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Whose side are you on?

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“Whose side are you on?”
When being a movement sympathizer does not necessarily help research

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The title of this presentation is of course an allusion to Howard Becker’s famous article “Whose side are we on?” (1967), in which he addressed the ethical position sociologists (and any other social scientists) should adopt towards the people (especially deviant ones) they study. The issue I’d like to address in my presentation is a rather distinct one, as the question at stake would not be asked by a sociologist to his or her colleagues but asked by social actors—in that case social movement activists—to the academics who intend to study them, often with a sympathetic (sociological) eye.

As a matter of fact, many sociologists choose to study social movements for which they share some kind of sympathy: they think that their causes are legitimate, their grievances are worthy, and their collective struggles need support. Even if many fear to be reproached by colleagues for an alleged defective axiological neutrality, and therefore face difficulties recognizing it within the academy, many hope that their study will help the movement they study to fulfill its aims, sometimes without knowing exactly how but at least that’s what they claim when they negotiate access to the field and ask for interviews. The situation can even be more explicit when the sociologist is him or herself a member of the movement s.he studies, participates to its actions, and publicly shares its aims and grievances. In that case, the observer does not need to negotiate access to the field—as s.he already is part of it—and participant observation is rather a given than a methodological challenge.

I’d like to address such situations as my own experience taught me that being a known sympathizer or a member of a social movement is surely helpful for data collection, but also exposes to some methodological and ethical difficulties that need to be considered. Two research confronted me to such situations and difficulties. The first one was conducted during the 1990s on the French prostitutes’ movement, that at the time took the form of self-help organizations mostly dedicated to AIDS prevention but that also developed claims for the official recognition of “sex work” (Mathieu 2001). The second one was conducted during the

early 2000s on the movement against “double penalty”—meaning the deportation of foreign delinquents after their prison term—that is for long a component of the French pro-migrant movement (Mathieu 2006). I will first expose how my closeness to both movements facilitated research and then how it led to tensions and problems. I will conclude with some general methodological and epistemological considerations on the issue.

I was not an advocate of the recognition of “sex work” when I started to work on the “community health” organizations that conduct AIDS prevention actions for prostitutes in the early 1990s, but clearly thought that their action was relevant and helpful for such disadvantaged people as most prostitutes were. Those organizations were based on self-help, as (former) prostitutes were hired and paid to participate in prevention teams that were also composed of nurses: the later had medical qualifications that guaranteed sanitary relevance, whereas the former had a direct experience of what it is to practice prostitution and could more easily join distrustful colleagues or clients. One political aim was to show that prostitution is not social degradation *per se* but rather grants its practitioners with skills and knowledge that make them indispensable protagonists in the struggle against AIDS, and that they should be recognized and rewarded. Another claim emerged from the constatation that the more precarious or clandestine situation a prostitute lives in, the weaker s.he is facing contamination by HIV; as a consequence, de-criminalizing and de-stigmatizing prostitution was expressed as both a political and a health issue.

Having been a participant to the sociological research that led to the creation of such an organization in Lyon—called Cabiria—, I had no difficulty pleading to observe its action and to interview its team members when I started my PhD in 1994. My first fieldwork proved decisive: I was tasked by a state institution to write a report evaluating the action that was led by Cabiria, while rumors were spreading among the French abolitionist movement that such AIDS prevention organizations were promoting procuring. This first ethnography showed that, contrary to rumors, nothing illegal was promoted or conducted within Cabiria, and granted me recognition from its team. I thus became some kind of honorary member, habilitated to observe its ordinary activity—for example by participating to night outreach on prostitution zones, welcoming prostitutes to the organization premises or sitting in the board of directors—and to whom interviews could be granted without any problem. Identified as a skilled writer, I was often asked to write letters to institutions or media, which I considered a reward for the help the organization was giving me for my research; I also helped the team members conducting many minor tasks, such as driving the organization’s van during outreach activities.

In that case, I accessed the movement first as a sociologist and, studying its claims and actions, I became a sympathizer, ready to defend it against what empirically appeared as unfounded accusations. The second case is different because I was first an activist of the movement against “double penalty,” and only thereafter its sociological observer. As a member of a far-left party and of a student trade union, I actively supported hunger strikes led in Lyon between 1998 and 2000 against the possible deportation of foreign men who had served a prison sentence but who had all their affective and familial ties in France. The poor results of such dispersed mobilizations led the main French pro-migrant movements to launch a national campaign to repeal the laws that allow such deportations, taking the opportunity offered by the upcoming 2002 presidential election to make the cause public.

I decided to move from activist to sociologist and to study the campaign as part as an historical study (from the 1960s until now) of the struggle against what activists call “double penalty” (deportation being a punishment imposed only to foreign delinquents that doubles the prison sentence). Being known for long by the campaign main spokesperson and animator, and benefiting from my legitimacy as a former activist and from my knowledge of the movement stakes, I was authorized to assist to coordination meetings during which I adopted a “neutral” attitude: I never participated to the tactical or strategic debates, and rather sat silently, taking fieldnotes. However, my personal conviction that the campaign was leading a legitimate struggle and my own activist inclinations could not be hidden, and this political and social affinity with campaigners obviously facilitated fieldwork.

From a methodological point of view, what had been my main benefit in both cases is that being or becoming a member—although of a specific kind—granted me access to what Erving Goffman (1959) calls *backstage behavior*, meaning the behavior adopted in social regions in which the control of information is reduced because no audience can assist to acts or listen to talks that could discredit the performance that is presented frontstage. Often, such backstage behavior adopts a conflictual tone, sometimes with cynical or despising tenor, that contradicts the conciliatory or unanimous posture that is adopted frontstage. This was clearly the case within the campaign against “double penalty”, that was led by a coalition of pro-migrant groups that, albeit they all denounced “double penalty”, had diverging views of both tactics (accepting or not the legislative reform proposed by minister of Interior Sarkozy) and grievances (a complete repeal of the law permitting “double penalty” or a partial one, as offered by Sarkozy). Observing similar backstage talks within the AIDS prevention organizations also offered me valuable data on competition between local groups, strategic divergences and tactical dilemmas that were discrete but major dimensions of the mobilization.

It should be added that the familiarity and proximity that results from a regular presence along activists—especially when the academic shares many sociological traits with them—helps to gather informally many personal data (on their social origin, education, political opinions etc.) that would be harder to collect during formal interviews. Although many social scientists only study movements' official stances and observe their public performances, being recognized and accepted as an honorary member of the contending group, and being able to assist to its backstage regions, clearly enhances the volume and accuracy of the data s.he is able to collect and, thus, his or her sociological knowledge (Collombon, Mathieu 2024).

But there is a “but”. The status of honorary member is not always easy to keep on the long term and exposes to difficulties that endanger relations between the social scientist and the movement s.he studies, and can reduce data relevancy.

A first difficulty has been stressed by French anthropologist Jean-Pierre Oliver de Sardan who coined the concept of *encliquage* (from *clique*, meaning coterie) to designate the fact of being included by just one part of a divided group and thus perceived with distrust by the other part (or parts)¹. This is clearly what happened when I was studying the national campaign against “double penalty”: having supported the previous Lyon hunger strikes and having been introduced by the campaign main coordinator (also coming from Lyon), I was associated with “his side”, meaning the more moderate one that did not claim for a complete repeal of the law and that was ready to accept the partial law reform proposed by Sarkozy. I was thus perceived with some kind of distrust by “the other side”, mostly composed of Parisian and intransigent groups and activists, that denounced such a position as an unacceptable renunciation. Consequently, fieldwork was made more difficult when I wanted to assist to some meetings or when I wanted to interview some activists who identified me with the “opposing camp”. I had then to recall that I was first a sociologist who wanted to collect everyone's point of view on the issue. In that case, respondents were sufficiently aware of the methods and stakes of social sciences to finally accept to give me data, but the same could have been more difficult with other movements (that could identify the sociologist as “the other side's spy”).

However uncomfortable it can be from a methodological (and personal) point of view, such a situation shaped with distrust is, in itself, sociologically relevant: it proves that the collective dimension of social movements is not a given and should not be taken for granted. Social movements are always composed of various and relatively heterogeneous elements

¹ See Broqua (1998) for a good example of *encliquage* while studying Act Up-Paris.

(groups or individuals with different ideologies, agendas, social traits etc.), and are the site of a competition between them for the definition of their goals, strategy, identity etc. The motives of distrust towards the sociologist are by themselves data that inform about the movement dynamics and composition, and that should not be neglected or shamefully hidden: they also have their positive side as empirical data.

Another problem can appear when the social scientist starts to disseminate texts (conference presentations or articles) in which s.he develops an analysis of data s.he has collected during fieldwork. As they address an academic audience, such texts must submit to the constraints of scientific writings: they affect neutrality, use theoretical rhetoric and, above all, rely on valuable and robust data to establish results and conclusions. Each of these three conditions can provoke misunderstanding when activists read such papers, especially when they are not accustomed to academic writing. The theoretical language can discourage such readers, who feel that what the sociologist has studied is unimportant or inconvenient, and the neutral tone can disappoint someone who thought the sociologist was as devoted to the cause as s.he is.

But conflict can arise when the social scientist exposes data that contradicts the movement's presentation of self, for example when s.he stresses that internal debates shaped the mobilization or that the contending group is hindered by contradictions. This happened to me on both cases. The AIDS prevention organizations were shocked when I published an article—of course based on convincing empirical data—that showed that, contrary to official stances and due to insufficient professional qualifications, (former) prostitutes had a subaltern role in those organizations that were in fact driven by health professionals and by middle-class activists. Their anger became harder when I published another article that considered prostitution as social disaffiliation resulting from the inaccessibility of “normal” job markets, and not as “sex work”. I was thus banned from the organizations and stigmatized as a villain traitor to the cause.

The book I published after the study of the mobilization against “double penalty” did not provoke such accusations but was however badly perceived by the coordinator of the campaign who felt that I had given too much importance to the internal debates and conflicts. According to him, those debates and conflicts were irrelevant, and I should have given a more positive view of the outcome, meaning the partial law reform he had supported. The fact that years after that he introduced me to some acquaintance as “Lilian, who wrote a book against me” shows how he resented me for what he also had experienced as some kind of treason.

“Whose side are you on?” is, in a way, the question social movement activists ask when social scientists portray them in what they perceive as a distorting mirror, that risks to undermine their legitimacy and, maybe, to support their antagonists (the state, a countermovement, a reluctant public opinion etc.) while offering them unfavorable arguments. The question contains the suspicion that the movement has hosted a hidden adversary that has falsely expressed support for a cause s.he has in fact undermined while publicizing its internal affairs.

Such a suspicion, and such accusations, can be hard to live for a social scientist who shares a sincere sympathy for the cause s.he studies and who candidly thought that shedding academic light on a movement is a contribution to its public recognition. It also confronts both protagonists to some kind of duplicity in what had first been an implicit transaction: the sociologist can have overemphasized his or her sympathy in order to be granted access to the field, while the movement can have overstated the benefits—in notoriety or legitimacy—it could expect from an academic study.

The reproaches can be hard to live when a study grants a sociologist some success in his or her academic career, while the movement sees no tangible results from its hospitality. Very often, the transaction is an unbalanced one: the movement has more to lose and the sociologist more to win from it. I would however conclude in favor of the later (this is of course a *pro domo* plea) in what can be understood as an allegiance conflict: the social scientist must first show allegiance to science, meaning to empirically based truth, and should never censor any relevant data on the basis that it could be detrimental to the movement s.he studies. Neglecting some facts on the basis that they can be embarrassing for the movement is, scientifically, not a legitimate reason; as those facts belong to social reality, they must be taken into account and analyzed with robust scientific methods to give a complete account of this social reality. As a scientist, and to remain a scientist, the sociologist must first remain faithful to truth, and only after, if s.he can, to his or her personal political options².

As such, “whose side are you on?” is also a question sociologists have to address to themselves when they analyze their data and write their reports or articles: do they have to remain faithful to the cause they have studied while being their members or sympathizers—and to neglect embarrassing facts—or to take side with science, and accept to take the risk to provoke disappointment or anger within the ranks of their (former) comrades? I have repeatedly

² This does not mean, of course, that a sociologist should use immoral or illegal means to collect data (e.g. by lying or hiding that s.he conducts a study). The debate can however be open in the case of immoral or illegal contentious activities, but such a discussion is more ethical or political than scientific.

been struck by the fact that PhD students who study movements they are members of, either do not finish their thesis and remain activists, either quit their movement (sometimes after a conflict) in order to accomplish a valuable scientific work: the difficulty to maintain an activist relation with what has become an object of science forces them to choose their side, whatever it is.

Following Wittgenstein (1958), one can consider that science and activism belong to two different life-forms and use different language games. They surely can intersect and enrich each other—many good sociological ideas come from activist experiences—but they certainly should not be conflated.

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