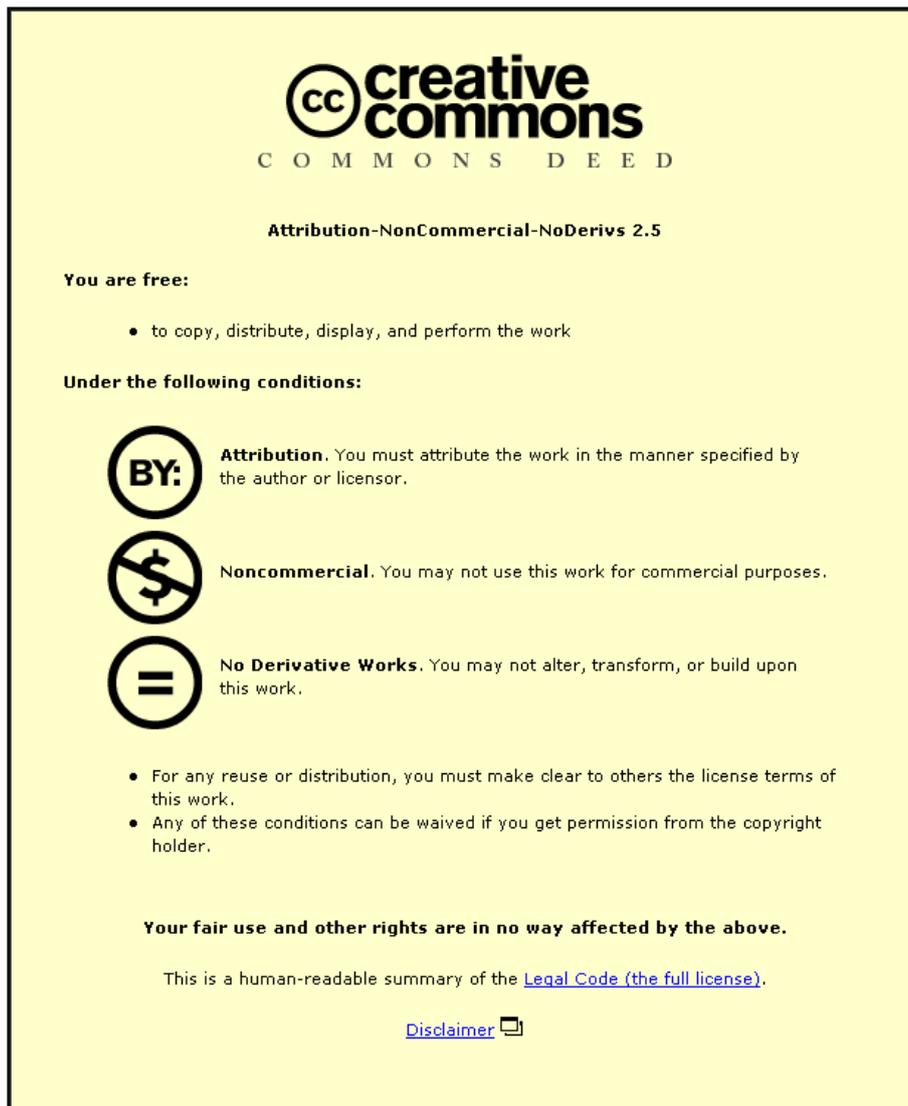


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Participant Observation and Research into Football Hooliganism: Reflections on the Problems of Entrée and Everyday Risks

Richard Giulianotti

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This paper discusses the author's fieldwork experiences while initiating and undertaking substantive participant observation research with two rival groups of Scottish football hooligans ("football casuals"). Key problems examined are those that emerge from attempted entree into the hooligan subcultures and the everyday risks of comparative research with violent fans. The author provides regular illustrations to highlight how dangers such as the researcher's personal characteristics, lack of guiding sociological literature, and interaction with police officers can threaten the urban ethnographic project. The resultant ambivalence of some research subjects toward the author is interpreted as one reason for minimizing the prospect of his "going native."

I understood that they would never accept me as a comrade, however much I might be a convict, not if I were in for life, not if I were in the special division. But I remember most clearly Petrov's face at that minute. His question, "how can you be our comrade?" was full of such genuine simplicity, such simple-hearted perplexity. I wondered if there were any irony, any malicious mockery in the question. There was nothing of the sort: simply we were not their comrades and that was all. You go your way, and we go ours; you have your affairs, and we have ours. (Dostoevsky, 1915, p. 247)

The first recognized definition of the social science method "participant observation" comes from Lindeman and is as dichotomous as it is literal. He underwrites the researcher's full engagement in the subjects' activities (participation) and the professional distinction of the discipline (observation).

The term [participant observation] implies not that the observers are participating in the study but that they are participating in the activities of the group being observed. . . . There are few such persons available and those who are must be trained. Such training involves its own difficulties. Shall the

participant observer be trained to look for exactly the same factors which are sought by the observer from the outside? This method would inevitably lead to error for the participant observer should be free to see many things which the outside observer can never see. (Lindeman, 1924, p. 191, quoted in Bruyn, 1966, p. 13)

To this definition, Becker (1958, p. 652) adds an interactional component: "Talk" with the actors to prise out the social meanings appended to actions.

The methodological origins of participant observation are spelled out less easily. It is possible to infer the limited use of inchoate participant observation techniques in very early community and anthropology studies. DuBois's (1898, 1899) works on race relations in the United States have been interpreted as the first participant observation studies in this field (Dennis, 1988). Equally, anthropologists such as Radcliffe-Brown (1922) practiced participant observation *avant la lettre*, driven by a dissatisfaction with short-term fieldwork and speculative analyses of traditional societies. An analogous imperative was behind the method's sociological adoption by the Chicago School in the interwar period, to focus on marginal or deviant social groups, such as hobos (Anderson, 1923), dance halls (Cressey, 1932), "jack-rollers" (Shaw, 1930), rooming house areas (Zorbaugh, 1929), and various immigrant groups (Drake & Cayton, 1945; Thomas & Znaniecki, 1927; Wirth, 1928). However, it should be noted that, particularly in early urban ethnography, participant observation was combined with various quantitative methods, such as survey and census research and statistical analysis-research methods with which post-war participant observation's hermeneutic element is strongly contrasted (Platt, 1983).¹

There are three important overlaps in the use of participant observation by anthropologists and sociologists. First, both are driven by Malinowski's (1926, p. 146) famous refrain that the researcher "relinquish his comfortable position on the verandah," to find out exactly what is occurring in arcane communities. Second, there is an implicit interest in studying communities typically considered to diverge significantly from the "normal" practices and mores of Western society. And third, both disciplines generally provide an empathetic presentation of these deviant communities, utilizing a relativistic approach to knowledge and social values. Upholding Weber's method of *verstehen*, Becker (1967, p. 247) notes that the participant observer in a prison must observe "through the eyes of the inmates and not through the eyes of the guards or other involved parties."

Accordingly, one criticism of such anthropological study is its propensity to suspend critique of the host culture, through disenchantment with one's own (Levi-Strauss, 1976, p. 502). Participant

observers regularly encounter such accusations of "going native," the imperialistic overtones of which tend to be displaced by the challenge to the researcher's professional integrity. The sociologist may be further endangered professionally by an originally "native" association with the subculture & died. The participant observation studies of newspaper offices (Park, 1922), jazz musicians using marijuana (Becker, 1963), pool players hustling (Polsky, 1967), criminals in London's East End (Hobbs, 1990), and Sheffield United football hooligans (Armstrong, 1993; Armstrong & Harris, 1991) were all enabled by established association and access of the researcher to the designated milieu. Reflecting the political and academic zeitgeist, allegations arose from established sociologists that Armstrong (and Harris, 1991) had gone native (Dunning, Murphy, & Waddington, 1991, pp. 467-468; Moorehouse, 1991, p. 491). Yet there are methodological precedents for being native in such research. Lindeman (1924, p. 193), in fact, recommended that the participant observation be carried out by a genuine insider, the emergent bias of his or her findings being construed as an actual research advantage in disclosing the sub-society's nature.

I return to a critical exploration of the important practical and epistemological differences between researcher qua insider or native in the concluding section. By way of contextualizing this discussion, I wish to concentrate on two of four major issues in participant observation, advanced by Weppner (1977, p. 31), which demonstrate the everyday difficulties of invoking the method: entree to the subculture, and the regular risks encountered by the researcher.² I illustrate these problems with extensive reference to my own research. The research was undertaken with Scotland's two premier football hooligan formations, the rival Aberdeen and Hibs (Hibernian Football Club of Edinburgh) "casuals" (see Note 6), and it remains an ongoing study.³ This consists of regularly introducing myself to new research acquaintances; renegotiating association with familiar casuals; talking with them, drinking with them, and going to matches with them; generally participating with them in a multitude of social situations; but disengaging myself from preparing for and participating in violence, within and outside of football match contexts.

Entree, Entree: The Origins of Comparative Research

Gaining continued access to the proposed research subjects, and entrée within their life-worlds, may be the most difficult part of a participant observation study (Hammersley & Atkinson, 1983, p. 54). Although the method's anthropological heritage and "private eye" patina encourage research with the socially marginal, there are micro-political reasons for participant observation's association with the disempowered. Powerful institutions are usually indisposed to the

ethnographer, "on their guard against unfriendly and unsympathetic investigation" (Smith & White, 1968, p. 153). They are far more amenable to funding investigative research into the discomforting activities of those social groupings that undermine the legitimacy of the existing socio-political milieu.⁴

The participant observer of deviancy is caught in a political double bind. Not only do his or her findings threaten to reinforce social inequalities when they are made public and acted upon; the researcher also takes all the risks in approaching the marginalized to study their practices. To compensate the liberal genius of the method, the researcher may explore critically the everyday construction of negative stereotypes about the marginalized (e.g., Damer's 1974 study of "Wine Alley" in Glasgow). But, criminal subcultures are as proportionately difficult to research through participant observation as they are exotic and attractive to the student. How to study the fascinating lifestyle of the hitman (Levi, 1981) or Ulster paramilitaries (Feldman, 1991) without becoming an accomplice, or a "stiff"?⁵

Prior access to the research group may be essential to actually commencing the proposed research. It has been an essential ingredient in my studies of Aberdeen casuals. I was a close personal friend of **3** of the 47 casuals "ambushed" by police in 1985, before a football match against Motherwell (Allan, 1989, pp. 101-109). I had been schooled, and continued to socialize regularly, with many of the first casuals in Aberdeen in 1982-85. Moreover, my personal characteristics were and remain almost identical to those of the research group, primarily in age, attire, and argot.

If any clarification is required here, however, it should be recognized that at no time have I defined myself as an Aberdeen casual, in the strict or loose senses of such identification.⁶ Nor was I ever regarded as a casual by my associates when the style was in its ascendancy. At that stage in my biography, I retained a more classicist reading of youth subcultures as harboring an identifiably radical socio-political component, which the casuals, through their pursuit of intragenerational and intercity violence, then appeared to contradict. Had I been aware of his work, I would then have empathized with the sentiment behind the naive response of Robins (1984, pp. 15-16) to his witnessing young fans fighting among themselves, in pointing them in the direction of more objective opponents: the club directors in the posh seats (cf. Armstrong, 1993, pp. 7-8). Since then, a personal refusal to engage in violence has become an ethical reification; what has altered is my reading of the significance, complexity, and cultural politics of the Aberdeen casuals in particular, and the phenomenon of football hooliganism generally.

During my undergraduate career (1985-89) I broadened my range of contacts to include much of the core group in the city center and wrote a short essay on the interactional sociology of the Aberdeen casuals. Full-time research on the formation began in 1990. It was only at this stage that I began socializing freely with the gang at football matches, traveling to and from matches within the main grouping of the Aberdeen casuals.⁷

The absence of prior access and discrepant personal characteristics threatened to abort the proposed research in Edinburgh with Hibs casuals, scheduled to begin late in 1990. I had had no prior contact with Hibs casuals, save for a brief exchange with two at the 1990 World Cup Finals in Italy. Initially, I planned to contact one who had been pointed out to me by Aberdeen casuals beforehand as a leader, but I was wary about affording too much credence to this advice.⁸ The only intelligence I possessed on Hibs casuals' meeting points was contained in salacious local newspaper reports about their criminal proclivities.

Personal characteristics were also a potential problem, although hardly an uncommon one for me. Prior literature on ethnography afforded encouragement on the degree of "passing" that could be achieved by the prima facie stigmatized (Goffman, 1963), through a skillful manipulation of personal and knowledge resources. The Caucasian Liebow (1967, p. 255) successfully dulled some of his obvious differences to the Negro subculture researched. Comgan (1979, p. 13), in a study of North-East English youths, faced an engrained antipathy toward "southern cream puffs," which he rebuffed through his expertise on London fashion and football gangs. I would argue that my personal misfit to Hibs casuals was of a greater magnitude than either case, for cultural and temporal reasons.

1. My highly anomalous Aberdonian biography and accent defied disguise. On entering the field, I could easily have been misconstrued as an ex-Aberdeen casual and dealt with accordingly. This misidentification would have been based on educated speculation, given their awareness that my approximate age coincided with those of over 1,000 young Aberdeen men who had identified with the city's monopolizing, casual youth style in the mid-1980s (Giulianotti, 1993, pp. 168-173).

2. My interest in researching and writing on the gang was badly timed. Recently, lurid newspaper articles had attributed murders, extortion rackets, and organized and random violence to Hibs casuals. On my first day in Edinburgh making inquiries about the gang in shops and pubs, I was advised by one shopworker to take the next train back to Aberdeen: "They feel they've been stitched up by reporters, and they'll take it out on the next one they meet."⁹ I ran the further risk of being misidentified as an undercover police officer. I later learned that at the time I had been

seeking entrée, the Hibs casuals had believed the police were about to mount an undercover operation against them (Giulianotti, 1994a), a strategy liberally used in England against football hooligan formations (see Armstrong & Hobbs, 1994).

In understanding the latter issue, I was drawn to Cohen's (1980) social constructionist theory of "folk devils" and "moral panics" and the culture of "secondary deviance," which the process precipitates. The media appeared to have become aware of fictive or unusual, but intrinsically criminal, activities of the gang and had presented these as real and typical, thereby fanning the flames of social unease at Edinburgh's foremost symbol of the underworld. More significantly for the research, there was also the then-inestimable impact of these discourses on the self-cognition of the gang. Symbolic interactionist theories of the looking-glass self and secondary deviance (Lemert, 1967) stipulate that the most outlandish of subcultures are sensitized a priori to their societal definition, and develop self, or collective (Young, 1971), images and identities accordingly.

The police and media representations of the gang, as well as those of my early contacts in Edinburgh, may have been amplified, or plain wrong, but I was about to parachute into the next phase of moral panic, in which such a publicized image of hooliganism may have become a self-fulfilling prophecy.¹⁰ Thus, I particularly feared being implicated in the anti-casual panic germinated by police and media (Giulianotti, 1994a); after all, how many youths from Edinburgh's Niddrie housing estate would know or care about the discrepant objectives of the broadsheet journalist, the plainclothes CID officer (Criminal Investigations Department of the Scottish police), or the sociologist? Each seems to ask pointless, if different, questions. I was developing the "labelling theory" impression of an obscure subculture becoming increasingly endogamous and xenophobic toward the wider public, a thesis not without evidence. Hibs casuals were also known as "The Family," connoting a surrogate kinship, where gang loyalties overtake familial ones. They had their own football side, banned from the local leagues for fighting and rioting. They even had their own rock band—the Guitar Casuals. How to introduce myself after others before me had merely reinforced the boundaries between the subculture and its wider social setting?

Becker (quoted in Hessler, 1992, p. 208) left me unsurprised that the relevant sociological literature offered barely any guidelines for action: "As every researcher knows, there is more to doing research than is dreamt of in philosophies of science, and texts in methodology offer answers to only a fraction of the problems one encounters." Few participant observation studies

are undertaken with both sides of a social rivalry, particularly criminal ones.¹¹ Hobbs's (1990) ethnography with East End criminals and detectives is a noble exception. His depiction of a shared entrepreneurial culture contained a useful, action-orientated field tip. This was to ensure that I had an effective response to the explicit or tacit quid pro quo, what Becker (1970) terms a "research bargain" to be struck with those studied. When I did eventually contact the formation en masse, after a week of searching, I had something to trade, to make my presence tolerable and a potential resource. They were very keen to know what the casual scene was like in Aberdeen and to contrast it with the situation in Edinburgh. In reciprocating, I could simultaneously establish my credentials as an accepted researcher in Aberdeen, in response to particular questions: "Do you know what X is up to?" "Who's that big Aberdeen lad with the ginger hair?" "What do Aberdeen say about us?"

Invariably, as the research progressed and more data were garnered on the individuals and activities pivotal to both casual formations, awareness of my informational resources about either side threatened to generate acute ethical and practical difficulties. As I illustrate later, the greatest everyday difficulty that I encountered relates to the ethical criticism of my potential betrayal of one side against another. Hobbs (1993, p. 57) describes how, in his research of both sides of a rival subcultural milieu (in his case, police and criminals), one research site was barred from him on the grounds that information about it might be relayed inadvertently to the opposition. Similarly, in talking with casuals on either side, I have been no stranger to the micro-politics of deliberate exclusion if a discussion turns to a forthcoming fixture and the proposed arrangements one gang is contemplating for tackling the other. (The element of surprise is often crucial to which side is successful in confrontations.) A more common practice has been for me to abandon one site of study in the run-up to these fixtures, ensuring that this altered research procedure is known to the group with which I retain contact and who are liable to increase inquiries about their rivals' potential numbers and likely meeting point on match day. The majority of the latter group accept my explanation that this temporary disengagement is purely precipitated by a desire to avoid being misread as a dangerous, intervening agent in their football-related violence. My candor tends to have a rhetorical dimension for the more committed casuals, who are not keen to acknowledge the implication that there is anything to fear from opposing casuals with whom I could have contact. En route to one match, a long-standing casual reassured me,

Some of the younger [casuals] might reckon you've let them [rival casuals] know what we're doing today for an ambush. But that's what we want! We

want them to come at us with their full mob for a proper "go." We're not worried about what they do. We'll wipe them out.

Additionally, there is no reason why information about the intentions and activities of the rival mobs on these occasions cannot be collected retrospectively. The subsequent interviews are typically graphic enough to provide illuminating accounts of the differing fits between the individual and collective thinking behind pre-matchday arrangements and their predicted consequences, and how events actually unfolded.

With both subcultures, the research was openly presented as "for a book." Active, covert research, or spying, would not be required in Aberdeen, or dared in Edinburgh. Armstrong (with Harris, 1991, p. 431), Corrigan (1979, p. 13), and Whyte (1955, p. 300) successfully adopted my research stance in initiating their respective fieldwork, and I saw no reason to depart from it. Hibs casuals would notice immediately a stranger in their midst, who would suffer the same ignominious fate as one erstwhile football sociologist in Leeds, in being chased back to the local train station (Armstrong, 1993). To support professional claims, I always carried my previous working papers on football hooliganism. Only a few casuals wished to know more on my funding source and other work at Aberdeen University; the consensus view remains not unfavorable, if only because I have a "cushy number" (easy job) researching football and youth culture (cf. Parker, 1974, p. 216). Indeed, many casuals are themselves not averse to the matter of compiling information and data on soccer hooliganism. Some have undertaken project work on the subject at school or compile scrapbooks and photograph albums at home. Primarily at major fixtures,¹² it is a common sight to see individuals breaking away from the main body to take photographs of the aggregate striding through city centers or preparing to engage in violence.

The major difficulty in sustaining the book-compilation line is any lack of productivity; redressing this imbalance may lead to problems where research is comparative. After a couple of years' staggered research with the two casual groups, and still no book, some would cast aspersions on my dedication or credentials. This was particularly difficult to handle in Edinburgh, where re-entry was infrequent and therefore less routinely achieved. After an excursion abroad with Hibs casuals, during which I was regularly chided about low returns, I felt I had to publish something on their activities. Fortunately, an article on Scottish fans in *The Herald* afforded an excellent entry point for the publication of ethnography on the Edinburgh hooligans (Giulianotti, 1992).¹³ It was well received by all the Edinburgh lads with whom I have spoken since publication; conversely, the few Aberdeen casuals to have read it maintain that it draws unwarranted public attention to their east-coast rivals.

The most effective strategies that the researcher has for renegotiating field access are prioritization and snowballing. Polsky (1967, pp. 124-125) defines snowballing thus: "Get an introduction to one criminal who will vouch for you with others, who in turn will vouch for you with still others." Snowballing may take years when starting with someone on the interstices of the research group and the wider society, or at the metaphorical base of the subculture's pyramid of status. Chambliss (1975) spent a decade in Seattle, working up from petty criminals and hustlers to the apex of political and financial corruption. Identifying and seeking out an influential "gatekeeper" can circumvent much of the time-consuming and stress-inducing experiences of entrée.¹⁴ Armstrong (1993) utilized the popularity and associations of two leading Sheffield United hooligans to snowball right across his loosely knitted research group. Plain luck can have the greatest influence on who is prioritized for entrée and snowballing: Liebow (1967, pp. 238-240), for example, literally bumped into Tally, his research's eponymous hero, after barely a day stationed in the field. Perhaps the most beguilingly fortuitous researcher, reassuringly fictitious, is Berger and Kellner's (1981) student of sexuality who meets a woman en route to an orgy.

Luck, snowballing, and prioritization were to the fore during entrée in Edinburgh. My first attempts to prioritize one individual failed dismally. A day before I was due to return to Aberdeen from my seemingly pointless first expedition to the capital, I could still be located retrying the shops and pubs identified by the media as known casual haunts. As I was leaving one fashion shop, the owner suggested I try one pub before going to the Hibs-Dunfermline football match. I followed this lead, and fortunately, an hour later, was in deep conversation with three long-standing Hibs casuals, one of whom had met a few Aberdeen friends at a Scotland-England fixture in 1985.

A second suggested I contact a doorman at a club that night who "you'll definitely want to speak with." The advice was too well sourced and apparently genuine for me to ignore, although I had been warned by some I had spoken to previously that the club was out of bounds to strange faces; its denizens had been implicated in a recent riot and a nearby murder on separate occasions. Unfortunately, I was early and the doorman late for our unscheduled meeting, so I spent an uncomfortable half-hour seated in the foyer, listening to the intoxicated casuals "bouncing" in the adjacent lounge. On arrival of the doorman, I spent a further hour presenting my research identity ("I'm not an Aberdeen casual, I'm not a reporter, I'm not CID") and trying to respond to his inscrutable questioning ("If you're not a casual, what's your interest in us?") by the end of which I was doubting my own fieldwork motives.¹⁵ My hesitancy must have been

rightly interpreted as Aberdonian rather than plainclothes in source, for I was invited to meet him at an amateur football fixture the next day involving the new casual team. At the game I talked with a further 20 casuals. I left my home number with one group to pass on to the reputed leader, whom I had originally sought to contact. His friends phoned me to arrange a meeting in Edinburgh, so that within 1 week I had secured a strong foothold among both middle-class and working-class Hibs casuals.

Fieldwork Dangers: The Politics of Observation

Conducting participant observation exposes the researcher to numerous physical risks and professional dilemmas, which must be negotiated and renegotiated. Axiomatically, the first order of physical dangers encountered in the field varies proportionally with the criminal or violent habitus of the subculture researched. This is acutely recognized when attempting entré, during which the ethnographer may find research and personal safety somewhat incompatible. A most notorious illustration was provided by Ken Pryce's (1979) studies of Afro-Caribbean culture, which resulted in his murder in Jamaica. Although hardly in this league, I was recently informed by one Hibs casual that during my first contact with the gang, some had held a private view that I should only depart "in a body bag."

Snowballing is not a fool-proof method of securing safe access to all associated with a deviant subculture. Armstrong (1993) asserts that throughout participant observation study, the researcher is continuously locked into a form of renegotiation with his subjects, no matter how ritualized or repetitious this may be in content. New faces are accidentally bumped into before introductions; others may retain symbolic autonomy from the influential by ignoring or rejecting the stated acceptability of the researcher. In testing circumstances, the researcher must guard against his or her own complacency and egotism, recalling the fragile contingencies of prior associations that legitimize his or her presence with the subject group. Testing the loyalty of those prioritized, toward a fellow member or the outside observer, is a scenario too dangerous for the latter, again underlining the parameters of going native.

Parker (1974, p. 220) notes one faux pas that he committed late in his research, when arguing with one of his young subjects; refusal to cede ground led him to worry that his security and the security of his research were endangered. My major blunder was committed during extended ethnography with Hibs casuals. After I and a dozen others had been released from a Belgian prison, another casual "pulled me up" at the train station, picking a sealed brown envelope from my back pocket. "Is this what they paid you for grassing us?" he enquired in all seriousness,

before tearing into my unwritten postcards. I made the mistake of laughing in his face, a folly only neutralized by the intervention of a friendly casual. But lack of sleep and judgment saw me err again, when I became involved in another disagreement with the same individual when we were finalizing travel arrangements. This time the others witnessed the altercation in silence. I had no choice but to take the extreme option of the stigmatized and remove myself from the fray (Goffman, 1963, pp. 122-123). He accused me of being "a grass" as I chased after the next train.

Only time in the field can allow the participant observer to decode accurately the gray areas between threat and mere banter. As Moore (1977, p. 96) notes, "There are no textbooks which tell the sociologist how to interpret expressive behaviour." Three times on a train journey to London one new face told me I'd be forcibly ejected; I was keen to alight voluntarily but was reassured by established contacts that this was merely his macabre sense of humor. It transpired that their advice was as truthful as his threats were prescient. At Huntingdon two fans fell out of a carriage door and were killed by oncoming traffic; meanwhile, at Victoria Station, the would-be combatant made his apologies to me, but the graphic banter continued.

I have faced similar harassment from Aberdeen casuals, usually premised on the comparative dimensions of my research and my professed favouritism toward their Edinburgh rivals. At the beginning of the 1992-93 football season, if the subject of the two casual gangs' respective capabilities was raised late on a weekend night, there was always the chance I would be backed into a pub corner with an accusatory finger in my face. In this period, Aberdeen casuals' fears of betrayal were at a height prior to a Hibs-Aberdeen match in Edinburgh. A long-standing Aberdeen casual met one of his Hibs equivalents while working in Edinburgh; he inferred, wrongly, from their brief exchange that Hibs had been informed of Aberdeen's likely travel arrangements, and that there could only have been one "mole." A week before the match, I spent several hours attempting to repudiate rumors of collusion, "grassing," and "clipping"¹⁶ but probably only succeeded in spreading the calumny.

I remained undecided about traveling with the Aberdeen casuals up until match day. I had avoided attending this match in the past, just after first contacting Hibs casuals, but had been to the last Easter Road (Hibs' ground) game. I decided that my absence would be interpreted as a tacit admission of guilt, and I met up with the travelers at the station, staying primarily with long-standing friends who would not question my integrity. It transpired that when we alighted at Haymarket, there was no welcoming party of Hibs casuals, an observation that was not allowed

to bypass the source of the rumor. But if, by chance, 100 "tooled-up" opponents had been waiting, my return journey would have been more eventful, and the research badly wounded.

A secondary danger faced by the participant observer is the influence that his research subjects may seek to have on his conclusions. There are inevitably stocks of knowledge bandied around within all deviant subcultures which the researcher simply cannot report, for fear of his and others' prosecution, as my field experiences simply confirm.¹⁷ More awkwardly, individuals may seek to present an idealized form of collective self, utilizing the researcher as an intermediary for wider, public communication. Both Aberdeen and Hibs casuals put it to me in forceful terms that each mob is number one in Scotland-the hardest and, to a lesser extent, the trendiest. Recent fights between the two have been inconclusive, inflicting more talk than injuries on each other. Subsequently, I refrain from making particularly conclusive statements on the objective superiority of either. But, even equivocation can have deleterious results that border on contamination of the research. One Hibs boy suggested, "If you don't say we're number one, I'm gonna . . . do ya, and that's a promise." Eventually, he insisted he would prove his point by running into the middle of the Aberdeen mob and shaking my hand the next time the two met. Although he would almost certainly have become involved in fighting had the opportunity presented itself, if he had attempted this ambitious maneuver it could still be argued that my silence had had a direct effect on the research participant's behavior.

Hobbs (1990, p. 7) notes that the ethnographer requires flexibility in dealing with a subject's criminal activities; otherwise, research relationships may be jeopardized. Polsky (1967, p. 127) is far more dogmatic, asserting that the researcher must choose where the limits of his participation lie.-In making the boundaries apparent to his subjects, the researcher must ensure that he is never tempted or engineered into their transgression. My own rules are that I will not get involved in fighting or become a go-between for the two gangs in organizing fights. Clearly, the latter activity would have serious ethical, legal, and methodological problems, in contaminating the research procedure by giving the two gangs a contrived advantage over the police, who are established actors in the social processes of football hooliganism. (It would also lead, of course, to my arrest and prosecution for masterminding serious breakdowns in public order, but most academics are more preoccupied with the "good" reputation of their profession).

Participation in football-related violence is even less viable for these reasons. whenever these outbreaks are likely to occur, the participants are under no illusions that I will offer any material or symbolic support for their actions. As Albritton (1991, p. 52) notes of his initial research with

the police, such a voyeuristic involvement contains no interactional obligations: "I observed all of this, but I did not participate in it. I could always walk away from it when tired or bored." Unlike Humphreys's (1970) "watchqueen" role in the participant observation of male sexual encounters, my research exigencies do not stretch to a facilitating role in which my presence during aggressive or violent confrontations can be construed as decisive. I have commiserated with casualties on either side on the lack of appropriate circumstances in which a full-scale battle could reasonably point to which formation had established a supremacy over the other *mutatis mutandis*. At no stage have I sought to intervene. Consequently, a finity to my participation in the host cultures' activities confirms my avoidance of Humphreys's (1970, pp. 169-170) antinomian form of sociology, which he christens "situation ethics," but which translates as a manipulation of individuals to the point of their personal endangerment in the perverse interest of contriving research findings (cf. Warwick, 1982).

Fortunately, I have never yet been in a "backs-to-the-wall" scenario, where involvement equates to self-defense. After experience in the field, I've developed some understanding of which individuals are best to follow in particular circumstances. The nearest I have been to personal attack was in Belgium with the Hibs casualties, when a battle broke out with immigrant Moroccans. Around 20 of us were chased to a metro station by knife-wielding locals, and it was only the punctual arrival of a tube-train that saved us from the skewer. I owe my own escape to following one "top boy," who was evidently disinterested in becoming caught up in this "jailbait" situation and headed for the front of the mob, while the violence went off behind.

Another danger faced during such research is arrest. This has occurred twice during my studies but has never advanced beyond the detention stage. Both arrests occurred following disorderly incidents in which serious injuries (serious neck lacerations, loss of an eye) had been inflicted on individuals. I had participated in neither incident but had been near enough to their respective loci to attract arrest by arriving police officers, in a rounding-up procedure that is routinely deployed afterward against football casualties.

Avoiding arrest by referring to my job status would introduce a variety of ethical difficulties. How could I regain the trust of my research subjects if I abandoned them to their fate? What guarantees could I give to the police that I did not involve myself in violence? What guarantees could I offer the research participants that my early release was not bought through providing evidence against them? Subsequently, when asked for my personal details at the station, I have declared my employment as "researcher," which generates no further enquiry.

This is not to suggest that the ethnographer's identity with the police and his subject group cannot clash more violently. During covert research at the 1982 World Cup, Williams (Williams et al., 1989, pp. 87-90) was arrested and beaten by Spanish police, an experience recounted almost as a sociological rite of passage. On the occasion of my ground ejection at Pittodrie at an Aberdeen-Hibs game, three research interests collided: ethnography with Hibs casuals, ethnography with Aberdeen casuals, and quantitative attitudinal research with Aberdeen Football Club, undertaken with the assistance of Grampian Police. Before the match, 150 casuals had fought in the city center, a nowadays exceptional piece of violence in Aberdeen representing a significant defeat for policing tactics, and which the local force was keen to rectify. Inside the ground at half-time, I tried to talk from the Aberdeen side, through segregation fencing, with some research associates from Edinburgh. Assuming that I was a hooligan arranging a post-match battle, an unfamiliar senior officer grabbed me, warned me he'd be looking out for me next time, and threw me out of the ground. The incident was more embarrassing for the fact that it would undoubtedly have been relayed on CCTV to the police control booth, manned by an officer whom I had interviewed in cordial fashion some months previously. I mentioned my research position only on the point of being ejected. Since then, I have been able to continue research with the assistance of both club and police; neither has displayed or expressed any doubts about my research status.

Native or Insider: The Political Semantics of a Research Insight

Lurking throughout this paper has been the dilemma over the differences between, and respective benefits of, an insider or native research position, and I conclude with a brief expansion on some of the surrounding issues. My argument is that, as a relative insider of the Aberdeen casuals, I have retained sufficient personal, professional, and ethical distance to eschew any identification as a native. As well as sharpening Lindeman's blunt equation of insider and native in a processual manner, this emphasis upon a distinctive classification goes against the research precepts of the ethno-methodologists (cf. Garfinkel, 1967). A Schutzian concern for disclosing the "common sense" behind actors' accounts combines with the ethnomethodologists' critical stance toward "scientific rationality" and, accordingly, any method that imposes meanings and motives upon research subjects (cf. Wallis & Bruce, 1983). The most explicit native accounts are regarded as the most valid, with the sociologist's function reduced to a simple, reportorial role in transcribing the actor's practical reasoning.

Epistemologically, this position accords excessive power to the actor's authorial intentions and explicative capacities in relation to his or her structural location. It deprives sociological research of a reflexive and critical potentiality and furnishes the discipline with a Pyrrhic insight on subjectivity that will then be superseded by methodological journalism or, more consistently, simple autobiography. In the research field of football hooliganism, there are already enough autobiographical studies (e.g., Allan, 1989; Ward, 1989) and prurient exposés (e.g., Buford, 1991; the video documentary *Trouble on the Terraces*, 1994) to satisfy the public appetite for describing fan violence, without genuflecting sufficiently toward an adequately social understanding.

Conversely, my personal experiences in the field with deviant subcultures confirm the positions of Gadamer (1975) and Feldman (1991) on the hermeneutics of social research. Social actors are bound intersubjectively by prior ontological frameworks (or prejudices) that an interpretive sociology can identify in the narratives of actors. As Feldman (1991, p. 13) notes, "In a political culture the self that narrates speaks from a position of having been narrated and edited by others-by political institutions, by concepts of historical causality, and possibly by violence." My research role of relative insider therefore manifests itself through an empathetic and pre-informed entree to the cultural values of the research subjects. The major benefit of this initial insight is identified in Bourdieu's (1977) technique of participant objectivation, which explicitly warns,

The kind of questions which an observer will ask are likely to produce normative, value-oriented statements about what it is believed ought to happen, rather than a valid description of "what goes on." (Jenkins, 1992, pp. 48-49)

Subsequently, I have avoided pursuing knowledge-building on the framework of previous sociological arguments on football hooliganism. Classic academic discourses on the phenomenon emit evidence for its "rules of disorder" (Marsh, Rosser, & Harré, 1978) or its "rough" working-class socio-genesis (Murphy, Williams, & Dunning, 1990, pp. 129-156), but which I interpret as manufactured through a lack of research rapport.

It will not have escaped the reader's attention that masculine pronouns have predominated in gendering both myself (and the researcher in general) and the casuals with whom I interact. This is neither accidental nor due to ethical lassitude, but it does harbor research prescriptions. There are female associates of both casual formations, but their involvement is usually limited to personal/sexual relationships with casuals themselves; they tend to be tangential to the groups' intra-social networks and violent competition with rivals. However, my skepticism on the

viability of female sociologists undertaking participant observation with football hooligan groups such as the Scottish casuals is based not on the publications evidence of there being no precedent. Rather, it is important to blend in (or pass) among the hooligans without exciting attention from rival casuals or police, to avoid significantly influencing the latter's actions toward the research subjects generally. The majority of the casuals studied share a culturally engrained, masculine deference toward women with regard to violence; the presence of a female researcher in confrontations would add an artificial degree of complexity to proceedings which the ostensible appearance of one more fellow/rival casual does not. Some forms of feminist research methodology theoretically may justify such an interventionist approach, in the interests of gender emancipation (Lentin, 1993). However, in explorations of subcultures that display a "commitment to violence beyond any reason comprehensible to others" (Katz, 1988, p. 100, quoted in Hobbs & Robins, 1991, p. 571), the minimal personal control that a catalytic researcher subsequently exercises over the precipitant events cannot, in my view, be effectively justified to academic peers, the court of law, or the hospital doctor.

Similar difficulties would be encountered by non-Caucasian researchers studying hooligan formations that were predominantly white. There are two black soccer casuals known to me who regularly attend matches, but the formations with which they are associated are noted on the Scottish casual scene for their ultra-nationalist, racist politics (in which one of the two openly participates). Again, a non-Caucasian researcher would highlight some of the latent bigotry underlying Scottish football's white, male fan cultures, but beyond these (already known) findings, the study may influence more than it observes. For those male researchers who have already undertaken studies of football hooliganism, their personal communications with me underline the greater role that they attach to gerontology in enabling research. One professed that he was abandoning the academic field of football hooliganism, as "I'm too old for the ducking and diving"; another researcher has since reported that his anomalous age was explained away to a group of hooligans by a friendly member, who claimed him as "big brother" (Finn, 1994, p. 122).

Therefore, my main political and ethical concerns with participant observation are located elsewhere in relation to its reception by academia. There remains the final danger of being seen to adopt the research group's perspective uncritically. As much of my experiences in the field suggest, the participant observer's humanistic interest in finding and disclosing empathetically the host culture's outlook is not achieved without subjective discomfort, the acuity of which is a fair index of the researcher's avoidance of going native. Upon

encountering particularly alien perspectives, the researcher may experience a sense of unease in squaring these with his or her native values (Karp, 1980, p. 94). Anxiety may be experienced in introducing himself or herself, coming to ask, "What right had I to ask these questions anyway?" (Newby, 1977, p. 117). Indeed, the researcher may become hypersensitive to his or her obtrusive role and may respond by being "invisible," with the result that research opportunities are lost in the process of becoming innocuous (Danzinger, 1979, p. 520). Alternatively, a more conspicuous presence may see social gaucheness become a source of humor for prospective research subjects (Parker, 1974). Past research has defined these field emotions of anxiety, stress, doubts, fear, and frustration as the "dysad-aptation syndrome." They are based on an ambiguous social identity: How close is the researcher able or willing to get to the new culture when trammled in the latent mores of his or her own?

Lear (1978) indicates one solution, which suggests that any translation between the host and sending cultures is irrelevant. He argues that in quickly learning a new language, and its underlying social and ontological conventions, the individual may jump freely between the native and acquired perspective. Going native is quite easily achieved in the process. But, against this, the social researcher is always moving, mentally and physically, between the new "tongue" and between the terms and conventions of established sociological or anthropological discourse. Intellectually and pragmatically, the former must remain an instrument of the latter.

To give an illustration from my own research, I have written a paper discussing the local social categories and terms that Hibs casuals use to differentiate themselves from other Edinburgh subcultures. But I have located these rather unique terms within more objective social categories (such as space, time, the body) that are sociologically premised (Giulianotti, 1994b). (I have already noted the researcher will place further, interactional barriers on going native by constructing personal parameters for participation, which appeal to research interests, e.g., no violence, rather than to interests of the research group.) As a consequence, the researcher is always aware of a relatively ambiguous status within the group. Outside of the vicissitudes of conducting covert research, only self-delusion or low opinion of research subjects would buttress the belief that the researcher had been accepted by the research participants as nothing less than an unqualified native.

This is not to argue that the researcher cannot fear for the collapse of his or her intellectual and sociological *bien-pensant*, as the cultural practices of the group attract a willing participation. Parker (1974, pp. 219-220) recalls a drunken evening in a youth club, when his own excesses symbolized his interpretation that "these were the days when I was very nearly one of The Boys." Hobbs (1993, p. 48) and Armstrong (1993, p. 18) confess to the necessity of drinking bouts with research subjects, and given the centrality of pub culture in the casual's match-day experience, I have no wish to proselytize on temperance. (Alcohol can loosen tongues and induce findings that years of research have missed; Punch, 1979, pp. 10-11.)

Additionally, when one is observing the anticipation and conjecture that a major fixture excites, sometimes months before match day, it is impossible to stem the flow of adrenaline, the psychophysical high of what Roland Barthes (1975) terms *jouissance*. At such moments, the chemistry of expectancy and foreboding is distilled into the first 4 or 5 hours of match-day casual aggregation and then compressed into the 4 or 5 milliseconds of collective agitation that greets the first sighting of the rival casual mob. However, this sensation does not convict its recipient as a native; it is, rather, an overpowerment by the psychosocial effects that the casual subculture can provide for those it shelters. Moreover, socialization within academia cannot be written off with the privileged epistemology that going native requires. Rather, my research experiences of *entrée* and everyday field risks highlight a more marginal identity between academia and research subjects. The ambiguities of this condition are succinctly captured by Jacob-Pandian (1975, p. 170), in a framework that reunites anthropologist and sociologist in their experiences of applying participant observation:

The anthropologist slips through the network of classifications that normally locate states and positions in cultural space and in relation to the group studied the anthropologist is in a state of liminality with a mind that is a blank slate, on which is inscribed the knowledge and wisdom of the group. . . The anthropologist acquires the wisdom but retains the ambiguous state of existence. The anthropologist does not become a native but is forever in the process of becoming a native.

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Notes

¹ Platt (1983) argues that before 1945, studies such as those by the Chicago School failed to apply singularly the modern definition of "participant observation" (an empathetic interest in cultural meanings) and therefore do not deserve the accreditation of the method per se. I would argue against this reading. First, Platt constructs an excessively rigid and canonical definition of participation observation. Second, an equally constrictive and wholesale application of meaning-orientated participant observation is neither desirable nor viable.

² The two other issues identified by Weppner (1977) are the representative and generalizable qualities of the data, and the extent to which the researcher influences the behavior of those studied. I discuss the issues of entrée and everyday risks only, partly for reasons of brevity but primarily because they represent the principal research issues in my fieldwork experience.

³ The rivalry between the two subcultures reached a peak in the late 1980s. In March 1985, a Hibs fan was critically injured after 500 Aberdeen casuals attacked 100 Hibs opponents; 7 months later, Hibs casuals threw a petrol bomb at Aberdeen casuals during fighting in Edinburgh. Generally, there has been regular fighting between the two gangs since the early 1980s, although the numbers involved have declined dramatically to a current maximum of 200, from a peak of 1,000-1,200 (cf. Giulianotti, 1993).

⁴ The political and ideological function of the participant observer is equally strong in an anthropological setting. Jacob-Pandian (1975, p. 171) argues that the anthropologist inevitably reinforces the power of the West in the receiving culture by being perceived as its representative and reinforcing Western ideals of appearance and demeanor.

⁵ Feldman (1991, p. 68) notes that the definition of the noun stiff has changed. Before the Ulster Troubles, it related purely to a corpse, but since 1968 it has acquired political connotations for terrorists and the sectarian communities.

⁶ The distinction is an important one. Casuals are strictly defined by their dedication to a particular form of fashion (expensive, designer leisurewear) and an interest in fighting opposing casuals. In Aberdeen, the ubiquity of the casual fashion style in the 1980s procured the emergence of "trendies," who were loosely associated with the genuine hooligans through appearance but not interest in violence (Giulianotti, 1993).

⁷ There are clearly epistemological and ethical dangers in claiming to have carried out participant observation while an undergraduate. Billig (1985, p. 445) criticizes John Ray's (1985, p. 441) claim that he joined the Australian Nazi party in order to conduct participant observation "as a sociologist." Ray's research began when he had just begun his student career, hardly a basis for claiming professional status. I was never involved in football-related violence before my student days, nor would I pretend to have undertaken participant observation in that period.

⁸ There were two reasons for my caution. First, I was aware of the possibility that the imputation of "leadership" lacked any significant evidence and might simply have been an instance of "cabalism," in which "behaviour which was to a large degree unorganized, spontaneous and situational, is seen as having been well planned in advance as part of some sort of conspiratorial plot" (Cohen, 1980, p. 63). Second, well-intentioned but speculative outsider advice on entrée to a subculture can prove potentially dangerous.

⁹ One week prior to my arrival in Edinburgh, a report published in Scotland's "quality" Sunday newspaper claimed to offer an inside story on Hibs casuals (Scotland on Sunday, November 11, 1990). I learned that some of the subjects the reporter had talked to had been so annoyed by the story that they had attacked him in an Edinburgh nightclub.

¹⁰ Jean Baudrillard (1983) defines this eventuality as one of "simulation," in which the real social phenomenon actually emerges as a reproduction of its image, the form in which it first was born.

¹¹ Dennis (1988, p. 44) notes that this lack of cross-cultural ethnography has an ethnic inflection in race relations research, with black researchers relegated to studying one culture, due to their perceived unacceptability in white communities.

¹² By "major fixture" I mean football matches involving the meeting of either two leading teams (e.g., Aberdeen vs. Glasgow Rangers) or two sets of supporters with particularly strong rivalries (e.g., Hibernian vs. Hearts, the "Edinburgh derby").

¹³ Here, the question of influencing the research subject's behavior arises. At no time during the excursion did I consider or openly reflect on the idea that I would specifically publish the accruing ethnography. Consequently, no one would have been acting under the impression that his behavior would receive quick reportage.

¹⁴ Gatekeepers are an attractive resource for field entrée, equipping the researcher with potential contacts and worthwhile codes of practice. But, they also possess the power to channel the researcher in directions that he might not otherwise have taken (Hammersley & Atkinson, 1983, pp. 63-68).

¹⁵ The post-war spread of covert policing tactics itself ensures that entrée to a subculture is more difficult than before. It is difficult to imagine, for example, today securing the smooth access enjoyed by Whyte (1955, pp. 291-292) to an Italian-American gang, via the social services.

¹⁶ The word is the present participle of the Scots verb *to clipe*, meaning "to tell tales" (Chambers7 Twentieth Century Dictionary).

¹⁷ The most recent BSA Statement of Ethical Practice underwrites a general, professional commitment to anonymity, privacy, and confidentiality in dealing with research participants: "Personal information concerning research participants should be kept confidential. In some cases it may be necessary to decide whether it is proper or appropriate even to record certain kinds of sensitive information" (British Sociological Association, 1992, p. 705).