Ethnographic Gameness: Theorizing Extra-methodological Fieldwork Practices in a Study of Outlaw Motorcycle Clubs

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Abstract
This article theorizes the fieldwork experiences that I gained while studying outlaw biker subculture. Drawing on Bourdieu’s practice theory and Goffman’s dramaturgical interactionism, I argue that ethnography in practice is pre-disposed by the ethnographer’s primary habitus, which shapes symbolic interaction. To substantiate this claim, I disclose my own upbringing in a troubled working-class family and my personal ties with outlaw bikers, both prior to and beyond my research. This article then illustrates how my habitus helped me to compensate for the vagueness of ethnography in theory with regard to three recurrent issues of fieldwork, which are the practices of (1) approaching/entering the field, (2) negotiating participation, and (3) managing (un)fortunate circumstances. After reflecting on my cleft habitus as the buddy and/or researcher in ethnographic practice, this article concludes with the metaphor of gameness. This concept, which is borrowed from early prize fighting, is used to outline and label the ideal-type of the ethnographer who is well-suited for the immersion into deviant, criminalized, or otherwise culturally elitist fields.

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Keywords
methodology of ethnography, outlaw motorcycle clubs, fieldwork practice, practice theory, interaction theory

Introduction
This article summarizes my practical experiences of more than a decade of field research on outlaw motorcycle clubs (OMCs). It also describes my general disillusionment with the usefulness of the methodology literature. Theoretical reflections on my fieldwork practices have led to the deliberately provocative conclusion that my ethnography almost exclusively depended on my specific human agency (habitus), which includes a distinctively socialized body and the formative past experiences that are independent of academic training in methodology. My overall claim is that these extra-methodological aspects of fieldwork significantly impact on the conduct of ethnographic research. To substantiate this argument, I will use quasi-autoethnographic data¹ to interpret the fieldworker–field relationship, which is constitutive of any ethnography. This article’s major contribution is to provide a heuristic to theorize and discuss the researcher’s corporeality and social dispositions, which account for explicable but as yet widely unexplained variance in the practice of ethnographic research.

The (Extra-)methodological in Ethnographic Practice
My entrenchment with the biker subculture dates back to my private life during the late-1990s, long before I started a series of interviews for my diploma thesis about the organizational culture of OMCs in 2006.² However, the data collection would not end there. In all its peculiarities and extremes, this field of research has affected me deeply, both personally and as a scholar of the sociology of organizations. Furthermore, the unplanned rapport with some of my informants provided me with additional and better opportunities for participation in the field.

As a Master’s student and novice ethnographer, I was unsure about the feasibility of this research project and how to go about it. However, my inexperience as a fieldworker and limited methodological understanding did not keep me from just getting started and trusting the process. As I became more familiar with the literature, my self-doubts about doing ethnography gave way to being more at ease with the methodological instructions and ethical discussions encountered in talking (about doing) ethnography. Although I understood that methodology can offer only a
vague orientation, I became increasingly annoyed when I realized how limited most of the ethnographic guidelines were when it came to providing me with practical help in situated fieldwork. Research-in-use is almost always more consuming, chaotic, and conflicted than research-in-theory (Miles and Huberman 1994, 309). I was reminded of Bourdieu’s critique of scholastic fallacy, which not only criticizes the theorizing of practical logics but can also be applied to methodology as the logics of practical research (Bourdieu 2000, 9–92).

Although there have been many attempts at consolidation since the 1970s, we still face a “carnivalesque variety of approaches” to ethnography (Coffey, Holbrook, and Atkinson 1996, 1): How many and what (sub)genres of ethnography are there? How can we categorize and evaluate all of these approaches (Eisewicht and Kirschner 2015, 658–62)? What is and is not ethnography? How can we become “good” ethnographers? Perhaps we have to accept that ethnography is simply everything that so-called ethnographers professionally claim and are believed to do. There are as many ways of doing ethnography as there are (self-)deceptions concerning how it is actually done (Fine 1993). Furthermore, ethnographies in action are always messy. Van Maanen (2011, 219) may be right in arguing that you only become an ethnographer by subjecting yourself to all of the trials and tribulations of just doing it. To the degree that ethnography is as invariably methodologically indeterminate, it is practically pre-determined by the researcher as the primary research instrument. It is the fieldworker who fills the gaps left by the need to interpret methodology and ethical guidelines in situated interaction. Consequently, methodological reasoning about the practice of ethnography must focus on theorizing the agency of the practicing ethnographer in the field (Agar 1980, 44; Lipson 1991, 80–82).

Several accounts in the literature allude to the bold and discriminatory claim that not every ethnographer is well-suited for every kind of ethnography (e.g., Harrington 2003, 606–13 and 620–21). However, which ethnographer is qualified for what kind of ethnography? To address this question, I consider the ethnographer in terms of a socialized body prior to, beyond, and (relatively) independent of academia. This includes the bodily characteristics, personality traits, and social skills of the researcher that cannot be acquired through academic training. The latter are critical in the practice of fieldwork but have been widely underexplored and undertheorized in the methodology literature.

Judging from my research experience, the importance of these extra-methodological dispositions of the ethnographer is generally contingent on the following determinants, which will be further elaborated throughout this article.
1. *The constitution of the field of study:* To what extent is the social life-world that will be explored culturally alien or dismissive towards academics and their methodology?

2. *Incomplete or vague research guidelines:* What are the circumstances and situations encountered in (improvised) fieldwork for which the methodology literature and ethical guidelines fail to provide sufficiently practicable and/or unambiguous instructions?

3. *Chance:* What are the (un)favorable coincidences in fieldwork that cannot be controlled by the research design or easily accommodated by opportunistic methodological adjustments?

If ethnographers choose to operate in deviant, criminogenic, or otherwise culturally segregated settings, then success or failure in fieldwork is arguably particularly dependent on the fit between the researcher’s proto-sociological dispositions with the structuration of the field. The ethnographer’s inherent sociopsychological capacities or “character” (Goffman 1967, 214–37) are more salient when the situations that are encountered in the fieldwork are exceptional, chaotic, risky, consequential, or fateful. These necessary extraproperties on the side of the researcher can hardly be created by academic education and they will be summarized metaphorically with “ethnographic gameness” at the end of this article. For the purposes of the following argument, *successful* fieldwork is defined as the quantity and quality of access to the most valid and rich information (Harrington 2003, 599). This would ideally consist of preferably unbiased or unfiltered conversations with all of the participants in the field, full access to all documents, and unrestricted opportunities for long-term co-participation and observation at any events of interest (Snow, Benford, and Anderson 1986, 379).

To further elaborate on the extra-methodological aspect of my ethnographic practice, I will first describe the German OMC subculture and provide the necessary information about my own social trajectory to then reconstitute my fieldwork practices in relation to the field of study.

**The Constitution of the Field: The German Outlaw Biker Subculture**

The German biker subculture has a rich and diverse history. It may be traced back to the Halbstarken-movement of the 1950s, when groups of lower working-class teenagers started disturbing the peace in West-Germany’s urban public. As elsewhere in the world, today’s spectrum of German motorcycle clubs (MCs) ranges from law-abiding bikers with only recreational interests in “biking and brotherhood” to the hardcore faction of the internationally
operating “one percenters” (abbreviated as 1%er; Kuldova 2019, 3–10; Quinn 2017). The latter were of primary concern for my research and can only be introduced in a grossly simplified manner.

The 1%er clubs are exclusively male associations that are well-known for their propensity to readily engage in self-righteous competition over territorial dominance, respect, and honor (Kuldova 2019, 185–90). Even small insults and provocations may escalate into outbursts of extreme brutality, which can lead to protracted enmities between the clubs and their adversaries. When asked to define a 1%er, a veteran club member concluded that: “I think this really boils down to taking a stand. Always ready to get the hell beaten out of you [for the brotherhood].” Since the repeated fatal clashes between members of the Hells Angels and the Bandidos MCs that started the so-called German Biker War in 2007, outlaw bikers have taken center stage in the media. They are commonly portrayed as hierarchically organized entities that are capable of operating in paramilitary fashion. Police authorities stigmatize most German 1%er clubs as “Outlaw Motorcycle Gangs” (OMCGs; BKA 2019) whose members are linked to violent crime, such as gang warfare, extortion, as well as the sex, drugs, and gun trades. However, the traditional objective or self-concept of outlaw bikers is to stand together and regain sovereignty (Kuldova 2019, 61–115). They despise undisputed obedience to state authorities or other moral entrepreneurs. Conversely, they pride themselves on rejecting any outside help or interference with their affairs, and they resort to vigilante justice if need be.

To preserve their deviant subcultural lifestyle and group cohesiveness, outlaw bikers rigidly regulate entry to their clubs by means of an extended trial membership. In rites of passage, the prospective member (“prospect”) will be checked for his sociomoral fit with his club peers and his unmitigated readiness to self-sacrifice for the collective. As one club president who runs a rather tight regime told me: “He is only credited with his very basic human rights, that’s about it!” Prospects are constantly assigned strenuous tasks. They may also feel tempted to commit criminal offences to validate their dedication to the outlaw biker ethics, and thereby gain the trust and loyalty of their club fellows (Kuldova 2019, 85–86). Eventually, the most crucial condition for participation in OMCs is the prospect’s willingness to jointly support and defend the biker brotherhood with all means necessary. Only after being unanimously voted in as a “full patch” are new members allowed to display their affiliation with club-sanctioned regalia (members-only apparel and tattoos), attend all club meetings, and have a say and voting rights in club affairs.

There are no statistics for the sociodemographics of German OMCs. According to many informants and my own estimation, they are primarily composed of people from the hedonist submilieus of the underprivileged
popular classes (Vester 2003, 50–52). These milieus nurture an underdog mentality, which individuals typically express by adopting an anticonformist attitude, distancing themselves from authorities, and striving for individual autonomy. In recent years, OMCs with aging members of predominantly German nationality have started to accept and/or recruit second-generation immigrants from deprived urban areas. The objective is to boost their ranks and reinvigorate their combativeness—thereby, they also culturally updated the attractiveness of their deviant lifestyle for a younger demography.

A very strict compliance with the code of silence ensures that OMCs are rarely successfully prosecuted by the police. Most contemporary 1%er clubs only communicate via designated “press officers” or lawyers. The rigid control of access and information also serves as an effective means of impression management for this ensemble of outsiders, who skillfully exploit their stigma for social effectiveness—they are to be respected! Thanks to strict audience segregation (Goffman 1959, 49 and 137–40), they are able to prevent demystifying insights into the backstage of what goes on within the clubs. OMCs are concerned with concealing internal disputes and incidents that might negate their public image of strength through brotherly unity and equality. Finally, the exclusive insider status of the full member intensifies the group solidarity of a self-proclaimed social elite.

For all of these reasons, non-affiliated outsiders are generally denied access and insider knowledge (Quinn 2017, 107). Moreover, the outlaw bikers’ social practices and moralities are likely to deter anyone alien to these cultural identities.

**Extra-methodological Researcher Dispositions**

Opportunities to gain access and insight depend on the quality of the social relationships between the ethnographer and the people in the field. In the literature, this relationship is predominantly conceptualized with recourse to sociopsychological theories of interaction and/or identity (Gengler and Ezzell 2017; Harrington 2003; Lipson 1991). Metaphorically speaking, this theorizing of fieldwork caricatures a dramaturgical approach of “we can all play ethnographer.” We simply have to be knowledgeable about the huge repertoire of methodology that provides us with countless “stage directions” for appropriate “role play” to help us gain access to fields and maximize our informational yield (e.g., Snow, Benford, and Anderson 1986). Even the chief dramaturg of all social theorists, Erving Goffman (1959, 72 and 252–55), admits that not all of the world is a “theatre stage” of deliberate impression management where we can put on just any kind of “[ ethnographic] play.” This could apply to participant “existential engagement” (Honer and Hitzler
2015, 549–52) in various (sub)cultural milieus that are incompatible with the researching academic’s social background and/or moralities. The assertion about the conditional feasibility of staging fieldwork should particularly apply to immersive research in elitist, deviant, or criminogenic social environments (Ferrell and Hamm 1998; Harrington 2016).

Identity claims in fieldwork are not easily performed by the researcher because they are usually not easily validated by the people studied. Therefore, we need to ask what are the constraints to the dramaturgical accentuation of (strategic) fieldwork interactions? How can we explain stylistic distinctiveness in the enactment of specified fieldwork roles? Is there such a thing as performed authenticity in fieldwork? And, what are the chances of reflexivity during the immediacy of situated co-participation in the field? To answer these questions, I propose to substantiate the conceptualizations of fieldwork via theories of symbolized interaction or identity with Bourdieu’s theory of practice (Bourdieu 1977; Bourdieu and Wacquant 1992).

**Researcher Socialization: From Biker Club Prospect to Researcher**

We can assume that the ethnographer’s socialization preconditions a specific, embodied *habitus* (Bourdieu 1977, 72–95; Wacquant 2016). If we take this at face value, then we ought to scrutinize formative influences of past affiliations with milieu-specific arrangements on the researcher’s *habitus*, and consequently the practice of fieldwork. As a prestructured system of social dispositions, the *habitus* is a prestructuring matrix of perception, appreciation, and action that guides *any* social practice. I contend that my past life has been the single most important prerequisite for the practice of my ethnography in the outlaw biker subculture. In this regard, my *habitus* formation can be analytically divided into two interrelated phases of a (primary) generic sociation and a (secondary) specific individuation (Wacquant 2014, 6–8).

*Generic sociation:* I grew up in a dysfunctional working-class family in a small Bavarian village. The social life of this village could be characterized by a widespread Christian, value-conservative philistinism where local dignitaries such as the mayor, priest, or school principal wielded supreme symbolic power (Bourdieu 1989). I was deeply disturbed by the patronizing attitude and hypocrisy among supposedly respectable people. At first, I was uncomfortable with the hidden life of male dominance, domestic violence, promiscuity, alcohol abuse, and racist ideologies, which was neatly concealed behind drapes and flower-decorated balconies. I later grew to hate them.
In my teenage years, I fell victim to the tyranny of the older village boys. Back then and there, bullying behavior was to be accepted and it was downplayed as an integral part of a boy’s initiation into “manhood.” Whereas others learned to accommodate, I separated myself in opposition. I discovered U.S. gangster rap music and started to dress in baggy clothes. This was in the mid-1990s when Hip Hop culture was barely known and was ostracized as some “GI crap” or “monkey music” in the rural regions of southeast Germany. In my search for comrades, I became interested in dogs and enjoyed the company of my first Doberman and later on an American Pit Bull Terrier. After I got my driver’s license at 18, I was able to join the boxing club in a nearby town. I was intrigued by gang culture and corresponded with pen pals in U.S. prisons, whose addresses I got from reading the Lowrider magazine. My deliberately deviant interests and behavior meant that I was further marginalized within the social fabric of the village community.

The effects of stigmatization, the identity crises, and the defiant struggle for self-assertion—which are characteristic of most outlaw bikers’ sociobiographies—are not alien to me. Because of my upbringing, I am familiar with the taste and style, the underdog mentality, the moralities, and emotions that are common among people of social milieus from which the majority of the traditional German outlaw bikers originate. Although I do not approve of the ideologies of predatory masculinity and the idiosyncratic codes of “respect and honor” that are so prevalent in the outlaw biker subculture, my socialization allows me to relate to them.

Specific individuation: The village that I grew up in was also home to other social misfits—the outlaw bikers. They congregated in my neighbor’s motorcycle garage. As a notorious figure in the regional biker subculture, my neighbor commanded great admiration and respect among his peers. Sometimes I assisted him in working on the motorcycles, most of the time I just enjoyed the company and became acquainted with the bikers. I liked the way that they appreciated me as a youngster and was fascinated by their nonconformist attitude and confidence. Over the years, my relationship with a specific group of bikers turned into comradeship. For instance, they would invite me to join them on their visits to biker club parties and nightclubs. They would also take me to events, such as a professional boxing bout in Berlin. Those trips were always exciting and fun times for a country boy like myself. I felt empowered to be around men who always commanded deferential treatment wherever they showed up.

At a height of 6’1” and with the well-trained body of a cruiserweight amateur boxer at about 200 lbs—oftentimes in the company of my Pit Bull—I was able to fit right in with this outlaw imagery. My biker buddies recognized me as one of their kind: an upcoming prospect. Little by little they let me in
on a few confidentialities and vouched for my reputation when introducing me to members of other biker clubs or underworld clientele. As I became more involved and committed to their lifestyle, I grew aware that my “easy rider” friends were outlaw bikers with a propensity towards violence and criminal intentions.

Things eventually took a turn after I graduated from high school and decided to leave for good to study in Munich. My contacts with the outlaw bikers were then reduced to sporadic home visits on the weekends. The geographical distance, new acquaintances, my studies, and several student jobs preoccupied me too much. As I also gradually concluded that this outlaw biker lifestyle was not for me, so did my biker buddies—who began to slowly dissociate themselves from me: “you’re either all in, or you’d better not be part of this lifeworld at all.” To this day, I can recall an in-depth conversation with a veteran member of the group who—like a father figure—looked after me. In contrast to the others, he told me that with my education, my life chances were better than theirs. He further advised me that I am probably too sensitive and decent a person to fit into the outlaw way of life in the long run.

In hindsight, my temporary association with these bikers provided me with valuable first-hand insight and experiences. I became familiar with the outlaw biker persona and some of the unwritten laws of this subculture. I learned the biker jargon, biker stories as well as the typical customs and mannerisms. I had the privileged opportunity to develop the “cultural tool-kit” (Swidler 1986, 277) that would, some years later, extra-methodologically inform and guide my fieldwork. If it were not for this specific socialization, it would never have occurred to me to study OMCs.

**Incomplete or Vague Guidelines: Extra-methodologically Informed Role Management**

Judging from my experience, the literature on role management in ethnography fails to give the fieldworker useful instructions when it comes to actual practices of co-participation in the field. The problems of applying the literature on ethnographic research are significantly magnified in deviant, delinquent, or otherwise elitist social worlds. Even the specified elaborations on doing ethnographies of deviance and crime (Von Lampe 2008; Ferrell and Hamm 1998; Horowitz 1986) help to a limited extent: What could an ethnography on German OMCs adopt from strategies applied by colleagues in bonding with Norwegian alcohol smugglers, Albanian Mafiosi, or U.S. street gangs? Furthermore, the improvised identities of fieldworkers can neither be negotiated independently of the field and its habitués nor—and even less
The Buddy–Researcher

With regard to informational yield, the overt status of the “buddy–researcher” (Snow, Benford, and Anderson 1986, 384–85)—a blend of buddy and researcher with changing accentuations—has proven to be the most profitable and viable one for my research. Building trusting relationships with different participants in the field has rewarded me with privileged access to events and insight. In contrast to immersive covert participation (e.g., as a club prospect or member), this role further allowed me to better manage the risk of getting involved in sticky situations.

As recommendable as the staging of the buddy–researcher proved to be in my case (cf., methodological impression management; Gengler and Ezzell 2017), I propose that performing this role is only feasible if the ethnographer is already equipped with field-adequate sociocultural competencies. On the one hand, the researchers influence how they are perceived by others via means of intentional “expressions given” (Goffman 1959, 4–16), which are to convey a certain favorable image. On the other hand, researchers are also perceived in terms of “expressions given off” (Goffman 1959, 2–4)—those little nonverbal and/or unintentional leavings in interaction which are, if at all, harder to control. The negotiations of rapport are, thus, contingent on meaningful practices of interaction communicated via physical characteristics, facial expressions, gaze, gestures, touch, body movement/modifications, proxemics/territorial behavior, posture, voice, and paralanguage (Knapp, Hall, and Horgan 2014; Waskul and Vannini 2006). Therefore, we must acknowledge practical accomplishments in fieldwork as being reliant on interacting physical bodies and symbolic body work (Coffey 1999, 59–75). If we substantiate symbolic interactionism with Bourdieu’s structuralist constructivism (Hałas 2004), then the impression management of the fieldworker must be viewed as enabled and/or constrained by the enactment and perception of their embodied expressive style (Hallett 2007, 153; Schwalbe and Shay 2014, 170–71).

Subcultures are constituted through symbolized orders of jargon, appearance, aesthetics, and stylized self-representation (Ferrell 1999, 403). Expression and style matter a great deal with outlaw bikers, who are very conscious of how to perform and make efficient use of their deviant image to their social benefit (Thompson 2009). Living the outlaw life, they become very proficient in presenting themselves in ways that generally demand
deference through intimidation. And vice versa, outlaw bikers are notoriously suspicious and adept at playing “expression games” (Goffman 1970, 1–82) that aim to uncover the extent to which others are strategically engaged in the manipulation of representations. Residents of honor-based, deviant, or criminalized subcultures are generally inclined to always check for those discrediting expressions in “face-work” that might attest to a “wrong face” (Goffman 1967, 5–45). Therefore, they pay careful attention to minute cues in face-to-face behavior that are especially difficult to control or they engage others in “character contests” (Goffman 1967, 239–70). In a conversation about prospects, a veteran 1%er claimed the following:

But sooner or later it’ll be noticed. And then these people will not be very happy about their decision. [. . .] Sure. You can pretend to be a biker. But that’ll be noticed, and they’ll fail the test right then and there. They won’t be accepted. They won’t be integrated. They stand out. Because of their conduct. Their behavior. (emphasis in the original)

The same applies to the ethnographers who approach outlaws—they will be checked and tried in some form or fashion. It is nearly impossible for the researcher to emulate the outlaw biker taste, style, and demeanor without running the risk of being perceived by them as either a “faker” or as an academic “culture vulture.” In addition, the outlaw biker milieu does not allow the ethnographer to slowly assimilate, learn, and/or imitate—there are hardly any second chances for renegotiations of rapport. As Hopper and Moore (1990, 69–70) validate, even minor remarks or behavior can cause significant issues when socializing with outlaw bikers. Despite the once achieved intimacy, trustworthiness, and joviality with club members, there remains a subtly discomforting feeling that the buddy–researcher status can never be taken for granted—moreover, already well-established relationships can be abruptly terminated. The closer you get to members of an OMC, the more you will also qualify as a potential threat to their brotherhood.

**Negotiations of Rapport: Different, but the Same!**

Outlaw bikers will either reject or tend to deceive the researcher who is limited to the role of the “acceptable incompetent” (Lofland and Lofland 1995, 56–57). The naïve ethnographer will therefore either be denied or granted only restricted and/or varnished access to data. My social dispositions and prior insider knowledge helped me to alleviate these problems and further directed me into the position of the buddy–researcher. It is important to note here that the performance of the buddy–researcher is about sharing
experiences and emphasizing similarities, but it is certainly not about trying
to pass as a native (Snow, Benford, and Anderson 1986, 385). Without a
verifiable background of having “earned your stripes” in the outlaw biker
subculture, any self-stylization as one of their kind will be considered to be
a serious offence.

To nonetheless bond and achieve credible rapport with outlaw bikers, I
drew from resources based on my sociobiographic experiences and habitus—
I am capable of joining in a conversation about fighting, the temperament of
working dog breeds, troubled family relations, tattoos, music, movies, or
motorcycles that outlaw bikers are typically interested in. Although I cannot
match the oftentimes massive physiques of outlaw bikers, I am visibly in
good shape. Although I do not qualify as a bodybuilder or powerlifter, I am
knowledgeable about ways to build muscle and strength based on my experi-
ments with different training protocols or diets. Although I have no street
fighting credibility, I have experienced physical conflict during my upbring-
ing in a tough rural environment and in the amateur boxing milieu. I am not
a motorcycle enthusiast, but it certainly helps to be informed about motor-
cycles. My extant knowledge about the German biker scene, its history, and
the gossip about club politics was also of great benefit to establish my field-
work role as the informed outsider. All these stereotype capacities generally
allowed me to successfully socialize with outlaws.

A female researcher’s access to the ultra-masculine outlaw biker subcul-
ture, for example, would be different and arguably privilege alternative
access options, perceptions, interactions, and insights. Kuldova (2019, 12–
16) reports a “feminine advantage” in researching outlaw bikers pertaining to
her ascribed fieldwork role and the gathering of knowledge presumably hid-
den from male researchers. Conforming with Hopper and Moore (1990, 371),
she rightfully contests naïve assumptions of gender solidarity in ethnography
and summarizes her fieldwork as “having an overall ‘easier time’” compared
to her male colleagues. Other female ethnographers in male-dominated,
honor-based subcultures also mention episodes of rather deferential and/or
protective treatment by their male research subjects (e.g., Horowitz 1986;
Van Hellemont 2017). However, gendered or sexualized identities are often-
times renegotiated in long-term, multi-sited fieldwork with corresponding
(dis)advantageous effects on the access to events and information (Horowitz
1986, 414–23). Without quasi-experimental designs in research on fieldwork
practices in such settings, we can only speculate about the rapport and experi-
ences that are categorically to no avail or favored because of gendered
researcher habitus. For example, the male colleagues Hopper and Moore’s
ethnography (1990) is seminal because they first recognized and explored
the hitherto faceless and changing roles of female associates within the biker
subculture. Dependent on the achieved intimacy with a few outlaw bikers and their female partners, relationship talk (e.g., about dating, sexuality, separation heartbreak, marriage/parenting) and other accounts of vulnerability (e.g., despair about personal, work/club-related issues) were no longer withheld from me as the male buddy–researcher. This was encouraged whenever I would open up about my own private and work-related troubles (e.g., my breakup with my longtime girlfriend, the tragic loss of my dog, my complicated relationship with my divorced parents, or my precarious working conditions in academia).

In summary, my fieldwork and research agenda benefitted from the incorporation of experiences that have been formed and acquired in the same or comparable social spaces that most bikers come from or where their habitus is further cultivated. In this vein, most of my buddy bonding with outlaw bikers came about by “elective affinity” (Bourdieu 1984, 241), which is the result of the intersection of mutually reinforcing interests and expressive signs (e.g., clothing, pronunciation, posture, bearing, and manners) that unconsciously create antipathies or sympathies between socially classified persons. This social mechanism generally worked in favor of my access to outlaw bikers as a male researcher from a working-class background and a prior history with this subculture.

Eventually, we need detailed and uncensored accounts to adequately analyze, compare, and discuss fieldwork as the dynamic product of “multiple correspondences” (Bourdieu and Wacquant 1992, 96–98) including gender, researcher sociobiography/appearance, prior knowledge/expertise, recreational/special interests, authority of sponsors/gatekeepers, official/hidden research agendas, research ethics, mode of participation, degree of immersion with/throughout the field, quantity/maturity of fieldwork interactions/face time, frequency/duration of field visits, settings visited, or situational framings.

**Adequate Fieldwork Improvisations qua Habitus**

Ethnographic fieldwork is performed in constantly evolving situations, and thus demands a great deal of improvisation on the spot (Humphreys, Brown, and Hatch 2003, 13). But what are ethnographers improvising on? They certainly cannot do so in a social void of arbitrary self-stylization. In line with Hallett (2003), I contend that the dramaturgical negotiations of the researcher’s *self* are characteristic of the situation and its participants. However, this process is invariably co-determined by the habitus of the researcher. Despite the creativity and malleability of the habitus (Hilgers 2009), we always bring our culturally inscribed and embodied dispositions
(corporeal hexis), our physical symbolic body, with us into the field, which shape the demeanor that we exhibit towards others (Hallett 2007, 153; Coffey 1999, 59–75).

If the fieldworker’s habitus is incongruent with the sociocultural dispositions of the other participants in the field, then they will face the constant challenges and consequences of “misrepresentation” (Goffman 1959, 58–66; Hallett 2007, 153; Harrington 2016, 139–40). If the researcher’s habitus corresponds with the doxical structuration of the field, then their fieldwork more likely benefits from a field-adequate “practical sense” (Bourdieu 1998, 25). Fieldwork improvisations are therefore guided by a better intuition of what is going on, what is going to happen, what should be done, and how it should be done.

Although methodical or ethical guidelines are available, they offer limited practical instruction when it comes to actually navigating through situations with sexist, racist, illegal, or overly aggressive behavior. Some of the questions raised by my fieldwork are: How to deal with racist or sexist overtones during talks in the field? How to respond to the glorification of extreme violence? How to resolve a situation where the club president’s drunk fiancée flirts so relentlessly with the researcher in the immediate presence of her fiancé that the situation could have easily escalated? How to turn down the invitation of a hosting member to sexually harass (grab) one of their female associates at a clubhouse party? How to conduct yourself in an unanticipated confrontational stare-down between club members with by-passers who are suspected to be supporters of a rival club?

Events of this sort entail agonizing self-reflections and self-reproaches from the ethnographer about their fieldwork practices, often only after things have already happened. Sudden confrontations with situations of physical and psychological risk (“where the action is;” Goffman 1967, 149–270) will rather evoke affective reactions than allow for reflexive action. The unforeseeable, ambiguous, and always evolving dynamics in fieldwork trigger ethnographers into pre-conditioned practices of perception, appreciation, and action that are regulated by their distinct habitus (Bourdieu 1977, 78–79). On the one hand, a culturally too affine researcher habitus implies a problematic fallacy of being caught up and getting caught in the “illusio” (Bourdieu 1998, 76–77) of the field. On the other hand, a corresponding habitus informs the improvisation of situated fieldwork according to the sociocultural axiomatic of the field, and thus can help the researcher to avoid ethical issues. For example, I once became alert to a middle-aged male who behaved aggressively before other patrons and stepped in when he suddenly turned directly towards the outlaws I was hanging with at a nightclub. The bikers took notice of my spontaneous intervention and carefully watched
how I fared in handling this encounter. Later on, this specific incident was addressed and a conversation about my research activities ensued. My initial impulse, however, was to prevent any possible trouble caused by this man confronting the 1%ers.

If outlaw bikers experience the ethnographer as visibly discomforted or overwhelmed by their sometimes outrageous conduct or discriminating ethics, then they will reconsider or end their relationship with the researcher. In the case of witnessing illicit behavior, they will pressure the ethnographer to reevaluate if they can be trusted as a confidant with “dirty knowledge” (Ferrell and Hamm 1998, 24; Hopper and Moore 1990, 370). To safeguard my observant participation over time, I had to acknowledge that this means “getting your hands dirty” somewhere in the process (Honer and Hitzler 2015, 552–54). I have faced the same conflicting dilemmas as encountered by Hopper and Moore (1990, 367–71): fascination versus repulsion, silent acquiescence versus voicing objection, commitment to research goals versus commitment to research ethics.

Eventually, all of the discussed prestructured and/or pre-reflexive practices of improvised fieldwork—the specific style of doing things together and self-presentation—extra-methodologically enter into the continuous negotiations of rapport and access.

**Chance: The Extra-methodology of Serendipity in Ethnography**

A third condition that demands extra-methodology in ethnography is chance. The ethnographer faces the constant challenge of having little to no influence over the course of their study. Yet, given that the ethnographic approach is meant to be open for the chaos of lived experience, fieldwork benefits from the capacity to cope with and/or capitalize on unplanned, accidental, or rare events. The following conceptualization of serendipity in field research will not elaborate on patterns of chance findings understood as “analytical serendipity” (Fine and Deegan 1996, 441–43), the focus is instead on chance access to persons and events that antecedes the possibility of analytical discovery.

**The Role of Unfortunate Coincidences**

At the very beginning of my study, I relied on the support of the outlaw bikers that I had known since my teenage years. However, in the meantime, they had been targeted by police investigations over a major criminal lawsuit. Lengthy court proceedings and a crown witness had stirred fear of
imprisonment among several of them. They were pleased to hear from me, but they were not in the mood to cooperate in a study of OMCs. Subsequent attempts to randomly recruit other clubs were to no avail—a myriad of emails, phone calls, and text messages were either rejected or ignored.

The Role of Fortunate Coincidences

Spurred by my personal and sociological interests, I was fatalistically determined to conduct this study against all odds. Eventually, my recruiting efforts were rewarded with the support of my first sponsor, who happened to be the editor-in-chief of a German biker magazine. He was convinced into cooperating for three main reasons. First, he seemed to acknowledge my personal history with outlaw bikers. We both shared a common fascination with the outlaw biker lifestyle beyond our professional assignments. Second, he seemed to be interested in my research agenda, which was not about the clubs’ alleged criminal enterprises. Third, he surprisingly turned out to be a fellow academic who had graduated with a PhD in philosophy and was therefore willing to support his academic peer. He did so by providing me with the email addresses of selected members from different OMCs throughout Germany. After he had briefed them about my persona and my planned research, it was still up to me to make contact, visit, and win over potential informants or gatekeepers.

Other than that, my recruiting strategy was seldom that of directly seeking out and approaching potential informants; instead, it seemed to depend on a great deal of chance. In recollection of all these years of researching OMCs, I now suppose that there was some regularity behind coincidences of being at the right spot at the right time (Fine and Deegan 1996, 439), and (re)acting the right way to gain access to club members or affiliates. Favorable circumstances are more likely to emerge when the researcher is personally committed, culture-savvy, and has been already immersed in the field. Although opportunities can be purely accidental, the odds can be beaten. As Fine and Deegan (1996, 435) argue in their examinations of classic fieldwork accounts, in observation chance favors the prepared mind. In theorizing the practice of fieldwork, the cognitive must be extended by the conative and affective dimensions of the ethnographer’s "body as geometer" or “conductive body” (Bourdieu 1992, 66–79 and 145–46; Wacquant 2014).

If researchers also privately frequent the social worlds where their research subjects congregate, then encounters with participants are less a matter of pure chance than one of tenure, intuition, and well-timed initiative. Again, this is where the “elective affinity” (Bourdieu 1984, 241) between people with a kindred or connectible habitus takes effect. The socially homologizing impact of comparable lifestyles and (recreational) interests generates opportunities of
access beyond chance. This mode of association is partly based on the pre-verbal mastery of mutual recognition and judgment (Bourdieu 1984, 470–75) and is usually superior to upfront efforts of approaching informants (Harrington 2003, 612; Fine and Deegan 1996, 440). Because outlaw bikers and associates do not always openly feature or sometimes even hide their affiliations, fieldwork benefits from the ethnographer’s practical mastery of identifying this social clientele by their companionship, their specific ways of bearing, decorating, and presenting their bodies, and through their interaction rituals. As one outlaw biker insisted in an interview with me: “We don’t recruit; we recognize!” The same mechanism sometimes applies to the ethnographer who recognizes and is recognized by, gets into contact with, and is attributively judged by members of the outlaw biker subculture. Many encounters and observations occurred in places and at times at which I had not intentionally pursued or forced my research project.

A selection of fieldwork anecdotes illustrates these unobtrusive practices of socializing for which there is no academic training or textbook advice. Working out in a martial arts or fitness gym brought me into contact with outlaw bikers or affiliates. Visits to Bull Terrier Club show increased the likelihood of contact with people from or people connected to the outlaw biker subculture. Living in an apartment in a district where a 1%er club has its clubhouse and where members frequent the local bars or restaurants raised the chances of fieldwork observations on the fly. A befriended tattoo artist is a good sponsor or informant for a study of the outlaw biker milieu. Friendship with a bar owner may increase the chances of connecting with other people in the local amusement district with ties to outlaw bikers. This is the context in which, for example, I was first introduced to a bouncer with whom I initially engaged in a lively conversation about mixed martial arts. We then randomly ran into each other on the street or met at our common friend’s bar. This acquaintance would later become a full-fledged OMC member. Another relationship dates back many years to a private party of one of my first research sponsors, where I befriended someone who has in the meantime become a higher-ranking 1%er. He would, for example, provide access to his club’s annual national meeting (“National Run”), where only club members or known associates are approved as guests. He also brokered a new contact with another local chapter of his OMC after I had moved cities for professional reasons.

The Double Stranger! Habitus Clivé and Ethnography

This attempt at theorizing the practice of fieldwork was to offer a heuristic to substantiate, examine, and compare the theoretically indeterminate and
widely underthematized relationship between the ethnographer and their field of research. Granted, if all social practice is of dispositional-interactive and relational (Bourdieu 1998, 3; Wacquant 2014, 5) nature, then so is the practice of fieldwork(ers). Therefore, we must adopt a more rigorous view of the researcher in the field to explore and explain hitherto unexplained variance in fieldwork. Despite the pedagogic efforts of methodological training or the instructions of ethical research guidelines understood as part of the academic “tertiary socialization” (Wacquant 2014, 7; Bourdieu 2008), the ethnographers’ impression management in situated fieldwork remains extra-methodologically informed by their embodied social agency (habitus). The underlying overall claim of this article that pure research interest and methodological savvy do not sufficiently grasp successful fieldwork practices is supported for immersion ethnographies of other social elites (Harrington 2016, 135–40). We need further discussions about the fit between the ethnographer’s social dispositions and the structuration of the field of inquiry as a significant condition for participation beyond the (most) perfunctory sort (Harrington 2003, 620–21).

What characterizes my fieldwork practices also manifests in my headwork and confessional textwork (Van Maanen 2011, 221–27): a working-class male’s disillusionment with academia and scholastic reasoning, a spoiled identity preoccupied with issues of social (mis)fit and resilience, or a self-stylization of accomplishment through arduous effort and tenacity. Social scientists may arguably be better equipped than lay people for critical self-analysis to gain conscious control over their agency (Hilgers 2009, 737–39), but they too cannot and should not deny their dispositions completely (Bourdieu 2003). The researcher’s habitus is not only a “method of inquiry” (Wacquant 2014, 4–5) but it is also an extra-methodological tool for inquiry. The classical argument favors a researcher–field misfit for its potential to turn strangeness into analytical leverage (Fine and Hallett 2014). For the cases of immersive ethnographies in socially elitist or closed fields, I add to the discussion by highlighting the necessities and/or benefits of a sociocultural fit between the researcher and their field. This is where the ethnographer can make competent and empowering use of a habitus, which has oftentimes proved to be dysfunctional in life (e.g., in establishing a career in academia). The same goes for the researcher with a privileged habitus of the dominant classes (Vester 2003, 41–45), which is well-suited for the immersion into equally difficult to access socioeconomic or sociocultural elites (Harrington 2016).

I recently worked on another publication (Schmid 2020) about my troubled experiences of an already alienated country boy with ties to outlaw bikers transgressing into the academic culture of an elite university. This brought
my attention to the literature about the practical and emotional implications that are faced by working-class academics (Lehmann 2013; Reay, Crozier and Clayton 2009). It reminded me of my own identity crises, which were the result of upwards social mobility achieved through higher education. Although I was quite successful graduating and later on received academic awards for my dissertation, the sociopsychological impact of my “habitus clivé” (Bourdieu 2008, 100–10; Friedman 2016; Wacquant 2016, 66–69) endures. Also referred to as a cleft or torn habitus, this concept captures the malfunctioning of a given habitus that encounters culturally divergent fields, institutions, or situations. The socially dislocated agent feels estranged from their social origin and simultaneously alienated by their new destinations. Such a double misfit can either lead to the transformation or, as in my case, to the durable dissonance of a habitus that is in constant renegotiations with its own ambivalences and divided allegiances.

This breaks up another layer of reflection about my ethnographic practice, which is far more existential than hitherto considered in debates about “existential engagement” (Honer and Hitzler 2015, 549–52) in ethnography. Fieldwork in socially segregated or risky settings demands the investment of a particular “disinterestedness” (Bourdieu 1998, 75–91)—a libidinous, passionate involvement devoid of strict extrinsic interests. Whereas my higher education studies provided me with academic reputation and secured my consecutive employment in funded research projects, my non-funded ethnography on OMCs is primarily motivated by my life history. This intrinsic quality of self-determined interestedness displays an authenticity that helps the ethnographer in accessing fields and associating with people who would otherwise be difficult to access for academics with primarily scholarly interests (Harrington 2003, 603).

Since the very start of my diploma thesis, my research on OMCs has helped to alleviate my torn habitus by taking advantage of the unique cultural capacities and flexibility (“cultural tool-kit;” Swidler 1986, 277) that it also endowed me with. The buddy—researcher (i.e., the buddy and/or researcher) is the exact expression of this internal divide put into ethnographic practice. My fieldwork role is constitutive of a complementary but also conflicted allegiance to the field of science and the field of my research—the “professional” stranger in both worlds. The label of the buddy to outlaws makes me discreditable in academia; and vice versa, the label and the tertiary habitus formation (Wacquant 2014, 7–8) of the “academic” contributes to my ultimate otherness in the outlaw biker subculture. Feedback, disapproval, alienation, misunderstanding, and informal attempts at censorship emerge from both lifeworlds (see also Ferrell and Hamm 1998, 4–9), but ideally stimulate new reflections and findings. The juxtaposition of strangers versus non-strangers in the
ever-contentious discussion about participatory competence and authority in ethnography (Fine and Hallett 2014) is too categorical to adequately capture the multiple, ambivalent, situated, ever-evolving, and non-linear positionality in fieldwork.

Got Ethnographic Gameness?

In closing this article, I propose a label for the ideal-type of the ethnographer who is well-suited for the immersion into deviant, criminalized, or culturally elitist fields. Therefore, I resort to the imagery of a close combat specialist who does not shy away from, effectively deals with, and thrives on adversity. The English language has a term to characterize a champion of this type, which is said to originate from the milieu of dog-fighting (Jessup 1995, 157–58) and early bare-knuckle boxing: “gameness.” Gameness means “to stick to a line of action and to continue to pour all effort into it regardless of setbacks, pain, or fatigue [. . .] not because of some brute insensitivity but because of inner will and determination” (Goffman 1967, 219). The intrinsic, dispositional nature of gameness becomes apparent in the fact that this characteristic appears only under conditions of selective breeding (e.g., the Pit Bull Terrier; Jessup 1995, 87–110) and/or socialization (e.g., the ghetto boxer; Wacquant 2014) for specific fields of practice (Wacquant 2016, 66): “game-bred.” Therefore, gameness is a distinctive quality of dedication to the game that is imposed in the mind and body of accordingly socialized agents (Bourdieu 1998, 76–88). Unlike the acquisition of sheer technique and conditioning in training, true gameness ultimately shows and further develops in fateful situations of real-life adversity. For the chaos of a fight as for immersive ethnography, a fixed strategy (“game plan”) almost never works out exactly the way that it is supposed to and this, again, calls for the practical mastery to adjust accordingly (“fight IQ”).

This concluding metaphor of gameness may come across as misguided self-indulgent and overall irritating in the context of the social sciences and humanities, but it perfectly serves the disruptive intention of my practical critique of methodology in theory.

Acknowledgments

The author appreciates the anonymous reviewers for their thorough and helpful comments. He is especially grateful to his dear colleague Paul Eisewicht for his initial inspiration which led to this article and for helping him a great deal in revising earlier drafts. Many thanks to the co-editors Andrea Ploder and Julian Hamann for their efforts to make this special issue happen as well as for their overall great support of guiding him through the process.
Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

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Notes

1. In unfolding the methodological reasoning on the following pages, I will make extensive references to myself. These should not be interpreted as the author’s self-centeredness (Goffman 1976, 118–19) but as the ecological narrative perspective grounded in the subjective experiences of his sociobiography and fieldwork.

2. Some findings of the unpublished thesis can be found in Schmid (2009, 2012).

3. For a short literature review on extant research on OMCs see Kuldova (2019, 10–12). Her ethnography on the Hells Angels Motorcycle Club is highly recommended because it provides the only up-to-date, thick, and insightful portrayal of European outlaw biker subculture.

4. Of course, I also encountered many members or affiliates who—for reasons mostly unknown to me—either ignored me, who were (first) suspicious of my presence, or eventually signaled no interest to connect with me whatsoever.

5. Johnny ”Big John” Moore was the former president of the Satan’s Dead MC. This was instrumental in getting permission to visit biker clubhouses (Hopper and Moore 1990, 367).

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