



## WHY ETHNOGRAPHY MATTERS: On Anthropology and Its Publics

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The projection of a public is a new, creative, and distinctively modern mode of power.

—Michael Warner, *Publics and Counterpublics*

How can one explain anthropologists' reluctance to reach an audience wider than just the specialists of the discipline, when their scientific approach is based on an experience in principle open to everyone, and when their works are for the most part written in ordinary language?

—Philippe Descola, "A *Bricoleur* Workshop"

When I woke up in my hotel room in Montreal on November 18, 2011, to finish preparing the lecture I was to deliver later that day at the American Anthropological Association annual conference, I was surprised to discover on my computer a recent accumulation—due to the transatlantic time difference—of electronic messages requesting that I urgently contact journalists from several French national television and radio stations to give an interview for the evening news in response to a statement made by the minister of the interior.

The reason for this unanticipated 15 minutes of fame that drew me out of my morning lethargy was the following. One month earlier, I had published *La Force de l'Ordre. Une Anthropologie de la Police des Quartiers* (2011), an ethnographic account of law enforcement in the "banlieues" of Paris, the outskirts whose disadvantaged

neighborhoods are mostly populated by working-class families from North and Sub-Saharan Africa. During 15 months, between 2005 and 2007, I had studied the daily routine of police work, accompanying patrols of uniformed officers or more often plain-clothes members of one of the special units known as the anticrime squads. This was an unusual time for such research, since it was conducted between the two major civil disorders of October 2005, when riots flared up all over the country after the deadly electrocution of two adolescents who had sought refuge in a transformer to escape an anticrime squad chasing them for a theft they had not committed, and of November 2007, when a more limited but no less impressive urban disturbance followed the fatal accident of two youths whose motor bike had been knocked down by a car of a similar unit under circumstances which led many to believe that the collision was deliberate. These were only the most recent victims of a long series of tragic events occurring during the last three decades, over the course of which ethnic minority adolescents and youths died as a result of interactions with law enforcement agents in housing projects.

But rather than focusing on these events, which generated recurrent urban disorders, I observed the everyday activity of the patrols, the relationships developed by the officers with the population, the differentiation of their attitudes according to the public, the spiral that sometimes ended in violent acts and near riots. And I attempted to inscribe them in the broader historical, political, and sociological context, analyzing in particular how, during the 2000s, the radicalization of governmental discourse and policy on issues of immigration and insecurity as well as the imposition of quantified objectives prompted the police to play a role far removed from their expectations and missions, the result being violent, ineffective, and counterproductive operations. On the basis of my empirical findings, I showed how law enforcement had become the enforcement of a social order with the policing of the housing projects serving to contain the frustrations of their inhabitants, who were more affected by increasing levels of poverty and unemployment than crime.

When the book came out, it received wide and favorable media coverage. It was the first ethnography of law enforcement in France; it revealed aspects until then unknown about the everyday life of these neighborhoods, including violent interactions with the police about which journalists had only heard the accredited side of the story via the public relations services of the Ministry of the Interior. I had endeavored to find a form of writing that would be accessible beyond academic circles, considering that the topic deserved more public awareness and public

debate. During the next five weeks, numerous articles were published in national newspapers and magazines, and long interviews were broadcast on the radio and television, surprisingly without reaction from the authorities, despite my having sent in advance the volume to the commissioners, unionists, and officials I knew. As some journalists were tempted to turn my analysis into a mere denunciation of police abuses on the basis of several scenes described in the book, I was cautious to reclaim the ethnographic project and avoid facile simplifications, but this was obviously easier to do in the 400 pages of the volume than in a couple of sentences extracted from an interview with sensationalist intentions. Being ignored by the police and the authorities was, for me, less a subject of disappointment than of astonishment, considering the way in which all public interventions on law enforcement issues during that period, in particular by hip-hop singers but also by magistrates, had been vehemently condemned by unions and occasionally prosecuted by Nicolas Sarkozy's government.

On November 18, this situation changed due to two events. The minister of the interior, unexpectedly inventing a tradition, decided to celebrate on that day the improbable 30th anniversary of the creation of the anticrime squad, a brigade that had never been honored in the past and for which historians actually gave 1994 as its inception date. Considered to have been the president's *eminence grise* during the previous two decades, Claude Guéant had defined and conducted the most repressive and stigmatizing policies toward immigrants, minorities, and Roma since the end of the colonial period, and was at the time widely known for having declared the superiority of Western civilization. During the celebration, several members of the feted special units were solemnly awarded a medal for their service to the nation. But the same morning, *Libération*, a national newspaper leaning to the left of the political spectrum that had not yet published a review of the book, devoted its headline and full front page to it. Overlapping a massive black-and-white photograph of the waistline of a heavily armed officer, the banner reversed my title: "Anticrime Squads in the Banlieues: The Forces of Disorder." There followed three pages of analysis, comments, and reports, with pictures of black youths being arrested and of the graphic badges of the squads. In interviews, residents of a large housing project in the outskirts of Paris complained about the abuses of law enforcement agents, whereas police unionists deplored the relational fracture between the officers and the population.

The timing of the two events was surely not coincidental. Informed in advance about the celebration, the editor-in-chief of the newspaper had decided to present a counterpoint and address the issue of the preoccupying shift in urban policing.



IMAGE 1. Front page of the national newspaper, *Libération*, November 18, 2011

At the press conference after the commemoration, the minister of the interior, pressured by journalists to comment on the findings of my research, made some contemptuous remarks. It was because of this criticism, of which I was not more aware than of the newspaper's front page, that the media were eager to reach me. Due to the time difference between France and Quebec, they had only a few hours to include my response in the evening news. Sleepy and stressed, I tried to articulate something coherent about urban policing and scientific independence. As for the actual television coverage, I learned later that, as I would have guessed, my reply was reduced to two 30-second excerpts on the national channel. In the following days, however, I wrote an editorial for the newspaper *Le Monde* to elaborate a more substantial and cogent response, expressing my gratitude to the minister of the interior for giving me the opportunity to do so publicly.

## WHEN ETHNOGRAPHY GOES PUBLIC

I take this personal anecdote as a point of departure to reflect on the significance of ethnography for contemporary societies, contemplating in particular the question of its relation to its publics. Analyzing the public dimension of the social sciences is certainly not new: Émile Durkheim famously asserted that they would not merit one hour of trouble if they were only of speculative interest without contributing to orient human actions; Max Weber resolutely distinguished the academic and political vocations to defend a scholarly ethic; and Franz Boas forcefully insisted that his discipline had an obligation to address the social issues of his time. Yet, the social sciences have always kept an ambivalent relationship with public involvement, contemplating the risk of an epistemological blur between a rigorous approach and a normative drift.

In recent years, however, there has been a resurgence of interest in this question, the adjective *public* serving to qualify both sociology and anthropology. Debates have certainly been richer and more intense in the first case—especially after Michael Burawoy’s plea for a “public sociology” (2005), which has generated stimulating discussions (Clawson et al. 2007)—than in the second one, relatively limited to Robert Borofsky’s self-admittedly modest “public anthropology” program (2000), which has mostly provoked skeptical reactions (Purcell 2000), although it can be argued that many anthropologists intervene in the public sphere without qualifying their endeavor as public anthropology (Checker 2009). Rather than entering the dispute over the definition and legitimacy of a public social science, be it sociology or anthropology, I want to explore some of the problems that emerge from the encounter between ethnography and its publics. I will do so by examining my experience, not because it is exemplary but simply for the obvious practical reason that it is easier to reflect upon one’s own case (Vaughan 2005), except if one were to develop a specific research program on these problems, which is not my intent. In doing so, I am not discounting numerous similar efforts, either collective and editorial, such as the series “Public Anthropology” at the University of California Press and the journal *Anthropology Now*, or individual initiatives, such as those deployed by Paul Farmer on health inequalities, Hugh Gusterson on the culture of security, or Lila Abu-Lughod on the representation of Islam.

The relations between ethnography and its publics involve two distinct although somewhat related operations, which have tended to be confused in recent discourses about public ethnography: popularization and politicization. Popularization refers to genre and medium (Vannini 2013). It is concerned with the ways of

writing to make one's knowledge accessible to large audiences and with the modes of communication corresponding to contemporary expressions. It deals with style and topic as well as magazines and blogs. Politicization refers to debate and change (Scheper-Hughes 2009). It is concerned with the opening of the public sphere to certain questions and with a potential impact on policies. It deals with forums and arenas as well as activism and reform. Obviously the two are related, but whereas politicization supposes some form of popularization, the reciprocal is not true. To transform the terms of a political debate, one needs to communicate with the corresponding publics, while acceding to wide audiences via alternative media does not necessarily imply any commitment to social change. In this essay, I focus on the latter dimension without ignoring the former.

Addressing more specifically ethnography—rather than sociology or anthropology—as a public undertaking, to follow Herbert Gans's (2010) proposition, is interesting on several grounds. First, being a method, it crosses disciplinary boundaries and concerns sociology and anthropology as well as, potentially, other domains including political science or communication studies. Second, dealing with fieldwork, it presents certain features linked to the practice of participant observation that define issues like the direct access to social facts and the ambiguous relationships to those studied, which poses specific problems when rendered public. Third, involving writing, it implies forms of description and argumentation that lead to discussing the borders of its multiple genres with literature or journalism, and its articulation with more abstract approaches developed on the same topics in the social sciences. Fourth, exposing the ethnographer, it has consequences not only in terms of the transformation of his or her image and place in the scientific field, but also in the long run in terms of the modification of the public representation of the field itself. Because of this set of singularities, the ethnographer's endeavor to go public raises both quite specific and very general questions.

At this point in the discussion, the interrogation that evidently comes to mind is: What is meant by public when one speaks of public ethnography? Or, rather, who are the publics concerned by this project? The issue is not always clarified by the sociologists or anthropologists who defend it. As Michael Mann (2002:67, 75) asserts, the public is “a social imaginary” which “exists by virtue of being addressed.” Is the public of public ethnography a generic, broad, and potential audience of the individuals who may read the book, or the sum of several more circumscribed groups or categories defined on the basis of their professional or personal involvement in the topic explored by the author? Should it include those who will not read it, but browse a review about it in a newspaper or hear someone

comment on it on the radio, or should it be conceived of primarily as those who are directly concerned, such as victims of a given situation or activists defending their cause?

Sometimes the public is understood as a vague readership that might be interested in the subject, sometimes viewed as a collective political actor who will be enlightened by scientific insights, and sometimes considered to be the policymakers who could make good use of thorough academic expertise. But there are also the scholars who will read, cite, and teach the book, thus expanding its audience, and the media who are both a public themselves and an intermediary to a larger public. In this range of possibilities, the ethnographer only has a limited capacity to choose by the way he or she writes and via the forums where he or she will speak. At most, the author can imagine the diversity of these publics when producing his or her work and encounter a definite portion of them on the occasion of a given debate. Such indetermination is certainly not an obstacle to the project of a public ethnography, but it is a fact one should not elude: the more social scientists strive to reach out to a broad audience, the more they ignore whom it comprises—and the more they tend to imagine it, at the risk of probably overestimating their role in the public sphere as moral heroes.

This imagination has consequences on the very process of writing (Descola 1996). The idea of reaching out to people who usually do not read social sciences books was indeed on my mind during the entire time I worked on my manuscript. Had I forgotten it, my two youngest children, then high school students, would have reminded me: affected by issues of urban policing via the experience of their friends in the housing projects, they insisted that the book should be accessible to anyone potentially concerned, and occasionally read excerpts of it as a test of legibility. However, this was a demanding endeavor. It meant finding an appropriate format for an extended readership while respecting the ethnographic epistemology. This was a fine line. I did not want to restrict myself to the first-person narrative (often eluding the social and historical foundations of the anecdotes recounted), which is sometimes used in studies of the police (Steinberg 2008) and gangs (Venkatesh 2008), but I considered it essential to retain the subjective presence of the author, as observer but also as citizen. I could not use the powerful resources of photography (actually any form of recording, even audiographic, had been explicitly forbidden by the commissioner) as in recent works on the abandoned (Biehl and Eskerod 2005) or the homeless (Bourgois and Schonberg 2009), but I endeavored to represent places, such as the housing projects, and those who inhabit them, whether the youths or the police.

Opening ethnography to a wider audience without losing its refinements and complications was thus a delicate task. It implied associating stories with analytical developments and linking theory with empirical materials. It also included more technical legibility choices, such as avoiding the usual academic apparatus of citations, which I entirely shifted to endnotes, and the intermediary titles in the text, which risked giving it a too explicitly didactic turn. But my principal effort was devoted to exploring a way of writing that John Van Maanen (1988) calls a “critical tale,” inscribing the description of scenes and situations in the social structure and the historical context—which is probably the terrain where the two projects of popularizing and politicizing ethnography intersect.

In the following sections, I will attempt to draw a few general lessons from my confrontation with the initially anticipated and later encountered publics of my book. I begin with the choice of the topic and site, then discuss the accounting and writing, later analyze the relation to the media and journalists, and finally contemplate the commitment and responsibility toward the various subjects of the research. But perhaps I should first clarify what should be understood as public ethnography. The expression simply refers to the principle of bringing to multiple publics—by which I mean publics beyond the academic circles—the findings of an ethnography analyzed in light of critical thinking, so that these findings can be apprehended, appropriated, debated, contested, and used. It is presumed that such a conversation between the ethnographer and his or her publics generates a circulation of knowledge, reflection, and action likely to contribute to a transformation of the way the world is represented and experienced.

### **BLACK HOLES**

“While most of you tend to investigate the same topics and sites, you should prioritize for your research these places tragically understudied in spite of the gravity of what happens there,” a French sociologist, who had been working for years in Rwanda, told my students at the end of the talk he gave in my seminar at the *École des Hautes Études en Sciences Sociales* in the late 1990s. His comment referred more specifically to the quasi-absence of ethnography, at the time, in the East of the Democratic Republic of Congo where more than two million persons were estimated to have died during the civil war. Beyond this case, which various recent works probably render much less an exception, I was struck by the general relevance of this observation. Whereas certain subjects, in Africa or elsewhere, attracted researchers and funds (including those involving violence in the past decades), others were almost completely neglected.



Exploring the black holes of ethnography seemed a good way to define scientific priorities.

Law enforcement was certainly not an ignored field for French social and political scientists, however. Since the mid-1980s a vibrant domain of study had developed, primarily based on the analysis of crime statistics, large surveys about officers, individual interviews with agents, and occasionally, short periods of observation at the station or on patrol. But in opposition to what is the case in Britain and the United States, where fieldwork has been practiced for half a century—from Michael Banton (1964) and William Westley (1970) to P. A. J. Waddington (1999) and Peter Moskos (2008)—there has been no real ethnography of the French police, a void all the more conspicuous since, as mentioned earlier, the multiplication of urban disturbances in the past three decades called for direct observation of the daily interactions between the officers and the population, especially in the neighborhoods where incidents regularly occurred. Why would social and political scientists in France overlook this classic method? I suggest three types of explanation: epistemological, structural, and circumstantial.

First, leaving aside the works conducted from a historical, genealogical, or philosophical perspective, the police as a contemporary institution have been mostly studied by social and political scientists trained in or influenced by organizational sociology and later pragmatic sociology. This theoretical background implied an emphasis on large processes and systems in the first case, formal models and grammars in the second, for which techniques other than fieldwork were privileged. Second, the specialization of a scientific domain dedicated to law enforcement was accompanied by the establishment of long-term relationships with officers and officials, who were precious sources of information but simultaneously reduced the initiative of the researchers to conduct more demanding empirical investigations, which could moreover contradict the version with which they were provided. The concentration of the social and political scientists studying the police principally in two institutions—one directly under the authority of the Ministry of the Interior, the other funded by the Ministry of Justice—also constrained their scientific independence, a situation about which several expressed concern. Third, the rare attempts to conduct fieldwork came up against the obstruction of the authorities. Although law enforcement agencies were never keen to be observed by outsiders, there had been some opening in the late 1980s under the Socialist government, but this period of liberality regarding the social sciences ended in the following decade with the return to power of



IMAGE 2. Interactions between the police and the inhabitants of the housing projects.  
 Source: "Shots Against Police after 'Ambush,'" *Le Point.fr*, May 17, 2009

the Gaullist party, and it has been since then increasingly difficult to carry out fieldwork.

In other words, the self-limitation of the conceivable methods for both intellectual and institutional reasons converged with the growing political censorship. As for me, more familiar with ethnography due to my different scientific trajectory and not inscribed in police networks, I unwittingly avoided the self-restriction of the possible, but also escaped governmental control, fortunately benefiting from the open-mindedness of the commissioner I initially contacted to obtain an authorization to conduct my study. This combination of circumstances explains how I gained access to the field between 2005 and 2007. Such a favorable configuration did not last long. When I requested to investigate a second site, I was denied permission by the prefects and eventually by the cabinet of the minister of the interior himself, in spite of the support of local heads of police. Law enforcement had then passed under the personal control of the newly elected president and no one would dare approve any research for fear of findings being rendered public.

But these difficulties have interesting implications. The self-limitation and political censorship to which it is submitted is telling of the value attributed to ethnography as fieldwork (Becker et al. 2004), especially when it deals with sensitive issues. Because it allows witnessing where those in power do not want evidence of what is ongoing to be seen, the method can be an object of avoidance, suspicion or prohibition—much more than is the case for questionnaires or interviews, which only give access to discourses and are carried out with no difficulty. There are definitely places where ethnographers are not welcome. However—and this is more

troubling—there are also places where they renounce venturing. Conspicuously, the reasons for this renouncement are not necessarily real or imagined institutional and political risks. They are often more prosaically the practical and psychological discomfort of fieldwork or of critique. In the case of the police, the inconvenience of long hours of random patrolling, the awkwardness of the observer's position, the slowness of the production of data, the embarrassment of displeasing officers or officials with whom one has become acquainted, and the comparative ease in preparing questionnaires and conducting interviews were probably more determining than the anticipation of problems with the authorities.

So far, I have addressed the reluctance of social scientists to conduct ethnographies of policing in France. Of course, one could reverse the perspective and ask, this time not limiting the question to the French context, why anthropologists traditionally do not study law enforcement, leaving this domain to other disciplines. Only recently did young anthropologists begin to invest in this field of research in various parts of the world, including South Africa, Brazil, and India. This long elusion should certainly interrogate the way boundaries are traced between legitimate and illegitimate objects in anthropology.

Be that as it may, these unobserved places are problematic in terms of the social sciences, but even more for what this implies for our understanding of the contemporary world. Indeed, entire segments of society, notably those whom the state precisely wants to substrate from the vision of the citizenry, remain unexplored by the social scientists and ignored by the public. When I started my study, I realized it was possible for most French people, as had been the case for me until then, to spend their entire existence in the vicinity of housing projects without being in the slightest manner aware of the life people lived there, and particularly of their trying experience with the police. Ethnography can perhaps partially fill this cognitive gap. Long absent from the studies of the police, anthropologists have a role to play here: professionally trained to develop a distant gaze and intellectually inclined to adopt a generalist perspective, they can complement the more involved and more specialized approach of their colleagues.

### CRITICAL POTENTIALITIES

“It made me see the banlieues and law enforcement in a completely different light”: this remark by a newspaper journalist, who usually covered urban issues and crime stories, was quite heartening. Probably it is for this reason that we practice ethnography—to make a difference in the way we comprehend the world. In this instance, the difference could be named quite simply: the ordinary. Whereas each

time the banlieues would make the news it would be for violent incidents, criminal activities, rioting adolescents, or drug trafficking, I depicted the everyday life of squads, the eventless nights of patrolling, the tedious routine of stops and frisks, the wearisome arrests of undocumented immigrants and marijuana smokers, the repetitive questioning of youths in the housing projects and of Roma people on country roads. In this “descent into the ordinary,” to use Veena Das’s phrase (2007), the most “spectacular discovery” of my research was the inaction characterizing police work in these disadvantaged neighborhoods and the profound boredom exuded by the long hours of roaming through the city.

If this mundane reality escaped even specialized journalists, it was probably because it took some time to go beyond the first impression of excitement aroused by the officers’ narratives about supposedly exhilarating moments they had experienced and by the emotions of their driving fast through the empty streets to mimic improbable adventures. The media relied generally on interviews with law enforcement officials and on spending one night out with the anticrime squad, both being strictly under the control of the department of public relations. Consequently, they missed the monotony of the activity and the tedium of the officers. Yet the ennui, the most prevalent “ordinary affect” (Stewart 2007) endured by the law enforcement agents, prompted them to take advantage of any minor event, to transform it into an engrossing expedition and try to turn it into a possible offense, a tendency strongly encouraged by the Ministry of the Interior, which had implemented the so-called “politics of the number” consisting in quantified objectives of efficiency, principally via the tallying of arrests. Since these goals were unattainable due to the relatively low level of criminality, the police tried to “make their statistics” with misdemeanors. But this rationale was not sufficient to account for their relationships with their public. There were more troubling indices of their gratuitous aggressiveness. During a lecture to a university anthropology department in the United States, I gave the following example.

On a night that had been particularly dull, the anticrime squad was informed that some youths were having a party in a small park adjacent to a housing project. For lack of a better thing to do, two of its vehicles headed toward the location. Upon arrival, the six officers did not observe anything wrong: the place was quiet and seemed empty. As they were leaving, however, one agent spotted a small group sitting at a picnic table, calmly chatting. The leader of one of the crews, an experienced and courteous man, asked his colleagues to ignore this peaceful gathering, but the officers from the other car insisted that they stop and frisk the youths, arguing that they should not be outside that late. The crews silently



IMAGE 3. Example of badge used by the anticrime squad as a troubling index of their gratuitous aggressiveness. *Source:* <http://collections-insignes.forumgratuit.org/t184-vente-ecussons-bac-athis-mons> Front page of the national newspaper, *Libération*, November 18, 2011

approached the group and suddenly announced the identity check. The youngsters explained that they were simply celebrating a birthday. Actually there were no traces of alcohol or drugs. However, they all had to submit to a humiliating body search, pockets emptied, hands up, legs apart. This episode, which unfolded in complete silence, lasted 15 minutes. Nothing illegal was found. We left the young men obviously vexed and bitter. On our way back to the vehicles, we turned around and saw them moving away. “We’ve fucked up their party,” one agent declared with satisfaction.

This anecdote is illustrative of the habitual sort of interaction between the police and the youths in the housing projects. It embraces every element of patrol work, including the combination of the dearth of activity and desire to hassle, leading to harassment and generating tensions. It is also indicative of the relationships within the teams in which, when disagreement occurs, aggressiveness prevails over moderation and sociability outweighs hierarchy. Yet, by comparison with other scenes I recount in my book, this one is quite benign: no brutality, no insults, no racist comments, no unjust arrests. It is far from the dramatic events that are the only facts of the life of these neighborhoods coming to the surface of the public sphere. What I described was the banal routine of policing.

At the end of the talk, the anthropologist who hosted it, evoking this episode, expressed her indignation that young people be treated this way and, even more, that it be the rule rather than the exception. She regarded the situation as epitomizing the discriminatory practices in policing and the oppression exerted on ethnic

minorities. I could not agree more, but I found her reaction quite telling of the power or perhaps, more suggestively, the discreet charm of ethnography. The simple depiction of a moment in the life of the inhabitants of the housing projects could vividly expose the social texture of the relations between the actors and instantaneously render perceptible its political and moral implications. By comparison, if instead of relating this minor episode, I had explained in a more general way that the police constantly harassed the youths in disadvantaged neighborhoods, which was a manifestation of the structural violence against the descendants of colonial subjects, the effect produced would have been different in terms of intelligibility and sensibility. The disparity in the public reception of this description of a scene resided in two complementary elements: the unfolding of events was regarded as “reality” (things did happen this way), as opposed to what could have been seen as already an interpretation; the narrative had produced an effect of “presence” (one could imagine things the way they did happen), in contrast with the distance introduced by more abstract statements. Of course, neither interpretation nor abstraction was absent from my discussion of the anecdote, but just as was the case in the expository scene of the Balinese cockfight (Geertz 1973), both were the product of the inductive reasoning of ethnography.

Publics vary, though, even within academe. When I used this same example, precisely because it seemed at once so anodyne and revealing, in a university sociology department in France, a young scholar appeared to be much less sympathetic to my ethnography, both for the way I presented it and the realities it displayed. He concluded a series of critical remarks with a question: how could I know that the officer felt satisfied when commenting on the ruined celebration of the youths? This somewhat surprising interrogation, inasmuch as the expression of pleasure was easy to recognize in the tone of the statement and to relate to the meaning of the phrase pronounced, had the merit of reminding me that ethnography does not obligatorily convey “reality” and “presence” in the same way to everyone — it may also induce resistance.

The reaction of my interlocutor, of which I encountered similar occurrences on several occasions in the French realm of social sciences, indicated not only a suspicion toward a method and its user, but also a more general distrust regarding critical thinking during the past two decades in France. This attitude was indeed symptomatic of the backlash against the “critical sociology” developed by Pierre Bourdieu and its substitution with the “sociology of critique” of Luc Boltanski (Bénatouïl 1999). Whereas the former credits the social scientist with the power to overcome the denial of the social by agents and to critique their spontaneous

sociology (Bourdieu 1984), the latter limits itself to the grammar of the arguments invoked in disputes and the modalities of the resolution of such disagreements, the only possible critique allowed to the social scientists being that produced by the agents themselves (Boltanski 2011). In large part, the pragmatic turn, which replaces critique as method with critique as object, is empirically grounded on an analysis of texts and discourses, rendering ethnography a negligible instrument.

For instance, when studying police violence, it is not the illegitimate or excessive use of physical force as observed in actual situations that is examined, but the criteria used by the disciplinary commissions to adjudicate the complaints of victims (Moreau de Bellaing 2009). In my own work, by contrast, critical thinking concerned both the category of violence, which I argued could not be restricted to brutality but had to include other forms I designated as moral, such as humiliation, vexation, debasing comment and racist insult, and the conditions making violence possible, within the organization as well as in the larger political context. The reaction I encountered among some scholars in France thus also had to do with the critical potentiality of public ethnography, and the reluctance they demonstrated was part not only of an intellectual shift in dominant sociology but also of a broader political evolution in academia characterized by conservatism.

By associating the potentiality of critique and the publics of ethnography, I want to reappraise the distinction Michael Burawoy establishes between critical sociology and public sociology, the former being addressed to an academic audience and the latter to a more general one. In the depiction of the scene I evoked earlier, the contrasting reactions of my two interlocutors indicate not only that public ethnography was inseparable from critical thinking, but also that writing for various audiences of non-specialists did not exclude the participation of scholars in the debate thereby prompted. In fact, critical and public approaches generally share convergent goals. But for all that, to have critique go public is not without danger.

### POPULAR TRANSLATION

“This is a difficult question, I know, and you devote many pages to it in your book, but we have less than one minute before the news, so could you answer straightforwardly: are the police racist?” I am not certain that my response on prime-time national broadcast in the allotted seconds did justice to the hundreds of hours spent reading and writing on the complicated issue of racial discrimination and institutional racism in law enforcement. At least, it later provided an opportunity for a former police unionist who had taken up politics and campaigned as a



IMAGE 4. Being asked the question, are the police racist, with less than a minute for the answer. Source: “L’invité,” *France Inter*, December 28, 2011, <http://www.franceinter.fr/emission-l-invite-didier-fassin>

right-wing candidate in the legislative election to vehemently protest against my alleged accusation that all police were racist—a statement I had, of course, never articulated in this form. The anecdote exemplifies the difficulties encountered when crossing the boundary between the academic realm and the world of the media, and having to adapt one’s discourse to a constrained format.

One should not, however, caricature the work of journalists. On the radio, I was frequently given the necessary time to account for the complexities and subtleties of the interactions between the police and the youth, for the importance of the historical background and political context, for the varieties of practices among law enforcement agents and the way these differences could be expressed: when programs lasted twenty minutes or one hour, it was not so different in length from a scientific communication or an invited lecture, respectively, even if the format and the audience were different. In the printed press, I frequently wrote the responses to questions, instead of participating in oral interviews, which permitted a control of my own expression and avoided the transformation of my words, and although my answers were subsequently edited, I always reviewed what I was supposed to have said. Moreover, most of the time it was clear that the journalists involved had read the book, or substantial parts of it, which made the conversation with them much more fruitful: in these favorable cases, programs or articles were the outcome of a productive and respectful collaboration.

Of course, I could not prevent some issues. When asked by the news magazine *L’Express* to submit a short contribution rather than be interviewed, I chose to narrate a brutal law enforcement operation from the symmetrical perspectives of





IMAGE 5. From “scenes of urban life” to “hunting scenes in the banlieues.” Source: Jean-Pierre Rey, “Des policiers de Saint Denis patrouillent dans une cité sensible,” April 29, 2008, <http://jprey.wordpress.com/2008/04/29/police/>

the police and the population, suggesting two distinct interpretations of the event. I titled the piece “Scenes from Urban Life,” inspired by Honoré de Balzac’s novels about 19th-century France. When the issue came out, I realized the headline was turned into the sensationalist “Hunting Scenes from the Banlieues,” in reference to Martin Sperr’s play and Peter Fleischmann’s film *Hunting Scenes from Bavaria* about a manhunt in prejudiced and intolerant postwar Germany. The apologies from the editor-in-chief of the magazine did not change what I could only imagine to be a disastrous effect on the readership. I subsequently learned that it was the assistant editor who could decide at the last minute, without consulting his superior, what title would work best for an article.

Along the same lines, having written about the conflicting perspectives of Ilongots and Westerners regarding violence—the headhunting of the former being considered horrific by the latter, while the deployment of soldiers in situations of war by the latter was repugnant to the former—Renato Rosaldo (1989) recounts how he was interviewed by a reporter from the *Chicago Tribune* interested in this exercise of comparative anthropology, who published it under the didactic banner: “Headhunting Tribe Provides a Lesson.” The story later caught the attention of other journalists, who transformed it into a criticism of anthropological relativism with a *Washington Times* article titled “And This Is How Profs Get Ahead,” and into a condemnation of barbaric practices with the *National Enquirer* headline “Headhunter Horror.”

However, it was much less probable that the Ilongots would peruse these articles than that the police would read my work. In fact, no one knows precisely who comprises the audience reached through the media. Sometimes, a small part of this audience may become identifiable, although anonymously, when reactions are published. One of the interviews I gave to the news website Rue89 was consulted 110,000 times in two days, and sparked 591 impassioned replies from individuals using exotic pseudonyms like “Lilliputh,” “Blackbear,” or “Cannibal Ferox.” Some bore witness to their generally painful experience with the police, others proposed little nuanced political analyses; some pleaded in favor of the anticrime squads, others called for their dissolution; some discussed the merits of anthropology, others denounced its uselessness. Most of the time the remarks were virulent and excessive, in one direction or another, although certain bloggers adopted a more restrained style. But it was clear that no one referred to the book itself—one person actually suggested that before writing about it, commentators should read it.

Thus, when ethnography goes public, what is produced by the media, whether reviews or interviews, generally becomes a substitute for the study. Because they are supposed to provide the core of its content in a few paragraphs or pages, such digests spare long hours of reading. And the knowledge most people—including academics—have about books is derived from what critics or analysts have written about them. This is true of all printed works, but is even more so when such works concern sensitive topics and have been conceived as public interventions. “When they read what the papers say we wrote,” as Ofra Greenberg (1993), author of *A Development Town Visited*, a study of a poor Israeli settlement, puts it. She narrates her painful experience with a journalist who issued a venomous review of this book and partially forged an interview with her. According to the reporter, the anthropologist depicted the people of the community where she had lived for five years as cruel, selfish, and obnoxious. The article understandably generated a wave of furor among the inhabitants and a virulent response by a local politician, who admitted in passing that he had not read the book, because “the article was good enough.” It took some time for the author to recover from this stain by intervening in the media in her turn.

I never encountered such an ordeal. However, I was interviewed by a journalist from one news agency who attempted to show me that he knew everything about my topic and later reproduced his own expressions with quotation marks as if I had articulated them. Several articles followed in the regional press repeating the original one, bearing titles like “Anticrime Squad Target of a University Professor” and describing my book, which they had not read, as a “pamphlet against the police.”

On most occasions of collaboration with the media, however, I was impressed by the respect journalists showed toward a scientific approach to questions they regarded as important using a method they valued, notably because it represented a practice of investigation most could not afford due to lack of time. Indeed, as Thomas McGuire (2008) suggests in his discussion of parallels between investigative journalism and anthropological inquiry, I realized that reporters were more comfortable and perhaps more familiar with ethnography than many experts in police studies, who were not only reluctant to employ the practice of ethnography themselves but critical of others who embraced it. From Nina Bernstein in the *New York Times* to Sonia Kronlund on France Culture, there are various examples of journalists who pursue their profession using an ethnographic approach.

However, one should not reduce the problems raised by the popular translation of ethnography to a discussion about the qualities of journalism and journalists. The very act of translating research to large audiences within a limited space imposes drastic transformations on its content. Even in an article over which one has complete control, the fact of restricting its length and writing for a general audience produces an object totally different from a book chapter or a scientific article. To adapt one's analysis to the format of a newspaper and the supposed expectations of its readership, one has to renounce the subtleties of theory and the precautions of method, to present a condensed thesis rather than thick descriptions, to abandon the narrative for the argumentative, to make a legible and convincing claim. In this exercise, ethnography seems to be reduced to a legitimizing instrument: the mention of the months or years spent in the field merely serves to attest to the authority of the anthropologist's account. But these challenges have their reward. An editorial is probably read by at least one-thousand times more individuals than a book or an academic paper, and may generate a discussion with or among the readers. If something of the ethnography is certainly lost in translation, something else is gained in the encounter with new publics.

### **PUBLIC DEBT**

"What he writes is exactly what we live," a young Arab man stated on a national radio broadcast during which my book was discussed with a journalist and a writer who had gathered narratives about the daily lives of ten youths from the same town. His parents were Algerian, he had grown up in a housing project and was now studying law in college, but, as he explained, he was still submitted to discriminatory and humiliating harassment by the police, especially the anticrime squads, when taking the subway to the university or chatting with friends in the

street near his home. I received several such testimonies, either directly or more often indirectly, from friends living in these neighborhoods or colleagues working there. A sociologist who had long been working with youths in the outskirts of Paris wrote to me that some of them would tell him: "It's good that this is a book by a scholar, because when we say these things nobody believes us." Such comments suggest two general observations. First, the young men, mostly from immigrant working class families who live in the housing projects, do not necessarily conform to the commonly conveyed image of social and economic deprivation as well as cultural and intellectual penury. Second, they are denied access to the public sphere under most conditions, except through deviant or violent acts that provide them with the only means to be seen and heard.

Beyond these two observations, the comments of these young men pose a question to the social scientist that, paraphrasing Alfred McClung Lee (1976), we can formulate in the following way: "For whom do we write?" The preposition "for" does not refer here so much to the public dissemination of the work as to its moral obligation: towards whom should we feel obliged? I begin the acknowledgments of my book asserting that, in writing it, "I am repaying a double debt: to residents of housing projects, particularly the young ones among them, whose experience of law enforcement, so little considered and so rarely heard, I seek to give an account of; and the police, especially the senior officers who, probably with few illusions about what might be expected from a researcher, accepted being made the object of study." Carrying on an ethnography is cumulating debts. Making it into an intellectual production is repaying them—at least in part. Of course, creditors are many, from the institution that employs the researcher or funds the research to the discipline and society as a whole, which is true of any scientific practice. But in the specific case of ethnography, I contend that we owe in priority those with whom we worked and those primarily concerned by the issues on which we conduct our work: indeed, including this second category is what distinguishes ethnography as critical. During my fieldwork, I patrolled with officers, but their policing unfairly affected a certain citizenry, which is why I consider myself to be indebted to both groups, although in different ways and with an unequal weight.

When putting my work into words, the difficulty was therefore to remain loyal to both sides, especially since their relationship is so antagonistic, as may also be the case in other contexts such as prisons (Liebling 2001). To make this distinction explicit, one could say that my loyalty to the police rests on deontological grounds, in the sense that it is defined by our everyday interaction in a professional context, whereas my loyalty to their principal public relies on ethical principles, in the sense

that it is determined by more general concerns about a certain state of the world viewed from the perspective of social justice.

For three years, before I could begin writing, my problem was simple: how to reconcile the two loyalties, which seem so incompatible? It would be quite excessive to affirm that I succeeded in doing so, but I can assert that, at least, I found a form that rendered writing possible. The solution I propose is thus somewhat distinct from that famously defended by Howard Becker (1967). To the question: “whose side are we on?” he answers that the social scientist is on the side of the “subordinate” to counterbalance the overwhelming “hierarchy of credibility” that only acknowledges the side of the “superordinate.” My approach is distinct in the sense that I do not choose one side—that of the youth—but being physically present on the other side—that of the police—I am in a position to observe what happens during the encounters between the two and later relate these findings to the larger picture of state interventions regarding certain populations.

In other words, I attempt to combine interactionist and structuralist approaches. On the one hand, I depict the way the working class of immigrant backgrounds, most notably residents of housing projects, are treated by law enforcement agents. I describe the stops and frisks, the insults and the provocations, racial discrimination and moral violence. I account for punitive expeditions and random punishment, which affect indistinctly individuals known to be innocent, and I reflect on the arguments provided to justify these acts. On the other hand, I discuss the sociological characteristics of the officers, most of whom come from country towns and rural areas and are posted in districts situated in urban areas with which they are not familiar and where they are encouraged to arrest certain categories of denizens. I relate their harsh policing to the role they are expected to play in terms of contention and repression of populations particularly affected by increasing inequalities. I reconstitute the genealogy of the politics toward immigration and insecurity in recent decades and analyze the substitution of a welfare state with a punitive state.

This approach situates protagonists’ experiences and practices in a broader framework, which they perceive but often cannot formulate critically. Not only could youths recognize themselves in my depiction of their interactions with the police and the logics underlying these interactions, but law enforcement agents could likewise use the analysis for their own purposes; for instance, when unionists criticized the harmful consequences of the politics of quantified objectives, or when officials condemned the counterproductive operations conducted by the anticrime squads. This was how some lawyers took up my work. I was solicited as a witness in

a lawsuit involving racial harassment and physical violence by a police officer against an African adolescent, and wrote a statement not about the specific case, which I did not know, but about the general conditions of law enforcement practices in projects, to provide a context in which the boy's words could be heard. I was also summoned as *amicus curiae* in the first trial against the Ministry of the Interior regarding ten complaints of racial discrimination in stops and frisks filed with the support of Open Society, and again contributed to this case via a short report to be read in court. These various interventions were probably not decisive, but because they involved the symbolic authority of academe, I participated, along with actions conducted by nongovernmental organizations on similar issues, in the reframing of the issues raised by law enforcement in the public sphere.

When I began my research, it was generally accepted that growing insecurity in housing projects implied specific modes of operation involving special units and including unlawful procedures: the riots confirmed this representation of dangerous classes that justified local measures of exception. The debate that followed the publication of the book questioned the legitimacy of these practices and the policies that promoted them: from mere victims of hostile populations, officers became perceived as part of the problem of the increasing tensions observed in disadvantaged neighborhoods. I certainly do not want to overestimate the impact of my study in this evolution, which has multiple and complex determinations, notably emerging social mobilizations in a context of rejection of a government that had precisely centered its discourse and action on the question of insecurity, stigmatizing minorities and immigrants while calling for more repression. Echoing these preoccupations, the critical ethnography I had developed simply gave empirical evidence of the issues as well as an interpretive framework to make a better sense of them.

### CONCLUSION

Ethnography matters for contemporary societies: such is the argument of this essay. This claim derives from the very activity of the ethnographer—a presence both involved and detached, inscribed in the instant and over time, allowing precise descriptions and multiple perspectives, thus providing a distinctive understanding of the world that deserves to be shared. Ethnography is particularly relevant in the understudied regions of society, but can be significant also in spaces saturated by consensual meanings: in the first case, it illuminates the unknown; in the second, it interrogates the obvious. To play its possible social role, ethnography must be simultaneously critical and public, in the meaning given earlier to these adjectives. Admittedly, neither is univocal and both involve

epistemological, ethical, and practical complications. Yet the combination of critical and public perspectives can make a difference in the comprehension of major contemporary issues, provided that the outcomes of such issues are discussed, contested, and appropriated by various segments of society. This plea for critical and public ethnography has no hegemonic objectives: other ways of conceiving and practicing ethnography remain entirely legitimate. But it is probable that the approach of which I propose a defense and illustration here can strongly contribute to the relevance and significance of the social sciences for contemporary societies.

This contribution depends, however, on how the relationship between the academic world and the public sphere is structured. Because it has a long history of negotiation of this relationship, epitomized by the figure of the “public intellectual” created during the Dreyfus Affair at the beginning of the 20th century, France offers a particular context in which the communication between the two realms is significantly greater than is typically the case in the United States. An indicator of this contrast would be the number and diversity of signatures of editorials in *Le Monde* and *the New York Times*, especially when referred to the respective demography of scholars in both countries. However, it is not the “public intellectual” capable of addressing almost any issue, in the manner of Jean-Paul Sartre, that I have in mind, but rather the “specific intellectual” defined by Michel Foucault in terms of his or her competence within a given domain. If ethnography goes public, it can only be in this more circumscribed as well as more qualified way. But again, it would be illuminating to count the anthropological and sociological volumes grounded in empirical research that are available in general bookstores in both countries, and also note the quasi-disappearance of the discussion of these works in newspapers and magazines, even literary ones, in the United States; as shown here, similar media in France at least continue to comment on such works. To make a difference, ethnography has certainly to regain its publics—everywhere, indeed, but in certain societies more than others.

Having begun my essay with a personal anecdote concerning Claude Guéant, I will end it with another one regarding his successor. After the 2012 presidential election, a new French government was formed. Manuel Valls, the new minister of the interior, invited me for a discussion in the presence of his two chiefs of staff. During one hour, we talked about the book, which he had at least partially read. Contrary to his predecessor, he explicitly, even if only privately, approved of my analysis, suggesting that his collaborators should peruse the study and asking my advice on various subjects. As he walked me out, he confided that it was difficult to

change law enforcement due to the inertia of the institution and the resistance of the unions. However, over the following year he did not propose any substantial reforms, even renouncing the promise made by the president during his campaign to institute a receipt system for stops and checks, a measure that was supposed to reduce the practice of racial profiling in the street. By citing this final episode, I want to definitely relativize the impact of ethnography on political decision and social change. Yet, however modest, this contribution still matters.

### ABSTRACT

*Based on the experience of researching and writing a book on urban policing in France and its reception by the media and various audiences, this essay discusses the challenges facing a public ethnography—distinguished from public sociology or anthropology. First, I differentiate two tasks (popularizing and politicizing) and multiple publics (imagined or encountered). Second, I plead for the exploration of understudied objects and terrains (black holes) and the attention to the ordinary as a way to transform the perspective on the world (critical potentialities). Third, I evoke the risks related to the appropriation of the work by the media (popular translation) and the loyalties toward the diverse and sometimes opposed subjects of the research (public debt). My conclusion underlines why rendering public the works of critical ethnography matters for contemporary societies. [public ethnography, critical ethnography, audiences, media, policing]*

### NOTES

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