidic Community* (1997). However, the importance of Shaffir’s (1998) discussion of sociability is to be found in its attention to central aspects of ethnographic research such as locating respondents, gaining access, examining how subjects understand the objectives of the research, and exploring how subjects’ perceptions of the researcher influence the project at hand. Shaffir (1998:63) concludes by asserting that:

We must reclaim the virtue of patience. When we enhance the pace of doing research, it is often at the expense of acquiring a deep appreciation of the research problem. As we sacrifice quality for quantity, we short-change not only those persons whose perspective we seek to understand, but also an approach to studying social life that holds the greatest promise for acquiring the most credible understanding of the dynamics of social interaction.

There is an implicit ethical stance articulated here, for this position speaks directly to the researcher/respondent relationship and the need to avoid privileging the interests of the researcher and thereby “short-changing” the other. As such, researchers are encouraged to attend to the central themes of genuinely hearing participants, listening to their words, attending respectfully to meaning, and becoming accustomed to the rhythms of their lives. Patience—sincere patience—nurtures genuine relations in the field. This paper takes up the spirit of Shaffir’s

<sup>1</sup> For a discussion of symbolic interaction in Canada see Helmes-Hayes and Milne (2017), Low (2017), and McLuhan and Puddephatt (2017).

<sup>2</sup> Other volumes that include Shaffir’s work and are developed, at least in part, from the *Qualitative Analysis* meetings include Pawluch, Shaffir, and Miall (2005), Puddephatt, Shaffir, and Kleinknecht (2009), Low and Bowden (2013) and Kleinknecht, van den Scott, and Sanders (2018).

(1998) position and attempts to extend his argument for the “virtue of patience.” Specifically, I examine the merits of “slow” ethnography by exploring the methodological importance of relational patience, perspectival patience, and the patience required to fully appreciate omissions, rarities, and secrets of the group.

## Relational Patience

Relational patience is a virtue for the researcher who seeks to occupy the place of the ethnographic other. Shaffir (e.g., 1999) has written broadly in the area of field methods and ethnographic research. A theme that connects this work is the shared emphasis on relationships in field settings—on how researchers form particularistic bonds with others and how those bonds locate the researcher in a community context. Central to this is a serious attentiveness to how participants in settings view the researcher. While getting in to a setting, learning the ropes, developing a sense of routines, and knowing when to write a project up are all key aspects of any research project and process, Shaffir (1998) suggests that the success of any field work project depends less on the research bargain, the purpose of the research, or the organizational requirement of informed consent, than it does on peoples’ perception of the researcher as an “ordinary human being who respects them, is kindly disposed towards them and is willing to conform to their code of behavior when he or she is with them” (Shaffir 1990:80). Shaffir (1990) discusses the importance of being defined as a *mensch*—a decent person with a sense of humor, a willingness to share, and someone who engenders the trust of others. While analytically distinct, the dual themes of reputations and identities are co-present here. Reputational integrity in the field requires maintaining a convincing presentation of self.

Shaffir’s position assumes something rather important; it assumes that there are research *relationships*. And it is posited that these relationships are meaningful ones—the kind that take time to build up, where the researcher is known as a person to those whose particular lives they seek to understand, where the researcher spends time in a setting where they are welcomed in and invited back, where, possibly, they are thought kindly of. These are relationships where researchers are challenged from time to time, embraced, and when absent may be missed. The research relationship is distinct from other dynamics in the field. For example, unobtrusive measures eschew a research relationship entirely. We can examine the number of nose-prints on the glass of a museum exhibit to gain a sense of the frequency of views of the exhibit, and examine their height to extrapolate age, but the research posture taken is intentionally non-relational (Webb et al. 1999).

While *research encounters* are joint acts in the full sense of the term (Couch 1984), nevertheless they are research exchanges marked by a more fleeting and instrumentally defined relationship. The researcher seeks “data” and the informant is of “value” in the research encounter relative to the information to be derived. Research encounters may be quite beneficial for some forms of social science research. A national census relies on such encounters as does rather crucial research on the immediate needs of persons displaced by natural disasters, or the demand for accessibility services for students transitioning from high school to university. While these research encounters may be precarious in various ways and they may fail to achieve their research objectives given attributions made by respondents (e.g., the encounter may be defined as deceptive, unsafe, too time consuming, or unwelcome in a variety of ways), the emphasis is placed on the encounter

as a means to accomplish an often clearly defined research end.

In contrast, *research relationships*, as used herein, denote the identity-rich and multi-faceted aspects of the particularistic bonds formed in the field. Research relationships are pursued and sustained where researchers have an interest in learning the ways of the subculture. Here researchers fully engage people's activities, perspectives, argot, and the various norms, folkways, and social boundaries of everyday life. By so doing, researchers come to appreciate the limits of humor, the socially constructed realities that sustain subcultural versions of "truth" and the worries, doubts, and uncertainties of the group. Researchers learn what is of subcultural value and how that value is sustained and maintained. In many ways deep, meaningful research relationships are a rather central aspect of achieving Blumer's (1969) elusive intimate familiarity of the social world.

Sharing some qualities with Schütz' (1944) framing of the stranger, the ethnographer as other occupies a place of some distance from those who are fully "in" and "of" the subcultural setting. Even where ethnographers are full members of the research site, their interests, questions, and concepts alter the relational dynamic in the field. While researchers may themselves be exotic dancers (Colosi 2010) or card hustlers (Prus and Sharper 1991), ethnographic relations alter prior relationships—as researchers become subcultural members who are "writing about us" and may become "our sociologist." Quite apart from how researchers may frame their own identities (e.g., as Wolf [1991] has suggested; he was a biker first and an anthropologist second), relationships in the field are modified by and through the reputations established by researchers in the field. These relationships, like the other more enduring bonds

that people establish with one another, require time and patience to develop. There is no substitute for this. To be present, over weeks, over months, and over years allows those whose lives researchers seek to understand a chance to observe our commitment to the field, our openness to the ways of the people, our willingness to be made uncomfortable, be inconvenienced, share emotions, and to be tested and not found wanting.

### Perspectival Patience

As Prus (1996) has argued, human group life is, in part, perspectival. The inter-subjective world of everyday life is realized through the meaning-based actions of people as they go about their everyday lives. The world of everyday life is our point of departure and our point of return with respect to the finite provinces of meaning that we encounter. Everyday life is the world we return to after giving ourselves over to the emotive experience of religious ritual, the passion of a lover's embrace, or the recreational terror of the horror film. It is within the world of the everyday that the realities of our lives reside.<sup>3</sup> The simple elegance of Blumer's (1969) assertion that people act towards the world of everyday life on the basis of the meaning it holds for them masks the methodological complexity of learning the perspectives of others. But, that is what is required if a deep and intimate familiarity with the world of the other is to be achieved (Lofland 1976:8-12). For researchers, patience in the field is required if a genuine appreciation of the complexity of the various worldviews, understandings, concepts, and perceptions invoked in any particular setting are to be fully understood.

<sup>3</sup> This framing is indebted to Schütz (1962; 1964) and Berger and Luckmann (1966).

As Shaffir's body of work pertaining to Orthodox Jewish experiences illustrates, not only may the worldviews that people hold be complex in their own right, participants may also face considerable challenges to perspectives held from those within their own community of adherents and from those who find the perspectives of subcultural members unwelcome, distancing, or otherwise troubling. For researchers, there is little in the way of a substitute for taking the time and putting in the work necessary to fully comprehend the worldviews at hand in any subcultural setting. Without a strong rapport with informants, it is challenging, indeed, for researchers to fully appreciate the nuances of perspective and meaning in play. Spradely's (2000) research with urban nomads is a helpful example here. By gaining the confidence of street affected persons, Spradely's work offers a ground-breaking rendering of the various understandings that people who may be formally identified as "unemployed" define work and working and the practical strategies associated with "making it" on the street.<sup>4</sup>

Drawing on various prior experiences, participants may experience significant misgivings, uncertainties, and cautions with respect to sharing their worldviews with others. It takes considerable rapport between informants and researchers for the various cloaks of competence that may cast shadows over the rituals of impression management to be seen, thereby allowing for a more detailed, forthcoming, and nuanced sharing of worldviews (Haas and Shaffir 1987). While no amount of time in the field may be adequate to overcome the misgivings of some, one of the virtues of patience is the oppor-

<sup>4</sup> For example, Spradely (2000) examines alternate forms of economic activities undertaken by urban nomads such as donating blood, obtaining resources from secondary aid agencies ("making the Sally"), and identifying appropriate targets for resources ("finding a live one").

tunity for researchers to more fully appreciate how participants come to understand the world as they do—to comprehend the everyday knowledge that is applied to situations at hand.

Without taking the time to learn the various worldviews within a subculture, researchers run the risk of discounting or diminishing the importance of *people's* worldviews to strong ethnographic research. If people act on the basis of meaning, then we have to do the work to understand those meaning sets if we are to understand the joint action that accompanies them. However, in the absence of time in the field and the full engagement with participant perspectives, one of the tendencies is for researcher perspectives to dominate the analysis and to restrict (if not sideline entirely) participants' voices. And when that happens, there is a distorting effect as the participants are written out of their own accounts in favor of the various agendas of the researcher.<sup>5</sup>

This can happen in a variety of ways, but three are particularly relevant to the ethnographic tradition in a contemporary context. Firstly, researchers may prioritize their own experiences in the field over the perspectives of participants. Here the members of the subculture at hand are not the "stars of the show," but rather are cast in secondary roles in favor of a detailed discussion of how difficult, traumatizing, and/or emotively draining the project was for the researcher.

Secondly, researchers may abandon an understanding of participants' perspectives in favor of

<sup>5</sup> Through various activities the research encounter may share some qualities with dominance and subjugation more generally (Athens 2015a). See Grills (2018) for a discussion of these themes.



embracing the opportunity to apply deviance designations to particular notions at hand. Here the designation of particular worldviews as somehow morally unacceptable replaces the pursuit of *verstehen*. We learn what the researcher finds offensive and what labels they are inclined to apply, but we learn little about the worldviews of participants or their lives, except as filtered through the lens of researcher as moral entrepreneur (Becker 1963).

Thirdly, researchers may impute meaning based upon their own prior conceptualizations and typifications. For example, where researchers are drawing upon observational data of joint acts and, at the same time, have limited access to the definitions of the situation of participants, the interpretation of these acts rests more exclusively on the perspectives of the researcher, not the participants. When this occurs, meaning may be rendered non-problematic, and while this may facilitate analysis, it is an oversimplification of the empirical world. The study of interpersonal violence is illustrative here. In contrast to dedicated field research, it is most certainly easier for researchers to distance themselves from interpersonal violence, to study representations of violence in the media, to attribute meaning to violence displays in professional sport, and to participate in denouncing violence and contribute to moral panics concerning violence. However, if we are to understand violence in everyday life, then we need to attend to the meaning sets that contribute to the construction of violence as an available interactional resource and the accompanying social processes of brutalization and violentization (Athens 2015b).

When researchers fail to grasp the worldviews at hand, their analysis is inevitably marked by a somewhat distorted understanding of the sub-

cultural setting. Colosi (2010:7) describes her experience of encountering an outside researcher's representations of exotic dancing, "It was my experience of working as a dancer that alerted me to the lack of understanding of and the many myths surrounding the industry. It was apparent that those attempting to tell my story and the stories of all my fellow dancers were outsiders, albeit strangers to our world." While researchers may not pursue "insider" status, one of the virtues of patience in the field is the ability to develop a complex and rich understanding of the perspectives of those in the setting.

An additional virtue of perspectival patience for field researchers is to be found in the development of perspectival fluency *over time*. Gubrium's (1991) attentiveness to local cultures and changes over time is instructive here. Gubrium found that the concept of "emotional disturbance" employed by clinicians in a juvenile detention center was an organizing principle for behavioral modification strategies employed by staff, but was virtually absent from the facility during weekends. The everyday reality that full-time clinical staff members were not present in the field site on weekends meant that the organizing perspectives in the setting changed. The ways in which youth behaviors were defined by the "weekenders" tended to reflect the perspectives of those who came from backgrounds more closely aligned to child-care traditions. Youth in custodial care routinely altered their behavior to attend to the distinction between weekday and weekend expectations. There is an important lesson in perspectival patience here. Not only does the acquisition of perspectival fluency require time in the field, it also requires being attentive to how perspectives may vary temporally—over the subculturally situated

rhythms of the day, the week, or from season to season.<sup>6</sup>

The various worldviews, realities, meaning sets, and typifications that are to be found in any particular subcultural setting may be challenging for researchers to take into account. Even where researchers may have higher levels of familiarity of the setting at hand, the concerted and direct efforts to develop more complete and comprehensive understandings of the full range of perspectives nevertheless require considerable time and presence. As Shaffir (1999:684-685) has argued, “the most creditable understanding of social phenomena requires the researcher to discover the actors definition of the situation...and that such discovery is best accomplished by placing oneself in the other person’s situation.”

## Patience and Voids

Advisory texts for those first entering the field note the importance of capturing aspects of the settings as they present themselves to researchers. Novice researchers may be encouraged to attend to the words used by informants, focus on what is occurring in the setting, be encouraged to avoid capturing only what researchers consider important, and to limit conjecture. This is sound, practical advice to be sure. In this we see the starting point of classic ethnography—hang around, see what is happening, listen, and observe. These activities allow researchers to attend to everyday life as it unfolds within the setting. But, what of what is not done, not spoken, those lines of action not taken—

<sup>6</sup> For a discussion of the importance of multi-contextuality in generating what constitutes sociological “evidence” see Zerubavel (2007) and Becker (2017).

the negative spaces in field research? That is much more of a challenge to attend to.<sup>7</sup>

I would suggest that one of the virtues of patience in the field is found in the opportunities afforded to researchers to fully comprehend voids—the nothings—in the setting at hand. The roads not taken may be as analytically and sociologically relevant as activities and missions more directly engaged and undertaken. As Scott (2018:1) argues, “Nothing is a sociologically neglected terrain, comprising negatively defined phenomena, such as non-identification, non-participation, and non-presence. Nevertheless, these symbolic social objects are created and managed through meaningful social interaction.” For example, Grills and Prus (2019) have examined the self in the context of experiencing management, being managed, and self-management. A rather central theme in this analysis is an examination of how those who hold offices may come to define inaction, delay, or doing nothing as a viable (if not at times preferred) course of (in)action. Managers may opt for inaction out of: (1) a lack of perceived strategic interest, (2) identity and reputational concerns, (3) resistance strategies, (4) distancing strategies, (5) competencies and skills-based concerns, (6) managing uncertainties, and (7) competing commitments and obligations (Grills and Prus 2019:185-186).<sup>8</sup> Obtaining an intimate familiarity with what is not done, an appreciation of not doing as strategic action, and the importance of office holders acquiring definitions of the situation that attend to the merits of inaction are rather central to understanding management in everyday life.

<sup>7</sup> Hillyard’s (2019) paper “The Rising Salience of the Absent: An Interactionist Analysis” offers a helpful commentary on this theme.

<sup>8</sup> For a more complete discussion of these themes see Chapter 8 of Grills and Prus’ (2019) *Management Motifs*.

However, as a methodological matter, it takes considerable closeness to a setting to understand what is unseen. For example, a substantial depth of understanding of a setting is required to be in a position to attend to the voids created by failed messianic prophecy (Shaffir 1995). Researchers are encouraged to attend to some of the multiple ways that absence, voids, and null spaces may be a useful analytical resource in field research. Researchers approach subcultural settings with a variety of prior constructs, understandings, personal experiences, and generalized anticipatory notions. It cannot be otherwise. Even where the ethnographer is entering relatively unfamiliar settings, an understanding of generic social processes may provide initial conceptual frameworks from which we may attend to self/other identities, joint acts, acquiring perspectives, and the relational dynamics to be found in any particular setting.<sup>9</sup> At times, however, anticipated aspects of human group life may be less readily available for researchers to encounter or engage.

Brymer's (1998) work on gangs and gangsters is instructive here. Brymer spent seven years studying Mexican-American conflict gangs. As he notes, he routinely spent time in the field with "gang guys." These are individuals with gang affiliations, who interacted with each other on the basis of small friendship cliques of 8-10 members, referred to as *palomillas*. While local discourse made reference to gangs, everyday interactions centered on these smaller group relationships. Brymer notes that it took more than two years of field research before circumstances arose wherein gang members came together to create a symbolic and actual move from *palomillas* members

<sup>9</sup> Readers interested in attending more fully to generic social processes and the symbolic interactionist tradition are directed to Blumer (1969), Lofland (1976), Couch (1984), Wiseman (1987), Prus (1996), and Puddehatt and McLuhan (2019).

to armed, potentially deadly gang members. Reflecting on the methodological implications of attending to this transition, Brymer (1998:148-149) writes,

So what does this tell us about field research?...Certain events such as gang fights are rare and exceptional events, as a researcher you may have to hang around for a long time before you can "see" them...[Such] exceptional events may be the missing piece in the puzzle, be patient.

Relatedly, we may also attend to the secrets of the group—that which is not present, not visible, and not available. These absences relate to the audience specific work of not saying, not revealing, and of concealment, and potential misdirection. Simmel (1906) framed the secret in sociological terms—attending to the secret as a more generic feature of social life. Simmel draws our attention to the power of the secret and how that power, such as it is, is realized only in the telling. But, by so doing, the power held by knowing the secret is lost in the telling, though the revelation of the secret may bring with it the ability to inflict certain harms or lay claim to the identity of a secret holder. Both Becker (1963) and Goffman (1959; 1963) attend to secrecy in the context of deviance and identity. The difference between the discredited and the discreditable may rest on the secret kept—the null space in the interaction sequence.

For Mitchell (1991), in field research it is rather essential to understand the secrets of the group. He draws out the helpful distinction between the egocentric secret and the ethnocentric secret. Egocentric secrets are aspects of peoples' identities and relationships that participants may seek to shield from various audiences. For example, participants in an environmental movement may conceal their potentially discrediting employment in the oil industry, members of the clergy

may conceal doubts and uncertainties about the nature of the divine, and those seeking public office may attempt to conceal prior illicit involvements. Ethnocentric secrets are secrets of the group, that were revealed, are perceived to place the group in some form of risk or consequences defined as unwelcome in a variety of ways. For field researchers who seek to occupy the position of full participant observer, learning the secrets of the group is essential to understand the subcultural setting. As Grills and Prus (2019:147) note, “access to the secrets of the team may rather centrally distinguish those who are ‘full team members’ from those more peripheral to the enterprise.”

One of the challenging aspects of field research is the problem of absences, voids, and that which is not present and/or not perceived. It can be difficult to differentiate between absence (that which is not present in a subcultural setting), omission (that which is a potential and available line of action that is untaken), secrecy (that which is concealed from the perception of the observer), and that which is unseen due to the everyday reality that the aspect of social life it reflects is a rare and/or isolated occurrence (as understood relative to the subculture at hand). For researchers who are committed to getting to where the action is and observing human group life, it is not enough to rely on cultural narrative (and mythologies) about gangs, or cults, or epiphanies of persons. The goal is to observe people as they undertake these various activities, to attend to the practical accomplishment of these aspects of everyday life, and to link or ground conception via perception (Blumer 1969).<sup>10</sup>

Therefore, one rather important virtue of patience in ethnographic research is that over time researchers

<sup>10</sup> Readers are specifically directed to the chapter *Science without Concepts* (Blumer 1969:153-170).

may, with greater levels of assurance, be able to differentiate between absence, omission, secrecy, and rarity. By so doing, the “no things” that are parts of the research process are more fully understood through time in the research setting. Simply put, there is no adequate substitute for patience in the field if we are to observe and engage the everyday voids that are a part of human group life.

## In Sum

There is no substitute for time in the field. But, time alone does not build research relationships. Shaffir has stressed throughout his teaching and writing the centrality of identity as a concept and how self/other identities shape the research relationship.<sup>11</sup> The research objectives of the project at hand may be much less relevant to informants/participants than the extent to which the researcher is seen as a “decent” person, as one who is genuinely interested in others, and as who can be trusted within the setting. Just as identities and trust cannot be established in fleeting encounters, nor can the understanding that is afforded by developing an intimate familiarity with the setting at hand. Relational patience and perspectival patience are in some central ways bound together. As research relationships develop and trust is enhanced, researchers come to more fully understand the worldviews of others. As members share the egocentric and ethnocentric secrets of the setting, researchers come to more fully appreciate the backstage regions of the setting and the worldviews that make the secrets of the group meaningful to members.<sup>12</sup>

<sup>11</sup> See, for example, Shaffir (1990; 1998; 1999) for discussions of the relationship between identity and reputation in the field.

<sup>12</sup> For example, see Mitchell’s (1983) study of climbers and the secrets within the community.

Field research can be time consuming, personally uncomfortable (Wax 1986), involve personal risks (Chomczyński 2018), and be emotively challenging (Kleinman and Copp 1993). It may be convenient in a variety of ways to adopt less personally demanding methodological strategies. By comparison, it certainly does not take much personal commitment to run interviews conducted by others through a qualitative analysis software program. While the product of this work may find a home, our shared enterprise certainly must not be to pursue the minimum publishable unit with as superficial an understanding of the setting at hand as possible.

There are other meaningful challenges, apart from the nature of the activity itself, to patient ethnography. I would be remiss not to note that many of those adopting alternative approaches to classical fieldwork do so while quite mindful of the advantages of the ethnographic tradition. Some may be adopting “methods of necessity” given various anticipated and realized barriers to research. For example, Newmahr and Hannem (2018:3) ask, “How can ethnographers and participant observers continue to do their research, without losing their jobs?” In response they argue for the value of what they term surrogate ethnography, and given their organizing question, the emphasis is not so much on the basis of the merits of the method alone, but on how ethnographers may, as a practical matter, address the excesses of ethics review boards or IRBs.

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Relatedly, the organizational life of universities that previously fostered the ethnographic tradition and the completion of strong fieldwork as a part of the doctoral preparation process has become increasingly challenging. As funding models have changed, restrictions on time to completion imposed on students have influenced research strategies. As graduate student enrolment has become increasingly monetized, departments are organizationally encouraged to increase graduate student teaching/supervisor ratios. Increasing demands for quantity of publication as opposed to quality have encouraged junior faculty members to pragmatically adjust research practice. And as Adler and Adler (2002) note, various forms of university oversight (which they associate with lawyers and policing) have rendered fieldwork in some settings virtually impossible. Taken collectively the challenges to detailed, immersive, and dedicated fieldwork have increased over time.

However, all of this does not exempt social scientists from adopting the research position appropriate for the task at hand. As Blumer (1969:50) writes, “to identify the objects of central concern one must have a body of relevant observations. These necessary observations are rarely those that are yielded by standard research procedure.” If researchers are in any way serious about heeding Blumer’s (1969:60) injunction to “respect the nature of the empirical world” and organize a methodological position to reflect that respect, then there is not only, as Shaffir (1998) writes, a virtue to patience, but an obligation to it as well.

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# The Gift of a Vocation: Learning, Writing, and Teaching Sociology

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## Keywords:

Mentoring; Writing;  
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**Abstract:** To write a sociological festschrift for a scholar necessarily means looking at a chain of influence instead of one person. In this essay, I honor William Shaffir, Emeritus Professor of Sociology at McMaster University, who taught me as I worked towards the MA. I examine what I learned from him by starting with my undergraduate experiences at McGill University, where Billy (I never heard anyone call him William) received his PhD. We shared influences there, including those who had studied with Howard S. Becker at Northwestern University. I then turn to my time at McMaster, and how Billy strengthened my knowledge of symbolic interactionism and qualitative methods, as well as taught me important lessons about writing. He also reduced graduate students' anxieties, including mine, through two words: "No problem." My experiences with Billy provided a model of mentoring that challenged the usual hierarchy between graduate students and professors. Those lessons were reinforced as I pursued a PhD at the University of Minnesota and spent two quarters at Northwestern University as a visiting student. These connecting influences helped me write and teach sociology in a largely quantitative department at the University of North Carolina-Chapel Hill, where I lacked the kind of support I had received as an undergraduate and graduate student. I taught there over 37 years, practicing the kind of sociology and mentoring that Billy generously modeled so many years ago.

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The era of smartphones has not relieved students—or professors—of the burden of convincing others that we are smart. Rather, technology has added one more thing we need to navigate, as I learned when I resisted getting a cell phone, even of the flip variety. I suppose it helped that I was at least 50; Luddite is a derogatory term to many, but has some cachet among old professors. And when undergrads discovered that crossing busy intersections could not stop them from keeping at least one eyeball on text messages and thumbing an immediate response, they developed awe (as in “awesome!”) for my ability to live without the temptations of the ready-to-wear machine. Wait long enough, and you might become cool again.

But, graduate students in sociology, even those working with young professors, must still prove they are smart in the old ways. They take classes, write papers and a thesis, pass comprehensive exams, present papers at conferences, write and defend a dissertation. They contend with mentors and other committee members who expect them to submit papers to journals. The presentations and pubs are not only for those who want a job at an “R1” university, a term I learned about 10 years ago, though I had been teaching at one for 27 years. Having a single-authored publication, perhaps from one’s MA thesis, and a few presentations at conferences, are expected for just about any academic position. Even students who whisper that they want a “teaching job” learn that having a PhD is not enough.

At every turn in graduate school, students know they must prove to professors, the gatekeepers of success, that they are hardworking (including evenings, weekends, holidays, and summers), intelligent, knowledgeable, insightful, and original

(“What’s your contribution?” “You haven’t answered the ‘so what?’ question”). Professors evaluate them, even in conversations over coffee; students must continuously show that they are worthy of the honorific title, “professional” (Becker 1970). As Jack Haas and William Shaffir (1977; 1991) found in their study of medical students at McMaster University, students learn to adopt a *cloak of competence*, maintaining gatekeepers’ impressions of them as deserving of the title.

And yet, I look back on the years I spent in graduate school and as an undergraduate as the most exciting years of my life in sociology. Like all students, I knew I had to convince professors of my smarts and had anxiety about the quality of my work. But, that did not form the *core* of my experience as an undergraduate at McGill University, an MA student at McMaster University, a PhD student at the University of Minnesota, or as a visiting graduate student at Northwestern University. I tearfully left graduate school after 5 years, leaving behind graduate student friends and supportive faculty for one of those R1 jobs.

What makes my positive experiences even more surprising is that I grew up with a perfectionistic mother who said when I got 98% on a test in school, “Did anyone get a hundred?” When I managed to get 100, she said, “Well, that’s geography; let’s see how you do in history.” My upbringing left me with enough doubt and anxiety about the quality of my work, and my self-worth, to last a lifetime.

And in case this sounds like mother-bashing, it is not (see: Kleinman 2002; 2006). My mother emigrated at age 6 with her mother, from Poland to Quebec City, to meet her father for the first time; he had left his pregnant wife to make enough of a living, as

a tailor, to bring her and his daughter to Canada. My mother, who loved school, quit at the end of grade 9 to help pay for the 4 children who came later. She liked to say she went to university with me because she read some of the books for my classes and many of my papers. She wanted me to become a real doctor, not a PhD, so I would have the money to live a life better than the lower-middle-class one I came from, or the poor one she experienced until she married my father. Much to my mother's chagrin, my father never used his Bachelor of Commerce (1935) from McGill University (he would have been an accountant), but instead took over his father's small printing press—permanent ink under the fingernails included—after his father died suddenly. Perhaps my father's underachieving and his corresponding unease about the high-status volunteer positions my mother held in Jewish organizations amplified my mother's investment in the accomplishments of her children. My brother, 12 years older, became an orthodontist; my sister, 5 years older than me, had test anxiety and did not get into college. Not surprisingly, she thought of herself as a failure and resented it at the same time.

So, how did I manage to escape the worst of the uncertainty and anxieties of being a graduate student? How did I come to thrive?

## **McGill and McMaster**

I was born and grew up in Montreal, and was expected to attend McGill University and live with my parents while doing so. I did not know anyone who "went away to college" until I met a student from the US who attended McGill, her apartment (I asked) paid for by her father. I told my parents about this American practice, and they said I had a perfectly good home to live in, even if it meant a one-hour bus

and subway commute each way. My mother, who always had the best lines, added, "Only a brazen hus-sy would want to live downtown!" Definitely grist for later feminist memoirs.

In my second year as an undergrad at McGill, I fell in love—with sociology. I did not fall for all of it, nor did I gravitate to a particular topical area. Rather, I connected with a perspective and the method that went with it: symbolic interaction à la Blumer (1969) and Mead (1934), and qualitative methods. I had no career plan attached to sociology at that point, but I somehow believed this love would save me from going to law school, the default for those who did not find a meaningful niche. This was the early 1970s, and I wanted an education that, as we said back then, was relevant—to my life, to the ways I did not fit in with my family, with the Jewish urban community, and with society (a reified term SI unraveled for me). By third year, if anyone asked about my major, I would say breathlessly, "sociology-but-really-symbolic-interactionist-qualitative-research." Sounds obnoxious, but being in love will do that.

At the time, McGill's undergraduate program was 5 years, with 5 courses per semester, so I had longer than usual to take classes, and to push myself to attend the office hours of professors I admired, especially Prue Rains and Joe Lella. I also took statistics with a qualitative sociologist, Malcolm Spector, who was a terrific teacher, but something about him scared me, despite the jeans and beard. Undergrads were not lining up at professors' doors, so I stayed for the two hours, or until they kicked me out. I talked with Prue and Joe (and occasionally with other profs) about papers I was writing, what intrigued me from the readings we had not gone over in class, and asked them to say more about their marginal

comments on past papers. My grades were good, so they knew I was not there to argue over marks. I still remember what Prue wrote on one of my papers: “This is an elegant analysis, but is it true?” I suppose that should have stung, but I had talked with her so many times that I could hear her voice, going over the analysis, rather than cutting me down. I was excited, anticipating the conversation we would have about her comment. Prue’s main point? Do not make the analysis overly complicated, just complicated enough.

Some of the intro classes were large, but those had smaller sections led by teaching assistants. I took advantage of their office hours as well. I also talked Prue and Joe into giving me independent studies, in which I did in-depth interview projects, transcribed the data, and wrote 30-page papers. Yes, like a graduate student, though I did not realize it at the time. I co-authored one of those papers with another undergrad, and learned about combining friendship and work. I caught the bug, and went on to co-author articles, chapters in edited volumes, and a book. I am retired, and still co-authoring.

What I learned, and then took for granted, is that interactions between students (even undergrads) and professors could be helpful, insightful, and intellectual, as well as informal, non-intimidating, and, dare I say it, fun. For those profs and TAs, I imagine what mattered was whether the student was passionate, hard-working, and curious. Our conversations were anchored in sociological work; I learned how to dig into a project, commit to a process that was alternately arduous and adrenalizing, build an analysis, and communicate it in an accessible way. Yes, I read a lot and used concepts from sociology, but the point was to produce a solid sociological analysis: maybe one day I would write one that was true *and* elegant.

I could be a student, not in the sense of making the grade (though getting A’s helped and I am my mother’s daughter and a daughter of schooling), but in the sense of learning, including making mistakes along the way. Even *mistake* is not quite the right word, as if one had the wrong answer on a math test. Rather, what they pointed out to me deepened my understanding or offered a new way of looking at a phenomenon. I internalized those lessons and turned them into good habits.

In case I sound like a self-confident nerd—having the chutzpah to monopolize professors’ office hours, asking for independent studies, including one with another student—I also worried that I would not do the work well enough to become One of Them. At some point I learned, mostly from teaching assistants, that if I wanted to continue doing this kind of work and paying for an apartment of my own, I would have to go to graduate school for several years and then apply for professor jobs. The number of years it might take in graduate school did not bother me; getting paid to be a student-for-life through TAing, teaching, and then working as a professor felt too good to be true. I assumed I was taking a vow of poverty, making my plan all the more virtuous, and I was thrilled at the possibility of having enough money to do what I loved. As I discovered in an undergraduate theory class, this is what Weber meant by having a vocation (Weber 1958; Ferrales and Fine 2005).

Looking back, Prue, Joe, and the TAs mentored me, and in ways that reduced the hierarchical differences between us; I think Prue and Joe were tenured associate professors at the time. They treated me as a serious sociologist-in-the-making. This kind of relationship between teacher and student is an example of what Jean Baker Miller (1987) calls tem-

porary inequality; the teacher's goal is to make the student (eventually) into a peer. Permanent inequality means what it sounds like; people in positions of authority stay invested in maintaining dominance over subordinates.

The profs and TAs were committed to doing good sociological work and I never thought of them as climbers. They did not give me mini-lectures in their offices unless I asked for one; office hours became an invitation to conversation. They asked me questions that mattered rather than questions that tested how smart I was. Maybe they were doing both, but I did not pick up on it. After years of living with my parents, schooling, piano exams, and pre-company ballet, I was likely to pick up on testing. The profs and TAs gave me the gift of treating me as a student-scholar at age 19, and I never felt called upon to satisfy their egos as scholars. Their informal, almost casual style put me at ease. They put sociological *work* at the center, not *me*.

Then the bad news came. I applied to Northwestern University's sociology department for graduate school (Prue had gone there for her PhD) to work with Howard S. Becker. Prue encouraged me to apply. I felt good about everything in my file—except my GRE scores. I did not know there was an industry, even back then, to help students improve their scores, including books to prepare for the test. [Or, am I still justifying those scores? Schooling's effects run deep.] Prue was surprised I did not get in, even after I confessed my mediocre scores.

I had also applied to McMaster (Allan Turowetz, a TA I came to know at McGill, planned to go there for his PhD), University of Minnesota (still something of an SI haven), and University of Toronto (a "good school" in Canada). I got into these depart-

ments, though I was disappointed that I would not get to study with Becker. The good news is that I got a one-year Canada Council MA scholarship that enabled me to go to a Canadian university. My fantasy, as an urbanite, was to get a job in Toronto after finishing a PhD. Just far enough away from my family in Montreal to avoid Friday night dinners, and close enough to visit several times a year. A TA told me that departments usually did not hire "their own," so I took the scholarship to McMaster rather than Toronto. I also kept the door open at Minnesota; my acceptance was deferred for a year.

And what a year that was. William Shaffir (Billy to everyone) was my appointed advisor. I also got to know another professor, Jack Haas, and learned about Billy and Jack's fieldwork at the campus medical school. Billy's PhD was from McGill, where he had worked with Malcolm Spector. Prue also influenced Billy, and Malcolm and Prue had both studied with Becker. In addition, Prue had chaired Allan Turowetz's MA committee at McGill, and Allan knew Billy. With all these connections, it is unsurprising that McMaster built on my work as a symbolic-interactionist-qualitative-researcher. Better yet, I did not need to announce the string of words to Billy and Jack; they already knew, and did not ask "What's your area?" Billy had studied the Chasidic community in Montreal, but was now studying a medical school; Jack had studied high-steel ironworkers and was now studying the medical school with Billy. Becker had studied jazz musicians and school teachers, among other groups. Moving across "areas" made sense despite the way most sociologists cut up the field.

I do not remember if McMaster asked for GRE scores, but Billy and Jack focused on my work. Billy also ensured that schooling would not interfere with my education. Each time I complained to him

about a bureaucratic rule that stood in my way, he said: “No problem.” At first I thought he was joking. But, he always came through. Billy never told me what he did, but the problems disappeared. Billy had my back.

I asked him to read a paper I had written for an independent study with Prue about sexual identity and experiences among college women (I had interviewed undergraduate heterosexually-active students). He gave me comments and asked if he and Jack could include it in a volume they were editing. The process became instructive. I had used Goffman’s (1961) concept of “moral career” in the paper, and Billy said he was not sure I was using it in the way Goffman intended. Uh-oh, big mistake? Billy said something like, “You could see if what you found really does fit with moral career, or argue that you’re using the concept in a different way because your case adds something to it, or just drop ‘moral career’ and use something else.” He said this in an unremarkable tone, as if what I faced was a typical matter that sociologists encounter. [“No problem.”] The lesson stuck.

Here is another lesson. I dropped into Billy’s office and found him looking at a manuscript on his desk. He turned it around to face me. “Sherryl, look at this.” He ran his finger slowly over the lines, and I could see cross-outs and substituted words. Two sentences in a row had a line running through them. What was he showing me? Beaming, he said, “Can you believe it? Howard Becker edited our paper!” I was confused; a famous sociologist had “corrected” the paper Billy and Jack were working on and Billy felt good about it. At least Becker had not used a red pen. Billy made it clear that Becker’s editing was a gift.

I also had a pang of envy, reminded that Northwestern had rejected my application and I would never

see edits on my work from Becker. But, the importance of having sociologists edit each other’s work stayed with me. As Daniel Chambliss (1989) wrote later in “The Mundanity of Excellence,” hard work is not enough; one needs to learn, and then practice, all the right things, including the little ones. Comments and cross-outs are valuable.

Billy also knew when to let a student go. After a few months, I talked with him about whether to transfer to the University of Minnesota for the PhD. I had visited the sociology department there, met faculty and graduate students, and heard it was possible to study at any of the Big Ten schools (I did not know it was a sports designation) for up to a year. Northwestern and Minnesota were on the list, and a current student from Minnesota was doing just that. Maybe I would get to take a class with Becker. Billy told me to go, that I would learn new things from other people. He wrote a recommendation letter for the 4-year PhD Canada Council fellowship that one could take anywhere. I got the fellowship, but I was nervous about leaving; McMaster’s sociology department felt like home. And who knew if near-collegial relationships were possible between graduate students and faculty in the States. Billy nudged me out.

### **Minnesota and Northwestern**

The informal and work- rather than performance-centered relationships I had with faculty at McGill and McMaster made it possible for me to develop a quasi-professional identity well before I received my PhD. I did not experience culture shock in graduate school in Canada or the US. I had not become test-wise (the skills of a student), but I had learned how to think sociologically. Prue and Joe and Billy and Jack made me feel like a sociologist rather than a student.

I have not said much about peers, though I mentioned co-authoring with an undergraduate at McGill. I wrote a paper at McMaster about first-year graduate students (yes, when I was one), publishing it several years later (Kleinman 1983). I chose that group not only for convenience. What surprised me at McMaster was that students, including me, pretty much developed a leisure culture; the content was anything-but-sociology. With the exception of one graduate student I became close friends with (we later co-authored an article), sociology became something one (presumably) did on one's own rather than in conversation with peers. In interviews, students said they even saw spending too much time with *faculty* as suspect; the goal was to prove oneself as a lone scholar. I wrote about the model of individualism in graduate school (do your own work, talking or working with others is close to cheating) and individualism (be original, hunching over your desk until you discover something no one has thought about before). Sociological work was meant to be asocial.

With the exception of a couple of students, I found the same peer culture at Minnesota. I talked about work with faculty, including a new assistant professor, Gary Alan Fine, and occasionally with two old-timers, Gregory Stone and Harold Finestone. Gary and I co-authored articles, based on the weekly meetings he initiated. I wonder if students' gossip, complaints about a department, and shared leisure pursuits tamp down competition (Puddephatt, Kelly, and Adorjan 2006) and allow supportive friendships to develop. After all, students will be getting letters from some of the same professors and will compete in a shrinking job market. In graduate school, talking about one's work or how much one accomplished in a day could lead to negative self-comparisons. Not a good basis for friendship, something every lone scholar needs.

After a year at Minnesota, I wrote Becker (Howie, once you meet him) and asked if I could take his fieldwork course through the program linked to the "Big Ten" schools. He said yes, and I flew to Chicago from Minneapolis in early January for the winter quarter. The first-year cohort adopted me. Most of them took the course because it was required; few expected to do fieldwork for their dissertations, and planned to work with other faculty. I got a dorm room in a Protestant seminary on campus because it was cheap and I expected to stay for only 10 weeks. Howie asked us to choose a setting on the first day, and I chose the seminary because I did not know Evanston or Chicago. My experience was immersive: I lived there, attended classes, ate in the dorm cafeteria, and hung out with the students. I became obsessed with the project, turning in voluminous fieldnotes and lengthy interview transcripts to Howie. During his office hours I shared my anxieties and excitement about all matters fieldwork, including the story I was trying to figure out. The intensity was heightened by the ticking clock.

At first I worried about impressing Howie; never mind his informal name and demeanor, I knew he was famous. I had devoured his work at McGill and McMaster, and... Northwestern had rejected me. But, I was not a student at Northwestern and knew I would be leaving soon, so I did not have much time to worry about what he thought of me. Instead I lived as a fieldworker. After 3 weeks, Howie suggested I stay another quarter to gather enough data for a dissertation. The D-word? I was only in my second year at Minnesota. He gave me independent studies for the spring quarter so I could continue my work and got me a TAsip for an undergraduate fieldwork course that another professor would be teaching in the spring.

At Northwestern I also learned about competition. Howie told me to get in touch with students whose committees he chaired, and that is where I found an unexpected chill. On the phone I found myself talking them into meeting with me, even after stating that Howie told me to initiate contact. Perhaps they resented the fact that Howie was treating me, a visitor from a lesser-status sociology department, as an honorary Northwestern student. These students might have wanted the TAsip; the undergraduate class was unusual, a special program in which students received a full quarter's credit (usually, 3 courses) for doing full-time fieldwork. I too found it surprising that Howie had called the professor to refer me for the job. I learned later that the Northwestern students I met had gone to elite liberal arts colleges, and Northwestern was a continuation of that kind of education. My mother considered McGill the Harvard of Canada, but graduate students knew almost nothing about Canada ("Toronto is the capital, right?"), including its universities. I also surmised, through my observations of Howie's students, that his informal style made it harder for anyone to figure out who would get the better letter for jobs. If memory serves, faculty at Northwestern decided which students should apply for which jobs, perhaps to cut down on competition between students in the pool of applicants. But, whichever way one cuts it, students who know each other may find themselves competing on the job market. I imagine they wondered, "Who was this outsider that had Howie's attention?"

I did not expect a letter and that freed me to learn. I watched carefully as Howie took a student's analysis in the fieldwork course and turned it on its head. *I want to know how to do that.* I kvetched to him in his office about paradoxes in my data and he asked the right questions to keep me moving. I gave him drafts of articles I was writing with Gary Fine, and he gave

us comments. At some point I confessed that I had not been admitted to Northwestern. "Well, obviously the committee was wrong," he said.

It helped that Howie's main criticism of graduate school is that it prepares students to become students (e.g., writing comprehensive exams) rather than professionals. Howie wrote comments on my drafts of articles and dissertation chapters well after I returned to Minnesota, and into my years as an assistant professor. That included light editing. And yes, he did write letters for me when I applied for jobs. When I called to tell him I had accepted the offer from the University of North Carolina (UNC), Chapel Hill, he said, "Well, too bad it's not in a city." Exactly.

### UNC-Chapel Hill

The performance model kicked back in when I got the job at UNC, Chapel Hill. I had never heard of the school or the department, but professors at Minnesota had, and those I had never met introduced themselves and congratulated me after word spread about the offer. Chapel Hill is a small town (I learned the concept of "college town" here), and I was one of only two women (the other woman, also an assistant professor, had arrived a year before). Almost all the profs were doing quantitative work, most of them senior in status and age, and they interacted in a formal way that made me uncomfortable. I felt like a kid among the grown-ups. The two male assistant professors spoke with professorial voices. The senior graduate students (around my age, some of them older) became my reference group and, to some extent, my membership group.

I had been in semi-collegial relationships with faculty for years by that point, but now my mind and body returned to childhood and schooling. Any untenured

faculty member surely feels performance anxiety; after all, the stakes are high. But, for me, moving to the department and town was no less than culture shock. I shrank at faculty meetings, felt anomic in the department and bored in the town, and had to learn to teach. Faculty in the department were polite, but where was the liveliness? The chair had said in our first meeting that my sole concern should be publishing (as if I did not know?) and that “it’s OK to be a mediocre teacher.” He said nothing about the content of my work, but the publish-or-perish message was clear. Lacking connection with students, I dreaded going to my undergraduate class of 80 bodies. I eventually found ways to teach and new courses that brought me closer to undergraduates—the very students I was supposed to see as standing in the way of research. And I wrote about it (Kleinman 1999; Kleinman, Copp, and Sandstrom 2006; Kleinman and Copp 2009).

My claim that I returned to childhood must sound like an exaggeration, but here is one story. At the start of my third year at UNC I found out that my dissertation on the seminary students, which I had revised into a book, would be published by the University of Chicago Press. I called my mother to tell her the news, and I mentioned Chicago (having informed her about the hierarchy of presses). She said, “The book. That’s good. And I know the press is important. But, there’ll be more books. You’re not the type to rest on your laurels.” After I said good-bye to my mother, I went into my department chair’s office to make the same announcement: “That’s good news about the book and Chicago is a great press. But, as you know, it’s what you do *next* that counts.” For a second, I thought my mother and my chair were in cahoots, believing they had found the way to ensure my productivity. But, they did not need to collaborate. It turns out that my mother had prepared me for the pressures of working in a highly-ranked academic department.

As she said often when I was in school, “You need to be ten steps ahead to stay afloat.” She did not say that because I was a woman, but because I was a Jew. And being secular would not help.

But, what *I* learned is that wearing the cloak of competence takes a heavy toll; the kinds of interactions I had with professors as an undergraduate and graduate student shored up my confidence and freed me to experience the work as meaningful. I came to do the same for students I taught and mentored, connecting with them in an informal style and challenging their thinking and practices in a friendly way. Graduate students at UNC suffered the anxiety that goes with the performance model, but my seminars became a place for us to talk about *that* (we read Jack and Billy’s article about the cloak of competence). I created spaces in which students could bond over shared vulnerabilities and analysis rather than connecting only through department gossip.

I encouraged students to create writing groups (I taught a writing seminar similar to the one I took with Howie). The students I mentored created qualitative working groups, and each of them knew I would comment on multiple drafts of papers/articles and chapters of dissertations, including light-to-heavy editing. They also learned to share early drafts among themselves, give constructive criticism, and edit each other’s work. I told them there were enough journals out there for everyone to publish, and that collaboration rather than competition would make everyone’s work better. It helped that SI and qualitative research held a marginal status in the department. You had to be highly committed to doing this work, knowing that other faculty and graduate students thought you had taken the wrong path. That status built solidarity among students.



It is hard being a new faculty member. There is no cohort to belong to and you have to figure out the department, the university, and how to make friends. Early on, I pushed myself to find qualitative researchers in other departments, then called department assistants to find out the professors' office hours, and showed up. They would *have to* talk to me. Yes, the same strategy I used as an undergraduate and graduate student. I also went to new professor events, looking for colleagues and friends. And I stayed connected to friends from graduate school through long-distance calls and visits, and met symbolic interactionists and qualitative researchers at conferences, something my mentors told me to do in graduate school. I also stayed in touch with Prue, Billy, Gary, and Howie, sending them drafts of papers.

I developed a variety of body pains, what I call sociosomas; I got a lot of work done, but did not have

the energy to apply for jobs. I hung on, got tenure, and two years later met Michael Schwalbe (at NC State University), another interactionist, and someone who lived out his politics more than I did. We have been partners for more than 30 years, and that relationship made it easier for me to bring feminism into my interactionist work and to become a thorn in the side of high-level administrators. Finding and working with progressive faculty across campus also brought new friendships.

I still do not care much for the town and the department never became a professional home. But, when I feel sorry for myself for having landed in a department of quantoids (Norman Denzin's apt term), I remember that I was lucky enough to get a job that allowed me to do the work I fell in love with so many years ago. Overall, as Billy would say, no problem.

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# Journeying into Academia via Immersion into Qualitative Research: Professor Shaffir as a Master Guide

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**Abstract:** The choice of a work career is one of the most important events in a person's life course and typically involves secondary socialization and identification with role models. This paper is concerned with the crucial role of my PhD dissertation supervisor at McMaster University, Dr. Billy Shaffir, in my choice of an academic career. I highlight and celebrate how, through the guidance of Dr. Shaffir, I experienced "immersive socialization" into field research and happily converted from an intended business career to an academic career.

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Studies of the life course tend to focus on at least one of the following three issues: careers, identity and self-esteem, and stress and satisfaction (DeLamater, Myers, and Collett 2015:99). This paper deals with the issue of "careers," by which is meant a "sequence of roles—each with its own set of activities—that a person enacts during his or her lifetime" (DeLamater et al. 2015:99). This paper outlines how my PhD dissertation supervisor, Dr. Billy Shaffir, influenced my choice of an academic career by providing me with a sense of safety while encouraging me to immerse myself into field research. My focus will be on how

Dr. Shaffir helped me navigate through the thorniest area of my dissertation research: my struggles with issues of validity.

In order to contextualize the impact of Dr. Shaffir's influence on my career choice, I shall first retrace my gradual movement towards an academic career, outlining some biographical reasons why a career in academia seemed rather improbable for me. Next, I shall discuss my immersion into qualitative sociological research and how navigating the issues of validity helped launch me into an academic career.

### **My Reluctant Journey towards Academia**

When I was growing up in Nigeria, the thought of becoming an academic did not even enter my mind. I fully expected to attend university and meet professors there, but I never even dreamed of or desired to become a professor. For one thing, I had some well mapped out career paths: my parents wanted me to become a business executive, such as a bank manager, and I wanted to become a journalist. Besides, since there were no universities in any towns where I grew up, I had no role models who were professors. Up till 1978 when I moved from Warri, Nigeria to Winnipeg, Canada as an International Student to study Business Administration at Success Commercial College, the only professor I met in person was a friend's uncle.

After successfully completing my business administration diploma program, I enrolled as a first-year undergraduate student at Dalhousie University in Halifax, Nova Scotia. My goal was to work towards obtaining an MBA degree in Canada, and some friends encouraged me to join them at Dalhousie University in Halifax, which I gladly did.

Although I liked Dalhousie University, I decided to move out of the city because the high cost of living, exacerbated by the high differential fees imposed on international students, made living in Halifax economically unbearable. I left Dalhousie University for the University of Manitoba (U of M) in Winnipeg in the spring of 1980. U of M did not have a differential fee system, and the cost of living in Winnipeg was significantly less than was the case in Halifax.

In the spring of 1982, I graduated with a three-year BA degree in sociology from U of M. Since I held a three-year degree, I was required to do an extra year of "Pre-Masters" courses before officially being considered an MA degree student. By early 1984, my thesis entitled "The Educational and Occupational Expectations and Aspirations of High School Students in Winnipeg," seemed to be on schedule.

To my utter surprise, just as I was arranging for an August 1984 one-way plane ticket to Nigeria, I heard rumors that my thesis supervisor was on her way out of the university. These rumors turned out to be true. My thesis supervisor left U of M in 1984. The rather sudden departure of my thesis supervisor left me in a quagmire. I realized that completing my MA degree on schedule was out of the question; I even had some doubts as to whether I would complete a graduate degree in Canada. Thankfully, in the summer of 1985, with the able guidance of a new committee and a new thesis supervisor, the late Dr. Larry Douglas, I successfully completed my MA degree program at U of M with a thesis entitled, "The Socio-Historical Analysis of Military Intervention in Nigerian Politics" (Etoroma 1985).

After obtaining my MA degree, I mailed the parchment to my dad in Nigeria and assured him that I still intended to obtain an MBA degree and that

graduate degrees in sociology would give me some advantages in the corporate world. Dad gave me his blessings. In the fall of 1985, I enrolled in the PhD program at the University of Calgary (U of C) where I was assigned the position of Teaching Assistant for a course on Quantitative Methods and Social Statistics, a position that I accepted very reluctantly. Thus, I went from completing a thesis based on historical methods at U of M to assisting students with quantitative methods-based assignments at U of C. I felt so out of place at U of C that, within a few months of enrolling there, I started looking for a different academic environment.

I transferred from U of C to McMaster University in the fall of 1986. Although I was committed to obtaining a PhD degree, my goal was to be a professional documentary maker and not an academic. I cherished the work of Canada's National Film Board (NFB) and had a desire to be involved with such an organization. I applied to McMaster because I liked its program, since it emphasized research/dissertation over course-work. My goal was to conduct a socio-historical study of the dilemmas of military disengagement in African politics. Based on the credits I received for coursework done at U of C, I was required to take only two courses at McMaster: Quantitative Methods with Dr. Alfred Hunter and Qualitative Methods with Dr. Billy Shaffir.

### **The Thrills of Immersion**

I first met Dr. William Shaffir in the 1986/87 academic year when I was a student in Sociology 742, his graduate level Qualitative Methods course. Since I was new to McMaster University, I did what most new students did: I asked other students for their opinions of both the professor and the course. The

consensus on Dr. Shaffir was as follows: (a) he will get you doing field research right off the bat, (b) he is very approachable, (c) he will tell you lots of stories about his research on "ultra-ultra-orthodox Jews," (d) he will tell you lots of stories about Montreal, and (e) he will get you connected with his friends and colleagues in academia.

I found all of the opinions above to be pleasantly true. I liked the challenge of the Qualitative Methods course because it seemed to be in line with my long-standing interest in journalism, promised to be quite informal, and promised to be a gateway to a wide variety of career options. Given my background as a young man struggling to discover his career path while trying to respect the wishes of his African parents who had sacrificed enormously to send him abroad, any heavy-handed attempts to force me into a career labyrinth would have been counter-productive. I needed options in my life, and both the Qualitative Methods course and Dr. Shaffir seemed structured to provide me with what I needed.

While at U of M I had acquired some knowledge of field research—particularly through a second-year undergraduate course in sociological methods. But, my first immersion into field research was in connection with Dr. Shaffir's Sociology 742 course. The following excerpt from the Fall 2017 syllabus for the Sociology 742 course captures the essence of the course:

This course is organized around problems of doing field research. Many of our discussions will focus on the practical problems confronting the researcher. The seminar will serve as a venue for participants to share their field experiences and consider the processes by which field research is accomplished.

The seminar is organized informally and driven, largely, by the kinds of projects that students undertake. In addition, readings are assigned and will serve as a basis for discussion.

As important as this reading material may be for learning about field research, however, it is not a substitute for being actively engaged in the field.

There are two course requirements. First, each student is expected to initiate a project using field research methods, and will submit field notes regularly...Second, you will be required to submit a paper based on your chosen research, to constitute the remaining portion of the final grade. [Shaffir 2017]

Dr. Shaffir told the Fall 1986 Sociology 742 class that he would show us the ropes, but that the best way for us to learn field research was to do field research. The point was for each student to be immersed into a local ethno-cultural or religious community, beginning with a major organization such as a church or a provisionary store.

What was it like to learn field research by doing field research? A cogent analogy is Richard J. Foster's (2011:133) point about meditation: "Thomas Merton rightly observed, 'You cannot learn meditation from a book. You have to meditate.'...the purpose of any book on meditation is to get us into the practice of meditation just like a book on the rules for the game of soccer is intended to get us out on the field and into the game."

Another analogy can be seen in Sidney Poitier's (2000:3-4) autobiography, *The Measure of a Man*, where he recounts the following story about learning to swim:

Cat Island is forty-six miles long and three miles wide, and even as a small child I was free to roam

anywhere. I would even go in sometimes and swim by myself. I had the confidence, because when I was very small, my mother threw me in the ocean and watched without moving as I struggled to survive... She watched as the ocean swallowed me, second by second. Then, mercifully, my father's hands reached under, fished me out, and handed me back to my mother...who threw me back in again, and again, until she was convinced that I knew how to swim.

In qualitative research, immersion invariably involves acceptance by one's study group. As Wax (1978:258) puts it, "Immersion or stepping into, or becoming a member of, a society or culture or living people is always a *joint* process, involving numerous accommodations and adjustments by both the fieldworker and the people who 'accept' him." Immersion is typically time-consuming as it involves a detailed examination of one's data—that is, study group, archival materials, et cetera (Borkan 1999). Thus, it is important for the researcher to be prudent in his or her allocation of resources. Immersion is a process that acknowledges and treasures the independence of each actor while providing experienced guides to prevent actors from running into destructive situations or problems.

Given my rocky journey towards and within academia, I was enamored by the opportunity of exercising the freedoms that an immersion experience would offer me. Particularly in terms of immersion in one's study, I was very intrigued by Dr. Shaffir's discussions of his research of Hassidic Jews, and I wanted to do a similar study. However, since I was new to Hamilton in 1986, I was confronted with a major obstacle: I did not know the city's communities well. Thankfully, based on both Dr. Shaffir's encouraging statements about ethnic religious communities and my experiences in Winnipeg, Halifax,

and Calgary, I decided that a Black church was an appropriate place to begin my study of Hamilton's Black community.

My acquaintances at McMaster University, most of whom were White, were familiar with and directed me to Hamilton's oldest Black church: Stewart Memorial Church (SMC) which was founded in 1835 by Reverend Josiah Henson. SMC has maintained vestiges of the practices of its original congregants who arrived in Hamilton via the Underground Railroad; thus, for example, old-fashioned plantation spirituals are still sung during regular church services.

When I phoned the church office and indicated my intention of studying a Black group in Hamilton, SMC's assistant pastor, Reverend Robert Foster, expressed his interest in the work and offered me rides to and from the church. These trips were very beneficial for me since they offered me opportunities to interact informally with Reverend Foster, who, in turn, used them as opportunities to introduce me to some of the neighborhoods where Black Hamiltonians once lived. In short, my study of SMC launched me into a field research of Hamilton's Black community and, ultimately, helped to anchor me in that community. By doing field research within Hamilton's Black community, I gained access to Black churches and to virtually every other significant segment of Hamilton's and, indeed, Canada's Black community.

After completing my PhD course requirements, I decided to pursue writing a dissertation on Black community building in Hamilton. I had enrolled at McMaster with a chip on my shoulder that was later reinforced by my reading of Wilfred Cude's (1987) book, *The PhD Trap*. Based on his negative

experiences as a doctoral candidate in a humanities program at the University of Alberta in Edmonton, Cude wrote his book about what he saw as structural problems in many North American PhD programs. By the time I started working on my dissertation, my mindset was such that an authoritarian style of leadership would have derailed my academic pursuits.

Given my positive experience with Dr. Shaffir and with his Sociology 742 course, I decided to approach Dr. Shaffir about the feasibility of having him as the supervisor for my PhD dissertation. I was thrilled that Dr. Shaffir granted my request. I, therefore, began my dissertation research by building intensely on my SMC study. After studying SMC, I studied each of the other Black churches in Hamilton. I then studied local Black businesses, such as provisionary stores and barber shops, and Black national organizations such as the Jamaican Association and the Ghanaian Association (Etoroma 1992).

Dr. Shaffir's masterful application of the philosophy of learning field research by doing field research manifested itself in many ways as I studied the Black community. One pleasant memory for me was when I informed Dr. Shaffir that Hamilton's Black churches were so different from one another that I could not find any particular ways in which these churches taught congregants about the Black experience. Dr. Shaffir referred me to his own research and pointed out that socialization is often passive. With time, I saw passive socialization at work in Black churches, homes, and other spaces in the form of photographs and other expressions of material culture; for example, SMC had a photograph of Lincoln Alexander, a Black man who was Ontario's Lieutenant Governor from 1985 to 1991, prominently displayed in its main sanctuary.



## The Thorny Side of Immersion: How Combating Issues of Validity Led to an Academic Career

In retrospect, my work towards a PhD degree was almost derailed by my doubts about whether my research was truly scholarly. I was particularly beset with the psychological burden of attempting to maintain the necessary distance from my subject matter. Primarily as a Black person studying fellow Blacks, I did not see how I could minimize the potential threats to the validity of my data; I was particularly concerned about the potential of “going native.” Although I was studying my own community, my work was a legitimate form of autoethnographic research (Holt 2003:20); the problem was that I had debilitating concerns borne of ignorance.

When I discussed my concerns with Dr. Shaffir, he assured me that while my consternations were legitimate, they were readily solvable. Dr. Shaffir provided me with two categories of responses to my concerns: (a) even though I am a Black man studying the Black community, as long as I am doing my research, I will likely be, at best, a “marginal native” within the Black community and (b) there are steps I need to take in order to minimize potential threats to validity.

Concerning the matter of being a “marginal native,” Freilich (1970:2) contends that field researchers’ involvement in study groups range on a continuum from “privileged stranger” to “native.” The “privileged stranger” has only instrumental membership in the study group in the sense that s/he participates in the study group to some extent, but is also allowed the privilege of interviewing group members and recording what s/he observes (Freilich 1970:2). In contrast, the “native” behaves and thinks in ways

that approximate those of the study group. Freilich (1970:2) contends that regardless of the role assumed, the fieldworker is invariably “a *marginal man* in the community, an outsider.”

According to Wax (1978), even if the researcher is accepted into an ethnic group, s/he would invariably remain as what may be variously referred to as a non-native, an outsider, or a stranger. As such, the only type of membership available to the researcher is “attached” or “instrumental” membership. Such a member is “a person who, though he always is and remains an outsider or non-native, may function in the society in a manner that is useful and agreeable to his hosts” (Wax 1978:264). Wax further notes that, unlike newcomers to a group, the field researcher is generally not socialized into a pre-existing role; rather s/he must continually team up with the group in inventing suitable roles (Wax 1978:266).

Regarding the matter of minimizing potential issues of validity, McCall and Simmons (1969:78) classified into three categories the sources of threats to the validity and reliability of field research:

1. reactive effects of the observer’s presence or activities on the phenomena being observed;
2. distorting effects of selective perception and interpretation on the observer’s part; and
3. limitations on the observer’s ability to witness all relevant aspects of the phenomena in question.

Since the most problematic of these threats for me was “distorting effects,” I shall focus the bulk of the discussion below on how, under the guidance of Dr. Shaffir, I successfully dealt with distorting effects. Moreover, in order to highlight the conse-

quences of subduing “distorting effects” on my eventual career choice, I shall save my discussion of these effects for last; I shall first discuss “reactive effects” and “observer effects.”

In general, two factors helped to minimize the potential “reactive effects” of my presence on my study groups. First, in spite of my efforts to broadcast my identity as a researcher to members of the study groups, my identity was known by only a small minority of the members of these groups. Lofland and Lofland (1984:24) contend that regardless of the researcher’s wishes, “all research is secret to some degree because the people under study do not always remember that the researcher is a researcher.” Another way in which I minimized reactive effects was by remaining long enough in the field. For, with time, group members are likely either not to notice the researcher’s presence or not take it into account (Becker 1970:43-44).

A major way in which “observer effects” were minimized was by playing back observations to various Black Hamiltonians in either verbal or written form (Shaffir, Stebbins, and Turowetz 1980:14). I made a habit of playing back findings to members of the Black community, partly because some interviewees requested these findings as a condition for offering any further information. Quite crucially in the context of “observer effects,” my status as a researcher generally made me only a marginal member of the study groups. I did not formally join any local Black organization, because I believed that, particularly because I am Black, membership in any of these organizations would undermine my desire to be insulated from the very probable factionalisms existent within the community. Since I did not become a formal member of any local Black organization, my socialization into roles within the various groups I studied was necessarily different from that

of new members. For example, I was never assigned any formal duties; whenever I was asked to do anything, members stressed that I was only “helping” and that had group membership been greater, they would not have “bothered” me with any work (see: Adler and Adler 1987:41).

“Distorting effects” refer to the fact that researchers’ perception and interpretation of phenomena are typically selective. Observer-based distorting effects, including the threat of “going native,” are typically minimized by taking what Posner (1980:209) refers to as a “psychic breathing space”; that is, leaving the field, every now and then.

Dealing with distorting effects was quite problematic for me, for it was virtually impossible for me to distance myself from the Black community for any length of time. Even when I decided not to attend any events organized by or in relation to Blacks in Hamilton, some friends and acquaintances would meet me at home or at the university and offer details concerning these events. Given my predicament, I could imagine only one viable way of avoiding the problem of “going native”: although I always welcomed information about the Black community, I sometimes did not read the materials brought to my attention for a week or longer.

Ultimately, distance from the Hamilton Black community was achieved when I moved out of the province of Ontario in the spring of 1991. Dr. Shaffir agreed to give me his blessings to move out of province on the condition that I commit to keeping in contact via email and regular mail. When I moved out of the province of Ontario, I became a *de facto* long-distance student. It is a great testament to the high regard in which Dr. Shaffir is held by his peers that sociologists at both the University of Manitoba

and the University of Alberta gladly provided me with Internet access when I introduced myself as one of Dr. Shaffir's graduate students.

Perhaps the most beneficial "psychic breathing space" for me was the opportunity of teaching a handful of courses. My first teaching opportunity was in the 1990/91 academic year when I taught off-campus evening courses at both McMaster and Wilfrid Laurier universities. It is precisely in regard to using teaching opportunities as buffers against "going native" that I owe Dr. Shaffir special gratitude. Dr. Shaffir encouraged me to and, indeed, expressly approved my foray into teaching, with the proviso that I provide him with evidence of progress in my research endeavors. The flexibility that Dr. Shaffir offered me was far more than I deemed possible in my originally intended career of banking. Furthermore, teaching helped me cultivate positive relationships with my students and allowed me to experience profound satisfaction in seeing my students thrive from the feedback they receive from me.

I ventured into teaching armed with an age-old Talmudic lesson I had seen exemplified by Dr. Shaffir: rebuff students with your weaker hand and draw them to yourself with your stronger hand. This is

the balancing act of teaching as outlined in Sotah 47a:6 of the online *William Davidson Talmud* (2019): "The Sages taught: It should always be the left, weaker, hand that pushes another away and the right, stronger, hand that draws him near. In other words, even when a student is rebuffed, he should be given the opportunity to return."

## Conclusion

Given my background, it is rather surprising that I have spent more than two and half decades teaching sociology full-time. I believe one of the most crucial things that prepared me for my vocation as a sociology professor is the way in which Dr. Shaffir guided me through issues of validity in my PhD dissertation research. In particular, I believe that by permitting me to use off-campus teaching as a "psychic breathing space" militating against validity issues, Dr. Shaffir effectively helped me experience the contagious joys of an academic career. Teaching opportunities provided me with the space I needed in order to see my research endeavors with new eyes; these opportunities also provided me with needed finances and kindled my interest in teaching. I found earning a living as a university instructor so rewarding that I knew I had found my vocational calling.

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# Feigning Incompetence in the Field

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**Abstract:** A basic feature of professions is specialized competence. Indeed, expertise grants professions and their members privileged, prestigious, and protected statuses. Members of professions thus face interactional pressure to appear competent in encounters with colleagues, clients, and lay publics, demonstrating that they, indeed, have the particular competencies expected of and associated with their position. For example, in a classic study of professionalization, Jack Haas and William Shaffir examine how medical students adopt a cloak of competence—presenting more-than-fully-able selves—in their training and work to convince gatekeepers, each other, and patients that they have the ability to do medicine. Similar competence-enhancing presentations are evident in other professions. However, a related dramaturgical phenomenon remains neglected: adopting a cloak of incompetence—presenting less-than-fully-able selves—in performing the professional role. Using the ethnographer's work as an illustrative case, the following paper examines this other side of managing professional competence.

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All professions trade in competence. Lawyers offer their clients advice on legal processes, access to legal protections, and resolutions to legal problems (Granfield 1992). Ministers offer their parishioners counsel on godly concerns, on the one hand, and worldly concerns, on the other (Kleinman 1984), comprising a suite of salvation services. Doctors offer their patients knowledge, abilities, and experience in body matters—diagnoses, prognoses, preventions, interventions, and so on (Haas and Shaffir 1987). Social scientists offer their audiences explanations for, critiques of, even interventions

into, patterns and problems of human group life (Buroway 2005).

The distinction between occupations and professions is “a social, historical, and political accomplishment” (Shaffir and Pawluch 2003:901), taken for granted and objectified over time (Berger and Luckmann 1967), offering professions comparatively privileged place as a consequence (Becker 1962). Credible claims to specialized knowledge and ability are essential to establishing and defending a profession’s honorific status (Shaffir and Pawluch 2003). Regulating the cluster of competencies that define the profession is an exercise in boundary maintenance—distinguishing insiders from outsiders and claiming expertise in and ownership over particular areas of work:

All social groups create boundaries and differences, view themselves in the most favorable ways. All individuals and groups strive to protect themselves from ridicule and charges of incompetence...Professional behavior is, or can be, understood as performance... The process of making some expert and more competent separates professionals from those they are presumed to help. [Haas and Shaffir 1977:85-86]

Prospective members who fail tests of competence are denied access to the profession and its attendant privileges. Established members who fail to fulfill the profession’s expectations of ability may face discipline and expulsion. Indeed, one of the most serious offenses with which colleagues may charge one of their own is professional incompetence, challenging fitness for the role. More than individual failures of performance, instances of professional incompetence threaten the collective reputation of the profession, blurring the lines that distinguish insiders from outsiders and keep the lay at bay. Part

of the profession-making process is establishing safeguards against these professional deviants: standardizing competence assessments (Claiborn 1982), developing peer reporting and review processes (Doyal and Cannell 1993), categorizing cases (Mennuey 2005), and disciplining poor performers (Morrow 1982; Overholser and Fine 1990). Maintaining the profession involves, in part, assurances that it comprises members with distinctive competencies that are in good standing.

There is thus pressure on professionals to demonstrate that they have the specialized competencies to stake a rightful claim to their honorific status. One of the responses to exaggerated expectations of ability is to adopt a “cloak of competence” (Edgerton 1967), presenting more-than-fully-able selves—more knowledgeable, informed, aware, skilled, experienced, accomplished, and so on. For example, in their study of the professionalization of medical students, Haas and Shaffir (1977:86; see also 1982; 1987) found “Expectations of competence are dealt with by strategies of impression management, specifically, manipulation and concealment. Interactional competencies depend on convincing presentations and much of professionalism requires the masking of insecurity and incompetence with the symbolic-interactional cloak of competence.”

The “cloak of incompetence,” presenting less-than-fully-able selves—less knowledgeable, informed, aware, skilled, experienced, accomplished, and so on—is also evident in work, occupations, and professions, though comparatively neglected and understudied (McLuhan et al. 2014). Routine practices of feigning lesser selves in professions tend to be guarded—known by insiders, but hidden from outsiders—for if these competence-concealing deceptions were to transpire, become evident, and de-

velop a “known-about-ness” (Goffman 1963), they may threaten the honorific status of the profession or otherwise make the work of its members more difficult: “Each job has techniques of doing things—standard operating procedures—of which it would be impolitic for those outside the guild to know. Illusions are essential to maintain an occupational reputation. Such actions are typically hidden in the backstage region from which outsiders are excluded” (Fine 1993:267). Maintaining the profession as a “going concern” depends on information control, disguising the “dirty work” involved in accomplishing activities and managing clients (Hughes 1971). Notwithstanding setting-specific dramaturgical strategies and interactional contingencies, feigning incompetence may be a feature of the trade-craft of all work, occupations, and professions, from the humble to the proud (see: Hughes 1970). Still, focused empirical cases of professionals displaying less-than-fully-able selves are needed to establish a comparative-analytical base from which to assess the cloak of incompetence as a generic self-presentational strategy.

Taking up this task, this paper examines the cloak of incompetence phenomenon in one particular form of work: ethnography. Experienced ethnographers tell us that field entries and relations hinge more “on the personal judgments made of the researcher” (Shaffir and Stebbins 1991:30) than on the scientific merits made of the research (Shaffir 1991). Impression management is essential to fieldwork: “Impressions of the researcher that pose an obstacle to access must be avoided or countered as far as possible, while those that facilitate it must be encouraged” (Hammersley and Atkinson 1983:78). At different points in the research process, the display of particular competencies can create problems for ethnographers. In these situations, ethnographers

may adopt a cloak of incompetence to pursue their professional objectives. In what follows, I consider some examples of feigning lesser selves in the field, organized around the “natural history of fieldwork” (Shaffir and Stebbins 1991)—getting in, learning the ropes, maintaining relations, and leaving the field. The analytical focus is on the various ways that ethnographers deliberately disregard, disguise, downplay, or diminish their competence—knowledge, abilities, experience, accomplishments, and so on—in doing fieldwork.

## Getting In

“Getting in” involves gaining access to research settings. Entry to any field site is best envisioned as an ongoing interactive process that researchers and subjects negotiate and renegotiate throughout the study, reaching a series of tentative, working agreements regarding the research relationship. These informal or formal bargains can delimit the scope of data collection in ways that can either enhance or nullify the breadth, depth, and import of the study (Shaffir and Stebbins 1991:28). Fieldworkers must be flexible and creative in their attempts to secure and maintain access, for “the situations and circumstances in which field observation of human behavior is done are so various that no manual of detailed rules would serve” (Hughes 1960:xii). Still, in ethnography, as in chess, there are standard opening moves: present our research and ourselves, informing prospective subjects of research purposes, methods, and risks.

The principle of informed consent, however, is a lofty one for those contending with the practical constraints of the research encounter. Roth (1962:283-284) identifies three obdurate conditions that tend to produce less-than-fully-informed research subjects: researchers do not know the precise direction or



shape their studies will take at the outset; researchers often do not want their subjects to know their more specific research questions or techniques; and researchers' training and experience prevent their subjects from knowing the research process as they do. For these reasons, "Secrecy in research is not something to be avoided or that can be avoided. It is rather a problem to be faced as an integral part of one's work" (Roth 1962:284).

How should one deal with the problem of secrecy, lies, and deception in gaining ethnographic access? The problem is not eliminated in further commitments to overt research or ethical mandates. Fieldworkers know that the assurances of informed consent they make to research ethics boards and prospective research participants are only partial truths, for "deception is, nonetheless, inherent in participant observation" (Shaffir 1991:77), endemic to ethnographic work. A more pragmatic professional response to the "problem of secrecy" is the call for ethnographers "to be cognizant of the choices that we make and to share these choices with readers" (Fine 1993:268), to lay bare our lies. In practice, however, these disclosures tend to be offered from the distant perch of graduate seminars, journal articles, and methodological appendices rather than the fateful grounds of the research setting. The moratorium on "confessional tales" (Van Maanen 1988) until retiring from the worlds we visit suggests a methodological utility in secrecy—deception is a valued implement of those who labor in the field. Fieldwork involves sales work: ethnographers trade carefully constructed images of themselves and their projects in exchange for access to subject worlds, and presenting less-than-fully-able selves can help close the deal.

In crafting our research letters and recruitment pitches, we take the role of our audience, for whom "the

best accounts are brief, straightforward, and devoid of academic jargon" (Shaffir and Stebbins 1991:26). For the lay subject, ethnography is enveloped in mystique. We dull our theoretical, methodological, and substantive knowledge when seeking access to their worlds (Bogdan and Taylor 1975:34), aiming to enhance their apprehension by losing our precision. Similarly, to manage subject reservations, ethnographers benevolently work under "shallow cover" (Fine 1980), announcing their general, mostly benign intentions, but withholding their specific, potentially concerning purposes. An effective fieldside manner, then, can involve presenting uncomfortable knowns as lesser-knowns or unknowns. Performing "dirty work" (Hughes 1971), but claiming clean motives, researchers attempt to achieve virtue by engaging in dishonesty. This is the moral, rather than the manipulative, purpose for adopting a cloak of incompetence when pitching our projects.

The manipulative purpose is self-interest: we must gain access to do *our* work and pursue *our* research agendas, so "ethnographers shade what they do know to increase the likelihood of acceptance" (Fine 1993:275). Researchers downplay their academic status (Shaffir 1991:78) and "any expertise...on the subject" (Schatzman and Strauss 1973:25) because their intellectual authority can "make the gatekeeper uneasy as to the likely consequences of the research, and the effects of its conduct" (Hammersley and Atkinson 1983:75). We align our research interests with those of our subjects (Shaffir and Stebbins 1991:26) when we know our studies will be of little to no practical benefit, perhaps even carrying risks of harm. We are vague about the detail and focus with which we intend to make observations and take notes (Bogdan and Taylor 1975:33-34), for less attentive observers can be disarming, appearing unable to uncover the "underside" (Hughes 1971) of the group

and therefore posing little threat (Douglas 1976:169-170). For fear of rejection and reactivity (Shaffir 1991:77-78; Fine 1993:274-275), fieldworkers obfuscate their methods and cultivate pretenses.

## Learning the Ropes

“Learning the ropes” involves “hanging around” and getting to know a social world. In many ways, ethnographers are similar to any novice member in their attempts to make sense of and become familiar with the language, perspectives, identities, relationships, emotions, and commitments characteristic of the group’s way of life (Prus 2007), engaging in an interpretive role-taking process as they strive for intersubjectivity with the other (Blumer 1969). The warrant for the ethnographic endeavor, however, is not becoming a well-socialized, contributing member of the group, but rather to grasp “the distinctive concerns and ways of behaving in the world” (Shaffir and Stebbins 1991:83) in order to generate analytic insights about human knowing and acting (Glaser and Strauss 1967; Blumer 1969; Lofland 1995; Prus 1996; Katz 1997). The road to theory in ethnography is paved with long and winding descriptions.

Satisfying the “so what?” of ethnography requires making observations and eliciting information, documenting the habits of action and thought natural to the world. One of the commandments of ethnography is the fieldworker shall not disrupt—to mitigate the effects of reactivity, the social order of the research setting must be more or less maintained. In making a scene, we move further from studying the other, closer to absorbing our selves. The nature of fieldwork is often foreign and thus potentially disruptive to those we study, for whom ethnographers are strangers—if not as persons, then at least as practitioners—taking up temporary residence.

People confront an ambiguous situation in becoming research subjects, unsure of what to expect from their interested visitors.

Herein lies the value of research roles, which provide behavioral expectations, placement and announcement of our motives and methods (Stone 1962). When roles are unclear, interaction is interrupted and data collection is delayed (Shaffir and Stebbins 1991:85). Successful adoption of a research role restores the smooth flow of subject worlds, incorporating ethnographers into everyday routines. The particular roles researchers assume in settings cannot be determined in advance, but are rather interactional products of the research encounter and subject to the contingencies of the research setting (Shaffir 1991)—roles are creatively made, not mechanically played. As “tentative offerings, possible forms of self” (Mitchell 1991:101), research roles are subject to negotiation, rejection, and replacement. Still, observers can attempt to influence the roles into which they are cast.

The “initial information” one intentionally gives and unintentionally gives off (Goffman 1959) can establish a researcher identity that serves to frame (Goffman 1974) the researcher and research throughout the study (Shaffir 1991). We see, for instance, ethnographers present lesser selves—disguising and downplaying their abilities—in approaching research subjects, for one of the most productive roles researchers can assume is that of “acceptable incompetent” (Lofland 1971). Approximating the qualities of the student role, being identified as an acceptable incompetent offers advantages in learning the ropes: “Such persons have to be told and will not take offense at being instructed about ‘obvious’ things or at being ‘lectured to.’ That is, such persons are in a good position to keep the flow of information co-

ming smoothly" (Lofland and Lofland 1984:26, 38, emphasis in original). Not only is the student role well known—and therefore a generic role adaptable across manifold situations and settings—but so too is its complement, the instructor role. By adopting the student role, ethnographers encourage reciprocal role-taking on the part of their subjects. Student-instructor roles provide definitions of the situation that allow joint action. In this way, time in the field can be more productive of data.

Although "the ethnographer enters the field with an open mind, but not an empty head" (Fetterman 1991:90), in interviews, ethnographers "pretend to know less about the topic than [they] actually do" (Shaffir 1991:80). Fieldworkers stake claims to selves that are less informed and skilled than they are, encouraging their subjects to spell out the details of what would otherwise be left unsaid: "The more that subjects think that the observer knows about a certain area, the less free they are to offer their own opinions. For this reason, the researcher should not show off his or her competence or knowledge. Let your subjects speak freely and say what's on their minds" (Bogdan and Taylor 1975:46). In diminishing our selves, we elevate theirs—subjects are "somebodies" in the interview encounter. Interviewees can remain similarly silent when they think the details are too basic to be relevant (Zerubavel 2015), but when instructing incompetents, irrelevancies become relevancies, allowing the ethnographer to find analytical novelty in the mundane. Subjects, of course, also feign incompetence, deliberately glossing over what they know (Douglas 1976:59-82) when they deem some information too unpleasant to the ear or too dangerous to the status quo. Researchers who are wise to reticent subjects may press for candor by playing dumb—pretending not to understand veiled and varnished references—forcing sub-

jects to explain what has been implied and "present a coherent description" (Becker 1954:32) that fills in the fieldworker's feigned blanks.

Participant observers may also benefit in carrying out their work under the cover of incompetence. Polsky's (1967:121) injunction to "Keep your eyes and ears open, but keep your mouth shut" reminds us that ethnographers learn more when they speak less. Observers who have successfully adopted the role of acceptable incompetent allow others to point things out, to be shown plainly the whats, hows, whens, and whys of everyday life in the setting, matters in which observers are ostensibly uninformed. There are also moments in the field when observers wish to remain unnoticed. Field researchers thus feign inattention, not hearing when they actually hear, not seeing when they actually see, not noting when they actually note the goings-on in the settings they study. The observer's senses appear most blunted when they are most acute, "worked at by displays of a studied lack of interest in one's fellows, minimal eye contact, [and] careful management of physical proximity" (Hammersley and Atkinson 1983:56). In this way, the ethnographer remains less visible, less evident, and therefore becomes less obtrusive (Goffman 1963). We can be paid no mind, taken for granted (Berger and Luckmann 1967), ignored as harmless (Zerubavel 2015), as participants carry on with their everyday lives and we carry on documenting them. Pretending not to be attending, the ethnographer's observational abilities lay quiescent, hidden and out of the way.

But, it is imprudent to feign incompetence in the field indiscriminately. To do so would decontextualize deception, ignoring that the credibility and utility of presenting less-than-fully-able selves is situational. For example, some "ascriptive charac-

ristics," such as age, can contribute to the acceptability and effectiveness of performed incompetence. Their ignorance having yet to be supplanted by the wisdom of experience, young researchers may be more readily cast as those with much to learn (Hammersley and Atkinson 1983:87)—the follies of youth mask the abilities of young researchers. Conversely, there may also be situations in which lesser selves are liabilities. Spector (1980), for example, suggests that in some settings, such as the study of public officials, researchers are better served in emphasizing their knowledge of and facility with the area under investigation, for authorities are loath to endure ignorant performances—we should be wary of playing down when studying up. Competence covering in the field requires selective shading of knowledge and abilities (Vail 2001).

## Maintaining Relations

"Maintaining relations" in the field involves the related processes of fitting in, getting along, and developing trust (Shaffir and Stebbins 1991:143). Far from being a peripheral matter, maintaining relations is the sine qua non of the ethnographic enterprise:

At first, the problems of getting along with the people in the field may appear to be of little scientific interest. Such an outlook, however, is hardly correct. The validity of the data hinges in part on achieving that delicate balance of distance and closeness that characterizes effective research-subject interaction. [Shaffir and Stebbins 1991:143-144]

A common problem for fieldworkers, then, is to develop and manage relationships throughout the study. But, the fieldworker's hanging around, digging for details, and discovering dirt pose standard subject risks: routine disruption, interactional

discomfort, and reputational harm. Who would ever tolerate an ethnographer in their midst? Adopting a cloak of incompetence can be an effective self-presentational approach to fitting in, getting along, and developing trust (McLuhan et al. 2014:368).

Whenever researcher-subject status differences are great, fitting in is often a challenge (Shaffir and Stebbins 1991:26). In some settings, for example, particular competencies are conspicuous, abilities out of place. Research suggests there can often be a stigma in academic achievement (Coleman 1959; Albas and Albas 1988; Tyson, Darity, and Castellino 2005; Shoenberger, Heckert, and Heckert 2012). Over-conforming to social standards, high achievers are "positive deviants" (Shoenberger et al. 2012). By virtue of their graduate degrees and institutional affiliations, researchers are often intellectual outsiders in the field, their competencies creating relational distance, perhaps even discomfort. Strong identification with their academic statuses will tend to position researchers in the role of "expert" or "critic," which can make subjects "uneasy as to the likely consequences of the research" (Hammersley and Atkinson 1983:75). Subjects suspect participant-observation will become "participant-intervention" (Fine 1993:287), finding the errors in their ways. Fieldworkers, then, downplay their academic statuses when they expect them to interfere in field relations (Shaffir 1991). Some ethnographers go even further in their cloaking efforts, approximating the appearance and speech of their subjects (Hammersley and Atkinson 1983:78-79), concealing their abilities in attempting to fit in (Liebow 1967:255-56; Shaffir and Stebbins 1991:26).

What we know can also present problems in getting along with others. Ethnographers "avoid

words that are comfortable for them but that their subjects are likely to misunderstand or to find pretentious" (Bogdan and Taylor 1975:57) and do not challenge subjects' clearly false but dearly held beliefs because such displays of ability would likely make subjects self-conscious and uncomfortable (Schatzman and Strauss 1973:72-73; Bogdan and Taylor 1975:46). Similarly, as is customary among trial lawyers, ethnographers may ask questions to which they already have answers, only to encounter subject deception via feigned ignorance or forgetfulness (Douglas 1976). To get along with some subjects, we go along with their deceptions. We feign ignorance of interpersonal (Haas and Shaffir 1987:124) and even ethical (Bogdan and Taylor 1975:53) issues to avoid confrontation because the study must go on. Interaction rituals of exercising tact, engaging in civil inattention, and performing deference and demeanor (Goffman 1967) are exaggerated in the field because "one cannot choose one's informants on the same basis one chooses friends" (Hammersley and Atkinson 1983:84). To protect the selves of their subjects and preserve the smooth flow of field relations, ethnographers hold their tongues when they know better.

Trust is essential to field relations. Subjects cooperate more when they sense fieldworkers can be counted on to handle sensitive information with appropriate discretion (Shaffir and Stebbins 1991:143-144). However, researchers also encounter pressure to reveal what they have learned, and they "cannot readily ignore or rudely refuse requests for information" if they wish to maintain good relations in the field (Schatzman and Strauss 1973:88-89). Adopting a cloak of incompetence may be useful, then, in maintaining confidences and managing information. Ethnographers present themselves as less informed than they actually are when dealing

with various inquisitive publics, "pretending not to know information that we, in fact, did know" (Adler and Adler 1991:180) and practicing "a degree of self-censorship, avoiding discussing potentially discrediting aspects of the setting" (Adler and Adler 1991:179). When subjects witness researchers tactfully deflect requests for troublesome information (Jacobs 1998:171), not only is trust enhanced but so too is the sense of reciprocity, of mutual obligation (Shaffir and Stebbins 1991:144). In claiming to know less than we do or be able to share less than we can, we attempt to walk the relational line between over-rapport and under-rapport, both of which threaten field relations and "destroy the delicate balance of external and internal considerations" (Shaffir and Stebbins 1991:145) essential to the ethnographer's insider-outsider status and its attendant advantages in achieving both proximity to and distance from the worlds they study and the subjects who inhabit them.

Presenting lesser selves, however, can incur personal costs. Some view "outright dissimulation both morally distasteful and difficult to execute" (Shaffir 1991:77). Others find avoiding competitions for esteem a challenge (Bogdan and Taylor 1975). Still others feel demeaned in assuming the diminished statuses their subjects expect (Evans 1991). The ethnographer, "as does everyone, has a self-concept to defend" (Bogdan and Taylor 1975:52). In doing ethnography, "You have to open yourself up to being snubbed. You have to stop making points to show how 'smart assed' you are...you have to be willing to be a horse's ass" (Goffman 1989:128), but there can be "personal and emotional difficulties of coming to terms with such estrangement" (Hammersley and Atkinson 1983:90). Field relations involve not only being with others, but also living with our selves.

## Leaving the Field

“Leaving the field” involves disengaging from the research site and disseminating the findings. Getting out is a comparatively neglected topic in discussions of fieldwork relative to the nuanced treatments of getting in, getting along, and getting results. The researcher’s decision to leave can be based on a number of practical criteria: the information obtained, analytical insights generated, available research time and funding, as well as a variety of external considerations to the project (e.g., institutional, interpersonal, and intrapersonal commitments and contingencies) (Snow 1980). Still, because successful fieldwork is contingent on establishing and sustaining relationships with a variety of subcultural members, leaving the field is complicated by the matters of relational distancing and disentanglement: “the leaving process is an aspect of an ongoing interplay between field circumstances and the way in which the researcher negotiates social relationships and a workable identity” (Maines, Shaffir, and Turowetz 1980:273).

Although ethnographers may never leave the field completely, as they may maintain relationships and connect various projects (Stebbins 1991), some relational distancing must occur, lest the field researcher be left with too many friends and too few publications. Relatedly, while disengaging from some relationships and settings is straightforward, “over-rapport” (Miller 1952)—getting too close to others—presents difficulties of disentanglement. Just as ethnographers “gauge how much rapport is necessary to get the cooperation required to continue the study” (Miller 1952:98), they also assess how little rapport is required to leave the field, attempting to “prevent relationships from becoming more personal than is desirable for the development of

insight” (Miller 1952:99). Here, ethnographers can adopt a cloak of incompetence to “inhibit, distance or sever relationships with others...a way of putting existing or potential associates off” (McLuhan et al. 2014:369), claiming to have less ability—time, funds, institutional support, and so on—to carry on with their research and related relationships than they actually do. Getting along fine in the research moment, but then retreating to their institutional homes, many fieldworkers are “temporarily friendly” (Fine 1993:272).

As students of the interaction order, field researchers are well aware of the centrality of impression management in achieving desirable selves and smooth affairs. Perhaps more than any other form of social research, ethnography adds a human element, dissolving the personal into the scientific throughout the research process (Fine 1993:283). The scientists’ creed is clear: no truth left behind. Full truths, however, are casualties of writing the worlds we examine. Readers are “betrayed by not having revealed to them all that one knows to be the truth” (Douglas 1976:43). Not only must we leave some data out to develop readable and publishable accounts, but we also may “shade some truths, ignore others, and create fictive personages” to protect the reputations of our subjects and ourselves (Fine 1993:287). In this way, the ethnographer withholds information that would spoil subject identities, “as do all the professions which deal with the problems of people” (Hughes 1971:436), and conceals the “dirty work” (Hughes 1971) required to meet professional demands. Sometimes the truth will beset you with obstacles and constraints. And so, in leaving the field, we bury discreditable bodies of knowledge, presenting ourselves as less informed than we actually are.

## Conclusion

We associate professions with specialized knowledge and ability—expertise and authority in areas that the community deems necessary and valuable. The rarefied place and collective reputation of professions rests on credible claims to a distinctive set of competencies and qualifications. Becoming and being a professional involves manifesting these abilities before colleagues, clients, and lay publics: “Ultimately, professional credibility depends on giving a convincing performance before an expectant legitimating audience” (Haas and Shaffir 1982:192). Members experience a performative pinch when their actual abilities fall short of audience expectations, challenging their personal fitness for the professional role. To protect against elements of doubt and disqualification, professionals envelop themselves in cloaks of competence, concealing deficiencies and exaggerating abilities (Haas and Shaffir 1977; 1982; 1987).

The other—analytically neglected—side of the problem of competence in professions is the burden of ability. Here audience awareness of specific competencies may present professional action problems, disrupting interactions, frustrating efforts, thwarting objectives, and complicating work. The practical demands of the profession, then, may also require members to selectively present less-than-fully-able selves—adopting a cloak of incompetence (McLuhan et al. 2014)—to perform the professional role. The ethnographer’s work, considered herein, offers an illustrative case. At particular points in each phase of the “natural history of fieldwork” (Shaffir and Stebbins 1991)—getting in, learning the ropes, maintaining relations, and leaving the field—ethnographers have found the deliberate concealment and diminishment of various abilities to be advantageous, if not essential, to accomplishing their work.

There are, of course, more examples of feigning incompetence in the field than I have considered here. Of those I identified, I provided only a sketch of competence-concealing situations and strategies that merit much more focused and detailed treatment. Future research on feigning incompetence in the field could reveal the panoply of impression management techniques ethnographers employ in dealing with problematic abilities. Research should also attend to the cloak of incompetence as an interactional accomplishment, identifying the researcher-subject dynamics, contingencies, and consequences of creditable and discreditable performances. A related matter concerns instances of research subjects feigning incompetence—not only when and how, but also with what implications for doing field research.

Though I present the particular case of feigning incompetence in the field, I claim more general analytic returns, for “the essential problems of men [sic] at work are the same whether they do their work in some famous laboratory or in the messiest vat room of a pickle factory” (Hughes 1958:48). If we find the problem of inconvenient, undesirable, or otherwise troublesome competence in one profession, we will also find it in others. The cloak of incompetence phenomenon is apt to be found in various manifestations in most work and occupations. We learn more about the generic features of social phenomena when we consider cases across manifold contexts (Glaser and Strauss 1967; Prus 1996; Zerubavel 2007; McLuhan and Puddephatt 2019; Puddephatt and McLuhan 2019). More research is therefore needed to not only identify specific cases of feigning incompetence at work, but also to leverage these individual cases for their formal theoretical insights through comparative analysis (Hughes 1970).

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# Agnostic Interactionism and Sensitizing Concepts in the 21st Century: Developing Shaffirian Theory-Work in Ethnographic Research

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**Abstract:** In this paper, we reflect upon our experiences taking a graduate qualitative methodology course with Dr. William (Billy) Shaffir. We highlight Billy's approach to ethnographic research and his declaration to "just do it." Rather than just absorbing theoretical knowledge from the literature, Billy taught us to be wary of the dangers of a prior theorization and how it can distort rather than shed light on empirical investigations. Despite his belief that sociological theory is far too often abstract and removed from real-world contexts, he nevertheless provided us with a latent theoretical commitment to concept formation, modification, and testing in the field that guides our research to this day. We explore Shaffir's agnostic and at times ironic approach to theory and demonstrate how his specific type of *theory-work*, derived from Everett Hughes' and Howard Becker's interactionist perspective on "people doing things together," influenced how many of his students study occupations and organizations via sensitizing concepts. Billy managed to get us to think differently about how we theorize in the field and how to cultivate a playful and healthy skeptical attitude towards its application. This type of *agnostic-interactionism* does not dismiss theory outright, but is always vigilant and mindful of how easy it is for practitioners of theory to slip into obfuscation and reification. We conclude with a Shaffir inspired *theory-work* that argues for the continuing significance of an agnostic stance towards ethnographic and qualitative inquiry; one that continues to sensitize the researcher to generic social processes through which agency-structure is mediated and accomplished.

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**A**dorjan, downtown in the city, is walking through the streets back towards home. He has a ridiculously large tape cassette recorder on hand, which he is using to capture his verbalized remarks about the interview he just had with a probation officer. He had received ethics approval to conduct a study with probation officers as his major applied project for William (Billy) Shaffir's graduate qualitative methods course. Through discussion of the research with the city's probation office, an agreement was made not to record interviews while they took place; he could take notes, and he was free to record his recollections of the interview afterwards. As discussed during Billy's seminar (which involved sharing one's experiences in conducting qualitative research), one's memory can fade fast, and as he knew his would fade faster than most, he wanted to ensure he got his memories on magnets as fast as possible. So there he is, talking to himself into this device larger than those first 1980s cell phones, and sure enough, he gets some sarcastic remarks thrown his way from observers on the street who probably could not figure out precisely what he was doing. It helps to make clear that the weather was quite warm, and many were enjoying the restaurant patios next to the streets he was walking down.

He did feel the duality acutely, both as an outsider to sociological research in general (he had taken a course-based Masters degree, and the research methods course did not involve engaging in original research), but also as an outsider-insider; a position which soon emerged by *getting out there and doing research* (Merton 1972; Adorjan 2016). He took the role of the hecklers, who did not mean any harm, but who led him to see his oddity reflected through their remarks (Cooley 1964). At the same time, he started to *feel* like a qualitative sociological researcher by getting away from campus, the textbooks and (however fascinating) theoretical debates, and diving into experience research first-hand. He had his written notes from the interviews, and his remarks into the recorder included details of his questions and the answers given, but also a variety of things that would help him contextualize the interview: his impressions of his own questions; his thoughts on the way the interview went; comments about the ergonomic features of the probation office and how this may come to influence the experience of probationers as they visited the office; and other factors that would help create vibrant and textured "thick descriptions" of the *culture* of probation work (Geertz 1973). He was certainly an outsider, too, to this particular probation office, but not to probation

work itself, as he had previous experience as a volunteer probation officer.

This all *positioned* him in a certain way—neither positively nor negatively—in relation to his participants, the questions asked, his interpretation of their answers, and so forth. Moreover, it is this positioning that is our window into the emphasis Billy placed upon direct engagement and experience with participants in the field. With Billy, much like many other interactionists who trace their sociological lineage to the Chicago School, methods are interdependent with theory (Clarke and Star 2008). Billy often instructed us on how to negotiate and manage boundaries with our informants, the data collected, and the theorization based on our understandings of our participants' social world. Acknowledging the barrier between himself as an ethnographer and one of his main informants, Billy (Shaffir 1999:684) writes,

While barriers between us have thinned, it is equally clear that they will never disappear completely. Whereas, previously, I saw this situation as a reflection of my inadequate field research skills, I now accept that successful field research requires a respecting of boundaries between researcher and researched, which, while potentially limiting the scope of the fieldwork, need not detract from it and is, moreover, inevitable.

In a similar vein, Billy taught us that good theorizing has the researcher creating a healthy metaphorical distance between what is found in the field and how such data can help be used to generate, modify, or refute concepts and theories that provide a sociological understanding of participants' worlds. Some distancing and managing the metaphorical boundaries between extant theory and data is a dance Bil-

ly constantly explored with us in class. In this paper, we first go into greater detail into Billy's qualitative research class and our experience of how he presented theory and methods within ethnographic contexts. We then trace Billy's theory-method lineage back to the Chicago School of Sociology, represented by Everett Hughes' comparative methodology and Howard Becker's analytic induction. Billy's students carry his Chicago roots forward into the 21<sup>st</sup> century with an *agnostic-interactionist* perspective informing their research, whether it takes up ethnography directly or uses other qualitative approaches. We highlight some of his students' more recent work that is arguably influenced by Billy's approach. We conclude with a description of Billy's inspired approach to the study of group life, which we term, *Shaffirian theory-work*.

## Qualitative Research as Rite of Passage

Ethnography's "...emphasis should always be on its practical accomplishments—the observation and description of the behavior of a group of people to understand their culture." [Shaffir 1999:677]

The experiences highlighted in Adorjan's field notes above, along with every other student's, were regularly discussed during Billy's weekly 3-hour seminar. He would often read and comment on our fieldnotes and ignore our theoretical memos. When asked why he was so concerned with our data and dismissive of our theoretical musings, he told us we are placing the cart before the horse. Kelly then asked him if he was implying that the data can be compared to a thoroughbred horse, and high brow sociological theories are merely wagons that need to earn the right to hitch themselves onto the experience of our participants. "You got it!" he replied with that classic Shaffir wink and a smile. "Next

time, just make sure the cart is loaded with Honeycrisp apples.”<sup>1</sup>

The time flew and we quickly grew to respect Billy’s directive not to put the theoretical cart before the empirical horse. Students may have felt initially anxious about sharing these experiences; we know we did. Admittedly, we wanted to “get it right” and impress the Zen Master with offerings of richly insightful encounters in the field. Billy’s emphasis, however, was not just on the data itself, but the encounters and *going concerns* which led to obstacles, frustration, and also reimagination and reconsideration (Hughes 1971). We learned much more from our failures than our ostensible successes. Ethnography, interviewing, and participant observation alike all required commitment. Some students shared challenges related to accessing participants in the first place—entering the field—and recalled stories of a “doc”-like character who would grant access to a new social realm (Whyte 2012). Others disclosed that they are not “morning persons” and abjured getting up early to meet with participants whose schedules demanded an early rise. Still, others raised questions of data and theoretical saturation—knowing when additional collection of data does not lead to new and significant knowledge. This, relatedly, would lead to questions about exiting from the field: how does one know when to stop, and in ethnographic work in particular, *how* does one retract from a group of people where often friendships have been made? And despite one having a formal role as a researcher, how does one negotiate “mutual influence” (Kelly 2010), “competing obligations” (Grills 1998), and the moral dilemmas

that make up the “underside” of ethnographic work (Fine 1993)?

No doubt the challenges of exiting were facilitated by the fact that we had a course to complete in a few months, with a final research report to submit. Nevertheless, while many qualitative courses feature readings and discussion about such things, it was integral to us in taking Billy’s course that we went out to experience research for ourselves.

Billy’s class reinforced the idea that qualitative research helps imbue “a clearer and sharper understanding of a slice of human lived reality” (Shaffir 1999:684), with an emphasis on “understand[ing] how social behavior is shaped and organized” (Shaffir 1999:685). Yet what are the “right” approaches to achieving this? As novice researchers, we expected a clear roadmap of steps to follow. Many of us who had little previous exposure to the craft (indeed, most of us had little to none), initially wanted Billy to provide more structure in the form of specific qualitative methodologies. We were, however, greeted only with the bemused smile of the Zen Master, who was trying to guide us on what appeared to be similar to *Leih Tzu’s Taoist pathless path* (Osho 2002). Billy’s approach to teaching and learning initially appeared unorthodox to us. Instructing students to “hang out” with research participants felt anomic; many of us simply did not feel adequately prepared to engage with the field and collect data on our own.

Furthermore, this “baptism by fire” approach generated a fair amount of anxiety that we may not have been collecting data properly. Class discussions, however, greatly helped us learn that our fears were common and, moreover, that we were encouraged to raise such issues to generate rich insights. Soon daunting aspects of fieldwork (i.e., entering the field,

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<sup>1</sup> The Honeycrisp is Billy’s apple of choice. Rare is the student who gets through his qualitative research course without eating at least one.

recruiting subjects, leaving the field) became issues we could grapple with. We often found a sense of confidence in *re-engaging* with our participants despite, or even due to, the challenges we faced.

Each week we would come to class and Billy would get all of us to discuss our experiences in the field openly. So each week through dialogue and collaboration, Billy would help guide us through our projects. We began to “see” our projects come to life out of a theoretical block of marble; our *craft* was not just extracting the perfect statue, but appreciating the process through which it was realized. Billy did not want our projects to be his; he wanted us to see them from our own informed sociological imaginations. Billy was always ready to help, and was immensely helpful, but his help rarely instructed us directly about how to pursue our own research. Billy encouraged us to create and answer our research questions, not his. He knew many of us to be impressionistic and idealistic and required that we learn not from what he wanted, but learn from the insights we gleaned from “just doing it.” With all of this being said, it would be a tragedy if the reader got the impression that Billy was merely a spectator and a passive and uncommitted teacher. On the contrary, he was and still is an inspiring mentor who has the patience, time, and openness to bring out the best in our work.

### **The Shaffirian Theory-Methods Toolkit**

After looking over each of our field notes at the end of the week, Billy would make suggestions on how to fine-tune and further explore the sociological questions we sought to answer. Guidance would consist of strengthening our analysis by slowly introducing us to a few sensitizing concepts and social processes under the light of the comparative method (we

elaborate on these concepts below). He often had us reflect on foundational interactionist tenants, like “role taking” and “definitions of the situation.” The two of us initially thought that a focus on these concepts appeared rather pedestrian. We assumed that “sophisticated” theory had to be front and center in the weekly memos we submitted to Billy; otherwise we were not conducting canonical sociological research (see: Luker 2009). Undergraduate training often conditions students to view all human experience through the lens of Marx, Durkheim, Habermas, Bourdieu, Giddens, or Foucault. It was enticing to apply what one experiences in the field to one of these canonical theorists; indeed, often we were inclined to see connections in many theoretical frameworks and had difficulty finding the ideal fit. Billy, however, encouraged us to see human life in terms of dynamic social processes, allowing us to understand and measure the fluidity of social action and organization in real time across different localities and permutations.

Billy encouraged us never to force sociological concepts, to be ever vigilant that theory should emerge from the data. Billy trusted us to mold theory in creative, innovative, and playful ways. And herein lies the irony: Billy urged us not to get trapped in theory proper, yet encouraged us to theorize. He pushed us towards maintaining a certain level of social distance from our participants; thus entertaining a type of realism which was necessary in order to theorize. We could not simply take our participants’ understandings of their worlds at face value, nor take them for granted. However, we were also not to take the theory derived from our textbooks for granted either. We soon realized the epistemological value in having a healthily balanced agnosticism towards our participants in the field and the theories we studied. Of course, this was not all made



explicit, but something we only later recognized as we made our way through Billy's class, comprehensive exams, and thesis preparation. Billy's approach, which we refer to as *Shaffirian theory-work* (see below) is most evident in his approach towards the study of occupational socialization and identity. We now turn to Billy's links to the Chicago School of Sociology to further demonstrate his unique approach to theorizing and how it shaped his students' research.

### **Agnostic-Interactionism: The Chicago Roots**

One overarching message Billy projected during our graduate qualitative methodology class: the data drives the theoretical application and modification, with an understanding that the two are inseparable. Billy's approach was inspired by his own mentorship with Malcolm Spector (1972; 2019) at McGill University, who was mentored by Howard Becker (1970; 2008; 2017) at Northwestern University. Becker was himself taught by one of the founders of the Chicago School of Sociology, Everett Hughes (1971). Billy, in 1972, was the first sociologist at McGill University in Canada to defend a PhD dissertation committed to symbolic interactionist theory and methodology. An auspicious occasion indeed, as Everett Hughes attended the defense with Howie Becker appointed as external examiner (Shaffir 2011, personal communication). Because Billy is widely considered "a principal interpreter of the Chicago School Diaspora in Canadian Sociology" (Low, this volume), we were very fortunate to experience this Chicago-McGill-McMaster connection, which at the time placed central emphasis on the insights gleaned from the Chicago School and symbolic interactionism. The Chicago School posited a clarion call for sociologists to embed themselves in social life in order to best understand the definitions of

the situation under observation. Rather than pursuing over-cogitated and reified theory claims, Billy pushed his students to respect the nature of the empirical world, where strict operational and "definitive concepts" may not easily map onto reflexive and often unpredictable behaviors and statements (Blumer 1969:146; Prus 1997; Maines 2001).

It is difficult to overestimate the specific influence of Everett C. Hughes upon Billy and, more widely, upon how interactionists approach the ethnographic study of occupations. Hughes sent his students out into the field to investigate and understand how people, situated along various points of occupational and professional strata (i.e., physician, custodian), defined and experienced their work. He showed how such people embodied "going concerns" found in everyday life and broader society in general (Shaffir and Pawluch 2003). Billy and his students not only took interest in the substantive topic of occupations and organizations from a Hughesian perspective, but also understood that his comparative approach, passed down to his students at Chicago (e.g., Becker, Strauss, and Goffman), is essential for any type of qualitative investigation (see: Fine 1995; Chapoulie 1996; Low and Bowden 2013; 2016; Archibald, Kelly, and Adorjan 2015; van den Scott and van den Hoonaard 2016; Kelly and Archibald 2019). Common experiential themes arise via comparison across occupations. Hughes argues for relational conceptualization that can handle social variation and homologies. He states, "We need to rid ourselves of any concepts which keep us from seeing that the essential problems of men at work are the same whether they do their work in the laboratories of some famous institution or in the messiest vat room of a pickle factory" (Hughes 1971:417). Hughes (1970:149-150) defines "the essence of the comparative frame" as one that "seeks differences in terms

of dimensions common to all cases." Also, in uncovering these "common dimensions, the differences become clearer, and more impressive" (Hughes 1970:150). Despite a variety of different statuses and roles, all occupations must manage a series of typical constraints that result in similar forms of adaptation (Shaffir and Pawluch 2003). In our class, Billy showed us how this comparative theoretical approach, sensitive to the emergence of common experiential themes, provides flexibility and playfulness that supports the use of sensitizing concepts (see below) and the discovery of generic social processes.

### **"The Crock" as Sensitizing Concept**

Theory is of value in empirical science only to the extent to which it connects fruitfully with the empirical world. Concepts are the means, and the only means of establishing such a connection, for it is the concept that points to the empirical instances about which a theoretical proposal is made. [Blumer 1954:4]

Billy was the first to introduce us to the methodological and theoretical value inherent in Herbert Blumer's sensitizing concept formation. A cornerstone of agnostic interactionism, a sensitizing concept avoids the sterility of "fixed bench marks [and] gives the user a general sense of reference and guidance in approaching empirical instances...directions along which to look" (Blumer 1954:7). Herbert Blumer, a colleague of Everett Hughes and Becker's social psychology instructor at the University of Chicago, felt sociological theory had become "conspicuously defective in its guidance of research inquiry" (Blumer 1954:4). By orienting the researcher to the direct happenings of the empirical world, the sensitizing concept alleviates the researcher from over-committing to deductive-nomological methodology and

all its flaws. Blumer recognized that researchers do not enter the field as a *tabula rasa*; that is, without any preconceptions or conceptual ideas that they wish to explore. The sensitizing concept serves the purpose of an epistemological placeholder, that is subject to (i.e., confirmed, refuted, augmented, reappropriated, etc.) testing against the lived experiences of people in their situated environments. Blumer (1954:8) here writes:

If our empirical world presents itself in the form of distinctive and unique happenings or situations and if we seek through the direct study of this world to establish classes of objects, we are, I think, forced to work with sensitizing concepts.

It was Billy's teaching of Howard Becker's discovery and application of "The Crock" as a sensitizing concept that "awoke us from our dogmatic slumber" and officially ushered us into a lifelong commitment to agnostic interactionism. Howard Becker, Blanche Geer, and Anselm Strauss, under the direction of Everett Hughes, conducted fieldwork at the University of Kansas Medical School (Becker et al. 1961). Becker began his fieldwork by "hanging around" third-year students in the Internal Medicine Department and "finding out what the hell was going on, who all these people were, what they were doing, what they were talking about [and] finding [his] way around" (Becker 1993:28). Upon first hearing medical students identify some patients by the demeaning term "crock," Becker pursued a line of inquiry via interviews and observations and discovered that a crock "referred in a derogatory way to patients with many subjective symptoms, but no discernible physical pathology" (Becker 1970:35).

Once Becker identified the term "crock" as a common usage among the medical students he inter-

viewed, it behoved him to inquire further and uncover the reasons behind the students' negative attitude towards these patients and how multiple meanings of a crock shaped student-patient behavior, and pointed to the broader social organization of the hospital. Upon closer investigation via analytic induction, he theorized that what first appeared as simple frustration with individual patients was actually refracting students' broader dislike and disparaging attitudes towards the medical school in general. A crock represented a lost opportunity to move beyond book knowledge and gain "clinical experience." Students had expectations that they would receive hands-on training in which to diagnose and treat disease. However, "crock" patients were seen to block professional pathways towards genuine "medical responsibility" necessary for acquiring medical expertise—"You can't cure anyone unless you can kill them" (Becker 1993:35). Viewing a "crock" as a patient without "real" symptoms who denied medical students' opportunities to learn best practices led Becker to link up student-patient interaction and the students' expectations of professional socialization. Billy had us read Becker's methods paper in class, describing "crock" as a folk concept that transcended its everyday use, sensitizing us to how the social processes of meaning-making within the student-patient interaction and student expectations of professional socialization connects to broader social structure and social organization. As Becker (1958:658) states,

Questions concerning the genesis of this perspective led to discoveries about the organization of the student body and communication among students... Since "crocks" were also disliked because they gave the student no opportunity to assume medical responsibility, we were able to connect this aspect of the student-patient relationship with...the value system

and hierarchical organization of the school, in which medical responsibility plays an important role.

The crock as a sensitizing folk concept is a subset of a Hughesian comparative generic social process, *anticipatory socialization*. Billy's own research, inspired by Becker and Hughes's *Boys In White*, would take Robert Edgerton's (1967) "cloak of competence" and use it as a sensitizing extant concept to further contribute to common patterns and variations associated with the social organization and experience of anticipatory socialization within medical professionalization in Canada (Haas and Shaffir 1987). The generic nature of social *assimilation* implicitly embedded within the experience of "the crock" and "cloak of competency," demonstrates how sensitizing concepts can migrate across social settings and offer greater generalizability. Will van den Hoonaard (2009; 2013), a friend of Billy's and an influential Dutch-Canadian qualitative sociologist, reminds us that we can bump up sensitizing folk concepts to higher orders of sociological analysis. He writes, "for this reason, sensitizing concepts are so highly transferable that they constitute a parallel model of explaining social behavior in other social settings" (van den Hoonaard 1997:74). In the next section, we explore the significance of the cloak of competence, demonstrating how sensitizing concepts can be applied, and extended, to varying empirical contexts. To do so, we refer to more recent work conducted by Billy's students.

### **Anticipatory Socialization: Extending Shaffirian Theory Work into the 21st Century**

When trying to understand people's experiences and interpretations of the world, Billy, clear with Blumerian conviction, expressed that we not con-

cern ourselves with individual internal psychological states, but rather explore what Becker (1970:276) calls, “social structure and its patterned effects on human experience.” Of course, in line with symbolic interactionism, Billy viewed social structure as an intersubjective and ongoing accomplishment. Gary Alan Fine (1991:164) once observed that “it is not that individuals cannot act as they wish. It is simply that they do not, knowing what they know of the world, whose forces are accepted by the self.” This approach to social structure again exemplifies the one Billy embraced; one that is not antagonistic to social structure *per se*, but one which takes seriously people’s understandings of the social contexts they are situated within.

Billy’s reflections on qualitative research evidence the saliency of these influences, perhaps most singularly Thomas and Thomas (1928). For instance, Billy (Shaffir 1999:684-685) writes:

the most credible understanding of social phenomena requires the researcher to discover the actor’s definition of the situation—that is, his or her perception and interpretation of reality—and that such discovery and understanding are best accomplished by placing oneself in the other person’s situation.

Note here that Billy is not suggesting *sympathy* for the participants one studies. Numerous interactionist examinations involve engagement with criminal populations that researchers may not necessarily sympathize with (Hamm and Ferrell 1998). Here Billy is suggesting the methodological significance of *empathy*, or what Cooley (1964) refers to as “objective introspection,” which is best achieved through a sustained embedding of oneself with the population under study so one can account for various perspectives and forms of social organization.

Several of Billy’s former students have taken his agnostic interactionist approach towards generating theory into the field; many successfully applying and modifying various sensitizing concepts that explore the lived experiences and social processes through which social structure is mediated and accomplished (Puddephatt, Shaffir, and Kleinknecht 2009a). Often the focus of our research was within occupations and organizations, perhaps based on Hughes’ influence on Billy. We recall Billy recommending to us Hughes’ classic work *The Sociological Eye* (1971). Billy was not “pushing” Hughes in a forceful way, but recommended we take a look at Hughes based on our interest in studying professions and occupations. The following case studies are not an exhaustive list, but a brief snapshot of Billy’s legacy at McMaster University in the agnostic-interactionist perspective on occupations and organizations.

Antony Puddephatt, a Canadian interactionist who studied with Robert Prus at the University of Waterloo and Billy Shaffir at McMaster University, took us under his wing during our first year of graduate school and showed us first-hand the inner workings of Shaffirian theory-work. Our reconceptualization of the *cloak of competence* is quintessential agnostic-interactionism. Originally coined by Edgerton (1967), Billy and Jack Haas use the *cloak of competence* concept to describe how medical students attempt to manage and elicit favorable impressions from critical audiences (i.e., patients, medical school evaluators, and each other) during their medical student role performance. These critical situations select for a feigned competency that results in successful reputational outcomes during the ritual ordeal of professionalization (Hass and Shaffir 1977; 1982a; 1982b; 1984; 1987). Extending this generic social process into oc-

occupational subcultures within the sociology of education and inequality, we argue that being overly concerned with managing other's impressions of one's competent sense of self, under conditions of intense asymmetrical interaction, can trend towards unhealthy manifestations of conformity. Indeed, reflecting even more widely on the graduate student subculture, we worked with Puddephatt on a response to an argument by Gabrielle Ferrales and Gary Alan Fine (2005) that suggested ways graduate students should conduct effective face work towards their supervisors in order to generate a positive impression. Our argument was, again, that too much concern among graduate students putting on airs of competence would produce anxieties and eventually supervisory problems (Puddephatt, Kelly, and Adorjan 2006). For example, the subsidized Canadian post-secondary system reflects a relatively flattened hierarchy compared to the heavily institutionalized symbolic capital found in the winner-takes-all ivy league privatized schooling markets in the United States. These structural differences highlight international variation in the socialization of graduate students. We found that otherwise insipid mistakes occurring in the classroom and during scholarly meetings tend to be much more costly evaluation rituals within elite social structures (i.e., top-tier American schools). A student's fear of making a mistake and appearing incompetent in the co-presence of faculty gives way to a toxic culture of learning that encourages *deceptive interactional routines* and inauthentic, cynical character contests. Such pathological forms of impression management may stunt creativity and innovation at the highest levels of graduate training. We call this occupational over-socialization process the *cloak of conformity* (Puddephatt, Kelly, and Adorjan 2006; Leigh 2017:614).

Arthur McLuhan, another student of Billy's, further extends this to consider the flip side of the concept, a *cloak of incompetency* within the sociology of everyday life (McLuhan et al. 2014; see also McLuhan, this volume). It is thus clear that a core sensitizing concept like "anticipatory socialization" can be refined to pinpoint certain behaviors found in particular organizational contexts, such as a "cloak of competency." The extension and in some cases reappropriation of the concept, for instance, of how a "cloak of incompetency" may be salient as a presentational strategy in everyday life, indicates the value of broad agnostic interactionism that is playful with theoretical concepts and their application.

Many of Billy's students conducted similar agnostic-interactionist research into occupations and organizations. For example, Carrie Sanders (2014) uncovered how a "hierarchy of credibility" contributed to what she terms an *ideological disconnect* between the intended design of emergency technology and its in-situ application by police, fire, and emergency medical services—ultimately resulting in the ambiguity and subsequent impediment of effective emergency interoperability. Santin and Kelly (2017) discuss how updated airline security policies after 9/11 provided female employees with new corporate guidelines that acted as normative resources in which they could draw on to empower themselves to be more assertive when interacting with untoward passengers. They observe that flight attendants now have more discretion in defining situations in which security trumps courtesy. Such new levels of autonomy not only reduced the emotional labor of female flight attendants, but also leveled the playing field within the gendered division of labor. Prior to 9/11, male flight attendants enjoyed a more pronounced status shield. Santin and Kelly termed this leveling process *role*

*shielding*. This concept links micro interaction and workplace emotion with broader cultural institutional change.

Scott Grills recently completed a book titled *Management Motifs: An Interactionist Approach for the Study of Organizational Interchange*. The book sensitizes us to *management* as a generic concept that can be extended across a number of organizational and occupations settings as everyday office related activities in which individuals or groups target others in an attempt to shape their experiences. *Doing management work*, *management teamness*, and *inaction as social action* are but a few key conceptual tools that help interactionists uncover and understand management as practical accomplishment (Grills and Prus 2019). Steve Kleinknecht explains how defeated politicians use “deflection rhetoric” as a strategy to save face, cope with unanticipated loss, and disengage from the political arena. They term these processes “un-becoming” (Shaffir and Kleinknecht 2005). The value of “front line” empirical research with occupational actors is also highlighted in Ricciardelli, Adorjan, and Peters’ (2019) research examining Canadian correctional officer experiences under changing youth justice legislative contexts. The value of this research is in pinpointing the perceptions and working experiences of correctional officers working within closed-custody youth facilities before and after the implementation of the Youth Criminal Justice Act. Overcoming the often misconstrued astructural bias of interactionism and its neglect of power, Sherryl Kleinman and Matthew Ezzell (2012) studied how upper-level campus administrators create positive public perception and control programs associated with reproductive rights with mission statements that hide powerful right-wing influence by framing the debate around the culturally acceptable rhetoric of “both sides,” “tolerance,” and “The Law.”

Billy illuminated the sociological insight that power is both a process to be empirically investigated and an intersubjective accomplishment. We are indebted to this approach since it enabled us to inform our doctoral research using these crucial insights. Both of us were inspired by a broader interactionist perspective on power and situated knowledge, so our focus became the *understanding of understandings* of people, reflected in their lived experiences, what they say, and what they do (Ibarra and Adorjan 2018). What’s more, we learned to attend to the negotiation processes and unintended consequences that constitute these emergent understandings within social interactions (Kelly 2017).

It was perhaps inevitable that the formative experiences encountered in Billy’s qualitative methods course led us to one of his colleagues, and another one of our mentors at McMaster University’s department of sociology—Dorothy Pawluch. Her branch of sociology, social constructionism (Woolgar and Pawluch 1985), led us towards an agnostic examination of social problems (Adorjan et al. 2012; Spector and Kitsuse 2017). Over the years, Billy and Dorothy’s students have produced what can be considered agnostic interactionist studies, including our own which center on discourses of youth crime and youth crime “stat wars” (Adorjan 2011a; 2011b), and ethnographic research on activist engineer-scientists’ attempts to mitigate climate change by developing and managing green technology with corporate, governmental, and lay end-users (Kelly 2011; 2017).

### **Discussion: The Value of Shaffirian Theory-Work**

Shaffirian Theory-Work is inspired by a Hughesian agnosticism towards “armchair” theorization that

his student, Howard Becker (1998:4), succinctly expresses in relation to his mentor:

Like Hughes, I have a deep suspicion of abstract sociological theorizing; I regard it as at best a necessary evil, something we need in order to get our work done but, at the same time, a tool that is likely to get out of hand, leading to a generalized discourse largely divorced from the day-to-day digging into the social field that constitutes sociological science.

In this paper, we have argued that Billy's lineage within the Chicago School of Sociology, grounded in a few core SI tenants, makes for an ambivalent and playful approach towards theorization in the field. We further demonstrate how Billy passed on this interpretive theory-method toolkit to students who continue to study occupations and organizations from his agnostic-interactionist perspective. We call this line of qualitative sociological inquiry *Shaffirian Theory-Work*. And while Billy encouraged a marked playfulness when it came to theory, his approach was still very systematic. The agnosticism applies as a tonic against overconfidence we may have in a theory *before* applying it to our research findings. It guards against being dogmatically wed to any particular theory/theoretical concept, but is flexible enough to apply, refute, or modify the theory given emerging linkages (analytically induced) to broader analytical sociological frameworks of explanation. Agnosticism, here wedded to interactionist perspectives on studying social life, eschews abstract theorization distantly removed from member's everyday experience. Howard Becker, for instance, advocated for a flexible theoretical approach within a qualitative methodology that is "open to multiple possibilities, discovered in the course of immersion in social life"; and further warned us to avoid theory built "on the basis of a priori con-

siderations, the truth of an already established abstract philosophical position" (Pessin 2017:104; also see Becker and Pessin 2006). This approach helps engender discovery, often unanticipated, in the field (Albas and Albas 2009; Puddephatt, Shaffir, and Kleinknecht 2009b). It emboldens students to not rest on their theoretical laurels, which merely recycles established academic doxa. It encourages scientific innovation, which often occurs when we explore what other scholars assume to be banal, uneventful, or mundane (Becker 1998:96).

It is also important to note that despite half a century shaping the interactionist landscape, Howard Becker does not believe a researcher can find a "Serious Theory" in his work and he would be disappointed if one could. He states, "What you could find would be a series of general ideas that oriented my research and writing about the various empirical topics I took up. A vague interest in a sort of Blumerian social psychology that could account for how people communicated and got it together to act jointly." The "theories" that he claims really affected him consciously, the ones he picked up and used, were almost entirely borrowed from people who studied the arts (i.e., Leonard Meyer, Barbara Herrnstein Smith, George Kubler [Becker 2016, personal communication]). This proves once more that a healthy agnosticism towards sociological theory sensitizes us to unexpected reservoirs of creativity; inspirational sources that can move us in and out of our own experiences in the field and those of our participants and the social scientific literature in general.

It is well known that Everett Hughes disliked social theory (see: Fine 1995). But, he nevertheless built outstanding scholarship around the conceptualization of ethnographic data. Roughly fifty years ago,

Everett Hughes' students provided him with a Festschrift. They made it very clear that their mentor was not completely averse to theorization. And today our dedication to Billy sounds eerily similar to what Howard Becker wrote of Hughes:

It may be because Everett needs concrete materials to anchor his magnificent capacity for conceptualization that he has sponsored field research among his students, and done field research himself. His mind is not an empiricist's, careful about facts, insistent that they not be smudged with speculation, skeptical of interpretation or theory. Rather, he has an extremely strong conceptual mind which operates with the materials of concrete reality, which functions by relating apparently disparate observations, presenting them in new perspectives, producing frameworks and concepts for organizing and integrating them. He prefers to develop analyses which retain complexity; to find value, at times delight, in variety...His general aim is to identify the systematic underlying the various; not by simplifying, but by making clear what is essential. We count it our good fortune to have studied with Everett Hughes. [Becker et al. 1968:x]

The *agnostic interactionism* that originated with Everett Hughes at the University of Chicago, perfected

by Howard Becker at Northwestern, and further articulated with Billy Shaffir at McMaster, continues to prosper within Canada and beyond.

Sitting in Billy's graduate qualitative methods class, we witnessed and began practicing first-hand his type of agnostic interactionism. Baked into this theory-method cake, is a distinct blend of comparative methods and generic sensitizing concept formation that we traced back to the Chicago School of Sociology and the occupations and organizational research conducted by Everett C. Hughes and Howard Becker. Billy passed on to us a symbolic interactionism that is agnostic towards theorization. Our interpretation of Billy's *theory-method toolkit* brings attention to subtle aspects of his implicit theorization. Our understanding of his conceptual playfulness and flexibility made us more cognizant of the importance of data being grounded in the emergent and processual nature of group life. As an agnostic interactionist, Billy helped us sit comfortably in this gap; not to fear it and rush to silence it with a pre-configured theoretical framework. We are fortunate to have witnessed and be guided by Billy's flexible and deft touch with sociological theory within his methods course, leaving an indelible mark on our own teaching and research style to this day.

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# Piecing Together the Meaning of “Dirty Work”

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**Abstract:** I reflect upon Dr. William Shaffir’s influence on my approach to ethnographic research and my study of homeless shelter workers. Dr. Shaffir introduced me to his own brand of the craft of qualitative field work, but also introduced me to important sociologists and ideas in the symbolic interactionist tradition. Most central was Everett C. Hughes’ notion of “dirty work,” which helped shape my research focus. Building from Hughes’ concept, but expanding it with Shaffir and Pawluch’s (2003) social constructionist approach to occupations, I was better able to conceptualize the process of how workers themselves piece together the meaning of “dirty work.” Beyond gaining these conceptual insights, I also reflect on Dr. Shaffir’s teaching philosophy of qualitative methods, that is, the importance of learning by doing. I conclude with some thoughts regarding Shaffir’s perspective on the wider ethnographic task of describing, *in situ*, members’ understandings and definitions. Following Everett Hughes, I call on interactionists to give more attention to “dirty work” as a generic and transcontextual process.

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I am deeply honored to have been given the opportunity to contribute to Dr. William Shaffir’s Festschrift. I have learned a great deal from him both through his lessons at McMaster University, and his writings on qualitative research. As a recent student of Dr. Shaffir, I discuss the influence of his work, and his approach to sym-

bolic interactionism more broadly, in shaping my investigation of "dirty work" among homeless shelter workers.

In order to write this essay, I have asked myself how I could share my own benefits from William Shaffir's teachings and writings with others, as clearly and succinctly as possible. I also want to demonstrate a sociological perspective for describing "dirty work" from the viewpoint of occupational members, in my case, those who work with the homeless. Like the many ethnographers contributing to this special issue of *Qualitative Sociology Review*, I have virtually absorbed his perspectives on field research and learned to tailor them usefully to the realm of work and occupational life.

Everett C. Hughes' enduring interest was in what he termed "dirty work," viewed as a part of "deviant occupations," counterposed to more legitimate or noble occupations and professions. This definition implicitly presupposes a definition of what "deviant occupations," "in fact," are. Although the assumptions we bring to "deviant occupations," and those who do "dirty work," may be an inevitable part of our sociological enterprise, I propose eschewing assumptions about "objective characteristics" altogether. I came upon this approach by reading Dr. William Shaffir and Dr. Dorothy Pawluch's (2003) "Occupations and Professions" in the *Handbook of Symbolic Interactionism*. They write: "rather than focusing on the objective characteristics of occupations and their interrelationships and place in the larger social structure, symbolic interactionists view occupations subjectively, as groups of workers constructing meanings" (Shaffir and Pawluch 2003:894). This means researchers ought to focus on how occupational members decide who they are and what they are about; what services they

are providing; what they deem objectionable about their work; and how they deal with client problems, among other issues.

Following this social constructionist approach, the meanings attributed by occupational members to the work they do becomes the heart of the matter. As such, what occupational members so name (as, say, "dirty work") constitutes the object of study. The definitional activities surrounding such forms of work, as observable collective enaction and interaction, are grounds for ethnographic inquiry. Following W. I. and Dorothy Swaine Thomas' famous theorem, importance is placed on the "definition of the situation," and what groups of workers themselves conceive as real and important (Shaffir and Pawluch 2003). What is to be considered "dirty" has to be defined by occupational members as such in the first place, and occupational members only. In this way, ethnographers of "dirty work" can focus on the definitional activities and processes whereby occupational members select, assess, and sort what aspects of their work are disagreeable and objectionable, rather than analysts presupposing, or worse, imparting, the overall "deviant status" and associated "dirty" elements onto the occupation.

As a narrative process, this paper expands on William Shaffir's influence on me during my Master's thesis work. I begin by discussing the ethnographic lessons learned, teasing out the pragmatist theme of *learning by doing*, which underlines Shaffir's approach to passing on the craft of ethnography. For me, doing ethnography was, to quote Herbert J. Gans (1968), a "traumatic introduction." I go on to explain the ways in which I used William Shaffir's work (and the symbolic interactionists who influenced him) in order to weave a particular approach

to studying the “dirty work” of people who work with the homeless.

### “A Traumatic Introduction”

During my undergraduate studies in sociology at Concordia University, in Montréal, Canada, I took my first field research course. A lot of that class focused on reading and rereading field research methodologies writ large, such as their epistemology and ontology. Much of it focused on “thinking about” field research rather than “doing” field research. With William Shaffir’s field research course, which I took in the winter of 2017, reading became less important than doing. Read what you can, be selective, but make sure to engage with the field, with people’s actual “doings,” above everything else.

I decided to do ethnographic research in homeless shelters in a large Canadian city to study the workers there. I had initially devised an interview guide prior to entering the field. During an office meeting with Dr. Shaffir, he would just tell me “go into the field.” To invoke one of Howard Becker’s memorable phrases of “people doing things together,” I soon learned that ethnography itself was a collaborative and collective process in which ethnographers made sense of other people’s “doings” or “interpretations.” It was thus almost counterproductive to predesign an interview guide before entering the field. I had to learn from what was going on there. Moreover, I learned to reflect on the process of accomplishing fieldwork. How did I do it? How did I establish a presence and accomplish rapport? One soon realizes that accomplishing fieldwork becomes an intersubjective accomplishment with others. I now see ethnography as such a process: a process through which anything, including matters of fact and concern, have been put-together, interpreted

and understood *in situ*. It is a social and collective process, requiring the complex joint efforts of the many people involved. Ethnography is a practical accomplishment (Shaffir and Stebbins 1991; also see Prus, Dietz, and Shaffir 1997).

My early experiences of entering the field reminded me of Herbert J. Gans’ experiences in Everett C. Hughes’ “Introduction to Fieldwork” course. No one talked much about participant observation, we just did it and Dr. Shaffir would listen, in a narrative process, to our ethnographic experiences. It was a “traumatic introduction.” As Gans (1968:301) recalls, “Everett Hughes gave us some words of introduction and of instruction, but good father that he was, he quickly pushed us out of the nest and told us to fly on our own.” The same can be said about Dr. Shaffir. I am sure many contributors to this Festschrift would attest to the fact that William Shaffir was “our Hughes” (Kelly and Adorjan, this volume; Low, this volume).

The first lesson I learned about doing ethnography is that it is a very daunting experience. In this first instance, the participant researcher is at the margins of those he or she is studying, or at least, I was. I knew nothing about working in a homeless shelter. I knew nobody who did. And in part, that is what seized my curiosity. Who was helping these people? Being in a marginal place in relation to those we are studying requires the ethnographer to take on a role, which, in effect, requires a performance. This performance is not always consistent with his or her real feelings. I always envied those who seemed to enter fieldwork settings with ready ease. For me, entering the field was plagued with the worst of anxieties. Dr. Shaffir constantly reassured me that this anxiety had more to do with the nature of ethnography than with me. He also said that, although the

experience of anxiety never fully disappears, it can be managed. Managing a convincing self-presentation could, indeed, mitigate, if not reduce, the experience of anxiety altogether. Since I was in a position of marginality, I had to make sure that my impressions worked, otherwise they would shun me. I took on the role of the naive learner and tried to exude a personable and professional image. As Shaffir (1991:74) himself wrote, "I have learned to cope with mild states of anxiety and uncertainty and now accept these as part of the field research adventure."

In *The Positive Functions of Poverty*, Herbert J. Gans (1972:278) argued that the existence of poverty ensures that "dirty work" is done and that many economic activities which involve dirty work "depend heavily on the poor." The poor and the homeless provide "jobs for professional and paraprofessional 'poverty warriors'" (Gans 1972:278). Without the poor, many occupations and professions would expire. Poverty directly produces work for various occupations and professions which serve the poor as their clients or "shield the rest of the population from them" (Gans 1972:279). As Gans (1972:279) writes:

As already noted, penology would be miniscule without the poor, as would the police, since the poor provide the majority of their "clients." Other activities which flourish because of the existence of poverty are the numbers game, the sale of heroin and cheap wines and liquors, Pentecostal ministers, faith healers, prostitutes, pawn shops, and the peacetime army, which recruits its enlisted men mainly from among the poor.

The unintended consequences of poverty, therefore, are the proliferation of responding occupations and professions. Penology, criminology, social work,

social services, caseworkers, poverty activists, sociologists, journalists, and public health depend on the existence of poverty. The image of the "poverty warrior" who lucratively benefits from the suffering of the poor and the homeless is a stigmatizing typification. During my early days of entering the field, I learned that this was the view that many outside agencies had of homeless caseworkers. I had initially visited a community legal clinic in James Town to speak with one of its representatives about the problem of "gentrification" in the city. The representative explained to me, quite judgmentally, that "James Town is a huge poverty industry. The homeless shelters in the city," she said, "are a big business." I knew then that I had to interview caseworkers who serve the homeless. My assumption was that these workers were "expert in their own lives" and that they knew what they were all about. I soon learned that caseworkers were often irritated with images of them as part of a lucrative poverty industry. As one caseworker reported, "when dealing with poverty, I really get offended when people say, 'Social workers don't want poverty to be fixed because then they will be out of a job.'" From Shaffir, I learned to focus on people's experiences and their interpretations of reality.

Dr. Shaffir would listen more than he would speak. He had a great deal of intellectual respect for his students and would listen attentively as they expressed their thoughts in the classroom. He was, from my perspective, very non-directive. Although he provided us with some readings, students soon learned that it was about reading with a purpose. At least that's what I did: if it was interesting and relevant and useful to me, I read it. Why read something that is not directly helpful to what is happening in the field? We were as responsible for the process of learning as he was. He never imposed his *teachings*

onto us. He was not doctrinaire. I do not even think we can effectively say that he had *teachings*. He did not impose a path on us. Ethnography, for William Shaffir, was an iterative process. The one lesson that stands out as most important to me is that of learning by doing, which lends focus and purpose to the pragmatic reality of ethnography.

## Learning by Doing

It was very refreshing that Dr. Shaffir did not teach his students any rigid “method,” since this had to be worked out in the field, nor did he push any one “theory,” since concepts had to emerge out of our research process. There is the commonplace notion that the teacher is more likely to play the role of a patron than a companion. That, for me, was the lesson. Finding the appropriate theory and method for a particular study is not something one can teach. As I had learned, there was nothing better than *working from the ground up*. Yet, William Shaffir gave me ideas that helped to shape my investigation. He told me to read Everett C. Hughes’ (1958) *Men and Their Work* and the idea of “dirty work,” and so I did. From this, I learned to reflect about what I was learning in my research project and to use that to shape the next steps I took in the field.

For John Dewey (1958), learning is a part of real life, which is the natural outlet of learning by doing. Learning by doing seems to mean learning on as needed basis. We learn because something has caused us to want to know. We learn because we doubt. We learn from everything we do. According to Dewey (1958:46), “education is not an affair of ‘telling’ and being told, but an active and constructive process.” But, of course, doing’s counterpart is reflection. Rather than being a matter of telling and being told, like the great teacher Dr. Shaffir was, he

rarely set constraints on my motivations to learn by doing. He would provide a subtle nudge here and there, but he was endlessly affirmative and encouraging. A rare combination of brilliance and approachability. I always felt like he had trust in me. Learning, therefore, appears to be more of an active, constructive process, namely, a cooperative process of learning from everyday situations with the necessary condition of the application of useful ideas and concepts—as analytical tools—to the interactive setting. Ethnography, therefore, is active learning rather than the application of methodological rules. Such learning, surely, is complemented by the use of one’s analytic faculties in a process of understanding and describing the meaning of situations. Construction, not instruction, is an active, ongoing process of learning. Learning by doing, therefore, is an important form of practical induction. We learn by reflecting on our ethnographic experiences. The way we acquire knowledge about the things we should learn to do is by doing them. That is the lesson I received from Dr. Shaffir. We learn the craft of ethnography by doing the ethnographic work we must learn to do. I can now say that students learn by constructing their own understanding of ethnographic encounters and experiences by building on what they already know, consciously and unconsciously, to constitute a perspective of the social world.

I once overheard William Shaffir say—as he was speaking with my supervisor—that, “My students...I never tell them what to do...they find out how to do it themselves.” Dr. Shaffir was on my supervisory committee. I read his work closely and we would speak together about my thesis work. When I mentioned my potential doctoral topic, he said, “Interesting...but how are you going to get in?” Shaffir asked the right questions. No easy answers,



because, there were no clear answers to many of the problems of ethnography, only better ways of doing things. In *Doing Ethnography: Reflections on Finding Your Way*, Shaffir (1999) recounts a parable, as told by the Rebbe, of a man who had been wandering in a forest for many days, not knowing the right path out. The young man went for a walk in the woods and upon his journey, eventually recognized he had become lost in its depths. He wandered alone. He wandered scared. Eventually, he encountered a woodman. Shaffir (1999:676-677) writes:

“How long did you say you have been lost in the forest?” inquired the woodman. “For three days,” the man cried. “You say you’ve been lost in the forest for three days?” asked the woodman. “Just look at me. For ten years I’ve been wandering the depths of the wood, unable to find my way out of the maze.” At this, the man who had lost his way burst into tears, saying, “Now I see there is no hope.” The woodman said with a gentle smile, “Still, you have gained something by meeting me. From my experience in wandering through the forest for ten years, I can at least teach you one thing of great value—I can show you which are the paths that do not lead out of the forest.”

Stories connect profoundly to our lives. They speak to us on different levels. Sometimes we have to hear or read a story many times before we discover its meaning. According to Shaffir (1999:677), “this fable is surely apt for those of us who pursue ethnographic research and guide students through it.” You could have a variety of great ideas, but at the end of the day, it came to the pragmatism of ethnographic research: could you get in or not? How would you manage to overcome the practical, social, and emotional dilemmas that you encountered along the way? How would you convince

those you wanted to study to take part in your research? How do you establish a presence in the field? Nobody can teach you that. There is no precise theory that can lay it all out. Yet, we can learn from others’ past experiences and gain wisdom from their mistakes. But, in the end, whatever path one takes must be accomplished, not just talked about. That, for me, was Dr. Shaffir’s greatest lesson, “Sure, a great idea, but now what?”

### “Dirty Work” as a Social Construction

We often remain silent while others manage the work of incarceration, death, garbage, and helping the homeless. Everett C. Hughes (1971:343) argued that work becomes “dirty” when it “in some way goes against the more heroic of our moral conceptions.” Dirty work can be physically distasteful, morally “dirty,” or socially and personally disreputable. Dirty workers “perform the lowly tasks without being recognized among the miracle workers” (Hughes 1971:307). In this way, it is best to view work as a collection of various activities, some of which “are the ‘dirty work’ of that trade” (Hughes 1994:62). Dirty work may be dirty in several ways, being merely “physically disgusting” or a “symbol of degradation, something that wounds one’s dignity” (Hughes 1994:62). Much of Hughes’ concern with “dirty work” was with the cleaning up what was morally and physically dirty and its consequences for the ongoing process of everyday life. Hughes was chiefly interested in the “social drama of work.” He often assumed the “dirty” aspects of work to be an objective element of the situation, from which such social dramas might unfold.

Yet from Shaffir and Pawluch’s (2003) writings on the sociology of work, and Dr. Shaffir’s (1999:684) advice to describe reality as it is experienced and

“expressed by those whom we study,” I emphasized the lived processes of work. Shaffir writes (1999:684-685), “my own position, crystallized over a variety of research, is that the most credible understanding of social phenomena requires the researcher to discover the actor’s definition of the situation—that is, his or her perception and interpretation of reality.” Elsewhere, Shaffir (2011:39) recalls the “Thomas dictum,” which is “a reminder that reality is socially constructed, and that people respond as much, or more, to the meaning a situation has for them than to the objective features of that situation.” This roots back to W. I. and Dorothy Swaine Thomas’ (1928:572) concept of the “definition of the situation,” namely, that “if people define their situations as real, they are real in their consequences.” I attempted to understand how caseworkers in homeless shelters define and make sense of what they do. Ethnography, therefore, is a descriptive practice that is attuned to meaning. Rather than ascribing “dirty work” to the occupation in question, I was interested in the social contexts in which “dirty work” is identified, defined, and then dealt with. “Dirty work” must be understood in its naturally occurring context.

By understanding the caseworker’s “definition of the situation,” I learned to understand “dirty work” as a subjectively derived category, and thus a social construction rather than an inherent property of the work itself. As Dr. Shaffir told me, “priests deal with street workers that come for confession, but this doesn’t mean that priests’ work is dirty.” Over time, through social interactions among members of an occupation, a consensus emerges as to how to define which tasks are the most objectionable. Therefore, I focused on the “occupational culture” of those who work with the homeless, to better understand the justifications, definitions, and explanations of

“dirty workers” with respect to their occupational activities. These definitions give their work significance, for “each construction serves to proclaim the occupation’s legitimacy” (Satzewich and Shaffir 2009:207).

I learned that “dirty work” does not merely result from dealing with vulnerable populations or unsanitary conditions. We can learn to be more reflexive and aware of the assumptions we bring to the table and work to correct many of these assumptions by learning from the perspectives of the other. If caseworkers do not experience the “dirtiness” of working with certain “stigmatized” clients, then we cannot claim it to be real. Instead, what they *designate* as “dirty” is dirty. This is far from a denial of reality. It is a call to respect members’ definitions. Therefore, phenomenologically, one must begin with the experience of work and only from there lead into what occupational members themselves designate and define as “dirty work.” By naming such “dirty work” as a fully definitional entity, I hope to draw careful attention to occupational members’ definitions, not our own.

Sociology has accumulated a long list of concepts to designate the “cultural” in our understanding of work and occupations, and “social reality” more broadly. These include norms and values (Parsons 1968), tools and toolkits (Swidler 1986), sense-making and accounts (Garfinkel 1967), excuses and justifications (Scott and Lyman 1968), frames (Goffman 1974), as well as narratives, themes, strategies, or myths (Lévi-Strauss 2013). These concepts serve as useful representations of culture, but more importantly, point to how people define situations and do things together. They indicate something that was clear in William Shaffir’s interactionist work, that is, people’s interpretive capacities and procedures for

social action and interaction. Social reality is built continuously *in situ*, often with skepticism towards taken-for-granted assumptions about shared values and the givenness of social situations. What we call social reality or everyday life exists precisely because people engage in social relationships and actions, the result of people “doing things together” (Ben-Yehuda et al. 1989). Actions, responses, and reactions to what we are doing—in this case, “dirty work”—are embedded in these interactions. I followed Dr. Shaffir in trying to understand people’s interpretations of reality: people making sense of the meaning of what is going on, using sense-making tools such as stories and frameworks to communicate it, and strategies to decide what to do next are the processes that constitute social action, interaction, and order. These processes are assembled and put together by people’s collaborative activities in the making of social worlds.

Rather than overestimating “dirt” as a source of disgust, we should understand that our senses are never culturally free, or naturally given. While pedestrians may “sometimes go so far as to cross the street in order to avoid anticipated interaction with the homeless” (Snow and Anderson 1993:199), as avoidance rituals, the reality of caseworkers tells a different story. Social constructionism draws attention to what people conceive to be real and what is taken for granted while conducting everyday life. Those definitions, as they relate to occupational worlds, come to be sustained by occupational cultures and institutions and are explained by social and symbolic understandings.

In using this framework, I sought to grasp how caseworkers working with the homeless defined their organizational mission. The focus was on what they said. Our interpretations, as sociologists,

should be about understanding the interpretations of our informants. All our stories, as ethnographic descriptions, are precisely that, stories: a combination of selected data into consistent narratives that reflect members’ understandings. If one believes something to be real and acts upon it as if it is real, then there are consequences in the “real” world. Schopenhauer (1901:15) had asserted that “it is not what things are objectively and in themselves, but what they are for us, in our way of looking at them, that makes us happy or unhappy.” Before Schopenhauer, Epictetus claimed that “men are not influenced by things, but by their thoughts about things” (cited in Schopenhauer 1901:15). The definition of the situation, however, implies that there is no private and exclusive correspondence between what is “objectively real” and people’s definitions of that “world.” Schütz (1962) referred to “multiple realities” to convey how the same “situation” or “object” can possess different meanings to different actors involved. Goffman (1974:43-44) extends pragmatist (e.g., William James) and phenomenological (e.g., Alfred Schütz) insights on “multiple realities” to consider the various ways actors “key” frameworks to mean “something quite else” for the participants involved. Goffman’s *Frame Analysis* (1974), therefore, provides fertile ground for exploring the ways in which occupational members frame and reframe their “dirty work” in ways that are meaningful to themselves. Workers, when confronted with work they identify as “dirty work,” must ask the question, “What is it that is going on here?” Some cognitive reframing work must be achieved. These reframing strategies become important sense-making practices to explore—in studying “dirty work”—illuminating the meaning-making processes whereby occupational members transform their first order experiences of “dirty work” into second order experiences and realities.

## Piecing Together the Meaning of “Dirty Work”

Entire occupations do not constitute “dirty work” in themselves. Rather, workers identify and define “dirty” elements or components within them. Doctors, typically seen as high-status professionals, also have elements of “dirty work” in what they do, for example, dealing with “alcoholics” (Strong 1980). In my own study, homeless caseworkers perform different tasks, and among them, there are those that are regarded as most disagreeable. Therefore, I focused on the experience of “dirty work,” those aspects of working with the homeless which are defined by shelter staff as objectionable and the ways in which they managed the experience of their “dirty work contexts.” Namely, I paid attention to how such “dirty work challenges” are mitigated, ameliorated, and overcome.

Working with the homeless is often seen as “dirty work” in two respects. First, it is seen as morally dirty, given the nature of the clientele. Caseworkers often worked with down and outers, addicts, those with mental health issues, and sex offenders, and often had to deal with client aggression, verbal abuse, and the unexpected drop-ins from undesirables. Second, the work can be seen as physically dirty, in that workers have hygienic responsibilities to deal with. Caseworkers must assist clients who need help with bathing, duties that involve contact with bodily fluids and other potentially infectious materials, assistance with personal hygiene, handling soiled laundry, CPR, and cleaning toilets, among other tasks. Some caseworkers objected to the “custodial work” that they were required to do as part of working in a homeless shelter. Although they pointed to hygienic issues, they also detested the work of cleaning dirty units, bathrooms, and doing laun-

dry, tasks viewed as illegitimate, and hence, “dirty work.” Many joked about not doing the laundry and spoke about the label of disrespect attached to such work. They should be working as caseworkers, not janitors.

My aim was to follow their lead during the interviews and informal conversations throughout my participant observation research. I wanted to understand the way they defined what they saw as objectionable and rewarding parts of their work. Once they began to speak about “dirty work” or “shit work” (Emerson and Pollner 1976), I sought to understand how they managed these aspects of their jobs on an everyday basis. I found that caseworkers developed strategies of reframing in order to get on with their work and provide legitimacy to their professional identities. When working with homeless sex offenders, for example, caseworkers often referred to their occupational ideologies of remaining “non-judgmental,” a concept I developed as “non-judgmental egalitarianism.” Moreover, I learned about the importance of dirty work through the use of humor.

Once having defined “dirty work,” those in the relevant occupations may reframe the meaning of their work, developing terminological definitions to make the work appear less dirty, concealing its repellant features, sometimes sloughing it off onto members of other occupations. As an example, caseworkers often referred to the homeless as “clients.” The way caseworkers use language to define *What is happening?* holds significance in understanding their definitions of the situation. Defining the homeless as “clients” and “guests” are strategies that caseworkers use to humanize their “clientele” that outsiders tend to dehumanize and bring respectability to their occupations. This serves to dignify human beings who are too often denied that dignity. This

reframing also has consequences for their occupational images, since caseworkers are framing (and thus redefining) their work as noble, valuable, and transformative. It is people who do the work that decide what is clean or dirty, and therefore, attention to their interpretations is cardinal. Van der Geest (2002) recounts an experience he had with Mr. Atia, a night-soil collector in Ghana, during his ethnographic research. Mr. Atia saw his work as “clean work” and was aware that people needed him. He knew his value and “had his price” (van der Geest 2002:203). Despite the “dirty work” involved in certain occupations, workers may show great pride and self-esteem in the work they do. Caseworkers, for instance, underscored the “heroic character” of their work as transformative and, as one informant said, the “emotionally and spiritually fulfilling thing of helping.” Success stories became emblematic symbols of their work.

## Conclusion

My hope in this essay is to reveal how my analytic constructions of “dirty work” were guided and sustained by William Shaffir’s teachings, and to reflect on how I learned to think like a constructionist. I have tried to be as candid as possible about the many influences Dr. Shaffir had in shaping my decisions and thinking in constructing an understanding of “dirty work.” “Social forces,” “social structure,” “theory,” and “society” have often been used to explain social phenomena, rather than being the very phenomena we must explain. In a circular and tautological way, sociologists of the “social,” mainly of the structuralist stripe, employ the taken-for-granted meanings of ready-made, already-assembled analytic concepts, using “the social to explain the social” (Latour 2005:3-5; Schneider 2018:13). Latour (2005) has recently argued that

there are no ready-made groups, only group formations. The aim is to study the making of group life, and thus paying attention to interactions between and among people as they do things together and generate new definitions and realities within these ongoing processes. Groups are assembled and re-assembled, they form and they un-form. Society as conceived *à la* Durkheim does not exist. We do not have a durable, external entity, but rather, following Becker, we have only activities and interactions that make the kind of stuff we (later) define as “society.”

From Dr. Shaffir’s introduction into the world of symbolic interactionism, social constructionism, and ethnomethodology, I have come to recognize the shortcomings of invoking “social forces,” “structure,” or “society” as referents to explain *What is it that is going on?* Instead of this, we should be describing, *in situ*, how members’ understandings of such issues are built out of and play out in social interaction. Concepts like “force” or “field” are difficult to empirically ground. People doing things together is observable in both an empirical and homely sense. For instance, “occupational *social worlds*” resonates more with people than technical uses of “field” or “force.” As Pawluch, Shaffir, and Miall (2005:1) have argued, “any question about society, ‘big’ or ‘small,’ is ultimately about people interacting with each other. Whether the issue is changing gender relationships, corporate deeds or misdeeds, class structures, or the school performance of children from cultural minorities, it all comes down to one thing—people doing things together.” It is through social action, speech, and communication through which we create and express, in Wittgenstein’s (2009) words, predictable “forms of life,” through which we become accustomed, and from which we sort and make meaning of the world around us.

I thus learned how to focus my research on how members construct accounts and frameworks about what they do and thus, what they make of the realities they encounter. William Shaffir, I have come to learn, constantly put knowledge and meaning at the forefront of sociological analysis. His work emphasizes the primacy of the symbolic, through members' subjective understandings and lived experiences. This requires a keen attention to the accomplishment of ethnography, following Becker's (1986) recommendation to study people "doing things together" (see also Plummer 2003).

New orthodoxies in sociology are emerging, however. These put "social inequality," "activism," and "public sociology" to the fore of sociological analysis (Harris 2006; also see Schneider 2018; Adorjan 2019; Nichols 2019; Pawluch 2019). This is, to my mind, very disconcerting. Politicizing sociology as an ideological instrument, and advocating mostly for left wing causes, is contrary to our craft of ethnographic description. If a descriptive science becomes simply ideology or political advocacy, it ceases to be a descriptive science. Recently, especially with quantitative methods, we have witnessed an obsession with the overspecialization and sophistication of research techniques at the expense of content and substance, what Peter L. Berger (2002) dubs "methodological fetishism." I am instantly reminded of Husserl's (1970:6) assertion that "merely fact-minded sciences make merely fact-minded people." The preoccupation with methodological fetishism, for Berger, goes too far. Positivist objectivism abstracts from everything "subjective" that we can reasonably ask if we are even studying human beings anymore. I have learned to strive to be apolitical when it comes to sociological activity.

As Hughes (1971:viii) wrote in his preface to *The Sociological Eye*, "Some say that sociology is a normative science. If they mean that social norms are one of its main objects of study, I agree. If they mean anything else, I do not agree." Hughes sought to bring an "informed, enlightened understanding of the world to those who would listen" (Strauss 1996:274). While some sociologists "theorize out of existence" (Garfinkel 1967:72-73) peoples' activities and interactions, producing them as "cultural or judgmental dopes," I have come to learn to not worry more than participants do. This, however, does not mean that we should avoid developing strong, sensitizing, and resonating concepts and completely eschew theoretical debates (Puddephatt, Shaffir, and Kleinknecht 2009). According to Blumer (1969:168), citing Kant, "perception without conception is blind; conception without perception is empty." As Puddephatt, Shaffir, and Kleinknecht (2009:6) persuasively argue, "some kind of theorizing is germane to all social scientific work, notwithstanding the various ranges of explanation, description, taxonomy, and the level of application intended."

William Shaffir assiduously emphasized the importance of experience, curiosity, and imagination that comes with *entering* and *learning from the field*, as a practical achievement. The important thing is that it is not preconceived methods or theories that should decide the ethnographic problem to be examined, but rather the issues that stimulate our curiosity in the field that should decide the ways we go about exploring them. The sociologist's ambition is to find something that is unexpected, not to generate predictable outcomes through the application of a politically slanted theoretical framework.

Many case studies of "dirty work" have been accomplished and more certainly should be done.

Everett Hughes, however, was also interested in focusing on the commonalities of a broad variety of experiences in order to account for social reality. This means looking for generic processes that cut across a range of empirical cases (Blumer 1969; Chapoulie 1987; Prus 1987; Shaffir and Pawluch 2003; McLuhan and Puddephatt 2019; Puddephatt and McLuhan 2019). Hughes (1970:149-150) reflected on his students who studied janitors, factory workers, furriers, and the like and recognized an emerging comparative frame of reference, where he became convinced:

That if a certain problem turned up in one occupation, it was nearly certain to turn up in all. There is no absolute virtue in studying one kind of work rather than another, if the inward frame of one's mind is comparative. The essence of the comparative frame is that one seeks differences in terms of dimensions common to all the cases. If one becomes over-enamored of a particular occupation, he is likely to describe it in terms which suggest that it is not comparable to others. If he seeks common dimensions, the differences between occupations becomes clearer, and more impressive.

Hughes thus emphasized “that generic themes were common to all work” (Shaffir and Pawluch 2003:895). Physicians, priests, janitors, nurses, firefighters, steel workers, musicians, scientists, recycling workers, night-soil collectors, all of which constitute different occupational universes, are “faced with similar kinds of tasks and problems to which they must adapt and respond either as individuals or collective to continue their work” (Shaffir and Pawluch 2003:895). Yet, mapping these similar kinds of tasks and problems has fallen by the wayside. Hughes insisted on comparisons and encouraged sociologists studying occupations

and professions (and “dirty work”) to develop transcontextual similarities (and difference) in the ongoing processes of work and occupational life. Such an undertaking is certainly worthwhile. As Hughes (1994:61) wrote, “until we can find a point of view and concepts which enable us to make comparisons between the junk peddler and the professor without intent to debunk the one and patronize the other, we cannot do our best work in this field.”

I have learned a great deal from the interactionists and constructionists I have thankfully chanced upon at McMaster University.<sup>1</sup> Dr. William Shaffir in particular, taught me *how to think*, *not what to think*. Dr. Shaffir's approach to education, sociology, and life has been, in short, deeply moving and intellectually stimulating. He is a man of few words and I've come to learn that can be the best quality in an ethnographer; to listen; to accommodate a perspective other than your own. Shaffir's style of humor was affiliative. His style conveyed real mirth and fun, his well-timed jokes a great reliever of tension, all of which embodied and expressed an entire worldview. I count it my good fortune to have studied with William Shaffir.

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<sup>1</sup> Dr. Shaffir is central here, but I would also like to mention Dr. Gregory Hooks and Dr. Dorothy Pawluch as a major positive influence.

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# Active Interview Tactics Revisited: A Multigenerational Perspective

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**Abstract:** William (Billy) Shaffir taught about what it means to be a true empiricist, a sociologist committed to naturalistic observation as the most incisive method in our scientific toolbox. His inspiration still resonates, two decades later, in the work of new emerging scholars with the same commitment to ethnography—or what Billy more modestly and wisely calls “hanging around.” This paper is a tribute to his legacy that highlights the contributions of the next generation of graduate students that the lead author has been privileged to mentor at the University of Guelph. It builds on work by Hathaway and Atkinson on tactics of active interviewing to establish a more nuanced understanding of the benefits and challenges of being recognized as either an “insider” or “outsider,” and the implications of attempting to be both.

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## Prologue

The first author is a former student of William Shaffir's at McMaster University (PhD, 2000) and Robert Stebbins at the University of Calgary (MA, 1995). In their book, *Experiencing Fieldwork* (Shaffir and Stebbins 1990), they observe that sociological field research is typically conducted on cultural phenomena within one's own society. As such, the group or institution being studied is embedded in a network of social relations of which the investigator as observer is an integral part. When people are aware of being observed, they ordinarily strive to make a good impression. Research subjects emphasize one of several selves that they deem appropriate in the observer's presence.

Self-presentation on the part of the observer is no less salient, and fraught with implications for research. Maintaining neutral rationality when emotion is expected reinforces expectations that the observer is just that: only an observer to be treated as such. More penetrating or investigative research methods are needed to gain access to more private spheres of life. Ethnographers are well advised to be especially attentive when people relax their guard (Shaffir 1999). Studying human behavior in natural settings also requires some measure of role-playing and self-presentation that cannot be fully calculated in advance. Self-presentation work evolves throughout the research process.

As a former student of Malcolm Spector, Shaffir recognized that better understanding social phenomena requires uncovering how actors define the situation and interpret their reality. The "bottom line" in ethnographic research is, put otherwise, the pursuit of a clearer and sharper understanding of human lived experience. Respecting the inevitable bound-

aries between research subjects and researchers need not detract from fieldwork. Shaffir's reflections on entering the field and self-presentation in his work on Jewish Orthodox communities highlight the often-unexpected uniqueness of each setting.

The social skills and circumstances of the researcher, regardless, tend to override professional claims about our research and determine the particular fieldwork strategies employed. Shaffir (1990) observes that although many research participants are more than willing to talk about themselves, many are also (understandably) indifferent about taking part in research of little relevance to them. Gaining some level of acceptance is essential, requiring the researcher to present a particular image to be granted access and secure cooperation. The proffered image cannot always be determined in advance, dependent as it is on the adoption of a role that reflects multiple contingencies encountered in the field.

Whereas the roles assumed evolve throughout the research process, the true measure of the value of any given role is the vantage point provided to the participant who plays it. Deception is inherent in the sense that the ethnographer is always more observer than participant, and is especially observant of what happens when the observed let down their guard. Deceit is largely unavoidable, since it is rarely wise or manageable to share all our research interests with the people that we study. But, overt deception is unethical; moreover, it is often difficult to execute in practice.

Rather than attempting to manipulate informants, Shaffir's experiences suggest that fieldworkers try to be as up-front as possible about their research interests. He notes that both our research aims and self-presentation strategies are shaped by the par-

ticipants with whom we interact. The roles we play are influenced as much at times by our own personal commitments and considerations as they are by academic interests and concerns. Attempting to display a particular image that the researcher interprets will be received favorably requires projecting personal and academic interests.

Formal introductions of credentials and objectives tend to matter less than explanations that group members develop to account for the appearance of a stranger who is eager to observe and understand their way of life. The role and status of the researcher are not so much assumed as assigned by others in a manner that reflects their own understanding of the outsider's presence. Academic credentials are ordinarily outweighed by personal(ity) traits conducive to being granted access and cooperation of the group. Downplaying academic status and vocabulary is also recommended to facilitate exchanges that ideally take the form of a casual conversation.

Pretending to know less about a conversation topic is a deceptive practice that falls short of outright lying and is as commonplace in research as it is in daily life. If our goal is to develop a better understanding of human lived experience, it stands to reason that the sharpest tools at our disposal are those linked to sociability and the ways we are perceived as ordinary human beings. In Shaffir's (1998:48) words: "The extent to which we are seen as likeable, friendly, dependable, and honest bears directly on our ability to collect rich and deep data with which to better understand and analyze the social world under study."

On having gained admission to a social circle or set of activities, convincing and persuasive self-presentation tactics are at the heart of the art of field research throughout one's time remaining in the

field. The research subject's understanding of the objectives of the study is more of a response to the researcher's human qualities, or how we are perceived, than to its scientific merits. Success in fieldwork is primarily determined by the performance of interactional skills that set the stage for the development and nurturing of relationships. Field research is accordingly more art or craft than science, being learned experientially rather than by formal training on research protocols for executing standardized procedures.

Pursuing intimate familiarity in fieldwork calls for more explicit recognition by researchers that we occupy several statuses simultaneously. The researcher role need not always predominate. Shaffir (1998) observes that some of his best insights came to light only after telling people more about himself. More important than obsessing about scientific methods is the need for sociability in attending to the human demands that shape relationships, and being open to exchanges that allow others to become familiar with our non-academic selves. Eschewing the obsession with collecting the "right data" in favor of more natural conversational dynamics frees us to inject our views and challenge those of others in ways that make for interesting and lively exchanges.

Acquiring the most credible and deep appreciation of human lived reality through social interaction requires that we immerse ourselves in the social worlds of others. In so doing, field researchers seek to cultivate relationships that grant us access to data we may not otherwise be privy to. Yet, despite the benefits of instrumental membership, we can never be true insiders. In the spirit of having more open and honest discussions about practices of ethnographic research, Shaffir's personal reflections of experiences in fieldwork illustrate the boundaries to

full participation that prevent us from abandoning our status as observers.

## Introduction

Effective and strategic self-presentation is essential for establishing relationships that enable us to navigate the advantages and disadvantages of pursuing more or less attached ethnographic points of view. The foregoing insights on ethnography are equally germane to interviewing, which is observed to have become the primary source of data in the social sciences (cf. Briggs 1986; Hammersley 2003; Atkinson and Delamont 2006). There is arguably a tendency to overly romanticize the insights gained from interviews as being more “authentic” than data gathered using other methods (Atkinson and Silverman 1997). Other critics argue that there has been too much emphasis on designing protocols with overly prescriptive schedules to ensure that interviews are “well-conducted” (Holstein and Gubrium 2016; Silverman 2017).

The resulting call for more creative use of interviews is by no means new (cf. Carey 1972; Miller and Tewksbury 2001). The literature on active interviewing is explicitly concerned with documenting and reflecting on new strategies for producing more revealing data. Active interviewing departs from standardized approaches by treating the participants as meaning co-creators (Holstein and Gubrium 1995). Self-presentations shape the narrative by (re)positioning respondents in relation to each other as the interview unfolds. In anticipation and reaction to the other, participants employ conducive narrative resources.

Interviewers draw on background knowledge and shared experience to build rapport with interviewees, as well as to establish and interpret themes emerging in the data. The aim is not to coax pre-

ferred responses, but instead facilitate an interactive dialogue that more closely resembles everyday conversation than a formal interview. The interviewer may introduce competing narratives in order to elicit a response that undermines or deviates from the interviewee’s previous account. The ensuing interaction between interview participants has been characterized as an unscripted interpersonal drama that neither of the actors can prepare for in advance (Holstein and Gubrium 1995).

From a constructionist perspective, the interview is understood as a social interaction or occasion where the dialogue is a social product of negotiation (Briggs 1986; Hammersley 2003). Interviews are forums for claims making (Spector 1980) that facilitate unstructured, open-ended talk as a performative event (Holstein and Gubrium 1995). Viewing interview exchanges as performances means bringing “heterogeneous stylistic resources, context-sensitive meanings, and conflicting ideologies into the reflexive arena where they can be examined critically” (Bauman and Briggs 1990:60).

Conducting active interviews provides an opportunity to better understand and draw upon the contextually embedded discourses or “social poetics” (Clifford and Marcus 1986) of the situation or subject under study. The “strange” and the “familiar” are subjected to greater scrutiny due to the cultural intimacy invoked by interview participants interacting from positions of commonality and difference. The quality of ethnographic research is often measured by the ethnographer’s ability to gain access to the “backstage” (Goffman 1959) of the social practice or setting being studied.

It has been noted that informants often make a conscious effort to conceal the back regions researchers

seek to access (Berreman 1962). In this vein, conflict methodologists go further in asserting that research participants are deliberately deceptive (cf. Lundman and McFarlane 1974; Christie 1976; Young 1976; Adler, Adler, and Rochford 1986). More in line with Becker's (1970) observations on the matter, the primary concern of the active interviewer is striving not to be too overly accommodating of the accounts of interview participants (see also Douglas 1985).

### **Exploring the Continuum of Tactical Engagement**

Active interviewers seek to critically examine interviewee's narratives while attempting to uncover perspectives that have not yet been disclosed. Hathaway and Atkinson (2003; 2005) envisioned a continuum of tactical engagement that can be drawn upon when doing qualitative interviews. Rapport is needed to gain trust and issue challenges to stimulate deeper narrative accounts. Invoking the personas of the "good cop" and the "bad cop," the initial stages of the interview are characterized as social lubricants to foster a greater exchange of information (see also Weiss 1994; Dewalt and Dewalt 1998).

In the early going, the interviewer is advised to use familiar terms based on prior knowledge and experience to establish trust that sets the stage for more aggressive and challenging lines of inquiry. By building on more passive, neutral styles of interviewing, a fuller range of tactics can be mobilized to include more pressing, provocative conversational strategies. Becker (1954) noted long ago that seeming to be skeptical or "playing dumb" about facts that were taken-for-granted can be used to elicit more candid responses (see also Hermanowicz 2002; McLuhan, this volume).

Whereas transforming into "bad cop" may not be advisable or feasible, less confrontational tactics like Becker's are indispensable for provoking fuller or alternative accounts. Returning to revisit and re-evaluate prior claims is another strategy he commonly employed. At every level of engagement there are both well-known and unknown risks that may inhibit the flow of interaction and the exchange of information. Impression management inevitably is part of the performance and contingent on the participants' presentation of self. The research literature on active interviewing is evolving, due to a need for more reflection on the intersecting ways that reflexivity, power dynamics, and positionality converge to shape the narratives produced.

### **Unpacking Power, Reflexivity, and Positionality**

A concern for reflexivity emerged from feminist critiques of the neglect of power dynamics operating during interviews (cf. England 1994; Mauthner and Doucet 2003; Day 2012). Interviewers and interviewees are necessarily reflexive. The influence of power and reflexivity cannot only be considered retrospectively, but also during "real time" (Weick 2002) or "in the moment" (Riach 2009) during interviews. That requires awareness of the dominant discourses that shape the narratives produced, and openness to viewing interview performances from multiple perspectives. The narratives produced are shaped by the positionalities of interview participants within their social circumstances. The interviewer must, accordingly, strategically adapt in view of shared experiences and social differences that influence the active interview performance. The unfolding dialogue is not only a reflection, but also (re)produces the existing power relations (Al  x and Hammarstr  m 2008).

The power-resistance dynamic is conceptualized as being somewhat fluid because participants might occupy positions of dominance and inferiority at different times. The narratives produced are open to interpretation through a myriad of shifting power positions that are adopted, imposed, and resisted by the interview participants. Positionalities reflect one's socio-economic status, occupation, education, gender, age, ethnicity, among other intersecting social characteristics. These positionalities shape interview exchanges (see also Song and Parker 1995); and the ways they intersect are often shifting and unstable. It is possible to occupy shifting positionalities by acting more or less attached at different junctures of the interview. At the same time, conversation and disclosure can be disrupted by participants adopting "wrong" positions (Al  x and Hammarstr  m 2008). These same "sticky moments" (Riach 2009) demonstrating lack-of-fit present new opportunities to study reflexivity.

The "insider" versus "outsider" dichotomy oversimplifies the complexity of social interaction during interviews. Interviewers, like ethnographers, are never fully either insiders or outsiders in relation to the other. The unfolding interpersonal drama is unpredictable. The plot and role of positionalities, and resulting opportunities, cannot be entirely anticipated in advance. The reflexive nature of active interviewing introduces inconsistencies in how participants interpret commonality and difference. Self-disclosure, for example, has both advantages and risks. As a strategy for making interviewees feel safer (see Song and Parker 1995)—and eliciting candor about sensitive matters—it can also make participants more hesitant or guarded, when it serves to highlight social differences instead. At the same time, sometimes acting more detached from interviewees can lead to fuller disclosure (Abell et al. 2006).

The present paper builds on observations by Shaffir (1990; 1998; 1999) and literature on active interviewing to establish a more nuanced understanding of the benefits and challenges of being recognized as either an insider or outsider and the implications of attempting to be both. We draw on illustrations from the interviews conducted by the two coauthors for their doctoral research projects.

## The Studies

Mostaghim (2019) conducted interviews with undergraduate students at the University of Guelph as part of the lead author's three-campus study of experiences and attitudes towards the use of cannabis (see: Hathaway et al. 2016; 2018). The interviewer's characteristics and positionality—as an Iranian male who used cannabis, and was in his late 20s at the time of the study—were noted to both hinder and facilitate responses, requiring flexibility in his use of probing tactics. Student attitudes and experiences, as users and non-users, were found to vary widely by ethnicity and gender.

The patterned variation of responses often followed exchanges in which interviewee's statements had alluded to common traits or differences with the interviewer. Use of the term "you know" (or "you don't know"), in particular, featured in exchanges in which responses served as cues that the interviewer had been positioned in a way that reinforced his "insider" or "outsider" status. Male students from the Middle East and Southeast Asia, for example, often used the term "you know" to indicate familiarity with common cultural understandings about the use of drugs.

## What to Do with "You Know" / "You Don't Know"

Being granted status as a cultural insider sometimes helps and sometimes hinders. It proved useful in

some interviews, to overcome self-censoring or a reluctance to elaborate, for the interviewer to share his own experiences as someone who was raised in the Iranian community. Anti-western views in some ethnic communities are aligned with stereotypes of marijuana use as a symbol of overly westernized youth. The interviewer's status as a visible minority facilitated probing into these issues by establishing rapport.

Interviewees seemed more open to discussing ethnic stereotypes with a cultural insider than they might otherwise have been. Shared understandings granted access to more sensitive subject matter, such as parents' attitudes towards the use of drugs. Ethnic students seemed more open to discussing their experiences following exchanges in which the interviewer was able to assure them that he understood their parents were not "stereotypically conservative," but merely culturally conventional in upholding rigid standards that prohibit marijuana use.

Insider status hindered interaction in some interviews, at times, when the assumed familiarity with ethnic attitudes impeded deeper probing of "brown cultures." When asked if he would ever date a marijuana user, a male student from India, for example, laughed and stated: "You know how it is. You are brown yourself." Adherence to traditional gender roles, which stigmatize marijuana use by females relatively severely, thereby went unspoken. As something "everybody knows" (if they are cultural insiders), such knowledge is typically taken-for-granted (see: Garfinkel 1984).

In such instances, the strategy of "playing dumb" was called on to facilitate vocalization of cultural narratives impeded by "shared understandings." The following exchange is an example illustrating the need to feign naivety at times to prompt explicit

it conversation about norms in ethnic families and communities. Another interviewee stated:

My parents aren't controlling, they just want what they think is right—you know what I mean?

[I am not sure. Can you elaborate?]

It's hard; like white people think that my parents are conservative and are like, you know, stereotypical brown parents. But, they are not; they just have their ways, you know? Like, you know how it is...they have their own point of view and they want me to respect that, unless I can convince them otherwise.

Expressing skepticism is another tactic that proved useful during interviews. When asked why they do not use marijuana, for example, some students remarked that "you know, it's not a brown thing" or "it's more of a white thing" to do. When challenged, by referring to the fact that some "brown people" do use marijuana, one Indian student clarified his statement by asserting that "it is the coconuts who smoke weed." Put otherwise, it is a sign of westernized behavior, by someone who appears brown but is white on the inside.

More generally, a non-judgmental tone proved most effective for navigating challenges and opportunities provided by the interviewer's inside and/or outside status. Overt differences observed in the experiences and attitudes of female interviewees cued a need for different tactics to probe beyond assertions like "you wouldn't understand" or "you don't know how it is." During interviews with non-users, whether male or female, they often seemed defensive for choosing not to use when so many of their peers are marijuana users.

Non-users sometimes prefaced their remarks by stating that they have no objection to using marijuana, or



that views of users are more important to the study. Such responses called for reassurances that their experiences and attitudes were equally important for the research, and that other interviewees had shared similar opinions. At times of hesitation, for apparent fear of judgment, assuring interview participants that their attitudes were not unimportant or unusual gave them “permission” to more fully share their views.

In interviews with students who were marijuana users, self-disclosure by the interviewer was another form of sharing that proved useful to establish trust, alleviating fear of judgment. Some cues that prompted self-disclosure of the interviewer’s status as a fellow user included the prefacing of statements with “I am not a pot-head” and/or “I am a good student.” One must be cautious not to “over-share” or overshadow the interviewee’s narrative by imposing one suggested by the interviewer. However, self-disclosure is a tactic that can help facilitate a non-judgmental tone.

Claiming status as a (sub)cultural insider, or having it ascribed to us as interviewers might also lead to “sticky” situations during interviews that can be both risky and revealing. Engaging with white students about ethnicity was often difficult, evoking “you know” statements that required a non-judgmental tone that gave them permission to be candid. One white student was asked, for example: “Why do you think brown people are less likely to be users?” He replied: “Well, you know, how it is man. Like they care about image and stuff a lot more.” [Can you tell me more?] “You know, man. Like I am not sure about you, but, you know, most of my brown friends are really into what their family thinks of them and stuff.”

Another white student responded that Asian parents are less likely to allow youth to have fun. “You

must know what I’m talking about,” he continued, “because you are brown as well.” When challenged with a skeptical reply, the interviewee assured the interviewer that he “wasn’t being racist” and asserted he would rather not discuss the matter further. Sensitivities surrounding race and gender thereby offer opportunities for probing based on presumed positionality; but interviewers must be cautious. Giving interview participants permission to be candid can, at times, seem threatening and disrupt the interview by counteracting efforts to establish rapport. To demonstrate the broader relevance of the foregoing observations during interviews with students, we turn to illustrations from a different kind of study. The experiences of the interviewer in our second study offer further insights into the benefits and challenges of being identified as an “insider” or “outsider” and what one stands to gain by seeking to be both.

### Interviewing City Planners and Officials

Sommers’ (2016) study of municipal law enforcement practices in Hamilton, Ontario involved ethnographic observation (600+ hours of “ride-alongs”) with officers and twenty interviews with city planners and officials. The focus of the fieldwork was examining the practice of municipal law enforcement in the context of official priorities which called for a proactive, “zero-tolerance” approach. In practice, it was found that law enforcement was primarily reactive, not proactive, and concerned with management of conflicts arising between neighbors and assuring public safety—rather than addressing signs of physical decay and social disorder in the downtown core.

Conducting interviews with city planners and officials provided opportunities to better understand

the observed gap between municipal policy and practice on the matter of bylaw enforcement. The interviewer's fieldwork afforded him some inside status as an informed observer, which proved useful when discussing politically sensitive matters. Attempts to dodge contentious issues like displacing homeless people from illegal dwellings, for example, included common references by interviewees to the "City's official position."

Moving past stock phrases that signified a "closing off" (Keating 1993) of critical inquiry was achieved by drawing on experiences in fieldwork, such as when it was observed that the displacement/relocation was due to safety issues that were in the interests of all parties to resolve. The interviewer's sharing of experiences thereby gave the interviewee the permission to speak more openly and candidly about difficult decisions and "grey areas" of law.

Interviewing city planners, whose work was more removed from the policy and practice of municipal law enforcement, also sometimes called for tactics in which insights gained from fieldwork proved useful in establishing rapport. Eliciting more candor in some cases was achieved by directly challenging responses that appeared to be rehearsed. For example:

[Could you describe what is meant by a "get tough approach" to municipal law enforcement in the City?]  
Our policy documents are quite clear...our get tough or zero-tolerance approach, whatever you want to call it...people throughout the City need to get the message...it's about improving the City's image.  
[Having spent some time observing the enforcement of municipal law, it seems officers use a great deal of discretion...In fact, somewhat contrary to a "get tough approach" officers seem very willing to work

with residents—especially in lower income areas of the City.]

Well, perhaps some of what has been written in City documents comes off as a little more aggressive than what unfolds on the frontlines...While I support what is outlined in our planning documents, I guess it's somewhat unspoken that officers use discretion, especially when dealing with vulnerable populations.

There are evidently limits to adopting more "aggressive" strategies relying on experiential knowledge. To illustrate, the following exchange is an example wherein the interviewee (a municipal law official) rejects the interviewer's inside knowledge as inadequate for understanding the bigger picture in which certain "unofficial" enforcement practices occur. The attempt to draw on insights gained from the fieldwork in this interview resulted in denial and abruptly closing off the line of inquiry:

[During my time in the field, officers discussed how they were encouraged to ticket and add fees for services when attending a certain downtown hotel. It was suggested to me on several occasions that these added fees would pressure the owners to sell the property.]

You mean Motor City?

[Yes, Motor City.]

Well, I'm not sure why any officers would tell you that.

[It wasn't just officers. I've also attended several public meetings that discussed how a zero-tolerance approach was being used on that specific property.]

I know you've been at this [research] for months, but what's happened recently at Motor City is only part of the story...I suggest you look into the decade of problems there...

[I understand...when exactly did the problems start at Motor City?]

I think I've said all I want to say...I'd like to move on.

At other times, for different reasons, the interviewer's fieldwork evidently hindered the exchange of information during interviews. Responses prefaced with common phrases like "as you know," "as you saw with the officers," and "as you experienced" were often preemptive of more detailed answers. Being granted status as an insider presented distinctive challenges when trying to adopt a more detached or neutral style of questioning in interviews.

Once insider status is conferred on interviewers, transitioning to the use of other tactics can be difficult. This is illustrated in the following exchange which required the interviewer, after several attempts to adopt an "uninformed" view, to assume the stance of an insider:

[What makes regulating the taxi industry in Hamilton so challenging?]

I think you know the answer...you've seen it firsthand, haven't you?

[I am wondering if you could discuss the challenges from more of a planning/licensing standpoint?]

Well...I think the guys enforcing on taxis probably have a more accurate idea of what's wrong with the industry.

[That's fair. However, I am wondering if this is something that the City could address through initiatives that start in the planning department?]

I am hesitant to go any further because I am not sure how much our official policies are helping the guys [officers] dealing with taxis...you know the laws; you know what happens out there.

[Okay then. My time in the field would suggest that only a limited amount of officers are dedicated to taxi enforcement. Can we talk about resources and funding more officers?]

At other times, downplaying inside knowledge ("playing dumb") to stimulate disclosure was suc-

cessful. The complexity and scope and high number of ongoing investigations observed during fieldwork made downplaying these understandings seem honest and credible. This tactic proved to be particularly useful in interviews with city planners.

In the following example, playing dumb resulted in disclosure that exemplified the gap between "priorities" and practice with regard to the commitment to democratizing access by holding public meetings to identify local needs. The interviewer asked if issues raised at public meetings were likely to be given equal consideration. The city planner replied:

You know how it goes in these meetings. It's probably no different for the bylaw guys. We [the planning department] have to prioritize what's important and what is not.

[My study focused primarily on the enforcement side of things.]

There is only a handful of people who show up to these meetings and often what they see as a problem in their neighborhood has little to do with our long-term vision.

[So why does the City bother having these meetings if the people who show up don't really represent the whole neighborhood?]

In some cases these meetings do help us identify issues, but in others we [the City] really don't get much from them.

[So what you're saying is some public consultations have little impact on funding?]

You got it. But, we [the City] can say that we did have a consult.

In sum, these illustrations further demonstrate the various opportunities and challenges of having inside knowledge. In preparing for the study, the interviewer's research training might contribute

to developing awareness of potential outcomes of adopting a particular persona or self-presentation strategy. It must also be acknowledged that the fuller implications of being cast as either an insider or outsider (or both) cannot be fully anticipated in advance.

Elastic reflexivity in positionality has evident advantages, prompting critical exchanges that have the potential to result in richer data (see also Moss 1995; Herod 1999; Rice 2010). Transitioning can also hinder exchanges moving forward and inhibit opportunities for further repositioning, once the interviewer is identified as having either an insider's or outsider's status.

## Discussion

This paper builds on observations by Shaffir on self-presentation in ethnographic fieldwork. As a doctoral student of Shaffir's, the lead author's approach to interviewing was informed by this and earlier work by Malcolm Spector on researching public figures. The focus of the present work builds on these contributions further by providing illustrations from the research of two recent PhDs at the University of Guelph. The collaboration demonstrates the value of pursuing a multi-generational perspective on research methods that draws on the experiences of established and emerging scholars. The result is a more nuanced and developed understanding of the advantages and challenges associated with adopting a reflexive approach to interviewing.

Arguably, all qualitative methods share a similar commitment to employing naturalistic observation. But, active interviewing treats participants explicitly as meaning co-creators to produce insightful data. Interview participants shape the emerging

narrative by (re)positioning and being (re)positioned in relation to the other throughout the interview. Sharing background knowledge and experience are narrative resources employed by interviewers to establish a rapport that is conducive to developing an interactive dialogue resembling the back and forth of open-ended talk.

Conducting active interviews provides an opportunity to better understand both the "strange" and the "familiar" through interaction premised on a natural conversation. Social interaction between interview participants is enacted from positions of sameness and difference reflecting relative "insider" and "outsider" points of view. Regardless of positioning, as the interview unfolds, the primary concern of the active interviewer is striving not to be overly accommodating in seeking information that has yet to be disclosed.

Pressing interviewees to be candid, hoping that they shed light on the "backstage" of performances, is facilitated by a variety of tactics that range from simply "playing dumb" to more challenging and confrontational interview techniques. The active interviewer must be flexible, adaptable, and be prepared to weigh rewards and costs of each approach. From overly aggressive to too accommodating, every strategy has risks that may counteract its benefit and ultimately inhibit the interactive flow.

The literature on active interviewing has been further developed with due reference to the research contributions of scholars emphasizing power dynamics, reflexivity, and positionality in qualitative methods (cf. Song and Parker 1995; Abel et al. 2006; Aléx and Hammarström 2008; Riach 2009). Interview participants are understood to be reciprocally reflexive "in the moment" in a way that

reflects positionality and dominant discourses that ultimately influence the narratives produced. An adaptive orientation to the task of interviewing necessitates awareness of positioning as fluid, reflecting power relations, social differences, and sameness in ways that manifest in different ways at different times.

Positionalities reflect different social characteristics that shape the form and content of interview exchanges and intersect in ways that are often unpredictable. Shifting positionalities can stimulate disclosure by cultivating commonalities or creating “sticky moments.” Exercising reflexivity in interviews can also disrupt the flow and counteract attempts to build rapport. There are benefits to being recognized as an insider and as an outsider (or an “alien observer” whose ignorance invites more detailed talk).

Attempting to wear both hats can be challenging and risky, but might allow the interviewer to have some “cake and eat it too.” Research contributions to the literature on active interviewing indicate that dichotomizing the potential role of the insider and outsider oversimplifies the intricate complexity of performing reflexivity and positionality on the part of interviewers. To offer a more nuanced view, we draw on illustrations of active interviewing in two very different studies.

Mostaghim’s interviews with students about marijuana use provided opportunities to explore the influence of being both positioned and positioning oneself as an interviewer based on ethnicity and gender, among other characteristics. Identifying as a user and being identified in interviews as an Iranian male graduate student proved advantageous in some situations where these social characteris-

tics demonstrated commonality as a (sub)cultural insider. Use of the term “you know” served as a cue for shifting tactics or positionality to a more detached position. Maintaining a non-judgmental tone throughout the interviews appeared to be the common thread facilitating the use of a variety of probes.

Sommers’ interviews with city planners and officials benefitted from the fieldwork he conducted in advance. It proved useful in establishing rapport as an insider to probe beyond the “closing off” attempts that he encountered when discussing politically sensitive matters. Showing empathy, as one who knows and understands, or resisting “canned” and sanitized responses were helpful tactics using inside knowledge based on fieldwork experiences to elicit greater candor during interviews. Resisting “you know” type responses required downplaying inside knowledge, and it was at times rejected indicating some resistance by interviewees seeking to (re)assert their authority.

Taken together, these case studies demonstrate the value of adopting a more “active” approach to interviewing. This paper draws attention to the intimate connections between ethnographic fieldwork and other qualitative methods that draw on insights gained from naturalistic observation and being identified as either an insider or outsider. Exercising reflexivity and positionality to foster more insightful conversational dynamics, or resume more fruitful avenues of dialogue, requires a working understanding of associated risks and helpful strategies to overcome them. Building on the academic lineage and legacy of Shaffir’s outstanding ethnographic contributions illuminates the crucial role of mentorship and highlights the enduring returns gained by “paying it forward.”

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