Very few tell in detail what a juvenile delinquent does in his daily round of activity and what he thinks about himself, society, and his activities … One consequence [of his insufficiency] is the construction of faulty or inadequate theories. Just as we need precise anatomical description of animals before we can begin to theorize and experiment with their physiological and biochemical functioning, just so we need precise and detailed descriptions of social anatomy before we know just what phenomena are present to be theorized about … We do not … [then] have enough studies of deviant behavior … [or of] enough kinds of deviant behavior. Above all, we do not have enough studies in which the person doing the research has achieved close contact with those he studies, so that he can become aware of the complex and manifold character of the deviant activity … If … [the researcher] … is to get an accurate and complete account of what deviants do … he must spend at least some time observing them in their natural habitat as they go about their ordinary activities.

(Becker 1960:166–170, quoted in Matza 1969:40)
The majority of studies of youth gangs are achieved in the grand tradition of positivistic social science. Taking natural science as the paradigm of good research, a premium is placed on the value neutrality of the observer, the scientific rigor of the methodology, the unpoluted character of the data and the generalizability of the findings, all with the aim of proving or disproving ideology-free testable hypotheses. In this vein US criminology has seen gang studies move increasingly away from the interpretive, holistic and probing approaches of earlier Chicago School analyses (as quoted above) to the certitude of survey-based truth claims, “jail house criminology” and an assortment of studies whose findings reflect the race, class and gendered position of the investigators and their largely uncritical domain assumptions. As noted criminologist Jock Young has commented:

What we need is an ethnographic method which can deal with reflexivity, contradiction, tentativeness, change of opinion,
Studying the gang critically posturing and concealment. A method, which is sensitive to the way people, write and rewrite their personal narratives. Our problems will not be solved by a fake scientifi city but by a critical ethnography honed to the potentialities of human cre-
ativity and meaning.

(Young 2004)

In this chapter I argue for a critical, anti-colonial ethnography which is best suited to study subaltern groups and their “hard to reach” populations whose lifestyles, “habitats” and purported character-
istics are highly stigmatized and pathologized by the larger society. Such an approach is based on the premise that all social and cultural phenomena emerge out of tensions between the agents and interests of those who seek to control everyday life and those who have lit-
tle option but to resist this relationship of domination. Drawing on the diverse methodological and theoretical work of researchers from across the disciplinary divide (such as Geertz, De Certeau, Whyte, Katz, Conquergood, Young, Mendoza-Denton, Fine, Ferrell, Scott and Cliff ord) and my own experiences in the field over two dec-
ades I argue for a mode of social inquiry that takes seriously Mills’ (1959:225) advice:

Before you go through with any piece of work, no matter how indirectly or occasionally, orient it to the central and continu-
ing task of understanding the structure and the drift, the shaping and the meanings of your own period, the terrible and the magnificent world of human society.

Such research at a minimum:

(1) is committed to the provision of multiple forms of data, ana-
lyses and theoretical constructions designed to critically engage, understand, humanize and holistically analyze subjects while resisting the dominant tropes of a society that pathologizes, devalues and obscures the humanity of these same subjects;

(2) is based on the ongoing dialogical relationship between the investigator(s) and the investigated, such that the research subjects
are presumed to be active, historical agents embedded in structured contexts and contingent situations; and
(3) produces knowledge that potentially contributes to social reform, self-empowerment and social justice.

**Critical gang ethnography**

If traditional, modernist ethnography is about studying the “folkways” of a community, painstakingly describing its everyday rhythms, its complex systems of social interactions, its relationship with the broader society and the different meaning systems that cultures and subcultures develop, then critical ethnography problematizes each of these notions, situating them within the asymmetrical power relations of a globalized capitalism within which we are all situated. A critical practice of ethnography is tied to excavating “the political underpinnings of all modes of representation, including the scientific” (Conquergood 2013: 81) and instead of asking “what is,” it asks, “what could be” (Thomas 1993:4), as was the distinguishing feature of critical theory itself.

Further, it does not take at face value the meanings of an action, speech term or observation but is always aware of the ironies within these spheres of “meaning-making” that speak to the contradictions and the symbolic and hidden meanings within the data. This distinction in methodological practice and epistemology is part of the general crisis of representation in the social sciences (Marcus and Fischer 1986), which in large measure occurred with the decline of imperial and colonial power after World War II. The critical turn has had a powerfully radicalizing effect on the legitimacy and assumed authority of social science research, which had primarily accepted unproblematically a research practice and epistemology of enlightenment based on the privileged nature of the text and assumed role of the author. While this post-colonial (and often postmodern) critique has had a great deal of currency in disciplines such as anthropology and sociology it has had less impact on the US version of criminology so long dominated by the criminal justice industry, and has been particularly resisted by a criminology common in gang studies.¹

As important as this critique of what passes for much gang research is, a host of questions still remain for those aiming to attain
a more consciously reflexive and political orientation to the praxis of gang ethnography. For example, how do we do this research without becoming the “zoo keepers” of the deviants (Young 1971)? How do we resist adopting the gaze of the colonizer and/or the middle-class paternalist? Or how do we prevent ourselves going “native” and losing all sense of perspective? These are some of the central questions with which we are confronted regardless of our good intentions and rigorous opposition to the dominant paradigms of a social science always constructed and conceived from above.

In the following I expound upon six methodological areas that have been at the core of much of the critical ethnographic research I have been engaged in, hoping to avoid what I have referred to as the “social scientific safari” 2: (1) the fallacy of the neutral observer; (2) researching from somewhere; (3) principles of collaboration, entrée and field work praxes; (4) data for a holistic study; (5) representing and (w)riting; and (6) converting knowledge into power, not oppression.

The fallacy of the neutral observer

The critical approach recognizes that the world is intrinsically unfair and that the material, ideological and cultural structures are heavily stacked against the subjects/participants. Therefore, when the subjects act or speak they do so from a peripheral position that makes attempts to hear and understand them difficult. No matter how one tries to objectively experience and portray the setting or engage subjects through interviews we always have an impact on the environment and vice versa (see Maher 1997). One cannot divorce our presence from our thinking and feeling, or what Galeano calls the need to realize that we all engage in “senti-pensante” (feeling/thinking) rather than being varieties of the fragmented Cartesian binary that orthodox social science usually maintains. As Galeano explains:

Why does one write, if not to put one’s pieces together? From the moment we enter school or church, education chops us into pieces: it teaches us to divorce soul from body and mind from heart. The fishermen of the Colombian Coast must be learned doctors
of ethics and morality, for they invented the word *sentipensante*, feeling-thinking, to define language that speaks the truth.

(Galeano 1998:32)

Thus ethnographic researchers are always part of the scene, always in a processual relationship with the subjects, negotiating positions, stances and strategies. They are thus embodied and embedded in the data that interacts with and through us, with the research act of observing and recording always a matter of interpretation. Further, we are not simply looking on but rather we are rupturing (whether wittingly or unwittingly) the normal patterns of life, often as we record those external ruptures that come from state agents arresting and engaging members of the subordinate class.
Studying the gang critically

Obviously the researcher tries to be hyper-observant but bodies of knowledge, codes of action, rituals, symbols and practices are not self-evident, coming neatly packaged with labels. So many meanings are frequently missed due to mis- and non-recognition or privileging different dimensions of the activity and perceived reality. The only remedy for this is time spent in the field to become increasingly acquainted with the terrain, the culture and the community, accepting that such co-presence necessitates proximity to the subjects. This intimate, intrinsic relationship requires an immersion into the group and its terrain that is appreciated in the methodological notions of action research, participant observation and “edge work,” and fundamentally contradicts the presumed and prescribed separation of the researcher from the researched (for good examples of these research methods in gang studies see Black 2010, Vigil 1988, 2007, Weide 2014, Maher 1997, Durán 2013). To this end I agree with those who argue that to get close to the naturalist meanings of the subjects, e.g. to have the possibility of the subjugated knowledges of the group or subculture revealed, the researcher has to be fully engaged and committed to the subject(s) yet open to different possibilities, contradictions and ambivalences in the encounter, in the making of everyday life (Schutz 1967).

Of course this is a risky, precarious endeavor and the pinnacle of power or location from whence the researcher normally descends, speaks or observes is at stake, but that is the price of doing research that does not promote what Conquergood called “epistemic violence” both to the researched subjects and/or the community. For example, how do we access “them” when “they” are so cut off from our own life-worlds? How do we trust what “they” tell us since most of us do not naturally, organically live in their life-worlds? How do we gain their confidence, relate to us their “insider” knowledges when they have little reason to trust outsiders who boast both the “expert” knowledge and status that reinforces lines of social hierarchy and cultural value? And how do we overcome or even recognize our own biases, given the power of hegemonic discourses in which these subjects are often judged to be inferior, pathological, remorseless and incorrigible (e.g. Bennett et al. 1996, Yablonsky 1963, Fleischer 1998).
Therefore we must remain mindful that we come from somewhere and that we are always positioned and situated. However, at the same time that there is a point of departure from which we speak, observe and interact through our experiences of race/ethnicity, gender, class and age, these identities and signifiers do not amount to a fixed impenetrable location in the social and economic borderlands. Rather it is our obligation to recognize that borders bleed (Trinh 1991), and the task of the ethnographer is to record and enter this zone of complex meaning-making, transgression and liminality, to share layered social actions, to reciprocate, to experience and to co-perform with the researched population through worked out processes of social solidarity, mutual respect and recognition. Such praxis might be seen as the ethnographer’s political statement, a form of ethnographic activism and a way to demonstrate the right of all of us to have a voice in this contested space we call society – a space that is at once home to the researched but also a watched terrain, a surveilled habitat constantly being policed, Othersed and exoticized (Di Leonardo 1998). In short, a space made to feel off limits and beyond our scope of “verstehen” (see Ferrell 1997).

Researching from somewhere

One of the first projects of critical ethnography is to recognize where we come from to assist us as we negotiate a way into a community or group. Further it is important to come to terms with the fact that our research and our site is not always chosen by us but rather it is chosen for us because of who we are. In Figure 4.3, of my father’s East End (London) shop taken at the turn of the twentieth century, is represented much of my identity. I was born in that building, raised in that environment and socialized by the traditions of artisans handed down through generations of my family. If I am interested in the marginalized and fascinated by the cultures of inner-city populations it has a lot to do with that sepia-colored photograph taken by someone more than a century ago. I make this point because all too often in research of subaltern populations the investigator appears to have dropped from another planet, such is his or her relationship with the researched population, and it is frequently the case that the
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investigator reveals nothing about him or herself to the subjects. In fact, there are many esteemed and well-funded researchers who have never seen nor met any of the research subjects. In the studies I have long been involved in my praxis is quite the opposite and I take great pains to explain to the group or members of the subculture over time who or what I represent in history.

Therefore, to do critical ethnography with gangs is to move consciously and reflexively from the center to the periphery, to establish a trusted location in the community under study that becomes accepted and natural to the study’s participants. As I already explained, the concept of situated space is crucial in such studies and this includes the situated place/space in which research is carried out. While it is true that many gang ethnographers try to do their research in the habitat of the subjects with interviews, for example, in the homes of research participants as well as in public spaces where the subject
feels comfortable and not under threat, if there is a neutral zone that can be found or at least a space that offers the subjects protection and reassurance of the study’s legitimacy, this is preferable.

In my own experience of carrying out gang research in different terrains on the East and West Coasts of the United States as well as in Europe, the Caribbean and Latin America, I have found the choice of location for this research to be quite critical to the success of the project. In San Francisco many of my fieldwork interviews were done in parks, public high schools and the homes of the subjects; in the Dominican Republic they occurred in public spaces that included restaurants but also within the confines of property belonging to a radical political party where some of the members regularly assembled. In New York, where many of the most revealing, detailed and richly textured interviews were performed, we were fortunate to have access to a church that provided a regular space to the group’s members and represented a symbolic counter-space to a society that was increasingly attempting to control the group on almost every level, spatially, culturally, politically, etc.

Consequently, such considerations as research space combined with the identities, personalities and skills of the researchers, the stated versus perceived auspices of the research and the stated versus practiced methods of the researchers are key to a study’s integrity and its ultimate success not only in the eyes of the researcher but in the eyes of the researched. The importance of this combination of subjective and objective factors was critical to the quality of our experience with various youth street subcultures during the late 1990s in New York City and allowed us to establish what we called “The Street Organization Project” in 1997 where we produced several substantially innovative pieces of work (including articles, books and photographic exhibits) along with well attended, highly inclusive national and international conferences and a range of interventions (from academic presentations, to community workshops and policy recommendations).

How did we achieve this without engaging in “fables of acceptance” (Di Leonardo 1998:23)? As stated earlier, one way we did this was through a church that became the site where the group’s members could meet to discuss their own affairs but also to convene with
trusted members of the community who were interested in furthering the radical political project that the group was now embarking upon. The church, then, became the project’s research and political hub in the community and was perceived quite differently to the academic center of the study that was still located at the college. This enabled the subjects to approach the inquiry not as something from a distance and external but almost as an internal project and as an integral endeavor of the group and of the community (i.e. not just as the tolerable idea of outsiders motivated by some vague commitment to social justice at best and at worst careerism and self-promotion). In a sense, it became over time “their” project as well as ours and a sanctuary that everyone looked forward to utilizing since this was where they could achieve respect and validation, feelings that were all too uncommon in their daily lives.

A second issue in addressing the distance between the researcher and the researched is the degree to which we reveal our other identities to the subjects. In our research we have always embraced this principle and find that a holistic knowledge of ourselves both solidifies and demystifies the relationship. For example, Luis Barrios, my co-principal investigator and a priest at the church, was a noted leader of the Latino/a community with a long history in the community’s struggle for social justice. He is also bilingual and bicultural and shared many of the experiences of immigration, social exclusion and oppression that featured in the accounts of the group’s members and their families. In time he became an important confidante to both the leadership and the rank-and-file, with his blessings and sermons helping the group to legitimate itself to the public while his spiritual and political analyses contributed to the group’s development and progress. Moreover, Barrios went through the struggle with the group and this was his way of doing “edge work.” He was, therefore, not detached, in that social scientific, value-neutral sense, but was a partisan in the quest for knowledge and societal respect.

My role was quite different. I revealed to the group my own history of struggle in the labor movement and my own lower-class origins and this allowed them to see me outside of my middle-class academic status and professional persona. In addition, I stayed with
the group across time and place, day after day, and did not opine negatively or disrespectfully about the group to the media or other agencies, even though I was frequently asked for interviews since the group was often in the public eye. At the same time I explained that my research role was to stand back and observe the proceedings with a little more distance though at all times I was expected to be there “when it counted.” This is not to imply that the group’s members did not test me on various occasions and on one occasion, in particular, my intentions were vigorously questioned as I was called upon to respond to the perceived transgression of another colleague whose actions had angered the group. The other field researchers did something similar, judiciously revealing their own historical narratives of shared oppression, not in any heroic, self-aggrandizing way but as a shared, human experience, mark of respect and discernible point from which empathy could be understood and accepted. We also had the collaboration (see Chapter 5) of the group’s leaders, with the President Antonio Fernandez encouraging the group to grant us access to all their meetings as well as individual interviews while his influential “lieutenant” Hector Torres constantly discussed with us the group’s development and helped to secure our entrée across multiple sites of the group’s activities in real time.

In addition, there were several other bilingual researchers (male and female) who would hang out at various locations for extensive periods of time with different branches of the same group, after the local leadership had granted them permission. Some leaderships were more welcoming than others but all of the different branches required an extensive amount of time to “accept” our intrusion in their private and public lives. Ultimately, we managed to successfully have multiple field sites running concurrently, all located deep within the group’s respective neighborhoods. This allowed us to see comparatively and holistically (see later in this chapter) what was happening across the group, between the group and other groups and between the group and the community, all of which increased our breadth of knowledge as well as our legitimacy. For, in the latter case, it demonstrated our ongoing commitment and our genuine interest in trying to understand the group in all its profundity.
The act of collaboration

unless community participants are actively involved in both research and its uses … both the research and its ultimate uses tend to be highly suspect. While this can be termed politicization, the alternative is not very pleasant either. Unless the community is involved, so-called objective research will almost inevitably be politicized beyond the researcher’s control.

(Moore and García 1978:10)

The third issue is a commitment to the practice of collaborative research as briefly elaborated in the quote from Joan Moore (above). Fundamentally, this is the formation of a relationship with the subjects/participants that is transparent, mutually respectful, built on trust and with benefits that both parties can understand in intrinsic ways. Why is this important for a critical research method? First, we need to be accountable for what we say about communities that have much less formal cultural power than we do. Second, because we cannot get close to these communities and subcultures without their consent and cultural guidance. Third, we need the help of the researched in designing the study, in coming up with questions that only they would know to ask while addressing issues appropriately in a language that is understandable and will provoke a response. Fourth, we need to think of constructive ways to redistribute the knowledge back into the community so that the community can utilize and claim this knowledge about itself for itself (see Moore’s quote above). Fifth, we need to think of research as mutually empowering, of being able to offer opportunities on both sides of the line, and to provide a means by which marginalized communities can think differently about themselves in order to gain more resources as well as to counter the criminalizing gaze to which they have long been subjected. Sixth, we need to be aware of what such communities can teach us about ourselves. And finally, we need to be open to collaboration outside of the community and be prepared to consult with a range of researchers who can bring different levels of expertise to the data.
I would argue that rather than see all human meaning as modeled on one type of code, we need to see social life as containing many different kinds of meaningfulness, incarnate in different practices and forms, layered and overlapping, connecting up in complex ways.

(Willis 2000:22)

If one of the basic tenets of critical ethnography is to humanize the research subjects it is incumbent upon us to collect data that more
fully explores the environmental contexts in which the groups and the subjects make their lives as well as what constitutes the subjects’ multiple identities, practices and social obligations/relationships. This basic reconceptualization of these “deviant” social actors as subcultural participants struggling for cultural meanings within massively unequal social, economic and cultural power relations compels us to develop an “ethnographic imagination” similar to the one advocated by Paul Willis (above). An imagination that goes far beyond the research parameters normally conceived in most gang projects.

Consequently, in conceiving of the contradictory agency of gang members on the streets, in institutions such as schools, prisons and work places, in their respective families, across and between local and

FIGURE 4.5 A blown-up photo of King Blood at an ALKQN meeting held to commemorate his birthday in 1997 (photo anonymous). Image courtesy of Antonio Fernandez.
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members routinely enter and leave gangs;
members go through different periods of development within gangs;
gangs themselves change across time;
laws toward gangs change across time;
gang styles go from the margins to the mainstream;
some gangs become intergenerational while others simply disappear;
some predominantly male gangs are run by women;
some hyper-masculine prison gangs allow gay and transgender members;
some gangs which formally only allow members of a certain race include members from a variety of races;
some gangs have overtly political doctrines while others have none;
gangs of various descriptions are increasingly found in countries across the developing and developed world.

Thus, considering the vast variations between gangs across space and time, how do we begin to understand or even compare their internal and external meaning systems? How do these systems relate to different societal reactions across a variety of contexts? Such questions can only be answered by the collection and analysis of multiple forms of data, many of which we do not know exist before the research begins. This variability within and across the groups can be gleaned from some examples of findings from my own research.

(1) Although the home of graffiti is considered to be New York, with the massive growth of this street aesthetic during the 1970s (see Cooper and Chalfant 2009 [1984]), this practice was not deemed...
that significant to the street groups we studied in New York City in the 1990s. Meanwhile, on the West Coast the Latino gangs were incredibly attached to their street texts and proudly used them as territorial markers for purposes of what Conquergood terms “affirmation through negation” as the group competed spatially, physically and symbolically with other gangs.

(2) On the West Coast groups produced little in terms of gang-related internal texts, but on the East Coast these were a fundamental means of self- and group expression through manifestos, poems, photographs (see above King Blood), essays, rap lyrics, etc.

(3) On the East Coast we encountered a vibrant oral subculture, with members regularly performing different forms of oratory via individual testimonies or public demonstrations of their commitment to the group, all of which had little of comparison on the West Coast – although similar practices were observed in Europe and in Latin America.

(4) On the West Coast the females in the groups were largely subordinate to the males and had little relative autonomy whereas on the East Coast female members were far more organized and expressed themselves through their own manifestos, meetings, activities and different forms of representation.

Once the researcher taps into this deep reservoir of both collective and individual practices and bodies of knowledge, which can take the forms of art, counter-memory, oral history, prison/street narratives, physical gestures, physical encounters, the particularities of language, clothing, figurative representations, etc., a complexly layered life-world emerges, which has been accessed not just through the usual practices of observation and formal interview exchanges but through the subject’s inner eye. These myriad practices, some of which are ritualized and rehearsed while others are more extemporaneous and not all necessarily understood at the time; their significance as cultural forms may not immediately become apparent. Nonetheless they all belong to the congealed, multi-level, historical experience of the group that we are attempting to capture.

It should be evident that limiting our research to prescribed, readily acceptable and available data severely reduces our ability to
appreciate the wide-ranging realms of self-expression and creativity of these subcultures and what Willis (2000:24) calls their “objective possibilities.” It also closes us off from the array of situations and environments in which the group performs and both the new and the old identities (both individual and collective) change and develop. I maintain that these are critical questions and issues that only a holistic approach to the group can enable us to contemplate, answer and address.

(Re)presenting and (w)riting the under-represented

But de Certeau’s aphorism, “what the map cuts up, the story cuts across,” also points to transgressive travel between two different domains of knowledge: one official, objective, and abstract – “the map”; the other one practical, embodied, and popular – “the story.” This promiscuous traffic between different ways of
knowing carries the most radical promise of performance studies research. Performance studies struggles to open the space between analysis and action, and to pull the pin on the binary opposition between theory and practice. This embrace of different ways of knowing is radical because it cuts to the root of how knowledge is organized in the academy.

(Conquergood 2002:141)

How do we resist reproducing the dominant culture’s pathological, exoticizing and negatively deviant categorizations of such groups and its limitless web of assumptions that normalizes “our difference” from “them”? This is an important and complicated question that de Certeau is alluding to in the quote above and, based on my experience, our success in this endeavor depends on: (1) the degree to which we are committed to a humanistic study of the group; (2) our willingness to represent the group through various mediums and art forms; (3) our practices of reflexivity; (4) writing and (5) the importance of theory.

**Humanizing the subjects**

By now it should be clear that my approach is to show the subjects (both individuals and groups) in all their multi-dimensional aspects and contradictoriness, being careful to situate them in deeply social and historical contexts. To do this means to let the subjects speak candidly, authentically and truthfully and to draw on as many voices as possible, always recognizing the problem of concealment but then also the likelihood of revelation and the various forms that “penetrations” take (Willis 1977) as subjects make “lived assessments of their possibilities” (Willis 2000:34) within their imaginaries.

In so doing we show the different positionalities of members and their associates and, if done with strong powers of observation, noting both patterns and changes, latent and manifest functions, the vocabularies of motives that describe action and the array of outcomes, be they intentional or not, will contribute to a humanizing research that does not have to privilege the text over the people.
Multiple data with multiple analyses

Just as we are committed to holistic data sets so we must be prepared for multiple types of analysis. Of course fine-tuned, rigorous descriptive analysis is of necessity but we must constantly think about the types of data collected and their need for specialized treatments, even if it means recruiting other analysts. For example the socio-linguist approach of Mendoza-Denton (2008), the performative, semiotic view of Conquergood (1997), our own recourse to social movement theory (Brotherton and Barrios 2004), Zilberg’s (2011) social geographic excavations, Miller’s (2001) and Maher’s (1997) interpretation of feminist ethnography, DeCesare’s (2013) photographic forays and Willis’ (1977, 2000) socio-symbolic reading of cultural production all provide different, fresh, innovative contributions to reveal more well-rounded lived experiences of groups and their members. Thus we must refuse to be methodological prisoners of the data, but rather use a radical inductive approach that does not recoil from crossing disciplinary boundaries but at the same is careful not to be dilettantish, borrowing analytical techniques that are not well understood or sufficiently processed.

Reflexivity and representation

I have already argued that reflexivity is a sine qua non of this kind of research, but implementing this is not easy, especially in writing up our accounts either in the present or in the past. It is difficult to reveal the effects and impacts of research as it passes through us, as we become moved by the events or actions, but it is critically important to show this side of the study relationship otherwise we have become supremely privileged. For our arguments may appear sound, with the subjects making appearances to fit the frames, but the “truths” on display will be suspect. I have encountered this on multiple occasions as events in the field sometimes turn into their opposites and our expectations are suddenly dashed. At the same time, however, it is precisely at such points when we are confronted with the unexpected that our observational and analytical abilities become most needed and our real commitment to remain within this bracketed existence is most tested.
(W)riting

Where possible we try to write with energy, verve and clarity. Rejecting the desiccated, neutral expositions of mainstream criminology/sociology we write against the dominant currents that police our disciplines, creating practices and products of the social scientific safari. Of course, this is easier said than done but it is something to be aimed for rather than settling for the pedestrian prose that passes for most social scientific writing. One way to do this is to look to great writers and artists to learn how they wrote with imagination, experimentation and style (e.g. Galeano, Agee and Evans, Mills, etc.)

Further we try to capture not simply the fixed and one-dimensional but the transcendental, the counter-intuitive, the soulful, the accidental as well as the epiphenomena of the quotidian. Mindful that we live in a period when “all that is solid melts into air,” this culture and political economy of extreme liquidity should influence our writing as we experiment with new and old vocabularies to capture figures moving through life, filled with hopes, disappointments, fears and certainties. With my own students in the field I advise them to write ethnography bearing the following in mind:

People suffer and enjoy life, so feel their pain and learn to appreciate how life is laughed at and enthusiastically created at the margins despite the obstacles.

Do not elide unless you have to. It is safer to err on the side of caution and create “thick descriptions” as a habit, playing with the prose to evoke and reproduce the aura of the scene and the existentiality of the moment.

Use your pen to create a canvas. It is a given that this messy web of interactions, histories, and stated and unstated vocabularies of motives is difficult to conceive on paper, and will necessarily be an act of creativity.

Use your pen to perform. For example, when describing graffiti think of the rush flowing through the veins of perpetrators; in a meeting imagine the front stage/back stage antics, as you listen to discourses and utterances remember the symbolic violence that often frames them and the resistances that might contradict the norms of social reproduction.
When writing of injustice tell it like it is. Find your voice, your style, your angle, your scream. You can always edit!

**Theory development**

While we all have theories about these groups, some of which are learned in the academy and some which emerge out of new experiences in the field, as critical ethnographers our obligation is to be open to counter-intuitive data, confounding experiences, a set of realities that sometimes conform to our theories and sometimes completely contradict them, but it is our stretch for theory at all times that is crucial for the furtherance of the study.

In our New York study, we paid attention to Burawoy’s notion of extended case study – the idea that theory needs to be developed and extended in the face of anomalous outcomes rather than simply rejected (see also Snow et al. 2003). In our case there was little in the gang literature that talked about political gangs or about gang politics so we had to go to other perspectives to find out how we might think theoretically about this movement on the streets. As critical ethnographers we are not wedded to a set of theories but rather to the subjects, the community, etc., and we need to be contemplative of all manner of theoretical explanations for the rational/irrational, hidden/opaque social and cultural processes. Consequently, theory drives the project and then the data drive the theory. As Willis (2000:114) makes clear:

To repeat and clarify: the original elements of a “theoretical confession” [i.e. the stated theoretical questions and conundrums being pursued through the ethnographic project] are not tightly structured positions looking merely for exemplification (the hallmark of pointless field work, merely the flip side of empiricism). They are the nagging issues which drive a curiosity within an overall theoretical sensibility of a particular kind.

As a research practice to further the goal of theory development we would have regular debriefing sessions among all the field workers, about once every two weeks. During these sessions we would summarize the data and then discuss patterns, contradictions and anomalies we were witnessing. Following these discussions we would
try to see how theory might come close to explaining our experiences or not. Over the course of the project such discussions and probings produce a substantial amount of analysis that become key to theory development in the final instance.

Using research knowledge for social reform and empowerment

Thinking carefully about de Certeau’s comment above (Conquergood 2002) prompts us to ask how we translate knowledge “about” into knowledge “for” and be true to one of our main principles: to return our research back to its social source so that it becomes shared knowledge. Such knowledge can increase the reflexivity of the subjects, enabling them and their communities to more cogently name the systems, apparatuses and practices that foster and justify the structures that bind and contain or the habitus that deepen processes of social reproduction rather than social resistance. But what’s the reality? How is this possible?

In our last two major projects there are several good examples of this recuperation process and I am glad to report that the knowledge, analyses and theories gained have been diffused in a manner that underscores the principle of reciprocity in critical research. Below I briefly describe three of these contestational processes that were direct results of the interventions.

The concept of the street organization

We came up with the concept of the “street organization” early in the research to supplant the highly pejorative and pathological term “gang.” By the 1990s this latter term had lost most of its sociological meaning and was being used in both popular culture and criminology as an ill-defined deviant youth grouping of the lower class unproblematically linked to the dominant class’s various moral crusades against drugs, terrorism, the poor, immigrants and so forth. Our replacement concept, complete with sharply contrasting ideal-typical properties, was adopted by various subcultures to counter the gang label, i.e. the ALKQN, the Ñetas, La Familia and Zulu Nation among others. The
ALKQN in fact used the term during their press conferences in the late 1990s to answer the usual question from journalists regarding why the group was no longer a gang. But the concept has gone much further and has entered the more general discourse on these groups. For example, in various policy and intervention efforts to stem the intra-group violence in sites as diverse as Baltimore and Chicago in the United States, London, Barcelona and Genoa in Europe and Quito and Santo Domingo in Latin America, the concept of “street organization” often replaces “the gang.” Increasingly in more critical sociological and criminological accounts researchers have invoked the street organization concept and even in political discourses, for example statements by Louis Farrakhan, we note the use of this anti-hegemonic concept.

This rupture of the hegemonic process is important and helps to break those loops of signification that sustain pathological categories of identification and essentializing properties of subaltern populations.

**Dangerous truths as contraband**

In our 2004 edition of the ALKQN book we wrote that we hoped to see this text used as a tool to show the levels of self-organization, resistance practices and modes of symbolic representation that reflect alternative modes of political empowerment usually dismissed as threatening or made invisible by the dominant culture’s power to erase and diminish oppositional behavior. But we did not expect the work to have such a reach with its presence now well established among a range of street subcultures, advocates for the poor and prison inmates. In fact, currently the book is considered “contraband” by prison authorities and viewed as dangerous literature for the incarcerated population despite its being a well-regarded social scientific text. For such a scholarly intervention to be feared and regarded as subversive to authorities in a total institution must mean that our inquiry has joined those lists of other “dangerous truths” deemed too unsettling for structures which already have extraordinary levels of power and control over both behavior and thought. Such a development points to the book’s counter-hegemonic critical content which, I suppose, is one of the best reviews one can receive.
Voices of the powerless strike back transnationally

In our most recent work on deportation, which included a number of testimonies about the influence of street organizations, this most vulnerable of populations caught between nation states has been able to utilize the narratives of lives in exile to defend themselves against further persecution and provide immigration lawyers with fine textured accounts to bolster their legal arguments for relief in the US courts of appeal. The result has been that the work is now widely used in the US immigration courts, while it has resonated strongly with Latino activists who see the research as confirmation of the extraordinary levels of criminalization and social exclusion that their communities have withstood for so many years, particularly during the last two decades. In this dissemination process, we see a commitment to making the invisible visible and the capacity for these highly marginalized voices to become powerful, unsettling narratives placing the deportee in a deeply troubling global context (since the deportee is now part of a worldwide adoption of the “deportation regime” (see De Genova and Preutz 2010)). Thus, instead of these testimonies stopping at the page of the academic they begin to circulate among a world of counter-memories and counter-signifiers that contribute to a growing movement to halt the mass exile of members of a social class now deemed surplus to US needs and caught in the racialized judicial flows and apparatuses of the “crimmigration” system (Brotherton and Barrios 2011, Stumpf 2006).

Conclusion

Sparks of dangerous sensuality may sometimes fly from bikers or street buskers, or from their flinty clashes with authorities—but as such groups and situations become the subject matter of criminology, those sparks are snuffed out, or fanned into flame, by method.

(Hayward et al. 2010)

In the above I have summarized briefly some of the major methodological lessons drawn from my work and that of others committed to critical research projects with “gang” and related “deviant”
sub-groups. There is, of course, much more to say on this, but for now I have made a case based on concrete experiences in the field that can act as signposts and principles of engagement for future and current researchers to avoid, as the cultural criminologists above aver, the “dangerous sensuality” that is such a part of the gang terrain being “snuffed out.”

Suffice it to say that the power dynamic between the researcher and the researched is of inestimable importance and any skirting this relationship will necessarily lead to a much less ambitious, probing and counter-hegemonic project than that which is intended. At the same time the road to hell is paved with good intentions and no amount of empathy and solidarity with the oppressed will be enough for the production of a powerful piece of social science that stands up to the inevitable charges of partisan scholarship and non-objective approaches to the subject. Nonetheless, there is truth in the stories we tell; or rather there are “truths”, but we need to craft them and reveal them with both care and precision for them to be believable to more than just a few. At the same time, we must always aim to bring together both the background and the foreground of the phenomenon we are studying and locate it historically as well as in the confines and contingencies of the broader society. As the Comaroffs have warned us:

> We require good grounds for claiming the non-existence of a system or a structure – the fact that we are unable to discern one at first blush is hardly proof that it is not there.

(Comaroff and Comaroff 1992:24)

**Notes**

1 A version I have already described as often characterized by a theoretically thin positivism suffused with the discourses of rational choice that eschew the cultural, the structural and the historical (see earlier chapters).
2 See Brotherton (2013) and my call for “ethnographic activism.”
3 The late folklorist Alan Lomax is a perfect example of this inevitable process. He recorded countless blues artists but he knew what he was looking for; he knew these art forms existed and he knew he had to preserve them for posterity. For him it was both an artistic and a political commitment and he did his best to get recordings that were as authentic as possible and
then to archive them and interpret them, leaving them for us to appreciate in their purest form. Without his thinking and feeling about this subject our cultural knowledge would be that much poorer today.

4 As I write an interesting debate is ensuing over the publication of Goffman’s (2013) On the Run, which some have called another example of the “jungle book trope” (Rios 2012).

5 It should not come as a surprise that those who carry out such research are frequently denounced by social control agents for their empathy with these marginalized populations. This usually takes the form of academic banishment, physical threats and sometimes violence, institutional red baiting and claims of enabling the deviance/deviants to develop.

6 The notion of edgework relates to what Lyng (1990) calls the corporeal transaction engaged in by the ethnographer working with subjects in high risk bodily transgressions, e.g. bungee jumping, illegal speed racing, graffiti writing and so on. The ALKQN definitely engaged in a lot of transgressions in their physical dealings with the police who harassed them constantly. Barrios, who had a long history of civil disobedience, was often on hand during such confrontations, which I consider a form of “edgework.” Part of the allure of the group in the barrio was its refusal to be intimidated by the state and Barrios chose to be in solidarity with this position through his physical presence and experience.

7 One way we did this was by employing the leader of the group as a consultant on the project as well as some of his or her most trusted aides.