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# Bias as a Research Strategy in Participant Observation: The Case of Intergroup Conflict

JOHN DRURY  
*University of Sussex*  
CLIFFORD STOTT  
*University of Liverpool*

*Participant observation (PO) is one of the more fruitful methodological approaches to studying crowd behavior. This article argues that, since crowd behavior characteristically takes place in a context of intergroup conflict, PO may involve having to take sides to gather data. Possible sources of bias within a partisan PO framework are examined, including bias in access, in observation, and in analysis. Two examples of partisan research on crowd conflict—a demonstration riot and an antiroads occupation—show that only the first of these forms of bias is unavoidable. However, it is posited that limited access to one of the groups in conflict is more than offset by the quality and quantity of data gathered from the other group and the subsequent objectivity this affords within the data analysis.*

**T**his article argues that partisanship in participant observation (PO) research may be necessary in research settings involving intergroup conflict. While a number of previous researchers have made the case for partisanship in practical terms (e.g., Fantasia 1988; Green 1993), the present article adds a systematic examination of possible sources of bias in partisan research. Through two examples of PO research, we will show how bias, in the form of taking sides, is necessary for data collection on crowd-police conflict. Whereas less-involved methods may still allow data collection, and while partisan PO may have a price in terms of access to one of the sides in the conflict being studied, it still enables access to a far greater amount and finer quality of data than do other frameworks.

The article is structured as follows. First, we briefly review methodological approaches to studying the crowd to illustrate the necessity of a PO framework. Second, we distinguish different types of potential biases arising from partisan PO. Third, we show the applicability of partisan PO and how we approached the potential problems of bias in studies of two conflictual crowd events: a demonstration riot and an antiroads occupation. Finally, we argue



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that the pursuit of objectivity itself requires that we acknowledge the inevitable role of subjectivity in social scientific research.

### HOW SHOULD WE STUDY THE CROWD?

How we study crowd events depends in large part on how we conceptualize them. Early writers on crowd behavior, such as Tarde, Taine, and Le Bon, were horrified by examples such as the Paris Commune and sought to explain and combat what they saw as a menace to civilization (Barrows 1980; Van Ginneken 1985; Reicher forthcoming). Since it was obvious to these “gentlemen observers” that the mob was essentially mad, bad, and dangerous to know, research could legitimately be carried out from the comfort of an armchair, using secondary sources rather than actually collecting data on crowd participants’ thoughts and feelings.

With the widespread adoption of the laboratory experiment, social scientific research on crowd behavior became more sophisticated. Thus, Festinger, Pepitone, and Newcomb (1952) initiated a tradition of controlled studies into the psychological processes underlying the presumed irrationality of crowd behavior by showing that the more anonymous participants felt, the more likely they were to express hostility. A key assumption of this “deindividuation” tradition was that involvement in crowds leads to a loss of identity and hence of normative control over one’s behavior. Deindividuation was, in fact, a direct descendent of the ideas expressed earlier by Le Bon (Cannavale, Scarr, and Pepitone 1970). Deindividuation experiments, therefore, merely attempted to elaborate on rather than question the conceptions of the crowd held by the gentlemen observers (e.g., Zimbardo 1970; Diener 1980; Prentice-Dunn and Rogers 1989).

A later series of experimental studies debunked the notion that participation in crowds leads to loss of identity and hence atavism, suggesting instead that particular social contexts may make particular identities salient (e.g., Reicher 1984; Spears, Lea, and Lee 1990; Reicher and Levine 1994). Thus, for example, Reicher (1987), using a version of the deindividuation experimental paradigm, showed that anonymity enables in-group members to express themselves in ways an otherwise powerful out-group might disapprove of—yet be in line with their own identity (for example, antinuclear comments). But the laboratory experiment still views the crowd (or, more precisely, *analogs* of the crowd) from the outside. Hence, these authors acknowledged that other methods are necessary to show how crowd participants might construct definitions of themselves and their social worlds (Reicher 1987). The experimental situation, in which participants have definitions of

context effectively imposed on them, may be a good imitation of much of everyday life in which people are not necessarily arguing or acting to change their social context; however, it cannot serve as an adequate model of social relations in situations, such as crowd events, where conflict and change are occurring (Stott and Drury 2000).

Within psychology and sociology, it was only in the 1960s, when academics themselves took part in crowd events—as in the anti-Vietnam protests, for example—that they began to take the perspectives of crowd members seriously. The first theoretical advance in the study of the crowd was the suggestion that crowd behavior was determined by norms rather than drives or instincts (e.g., Turner and Killian 1972). In contrast to earlier approaches, this “emergent norm theory” was as concerned with peaceful crowds as violent ones. However, like the earlier approaches, emergent norm theory examined the crowd in isolation and therefore sought the explanation for collective behavior solely within the crowd itself. Yet, both historical studies and more recent research within social psychology suggests that crowd events are characteristically *intergroup* encounters, where the rioting crowd is in conflict with another group—often the police (Stott and Reicher 1998a, 1998b; Reicher 1996, forthcoming).

So, to understand the development of behavior and perceptions within a crowd—for example, how participants turn from peaceful protest to violence—researchers need to examine the *interaction* between crowd and police. This means studying not just crowd participants’ behavior but also how they understand their social context, including the behavior of other groups involved in the crowd event.

Research on social movements by sociologists and social historians has attempted to take seriously this argument that collective behavior can best be understood by examining participants’ own understandings in relation to their (changing) social context (e.g., Thompson 1980). Thus, for example, articles in a volume edited by Tilly and Tilly (1981) examine the way participants in various class struggles in eighteenth-century Europe came to organize themselves into social movements around the theme of injustice, and the changes in such movements in the course of history. Similarly, Fantasia and Hirsch (1995) analyze how the Islamic veil became transformed from a symbol of traditionalism to one of female emancipation through women’s involvement in the independence movement in 1950s Algeria.

Various source types have been used in this kind of research, including newspaper accounts (e.g., McAdam 1982; Kriesi et al. 1995), legal documents (e.g., Thompson 1980; Stevenson 1987), and in-depth biographical interviews (e.g., Della Porta 1992). Although some of the material analyzed in such studies would count as being contemporaneous (movement docu-

ments, for example), the approach is largely post hoc. This means that details of the nature of interactions in actual crowd events would be subject to participants' memory limitations and distortions, as well as their functional reconstructions of these past events.

To overcome these problems, it is necessary to gather contemporaneous data and to sample from behaviors and perceptions over the duration of a collective event (Klandermans 1992; Benford and Hunt 1995). Extensive literature, however, points to the practical problems in obtaining contemporaneous data on crowds. For example, crowd events may happen quickly, often with no prior warning (at least to outsiders), so researchers may have difficulty being at the right place at the right time (Milgram and Toch 1969; Wright 1978; Reicher 1983; Adams 1994). In relation to student protests, for instance, Hirsch (1990) points out that the organizers' need for secrecy makes it difficult for the researcher to know about events in advance.

What is needed is a methodological approach to data gathering that is of suitable flexibility or "opportunism" to fit the unpredictability of crowd behavior. PO, or ethnography, is the supremely opportunistic data-gathering framework, being by its nature flexibly adaptive to possible changes in its research topic and setting and open to the unexpected. In their review of crowd research, Milgram and Toch (1969:603) give PO the highest recommendation: "The surest road to theoretical advance in this field is . . . participant observation by social psychologists of collective outbursts." There has also been a recent turn to PO in social movement research (e.g., Johnston and Klandermans 1995; Melucci 1995).

Since the forms of crowd behavior of interest here should be understood in terms of intergroup conflict, PO would mean joining in with one of the groups involved and opposing the other. Although bias may be an inherent danger in PO, most ethnographic researchers believe it can largely be avoided. But when the researcher openly takes sides, critics might argue that bias, and hence tainted research, is inevitable. To respond to such criticisms, we need to examine more closely why partisanship might be necessary in PO research on intergroup conflict. We can then go on to distinguish types of bias to suggest that bias in the form of taking sides need not mean bias in analysis and research reporting.

### WHAT MIGHT TAKING SIDES MEAN FOR THE STATUS OF OUR DATA?

Taking sides in intergroup conflict in the course of research appears to entail at least two forms of bias from the outset. First, the mere fact of align-

ing oneself with one group is itself a bias and seems to conflict with the usual stated aim of social scientific research to put distance between researcher and researched. Second, such a role seems to entail acting as an advocate in the wider society in which the intergroup conflict is situated. As such, partisanship in PO appears to overlap with action research (e.g., Rowan 1974).

However, Fantasia (1988) contends that, if research takes place in a conflictual context, partisanship is a necessary basis for any form of data gathering. When opposing groups have a great deal at stake, getting any data may be difficult since there may be suspicion of it being used against them.

In the case of the wildcat strikers Fantasia (1988) was studying, there was an understandable mistrust of people with notepads and clipboards, due to the previous role of such people in helping to manipulate the workers' workplaces and communities for management's interests. Fantasia's solution to this problem, and to the problem of being in the right place at the right time, was to participate as a worker and striker; indeed, Fantasia was a "complete participant" before he doubled up as an observer by starting to take notes on the strike.

In the second strike Fantasia (1988) studied, he was one of four workers active in discussing the prospects of industrial action over the lack of heat in the break room and in persuading others not to go to work. He later attended meetings with others attempting to challenge the union leadership in a subsequent election.

Similarly, Green (1993) argues that, when a minority with strong nonmainstream views is the object of investigation, it may be essential to show a basic sympathy with them to get started at all. Her criminology department colleagues criticized her, questioning how her research on the 1984–1985 UK miners' strike could be objective when she was involved in collecting money for the strikers. Yet, as a socialist, she was not detached from the subject matter: She wanted to help the miners win the strike, rather than merely describe what they were doing.

Wright's (1978) study of crowds shows that it is possible to get data even when maintaining a distance and not actively supporting one side rather than another. On the other hand, Wright conceptualized crowds within the "collective behavior" framework, whereby all crowds are seen as more or less interchangeable primitive entities groping toward structure: From this perspective, access to participants' subjective experiences was less important than simply documenting behavior.

By contrast, Fantasia (1988) and Green (1990) understood and analyzed collective action in its context of a developing movement. In this perspective, different crowd identities are important, and the behaviors of crowds cannot be understood abstracted from the intergroup relationship in which they are

embedded. By taking sides as they did, Fantasia and Green were each able to collect data on changes in the consciousness of participants.

In carrying out PO research on movements that were trying to resist or bring about change, Fantasia (1988) and Green (1990) themselves had to act to resist or bring about change. Undoubtedly, Green, and more especially Fantasia, influenced their setting through their partisanship: They helped maintain and possibly even escalate the very disputes they were researching. But in the case of research on collective struggles, the best research instrument may be someone who is already active and hence willing to lend their experience to the research process. In the studies of collective action by Fantasia (1988) and Brent (1992), for example, it is apparent that, if there were not such already committed people willing to adopt the dual role of activist/participant observer, that research would never have been done.

If “bias is the human condition” (Aguilar 1981:22), we need to go beyond simply condemning partisan PO because it entails social alignment and advocacy. Instead, we need to (1) examine the particular epistemological consequences of any such biases; and (2) consider the epistemological consequences of the alternatives. Taking the second of these first, it is commonplace in ethnographic research that the researcher’s presence in the setting may entail reactivity. In some contexts, the further the researcher attempts to stand outside the world she or he studies, the more she or he may influence those being researched to act in an artifactual way in response. Attempting to be objective or neutral is still a choice of position (Parker 1993), and the researcher’s “neutral” behavior may be subject to the interpretations of others, which may in turn artificially affect the setting or otherwise inhibit the collection of data.

This point becomes particularly acute where there is intergroup conflict. Some groups in conflict may regard researchers with suspicion (Wright 1978; Reicher 1983; Adams 1994). Thus, Kriesi (1992) discusses the rebellion of the research “objects,” who feel threatened by the interest of academics in their protest activities.

By treating competing collectives equally, the ethnographer risks the condemnation of both—as happened to Wright (1978), who was cast into a “villainous role” (p. 169) by both Chicano demonstrators and police—which hardly helps in collecting data. In his PO study of a prison, Jacobs (1977; cited in Whyte 1984) adopted a neutral position in his relations with rival black gangs; but since he was white, he was condemned as a “racial traitor” by white inmates: “No longer was there enough common agreement on social identities and social tasks to support more than the most superficial communication. Perhaps this demonstrates that neutrality itself is a role enactment subject to interpretation” (Jacobs 1977:223; cited in Whyte 1984:72). While

taking sides in research on intergroup conflict may therefore entail biases in the forms of social alignment and advocacy, it may also minimize the bias of reactivity.

We outline below the possible epistemological consequences of partisan PO in intergroup conflict and then show how we dealt with them in our own research on crowd conflict. Such epistemological consequences are themselves usually conceptualized in terms of bias (e.g., Aguilar 1981). The types of bias that might be particularly relevant for research on intergroup conflict in crowd events can be divided into three areas: partiality of access to materials; partiality in the researcher's observations; and partiality in the analysis.

#### Partiality of Access

Arguably, fellow participants are more likely to be willing to give the time for interviews and help make available other data (movement documents, for example) than participants in experiments or other methods in which the researcher has control: Shared identity increases cooperation (Turner 1981). Thus, Green (1990, 1993) argues that her close association and political sympathies with some of the striking miners gave her a credibility with the others that allowed for further introductions and interviews.

For the single PO researcher, in situations of intergroup conflict, the reverse side of the coin of this enhanced access to one side is, of course, restricted access to the other group involved (Whyte 1984; Jorgensen 1989). Green (1993) wanted to interview working miners as well as strikers, but only a minority were willing to help her when she was so obviously on the side of the strikers. Similarly, years after Fantasia (1988) left the workplace in which he had carried out PO, he tried to gather retrospective data on the strike, but a request to the company to see their records was turned down, probably because of his earlier involvement with the strikers.

Although the interest in the present case is primarily in the perspective of crowd participants themselves, since the crowd event is understood in intergroup terms there is also an analytic need for a certain amount of out-group (i.e., police) data. However, access is not the only potential impediment to grasping the out-group perspective.

#### Partiality in Observations: "Going Native"

Participant observation involves not only adopting the behaviors of those being studied but also their perspective: The researcher "becomes the phenomenon" (Jorgensen 1989). Ethnographers often argue that objective and truthful findings are more rather than less likely as the researcher becomes



involved directly with people in daily life; the potential for misunderstanding and inaccurate observation increases when the researcher remains aloof and distanced physically and socially from the subject of study.

This is part of the rationale for the turn to indigenous anthropology among researchers whose discipline has traditionally made a virtue of studying the Other rather than the Self (e.g., Aguilar 1981; Messerschmidt 1981; Stephenson and Greer 1981; Fahim and Helmer 1982; Altorki and El-Sohl 1988). As these discussions identify, going native—or in this case already *being* native—carries with it a danger of relativism (Fahim and Helmer 1982). In the search for subjective validity, the researcher may be reluctant subsequently to reflect on her or his own and participants' perspectives in terms of the overall picture. So, for example, Hammersley and Atkinson (1995) argue that the ethnographic study by Willis (1977) of school-student cultures of resistance uncritically adopts the perspective of one set of students in his analysis of the whole social situation. In the case of partisan PO research on collective action, the researcher's observations of an incident of intergroup conflict are likely to reflect the perspective of the in-group (crowd participants) rather than those of the out-group (police).

#### Partiality in Analysis

At the analysis stage of the research, the possible concern is with how pieces of data are interpreted. In the first place, what is the status of the accounts of fellow in-group members? Is the partisan researcher more likely than a researcher maintaining a distance to take in-group participants' accounts of conflict on trust (i.e., uncritically)?

In the second place, even where we are satisfied with the status of a particular piece of datum, where do we place it in our analytic scheme? The data gathered in PO research are characteristically treated to some kind of qualitative analysis, such as grounded theory (Glaser and Strauss 1967) or thematic analysis (Kellehear 1993; Miles and Huberman 1994). In the case of partisan research on intergroup collective conflict, the choice of analytic categories in which to place pieces of data might conceivably be shaped by the researcher's wish to produce results in line with the in-group perspective. All methodologies are potentially vulnerable to the researcher's desire to support particular theories and arguments. However, it might be argued that since in PO research on intergroup conflict, the data-gathering method relies on adopting the perspective of those being studied, and, since the analytic method is essentially about interpreting each piece of data after it has been gathered (rather than number-crunching preinterpreted data as in a questionnaire, for example), there is more danger, perhaps, of data being categorized

to tell the story that the researcher (*qua* in-group member) would like to see told.

### PARTISAN PARTICIPANT OBSERVATION IN PRACTICE: TWO EXAMPLES

We now examine two examples of research on crowd behavior to illustrate partisan PO as a research strategy. We detail how we dealt with the dimensions of bias described above and argue for the necessity of partisanship in this kind of research topic.

#### The Trafalgar Square “Poll Tax Riot”

On 31 March 1990, around 200,000 people marched to Trafalgar Square in London as part of a nationwide campaign against a new local tax system, dubbed the *poll tax*. The demonstration began peacefully but eventually became one of the biggest riots in the capital in 100 years. The research problem was to explain this transition in social-psychological terms. We present here a summary of our study of the event, based on articles published elsewhere (Stott and Drury 1999, 2000).

Stott participated in the crowd event as a demonstrator and used this role to record semistructured interviews and the “soundtrack” of the demonstration (e.g., shouts and chants from the crowd) on a hand-held tape recorder. Field notes in the form of a commentary, dictated into the tape recorder, made the soundtrack comprehensible (see Hammersley and Atkinson 1995). These contemporaneous data were supplemented by video data, newspaper accounts, thirty-eight post hoc interviews with participants, and police accounts (see below).

The study of the poll tax riot aimed to relate the pattern of events to the perceptions of participants. It was necessary as a primary step, therefore, simply to produce a description of what happened. Given Stott’s involvement, there was a potential problem here in terms of partiality in observations. Stott certainly identified with the anti-poll tax movement and did not feel “external” to the conflict he experienced—indeed, as a member of the crowd, he felt threatened by police charges into the crowd in the same way as other crowd participants.

Our principle in dealing with this potential bias was to treat Stott’s observations as simply another piece of data rather than as an objective version that could stand above and arbitrate between competing accounts. Of course, to stand back after the event and treat observations in this way means to shift

between roles. But, as Naples (1996) argues, “insiderness” and “outsiderness” are shifting locations in any case; indeed, in everyday life, people routinely perform multiple roles (Jorgensen 1989).

Thus Stott’s own field observations were triangulated with other material (Denzin 1989), including that from other demonstrators who may have seen events differently, as well as that material from other parties, such as police and press. “Consensus” was operationalized, and a triangulated account constructed, on the basis of agreement between the different parties (e.g., crowd participants on one hand, and police or press on the other) or between statements by any one of these parties on one hand, and photographs, audio recordings, or videos on the other. Where accounts diverged or only one source made a claim in relation to an event involving both crowd participants and police, the origin of the claim was cited in the research reports.

As with similar research (e.g., Reicher 1984, 1996), the study established that, although there was disagreement over the sequence and details of some aspects of what happened, there was a level of consensus within and between the two sides (police and demonstrators) over the major elements. They agreed, for example, that conflict began to escalate in Downing Street, that large numbers of riot police were deployed there and in Trafalgar Square, that property was burned in the square, that police cars were attacked, that looting took place in the West End, and so on. Thus, a story about the riot could be constructed over which both sides could agree. As with any description of an event, such an account was constructed rather than absolute. Yet, to the extent that it was consensual, it represented the reality as understood by the various parties and to which they jointly orientated. Hence, as well as providing a guide to the reader, the account served to identify the features of the event that were the focus of the analysis.

The analysis itself examined the differing perceptions of police and crowd participants to explain the pattern of events found in the consensual account. First, the starting point for the explanation was a conflict of perceptions between the crowd and the police: For example, while demonstrators saw the aggressive actions of a small minority within the crowd as antinormative and unrepresentative, the police saw the minority as representing the potential threat contained within the crowd as a whole. Second, the police had the power to impose their perception on the crowd as a whole, through organizational resources and use of riot-trained officers and horses. Third, crowd members saw the police’s action toward them as both illegitimate and indiscriminate: The whole crowd was now located in a context in which the police’s behavior posed a threat to their safety. Within this new set of social relations, violence against the police became redefined as legitimate self-defense and hence normative for the crowd as a whole. Moreover, the

shared identity in relation to the police meant that power relations became reversed. Where previously the crowd had contained divisions (“nonviolent majority” versus “violent minority”), the indiscriminate treatment by the police unified crowd members as expectations of mutual support within the crowd increased for actions against the police. Hence, crowd members felt more empowered to enact their identities. Finally, the violent response of the crowd confirmed police fears and expectations of a collectively violent crowd and led them to escalate their own defensive behavior.

Issues of validity, and thus the possibility of partiality in the analysis, arose in the study of the poll tax riot at two levels: first, in terms of the status of particular analytic claims about participants’ perceptions and second, in terms of the analysis as a whole.

When crowd participants recount their own feelings and behavior after a conflictual event, memory, attributional, self-presentational, and other social-cognitive biases might be expected to operate (Thompson 1978). Discursive psychology adds to the skeptical chorus on this point, arguing as a matter of principle that we cannot extrapolate from accounts to perceptions (Edwards and Potter 1992): All accounts are functional; they are intended to achieve various ends and should not be treated as a neutral medium revealing underlying cognitive states. However, Billig (1995), following Grice (1989), suggests that one of the functions of talk is the act of communication; the aim of the utterance may be to cooperate, to convey certain information about feelings and thoughts rather than to justify a particular self-construction.

The case for either self-constructive strategies, cognitive biases, or simple communication as an explanation for the content of post hoc material depends on the particular issue at hand. The more controversial an issue is between interviewer and interviewee, the more plausible is an explanation in terms of rhetorical strategy or memory omissions. By the same token, however, where there is a shared perspective between researcher and researched there will be less controversy and hence less motivation for the latter to distort her or his account.

Undoubtedly, actions that occurred during the poll tax riot were deeply controversial and may lead to questions about the validity of some of the participants’ accounts of their actions and perceptions. But, since most accounts of perceptions (e.g., “I felt angry with the police for their illegitimate behavior in acting against people who were just sitting doing nothing”) were consistent with contemporaneous data (e.g., shouts against the police for moving against those sitting down), we had some confidence in treating such accounts as reflective of perceptions at the time rather than as post hoc pretexts. Furthermore, given that both post hoc accounts of perceptions and contemporaneous material helped us make sense of the consensual evidence

(e.g., agreement that shouts and collective violence occurred for the first time at the point when the police confronted demonstrators at Downing Street), we felt justified in arguing in the analysis that the perceptions were used to guide behavior at the time.

Our approach to the validation of the analysis as a whole was based on the principle of coherence (see Habermas 1971)—that is, whether the consensual account and the analysis itself fit together as a whole. In this case, the *theoretical* account given in the analysis (conflict of perceptions, dynamics of power—see above) on the basis of participants' perceptions and feelings (about legitimacy, threat, self-defense, etc.) allowed us to explain coherently the evidence in the consensual account of what actually happened in the riot (see Reicher 1996).

The study of the poll tax riot produced far more data from crowd sources than from the police. Approaches were made to the Metropolitan Police in the week after the demonstration. While at first the senior officer contacted was eager to allow access to officers involved in policing the event, this access was never forthcoming. The fact that many of the cases arising from the investigation were at that time sub judice may have been a contributory factor, but other studies suggest that the police may have been more forthcoming had the research request originated "from the inside" (see Cronin 1995).

Subsequently, a small body of data from three participant officers was obtained by contacting another senior officer who was willing to help. One of them was the commanding officer of the police stationed in Whitehall during the conflict episodes. The accounts of these three officers were supplemented, and the analysis as a whole validated, through other sources of police material, including television and newspaper interviews, the official police debriefing report, and a separate study of riot-trained officers who commented on videos about the poll tax riot (Stott and Reicher 1998a).

The ethnographic framework was crucial to the analysis of the poll tax riot in two ways. First, participation provided Stott with an orientation to the event and suggested questions to ask. Second, and more important, it enabled Stott and Drury to collect a variety of different types of data. Our identity as participants in the anti-poll tax movement facilitated access to sources (e.g., letters from prisoners, legal statements, and subsequent interviews) that participants might not wish to pass on to a "neutral" researcher. Waddington (1992) is an example of an "external" analysis of the poll tax riot, which in some ways is consistent with the account presented above of escalation through interaction. Yet Waddington acknowledges the limits of his data (no firsthand observation or interviews)—limits that would prevent the analysis of participants' changing definitions of self and context offered in the present case.

### An Antiroads Occupation

This study of a crowd occupation at the building of the M11 link road in Wanstead, London, in 1993 examined not only the dynamics within a crowd event but also their aftereffects (Drury 1996; Drury and Reicher 2000): How did participants come to define themselves and their social world after (unexpected) conflict with the authorities? The campaign against the M11 link road was part of a vibrant and UK-wide antiroads movement that flourished in the mid-1990s (e.g., at Twyford Down, Newbury, and Fairmile). Those involved in the movement included not only the local people living around the locations of the proposed roads but hundreds of others motivated by ecological principles, many of whom adopted and propagated (with some success) an ideology of “nonviolent direct action”—an ideology that informed the collective identity shared by those at the M11 occupation.

The crowd event in question comprised the police removing participants occupying the base of a chestnut tree scheduled to be demolished to make way for the road. Drury, the principal researcher, had already been taking part in campaign actions and collecting data for more than a month previously as part of a longitudinal research project. He participated in resisting the tree’s removal by sitting down with the rest of the crowd and being forcibly ejected by police—being thrown into a mudbath, in fact.<sup>1</sup> He then remained with the crowd, which attempted to block the road contractors’ vehicles and breach police lines for most of the rest of the event, which lasted around 12 hours. Several hours of soundtrack recordings were made during the event and observations were recorded by hand or into the tape recorder. Drury was also able to interview participants immediately before the police arrival, during lulls in the actual conflict, and again in the days and weeks afterward.

Once again, since Drury identified with the other crowd participants (being himself against the building of the road) and was treated by the police no differently than anyone else, he certainly did not look at the event from the outside. As with the poll tax riot, then, a “consensual” account was subsequently constructed, as part of which Drury’s own observations featured as just another piece of data. The same principles were adopted, with one small difference. This followed from the fact that for the M11 event, there were times when only crowd participants were present (in particular, a rally and nonviolence briefing immediately before the police arrived); hence, accounts here could not be triangulated with those of noncrowd participants. In these cases, agreement between different crowd participant accounts was considered as consensual.

There was agreement that police had forcibly removed people, but some disagreement about the behavior of crowd participants: Police claimed that

the crowd was threatening and a minority was aggressive; the crowd claimed to have suppressed aggressive elements and maintained the principle of non-violence. However, the main point for the research was to explain how crowd participants saw themselves and their social relations in the light of the encounter, which all agreed was conflictual. The analysis suggested that, where crowd participants defined themselves as “individual citizens” enacting their “democratic right to protest” and yet were physically treated by police as “a threat to public order,” they found themselves repositioned in a new set of antagonistic social relations with the authorities and redefined themselves accordingly. Thus, where the police were no longer seen as protecting “individual rights” but as defending the power of the state to impose an illegitimate road-building scheme, the self became redefined as *oppositional*. For those participants affected, oppositional attitudes toward the police and an enhanced willingness to engage in oppositional action (occupations, trespass, etc.) endured for many months after the crowd event itself.

Since the aim of the analysis was less to explain how crowd conflict developed than its consequences, the issue of validation—of countering possible partiality in analysis—relates to the evidence for psychological change. The study gathered data on psychological change at two levels: first, indirect measures, for example, as when participants described themselves differently from Time 1 to Time 2, and second, more direct measures, as when participants described themselves as having changed or seeing their social relations differently. In terms of validity, clearly neither of these types of evidence for change was sufficient in themselves. The validity of the analysis was therefore argued to be cumulative in that, together, the various pieces of interpretation formed a coherent whole that, again, made sense of the pattern of data. Indeed, although not everyone interviewed claimed to have changed, those who were explicit that they had not changed were found to hold distinctive views about the nature of the police before the conflict occurred (e.g., “agents of the state”).

The question of artifactuality or reactivity was perhaps more of an issue in the antiroads occupation research than in the poll tax riot study. In general, participants may want to “help” a researcher by giving the (interview) answers they think the researcher wants to hear; this may be exacerbated when researcher and researched share an identity since, as mentioned previously, shared identity enhances cooperation. However, where what is of interest is psychological change within a crowd event, there is the added possibility that asking participants certain questions might in itself provoke a process of self-reassessment and thus change (see Thomas 1993).

Green (1993), who wanted to examine the role of policing strategies in radicalizing striking miners, argued that the willingness of striking interviewees to tell their stories, often without regard to the particular interview questions, was evidence that “interviewer effects” were minimal. The same phenomenon occurred in the present case. Moreover, most who described themselves as “changing” after the M11 occupation event did so without being asked directly about change. Yet, the stronger argument against the danger of this type of reactivity is our utilization of a range of different sources of evidence of change: as, for example, in the case not just of the participant herself giving evidence of change but also of her friends and family describing her as having changed.

As with the study of the poll tax riot, it was easier for us to collect data from the crowd participants than from the police involved in the M11 occupation eviction. Indeed, although the antiroads occupation was a far less violent event (no police batons were drawn, for example), and perhaps less legally sensitive from a police perspective, the problems cited above were similar. Obviously, during the crowd event itself it was simply not an option to ask the police “for their perceptions of the crowd,” let alone to request a formal interview. Indeed, it was the police, rather than Drury, who were asking all the questions. There is a rich—and sometimes entertaining—literature on the practical pitfalls of PO, including assault and arrest (e.g., Humphreys 1970; Armstrong 1993), and Drury suffered a similar fate. Shortly after some of the participants had blocked a road to prevent contractors’ vehicles from reaching the tree, the police arrested Drury who was standing nearby, along with eleven others, for “breach of the peace.” All were later released without charges being brought.

In subsequent weeks, although one senior officer at first expressed a willingness to cooperate with the research by arranging interviews with officers, this did not materialize. No reason was given, although our impression was that the change of mind was due to the senior officer’s recognition that Drury was “one of the protesters,” irrespective of his university affiliation. Police data were therefore obtained indirectly: from mass media sources; from a student project (for which Drury supplied an interview schedule); from legal documents; and from a tape recording of a meeting between senior police and antiroad campaign organizers.

The availability of these latter three data sources makes the point that what was lost in data from the out-group was more than made up for with data from the in-group. It was precisely because Drury was known personally to the antiroads participants as “one of us” that they were willing to cooperate with him. Thus, Drury had access to fifty-seven witness statements, as well as



video recordings taken by participants, diary material, and campaign documents. It was significant that so many participants were willing to donate their time: Fifty-six people were interviewed in relation to the event in question, several of them more than once. To make full use of the PO framework requires that the researcher explicitly enlist the help of those being studied. In the present case, we needed not only interviews but access to campaign literature, introductions to other people, copies of videos and photographs taken by campaign participants, and so on.

We could not have expected people who were making such a commitment to the antiroads campaign to cooperate with us in these ways unless we were on their side. Why should they help us unless we were helping them? If Drury were simply in it for his career, he would have been seen (correctly) as a parasite. So, Drury helped the campaign not just by being one of the numbers in the collective actions, but also in more mundane ways such as folding leaflets and barricading buildings. In principle, there is a possible conflict of roles where the researcher gathers data through involvement in a particular activity in a culture or movement: It may be difficult to retain the role of researcher if one is caught up in the demands of participation (Stephenson and Greer 1981). In the present case, however, such involvement only rarely obstructed the research; quite the contrary, it meant being in precisely the places where Drury could meet more campaign participants who could then help in the research.

## CONCLUSIONS

While PO is an established research framework for anthropologists and sociologists, it is less favored for psychology than are experiments and questionnaire surveys. Beside offering greater control to the researcher, these methods are also said to offer greater objectivity—the implication being that in PO the subjectivity of the researcher is likely to contaminate the research object. In this article, however, we argue that subjective involvement is actually necessary in research on crowd conflict since such events should be understood as intergroup encounters.

Most crowd events are not conflictual (Benewick and Holton 1987), and many collective conflicts of interest do not always express themselves openly. Arguably then, partisan PO has only limited research applications. Yet, we contend that the issues it raises have wider epistemological significance. Objectivity and subjectivity are, implicitly if not explicitly, often cast essentially as polar opposites. The rigid Kantian antinomy of the subjective and

objective realms offers the comforts of a world that can be studied apparently without “getting one’s pants dirty”—except that it involves an epistemological sleight of hand whereby subjectivity is secretly smuggled in. Every type of methodology entails interpretation by the researcher, and interpretation presumes an interpreting subject. In other words, the scientific community cannot escape subjectivity since we are always and irreducibly subjects (Drury 1994). Subjectivity and objectivity are not exclusive realms but mutually constituting; one of the underlying arguments of this article has been that it is *because* we have subjectivity that we can grasp the truth—the objective. The rationale for partisan PO is that it recognizes and makes use of our inherent subjectivity rather than pretending it can be wholly replaced by objective techniques. Social science should consider itself fortunate that there are researchers willing to lend their subjectivity to research on intergroup conflict.

## NOTE

1. “Go get the seat of your pants dirty in *real* research” (Robert Park, quoted in McKinney 1966:71).

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*JOHN DRURY is a lecturer in social psychology in the School of Social Sciences, University of Sussex. His research interests include crowd dynamics, power and resistance, and social influence. Recent publications include “The Intergroup Dynamics of Collective Empowerment: Substantiating the Social Identity Model” (Group Processes and Intergroup Relations, 1999), with S. Reicher, and “Representations of Teenagers among Police Officers: Some Implications for Their Communication with Young People” (Youth & Policy, 2000), with C. Dennison.*

*CLIFFORD STOTT is a lecturer in psychology at the University of Liverpool. His research interests include crowd dynamics, football "hooliganism," and intergroup stereotyping. Recent publications include "Crowd Action as Inter-group Process: Introducing the Police Perspective" (European Journal of Social Psychology, 1998), with S. D. Reicher, and "How Conflict Escalates: The Inter-Group Dynamics of Collective Football Crowd 'Violence' " (Sociology, 1998), with S. D. Reicher.*