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## 12. Rigorous ethnography\*

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This chapter addresses two questions: what ethnography is, and what makes it rigorous (see the chapter by Raub, De Graaf & Gërkhani on rigorous sociology in this *Handbook*). The text that follows does not aspire to be a complete literature review. Its only aspiration is to answer cogently the questions I set for myself, in line with the overall aims of this *Handbook*. In the next section, I present a definition of ethnography. Subsequently, I discuss what I mean by ‘rigorous’. My remarks are guided by many references to the seminal study by W.F. Whyte, *Street Corner Society* (SCS), a book that has set the standard for rigorous ethnography (RE). The final section concludes the chapter.

### 1. WHAT ETHNOGRAPHY IS

Ethnography is a method of data collection that involves spending an extensive period in the field and conducting prolonged observations and interviews with the subjects of the research. Time in the field helps building trust, access difficult-to-obtain data and observe variation. Several definitions mention the requirement of spending a lengthy period in a given setting (e.g. Baily 2007, p. 206; Hammersley & Atkinson 1995, p. 1). William Rivers (1864–1922), an eminent anthropologist of the early twentieth century, contrasted ‘survey work’ with much needed ‘intensive work . . . in which the worker [*sic*] lives for a year or more among a community’ (Rivers 1913, p. 7; see also Forrester & Cameron 2017, pp. 57–99). More vividly, Bronislaw Malinowski—the founder of modern anthropology—urged fellow scholars to be ‘getting off the veranda of the missionary compound’ and to go and live with the natives (Malinowski 1926, p. 92). Malinowski himself spent ‘about two years’ in the field (Malinowski [1922] 1966, p. xvi). However, while anthropologists would seek out ‘distant sandbanks and . . . foreign tribes’ (Malinowski 1926, p. 93), sociologists were from the start conducting fieldwork within Western societies, often in their own country of birth.

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\* K. Gërkhani, N.D. de Graaf and W. Raub (eds) (2022), *Handbook of Sociological Science: Contributions to Rigorous Sociology*, Cheltenham, UK and Northampton, MA, USA: Edward Elgar Publishing. ISBN: 978 1 78990 942 5; <http://dx.doi.org/10.4337/9781789909432>.

† This chapter expands on two essays I have written: ‘Ethnographies of Organized Crime,’ due to appear in Sandra Bucerius, Kevin Haggerty and Luca Berardi (eds.), *Oxford Handbook of Ethnographies of Crime and Criminal Justice* (Oxford University Press, in preparation), and ‘Comparazione e spiegazione nello studio delle mafie,’ in Rosa Mulé and Sofia Ventura (eds.), *Lo studio della politica* (Il Mulino, 2019, pp. 119–142). Funding for working on this paper comes from the ERC advanced grant CRIMGOV 101020598.

I am most grateful to Paolo Campana, Ivan Ermakoff, Diego Gambetta, Klarita Gërkhani, Jonathan Lusthaus, Werner Raub and Zach Schrag for their comments on an earlier version of this chapter.

I dedicate this paper to the 2020–2021 Research Design Class (MSc Sociology, Oxford University) who had a strange and challenging educational experience, and with whom I discussed some of the issues raised in this chapter.

The ethnographer collects primary data. The distinction between primary and secondary sources relates to the distinction between *le cru et le cuit*. A source is secondary because the evidence has already been selected and processed by others (*cuit*), while primary evidence has not (*le cru*). Naturally, the ethnographer will rely on data collected by others in his or her study, but the collection of secondary data does not qualify as ethnography. Any study of a given context will be based on a variety of data, such as narratives and statistics.

Vaughan defines ethnography as ‘research conducted by situating oneself in a social setting to observe and analyse individual interaction in order to understand some complex social process, event, activity, or outcome’ (Vaughan 2009, p. 690). In her view, the aim of ethnography is not exclusively to elucidate the actors’ subjective definitions of a situation or the meanings they give to it. It also aims to uncover a meaning that might escape the actors’ self-perception entirely: what might appear to be a game of bowling for some participants is for the ethnographer a representation of the gang’s authority. This perspective is firmly individualistic: it seeks to collect individual level data (such as ‘who plays card with whom’; ‘who votes for whom’) and to produce accounts of events and outcomes focusing on actors and actions. Ethnography need not assume that there is a monolithic ‘culture’ into which people are highly socialized, although prominent studies are based on such premise (e.g., Geertz 1973; Clifford & Marcus 1986). Indeed, critics of the ethnographic method have suggested that fieldwork-based studies are unable to account for variation within the community under scrutiny (Goldthorpe 2000, pp. 74–79). As long as ethnography focuses on individuals’ interaction, such objection does not apply.<sup>1</sup> It is now common to read academic studies in journals, such as *Social Networks*, based on ethnographic collection of relational datasets, which are then analyzed using advanced statistical modeling of the evolution of the actors’ interactions (for a recent example, see Basov 2018; see also White et al. 2005). While the method of data collection is ethnographic, the collected data can be coded as quantitative and be subject to statistical analysis.

Where does modern ethnography in sociology come from? The so-called Chicago School, which grew out of the Department of Sociology at the University of Chicago and was led by Park and Burgess in the period 1917–42 (Deegan 1988), is often credited with having invented the ethnographic method in sociology, making extensive use of participant observation and systematic collection of data in the field (e.g. Harrison 2014, p. 25). Yet participant observation was not given a significant role at Chicago until the mid-1940s and, when used, it did not produce significant insights (Harvey 1987; Platt 1994). We had to wait for the publication of *Street Corner Society* (SCS) (Whyte [1943] 1993) for the study that will come to define modern academic ethnography in sociology. SCS, first published in 1943, is the account of life in an Italian-American ‘slum’ (cheaply built residential housing). The author calls it ‘Cornerville,’ and later reveals that it is located in Boston’s North End. The social relationships among the ‘corner boys’, ‘college boys’,

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<sup>1</sup> The appreciation that there can be a high degree of variation within a context was not foreign to anthropology either, although this discipline has done much to promote the view that the scholar is going to the field to uncover people’s unique ‘culture’. For instance, writing in 1912, anthropologist Robert Marett recognizes that ‘the immobility of custom, I believe, is largely the effect of distance. Look more closely and you will see perpetual modification in process’ (quoted in Wallis 1957, p. 790, and Harrison 2014).

politicians, and racketeers come to life in the pages. SCS uncovers how status and prestige play out in the daily interaction of the people in the slum, a reality that appears disorganized and chaotic to the passer-by and the superficial observer. Instead, Whyte documents a hierarchy of informal relations based on ‘reciprocal obligations’ (Whyte 1993, p. 272) that cross the legal and the illegal.

In 1955, Whyte adds a lengthy methodological Appendix, ‘On the Evolution of *Street Corner Society*,’ to the second edition. In the 1955 Appendix (Whyte 1993, pp. 279–342), the author spells out his research strategy in the field and does not shy away from revealing the false starts, the ethical dilemma, the criminal acts he committed (e.g., he voted four times in a congressional election), as well as his general epistemology and theoretical orientation. For Whyte, the researcher should (1) embed him- or herself among the subjects for an extensive period of time;<sup>2</sup> (2) work independently of governmental agencies; (3) make sure that the inquiry does not jeopardize the subjects’ welfare; (4) seek to understand their meanings, beliefs, language and practices; and (5) focus on interactional patterns. These five points are of a composite character. Points (1) and (2) are methodological, (3) pertains to the ethics of research, while (4) and (5) set epistemic goals. By setting epistemic goals, Whyte also inevitably advances a research agenda for future studies. This document has since been read as a manifesto and practical guide for participant observation studies to follow. We now turn to a discussion of what makes ethnography rigorous.

## 2. RIGOROUS ETHNOGRAPHY (RE)

### 2.1 The Epistemology of RE

Rigorous ethnography subscribes to the view that the world exists independently of ideas and language (Vaughan 2009, p. 609). Such a position is clearly articulated in Whyte’s work, which is grounded in the (Western) rationalist tradition as outlined, for example, by Russell (1906; see also Searle 1993, pp. 60–68). Whyte wants to contribute to a science of society and accepts the key distinction between objective and subjective reality. He believes that there is ‘one world out there’ and a researcher can study it. As a philosopher would say, there is a ‘mind-independent external reality’ (Searle 1993). Language does not communicate meaning only, but also refers to events and objects that exist independently of language. Whyte accepts that causes can be established and explanations put forward. Rather than subscribing to a ‘coherence’ or ‘narrative’ theory of truth, RE holds that statements can be true or false depending on how much they correspond to the objects they describe. Creationism is not as true as Darwinism (Searle 1993; Spiro 1996, pp. 764–765). Ultimately, truth depends on empirical support, not the identity of the speaker, although the bias introduced by the culture, the social position and the background of the researcher must be accounted for and minimized. In turn, empirical support depends on the procedures and methods used, and these have to be as transparent as possible.<sup>3</sup>

<sup>2</sup> Whyte lived in ‘Cornerville’ for three and a half years.

<sup>3</sup> A different view of ethnography has emerged since the late 1970s in both anthropology and sociology. Around that time, the ‘Linguist Turn’ in philosophy and literary studies influenced the two disciplines. A new type of ethnography emerged under a variety of names: ‘reflexive’, ‘critical’, ‘post-foundational’, ‘postmodern’.

## 2.2 Theories and Causes

Rigorous ethnography posits that explanations can be found, and that testing theories is a core objective of the research. Data adjudicate answers to research questions. Working alongside other methods of verification, RE contributes not only to the ‘context of discovery’ by suggesting hypotheses that can be tested on large datasets. It also plays a key role in the ‘context of justification’ by providing evidence to support or reject theoretical predictions and causal claims.

The ethnographer enters the field with a research question in mind, rather than letting the field suggest to him or her what to write about (thus, it differs from the grounded-theory approach). If the question changes, the research design must also change (Abramson & Sánchez-Jankowski 2020, p. 59). Coherent with this view, Whyte sets himself the task of ‘tak[ing] the theory to the field’ (Whyte 1993, p. 287), while being open to changing his initial assumptions on the basis of the evidence collected. He sees his work as testing a theory and the selection of data reflects such attempts, but data are not selected to prove a preconceived assumption or hypothesis. In SCS, Whyte is mainly interested in testing what we might call social network theories and discovering the formal structure of a phenomenon. He starts with the proposition that repeated associations among actors, e.g., who plays cards and/or drinks with whom, would map onto ‘political’ factions when, for example, the time comes to elect a new club president. He draws ‘positional maps’ of where people sit when playing cards over a long period of time. He finds that most people interact with the same individuals. Only 16 percent of the actors he observes fluctuates between the two main groupings. The reasoning he describes in SCS (Whyte 1993, pp. 334–335) is consistent with community detection algorithms routinely implemented in social network analysis (SNA) software and amounts to a technique of data formalization and reduction.<sup>4</sup> Whyte’s approach is consistent with what Ermakoff calls ‘morphological inquiries,’ elaborating causal arguments by uncovering patterns in the data (Ermakoff 2019, p. 1). To put it differently, Whyte shows that homophily obtains in two social domains he is studying and shows the degree to which this happens quantitatively with longitudinal data.<sup>5</sup> He also identifies the mechanisms that explain the outcome, namely reciprocal obligation between individuals. Advances in computational methods allow the use, for example, of heat-maps and SNA diagrams to represent data collected by ethnographers. Such representations can include a large number of observations and can be anonymized to protect respondents (Abramson et al. 2018).

RE has been used also to lend support to theories generated by deductive reasoning and game-theoretic formal modeling, what Ermakoff (2019) calls ‘genetic analyses.’ For instance, Gambetta & Hamill (2005) test aspects of signaling game theory by examining

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and ‘interpretative’. This view subscribes to a version of metaphysical idealism, denying the existence of an objective reality, the possibility to produce statements with a degree of truth-value and criteria to rank statements based on their empirical support. It should be clear that a discussion of this perspective falls beyond the topic of this chapter.

<sup>4</sup> For an early example of ethnographic observation of patterns of action over time leading to clustering, see, for example, McClintock et al. (1979).

<sup>5</sup> SCS was a key influence on Homans’ (1950) view that group members interact more among themselves than they do with outsiders.

the choices made by taxi drivers in New York and Belfast. In my own doctoral thesis, I study mechanisms that foster exchange. The theoretical proposition that I have strived to test is that, in the absence of a legal third-party enforcer of contracts, other organizations or informal mechanisms emerge to ensure that promises are respected. The mafia is an organization able to provide third-party enforcements of deals and promises (Gambetta 1993). I have argued that extra-legal forms of protection emerge in Russia at the time of the transition to the market economy, when the state is unable to define and protect property rights (Varese 1994). In order to avoid a functionalist fallacy, I also identify the source of the supply of the people who join such organizations (in other words, the existence of a gap, or a demand, does not automatically mean that such a gap will be filled). This work is, to some extent, an attempt to test predictions emerging from the case of Sicily made in Gambetta (1993) and to engage in cumulative social science. To lend support to that theoretical statement I collected a variety of data, including in-depth interviews with, among others, officials, Mafiosi and businesspeople. Such interviews took place in the context of a lengthy ethnography in a Russian city in the mid-1990s. The research design included the study of different types of businesses, located in different parts of the city, in order to ensure a degree of variation. On the other hand, the city was relatively ethnically homogeneous, and my work was limited to the study of ethnic Russians. During that time, I observed businesspeople in their natural settings searching for protection and making choices between state-supplied forms of enforcement and dispute resolution systems (in the form of a Court of arbitration), and mafia-provided enforcement. I concluded that members of the former Soviet nomenklatura were more likely to utilize state-supplied enforcement, although they accessed it not as a right open to all, but as a favor available to them only. Less connected businesspeople were more likely to turn to the mafia. This knowledge allowed me to delineate who was able to access which service and the nature of the service (Varese 2001). While in principle this result could have been obtained by a large survey instrument, the nature of the behavior I was studying would have made it very unlikely for actors to respond to a questionnaire. Hence, the method of data collection is a function of the nature of the data to be collected. Fieldwork also allowed me to outline the structure of the mafia groups in the city and compare them with mafia organizations present in Italy and the USA. I also specified the conditions under which I expected a mafia group to last over time or turn into a purely predatory group, and used field interviews to lend support to my expectations. By specifying the conditions under which an outcome occurs, scholars can make their claims generalizable and, *ceteris paribus*, predictive. I believe that the length of my stay in the city and my commitment to the topic acted as a credible signal of trustworthiness in the eyes of the people I was seeking to speak to, who then were more willing to share key information with me.

In subsequent work, I studied contexts where a mafia might be costly to use or simply is not present. The theoretical expectation is that, in the absence of a mafia, actors involved in illegal exchanges need to show that they are committed to fulfil their promises through the deployment of informal mechanisms, such as reputation-building, repeated interaction, hostage-taking and sharing compromising information (Schelling 1960; Axelrod 1984; Raub & Weesie 1990; Gambetta 2009; Campana & Varese 2013; Gambetta & Przepiorka 2019). For instance, in a study of informal bankers in China, an informant told us that the significant investment in her shop was a signal of her commitment to her

customers: 'I have a large shop here in the mall. You can always come back and check on me if there is a problem.' Having invested in a large shop is a form of self-binding. She was cutting off options in order to facilitate a certain type of interaction with her customers, in the absence of a third-party enforcer of the deal. We also uncovered how informal bankers and customers often spend time engaged in the consumption of commercial sex, which is illegal in China. Underground bankers who accept prostitution services are in effect sharing compromising information with the VIP customers with whom they are trying to build trust. Individuals involved in informal exchanges are keenly aware they have to signal their trustworthiness to customers and adopt several strategies to do so (it should be noted that formal bankers were also discussed in the paper, see Varese et al. 2019).<sup>6</sup>

RE can exploit natural experiments to exclude possible causal factors and focus on a narrower set of candidate explanations (Diamond & Robinson 2010). For instance, Varese (2011) explores the conditions under which a traditional mafia is able to become rooted in a different, distant territory. The study includes a set of controlled comparisons, which vary for the outcome variable (successful or unsuccessful transplantation). Among other cases, I focus on a single mafia group, the Mazzaferro clan belonging to the 'ndrangheta', attempting to become rooted in Bardonecchia (in the Piedmont region; success) and Verona (in the Veneto region; fail). By choosing the same clan and two settings both located in the same northern part of the same country, I am able to discard some causal hypotheses, such as 'culture', language, level of trust, corruption, and effectiveness of local law enforcement, which are broadly similar (but not identical) in the North of Italy, and organizational features of this mafia. In the end, the study concludes that the size of the territory and the growth of local unregulated markets predicts mafia transplantation. These findings have high external validity (Varese 2020).

Once we have specified the conditions that generate an outcome and the mechanism at work, we can deploy various validation strategies, such as suggesting observable implications. Given that a particular process generates an empirical regularity, what other regularities should we expect to find (Goldthorpe 2000, p. 90)? As noted by Stinchcombe (1968, p. 23), the more implications are supported by the theory, the more general the theory. For instance, an observable implication of Varese's (2011) conclusion that the mafia in Bardonecchia provided genuine protection to construction companies and reduced competition is that the mafia provided its protection in other domains as well. Indeed, the criminal group was also active in settling disputes between employees and employers hired informally by the local construction companies.

Agent-based models can complement RE by reproducing the micro foundations and the dynamics hypothesized by the ethnographer and observe the outcome. Agent-based simulations allow us to examine whether, and to what extent, the actual outcome fits the simulated one, and how the causal claim (which needs to be clearly specified) can be obtained through the simulation (see the chapter by Flache, Mäs & Keijzer on computational approaches in rigorous sociology; Manzo 2014). For instance, Ackland et al.

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<sup>6</sup> Similarly, Hamill (2010) studies how the IRA acts as a form of extra-legal governance structure in a catholic West Belfast community, punishing deviants. She endeavors to test aspects of signaling game theory. The author shows that paramilitaries are known to exercise violence. This in turn suggests that the 'hoods' are willing to face the threat in order to signal their strength and defiance, ultimately enhancing their social status. The ability of paramilitaries to punish motivates the 'hoods' to defy the local, extra-legal justice.

(2019) present an agent-based model for a pharmaceutical supply chain operating under conditions of weak regulation and imperfect information. The study explores whether poor quality medicines are sold, and how buyers can learn about the quality of sellers (and their medicines) based on previous successful and unsuccessful transactions. The starting point of the simulation is an ethnographic study conducted in Ghana and Tanzania in 2015–2016.

### 2.3 Data Collection

How can we be sure that data collected in the field are valid and reliable? Such issues were at the forefront of Whyte's work. Let's take several issues in turn. First, Whyte had to choose a site. Although Whyte modestly reports that the selection was based on 'very unscientific grounds,' he followed a criteria that was unrelated to the argument he was about to develop in the book. In his search for a site, his guiding principle was to find an overcrowded neighborhood. The chosen one 'had more people per acre living in it than any other section of the city' (Whyte 1993, p. 283). The choice of field site was unrelated to the dependent variable. Other ethnographers have followed rigorous sampling criteria. Sánchez-Jankowski, in his study of 37 gangs in the USA (Sánchez-Jankowski 1991), randomly selects the gangs from a list of potential cases stratified by ethnicity. Naturally, there are limitations to his access: since the author is of Hispanic origin, he finds it particularly difficult to study gangs hostile to Latinos. Yet Sánchez-Jankowski makes such a limitation clear. More generally, *Islands in the Street* remains an admirable example of RE in action for the study's attention to the issue of case selection as well as its comparative research design.

Whyte writes at length on how he gained entry into the social world of the slum (Whyte 1993, p. 291). His key contact was Doc, whose role was not that of an oracle, to whom Whyte turned to obtain an adequate account of a given situation or institution. Rather, the ethnographer needed to penetrate a hidden world and Doc offered to be his guide. Once in, Whyte observed the behavior of all the people involved in a particular activity or setting, such as the gang or the club. Since Whyte focused on a small group of people, the issue of non-responses did not arise, contrary to what happens for large surveys (see Goldthorpe 2000, p. 79).

Reflexivity is central to an ethnographic project because the ethnographer is the instrument of data collection, and might well introduce biases. Whyte discusses the position of the author in the field, including his intellectual trajectory. Such a discussion is to the point and refers to the features that can have an impact on the data collection process, such as the fact that Whyte was an outsider to the community, from a different ethnic, religious and social background (Whyte 1993, pp. 280–283). The ultimate goal is to reduce the biases introduced by the ethnographer. Spending a lengthy period of time in the field serves to minimize the impact of the outsider, as he or she can observe more interactions and gain trust. Yet active reassurance to the study's participants about the scholar's intention is also crucial. Academic ethnography has the advantage (*vis-à-vis* journalism) that the research is published several years after the events and exposing a particular individual is not the aim of the work.

In the field, Whyte followed basic rules to reduce distortion introduced by time lags and ethical revulsion. Whyte typed up field notes as soon as possible (Whyte 1993,

pp. 287, 365). He strived to keep observations ‘completely divorced from moral judgement’ (Whyte 1993, p. 287) and to not argue with people or judge them (Whyte 1993, p. 302; for a discussion of the issue of moral revulsion, see also Varese 2001, p. 12). It is clear to him that the ethnographer should not shape events in the field, although he admits that it was not easy to remain detached and breached this principle at least twice. In RE, the ethnographer does not routinely become the object of interest.

Data triangulation is also a good practice. In my own fieldwork, I always checked if the information I was given in interviews matched open sources and other confidential sources. A most scrupulous approach to fact checking and triangulation is to be found in Desmond’s *Evicted* (2016). Desmond follows eight families in and around Milwaukee, Wisconsin, struggling to pay rent and living in trailer parks and unsanitary houses at the time of the financial crisis of 2007–2008. He regularly fact-checks his interviewees’ assertions and never takes them at their word (Desmond 2016, p. 327). In one instance, he omits a story that he is not able to confirm, although it does fit well with this overall argument, ensuring that he does not fall in the confirmation bias trap. He even hires an assistant who ensures that his conclusions follow from his field notes (2016, p. 404, fn 7).

Ethnographers should strive to be precise about what they count as data, how information is collected, over which period of time, and verify the information given (Reyes 2018, p. 205). There are no structural impediments for RE to achieve the standards of rigor to be found in other social sciences.

## 2.4 Anonymity

Anonymity is generally acknowledged to be a key requirement of academic ethnography. Participants’ real names are routinely masked. Occasionally places are also given fictional appellations. More extreme forms of anonymity include changing key features of the participants (such as gender) and of the field site. A further practice is to create composites of the people interviewed. Thus, anonymity protocol can morph into *de facto* falsification of data, as in the case of composite characters. Surely, there are many instances where anonymization is appropriate and required. When sensitive and/or highly personal information is being shared, or when participants will talk only if their name is anonymized, there is a good case for changing people’s names. Compromising the confidentiality of the participants puts them at risk and undermines attempts by other scholars to study the same or similar communities. As consent is a key requirement of ethnography, the scholar’s options are limited: report anonymously or not at all. Yet anonymization has evolved into a default position, routinely used even when unnecessary. Furthermore, there have been several cases of bad anonymization, where offering to change names in no way leads to the protection of the participants’ identity and the process becomes futile. Thus, there are benefits but also serious costs to anonymization and an assessment should be made case by case. When it is possible, I suggest scholars ask participants to ‘go on the record,’ as done by journalists with their sources, and try to avoid masking places. RE should strive to keep anonymity at a minimum because the practice of hiding or distorting identifying information reduces the ability to construct cumulative social science (Jerolmack & Murphy 2019; see also the chapter by Auspurg & Brüderl on reproducibility and credibility).

Let us start with the practice of masking places. Several ethnographies, including SCS, follow this protocol.<sup>7</sup> As noted by Reyes (2018, p. 206), this practice takes three forms: changing the name of the region, of the city and/or of the specific community. Some such attempts at masking are futile. In the twenty-first century, widespread social media documentation can make it easy and quick to reveal locations (Harrison 2014, p. 237). In addition, ethnographers themselves, during the course of a lengthy fieldwork, might post photos or even online reviews of places they visited, making it hard to hide field sites in their academic monographs (Jackson 2012). Even in the past, masking places also did not ensure anonymity. Marquand's novel *Point of No Return* (1949) is a fictional account of the (real) fieldwork by the American sociologist Lloyd Warner in the city of Newburyport, Mass., which is referred to as *Yankee City* in Warner's books. Marquand happened to be from Newburyport and came across Warner during his time in the city. In *Point of No Return*, Marquand mocks the sociologist's clumsy attempt at disguising places and family names (Marquand 1949, p. 58, cited in Ingersoll 1997). Goffman's efforts at disguising the place of her fieldwork also proved futile: a journalist quickly discovered it and was able to confirm some aspects of Goffman's narrative (Singal 2015).<sup>8</sup>

A rather more dramatic (and real) instance of bad anonymization is the ethnography of mental health in the Irish village of An Clochán by Scheper-Hughes ([1979] 2001). The author tried to disguise the field site by calling it 'Ballybran'. After publication, a journalist from the *Irish Times* discovered the village and identified several of the people Scheper-Hughes interviewed. When the author returned to the village in 1999, she was no longer welcome, as the people felt betrayed by her work (Scheper-Hughes 2000). Reflecting on her work in a 2000 paper, she conceded that changing the name of the field site (and altering certain features of individuals) gave a false sense of protection to the interviewees and an easy way out of complex ethical dilemmas for the ethnographer: 'I have come to see that the time-honoured practice of bestowing anonymity on "our" communities and informants fools few and protects no one – save, perhaps, the anthropologist's own skin' (Scheper-Hughes 2000, p. 128).

Changing the name of the locality makes it particularly hard to judge the external validity of the work. It is not uncommon for a field site to be described as 'typical' of the phenomenon under study. By hiding the name, critics find it harder to challenge the claim to generality. The practice of hiding the name of the field site dates back at least to Lynd & Lynd's ([1929] 1956) study of a supposedly representative American city, *Middletown* (which was indeed Muncie, IN.), Hollingshead's study of a 'typical Midwestern community,' *Elmtown* (1949), and Warner's *Yankee City* (1963), known to be Newburyport, MA. Withers (1945) took the extreme decision to disguise not only the name of the town

<sup>7</sup> Tocqueville did not print the names of the people he interviewed during his trip to America: 'When information was confided to me, I wrote it down immediately, but these notes will never leave my files' (Tocqueville [1835] 1990).

<sup>8</sup> When contemporary historians tried to follow the sociologists' lead at masking place names and interviewees' identities, they also failed. For instance, in *The Nazi Seizure of Power: The Experience of a Single German Town 1930–1935*, Allen (1965) tried to mask the name of the city, calling it 'Thalburg'. When the book came out in Germany in 1966, the magazine *Der Spiegel* had no difficulties in identifying the real place and even some of the people the author had interviewed. See Schrag (2020), who offers more examples.

he wrote about (dubbed *Plainville, U.S.A.*) but also his own name.<sup>9</sup> Extreme anonymization of places leads to losing important contextual information and makes fact-checking hard. For instance, in *The Mafia of a Sicilian Village 1860–1960*, Blok (1974) studies a village (some 2600 inhabitants) in the interior of Sicily, 81 km south of Palermo. While Blok tries to mask his chosen site and calls it ‘Genuardo,’ Sicilian scholars quickly understood that it was Contessa Entellina. Furthermore, the community, as pointed out by Catanzaro et al. (1987, pp. 188–189, pp. 193–194), lived on a remote high plane and had a peculiar history: the inhabitants of Contessa Entellina, including the peasants, were of Albanian descent, while the landowners were ethnic Italian and did not live in the village. Rather unusually for Sicily, the village elite was hostile towards large landowners, according to a survey conducted in 1908. Such an ethnic composition made for special dynamics, which went unaddressed in the book. Removing the name of the place prevented Blok from exploring key aspects of his field site, including political and electoral processes (as noted by Catanzaro et al. 1987, p. 190; for a response see Blok 2001, p. 65). Blok drew general conclusions regarding the nature of the Sicilian mafia based on a very special case. Such conclusions—such as the predicted demise of the Sicilian mafia—proved to be incorrect.<sup>10</sup>

Internal validity is also undermined by the decision taken by some ethnographers to alter dates, age and gender of informants. For instance, in his study of an elite hospital, Bosk ([1979] 2013) changes the gender of the only surgical resident who is denied promotion in the program. It turns out that the person is a woman and her gender might well explain the outcome Bosk observes (in the second edition, Bosk admirably discusses—and regrets—that choice). Another practice that undermines validity is the habit to create composite characters out of real personalities. For instance, Warner & Lunt in their book on Yankee City write that ‘No actual individual in Yankee City is depicted, rather the lives of several individuals are compressed into that of one fictive person’ (Warner & Lunt 1941, p. 127).<sup>11</sup> Such a practice is questionable as it takes away agency from people (Lubet 2018, p. 95) and undermines the research. Rather than removing information such that someone cannot be identified, this practice entails changing (inventing) facts and features. Composites’ descriptions produce false data.

Anonymization of people has usually been justified in two ways. On the one hand, it is what the informants want. Yet ethnographers seem to routinely grant anonymity without even asking. Informants themselves might wish to be heard in ways that are not mediated by the ethnographer’s reconstruction and want to be known by their name. For instance, Lusthaus (2018, p. 228) finds that (former) cybercriminals providing sensitive information were happy to be named. A second justification for anonymization has been to protect the safety of the informants, a practice often requested by Universities’ Ethical Boards. Safety is surely a crucial issue and in some contexts—such as, for example, when writing about people who might have been close to the IRA (Hamill 2010) or who might

<sup>9</sup> See also the earlier study by Williams (1906) of Waterville, New York, named ‘Blanktown’ in the text. For a discussion of this early case, see Schrag (2020, p. 74). These studies have been criticized extensively by, for example, Mills (1942), Pfautz & Duncan (1950) and Madge (1962).

<sup>10</sup> Another example of extreme anonymization in the field of mafia studies is Ianni & Reuss-Ianni (1972), a book that comes with a Foreword by Whyte himself.

<sup>11</sup> As seen above, the two authors ultimately failed to scramble the identities effectively (Ingersoll 1997, p. 141). For a contemporary critique of the book, see Handlin (1942, pp. 554–557).

be persecuted under the recently enacted Chinese Security Law (Wintour 2020)—repercussions for talking to scholars can be severe. Yet Wong (2015) suggests that there is a tendency among some fieldwork reports to exaggerate how ‘dangerous’ the research is, hence the need for extreme anonymization. This tendency helps generate the trope of ‘the anthropologist as a hero,’ as seen in works by, for example, Goffman (2014) and Venkatesh (2013; 2008. Cfr. Sontag [1966] 1978). Key aspects of both Goffman’s and Venkatesh’s investigations have been challenged by other scholars (especially Lubet 2018), although such a critique was made more difficult by the choice to anonymize.

The New York University Faculty of Journalism has produced a handbook detailing ethical behavior and good practice (Penenberg 2020). Guidelines on integrity and the use of human sources are clearly spelled out, encouraging journalists, among other things, to avoid printing ‘not for attribution’ conversations, where the reporter agrees not to identify a source by name. Ethnographers might want to pay some attention to journalism’s professional code, especially in dealing with sources. In addition, scholars should signpost the anonymized names, to distinguish them from real names clearly. In Varese (2011), I adopt the convention to write the few anonymized names in italics throughout the book and in the Index, so that the reader is always reminded of the masking. More generally, RE should keep anonymity at a minimum, interviews should be clearly dated and, unless strictly necessary, a place should be indicated, as well as a summary description of the role/profession of the person. It is important to note that the ethnographer cannot guarantee to participants that anonymization will be successful. This risk should be made clear when the promise of anonymity is made. Such a promise can give a false sense of security to participants and researchers should be open with participants about the limits of this practice.

## 2.5 The Small-*N* Problem and Generalization

How general are the conclusions reached by ethnographic research? RE can point to the presence of a given mechanism—such as reciprocity, homophily or transitivity—in an instance, e.g. a given gang, that we know is present in other cases. Such a result would increase the external validity of the research. Random sampling of cases is possible, as shown by Sánchez-Jankowski (1991). Yet, the ethnographer might face constraints due to his or her background and knowledge, which leads him or her to study a particular case. Yet the ethnographer should discuss the universe to which the case belongs. On the other hand, non-random sampling can take place, as it does in many quantitative research projects.

Ethnography normally involves the study of a small number of people, the so-called ‘small-*N* problem’ (Lieberson 1991) although this is not a defining feature of ethnography. Yet, even in the study of small groups, the scholar can observe a quite large number of interactions, which will form the basis for making empirical valid claims. Indeed, Sánchez-Jankowski & Abramson have argued that participant observation methodology strives to maximize the number of observed events rather than individuals (Sánchez-Jankowski & Abramson 2020, pp. 37, 47; along similar lines, see the discussion of qualitative methods in economics in Skarbek 2020, p. 411). To the extent that the scholar collects relational data in the field, the dataset can be tested for statistical significance. Since observations are by definition not independent of each other in SNA datasets,

statisticians have developed techniques to estimate significance by drawing simulated networks from the distribution (e.g., ERGMs and SAOM models; see the chapter by Steglich & Snijders on stochastic network modeling). We can then say to what extent the observed network differs from chance. Yet, if a particular gang is an outlier in the population of gangs, it will remain so. Thus, a discussion of ecological validity is appropriate.

## 2.6 Replication and Re-study

Replication is a goal for any scientific discipline, including ethnography. Indeed, this method has the advantage that it is comparatively cheap. There is no need to set aside valuable and costly lab time. Yet there is a degree of confusion over what replication means in this field (Abramson & Sánchez-Jankowski 2020; for a discussion of replication in other social science fields, see the chapter by Auspurg & Brüderl). Many authors consider re-study of a given field-site the equivalent of replication. For instance, Lewis (1951) conducted fieldwork in Tepoztlán some 20 years after Redfield (1930), Weiner (1976) in the Trobriand islands some 50 years after Malinowski's *Argonauts of the Western Pacific* ([1922] 1966), Duneier (1999) in the Greenwich Village more than 30 years after Jacobs (1961), and Hunter (2013) in the Seventh Ward of Philadelphia more than a hundred years after Du Bois ([1899] 1973). It is highly likely that the object of study has changed significantly in the intervening decades, thus replication would be impossible. Such research would be more accurately defined as a second study of a changed community (on communities restudy see Crow, 2012). To use the language of quantitative social science, the new study uses new specifications and new data (see the chapter by Auspurg & Brüderl). In some cases, scholars have gone back to check whether the original study was fraudulent or severely biased. Freeman (1983) travelled to Samoa trying to prove that Mead's fieldwork was deeply flawed, Boelen (1992) went back to Whyte's field site and interviewed some of his informants, and Duneier (2006) revisited critically Klinenberg's (2002) study of the heatwave that occurred in Chicago in 1995. Checking on the work by others and re-interviewing some of the participants years later do not amount to replication as understood in the laboratory tradition (Sánchez-Jankowski & Abramson 2020, p. 80). Obviously, if an ethnographic study produces a quantitative dataset, then other scholars can access the data and replicate the analysis.

Scholarly scrutiny is made easier when field notes are made available, as well as information on the site, the participants and more generally the process of data collection (see the section on anonymity above and Breznau 2021). For instance, Whyte deposited his field notes in the form of archival data, which now are available to anyone (Vidich 1992, p. 81). While it might not always be possible to follow Whyte's example for reasons of safety and confidentiality, field notes should be retained and not destroyed, as, reportedly, done by Goffman (2014).<sup>12</sup> Ethics Boards normally require that data should be retained for a period after publication (e.g., 7 years). Yet there is also a presumption that data that may be personal/sensitive should be destroyed at some point to protect participants. Thus, Goffman might have followed this prescription but ended up destroying all notes. Such documents could be kept in an escrow-type service and released only after a

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<sup>12</sup> Melamed (2014) reports that Goffman destroyed her field notes. On the importance of keeping and possibly sharing field notes, see Lubet (2018, p. 134) and Smith & Roberts (2016).

period of time, which could be very long. Yet sharing field notes is no panacea (Abramson & Sánchez-Jankowski 2020, p. 79). It might be impossible to share data if there is no clear informed consent from participants to make the data available: others who did not give consent may be compromised. In addition, consent might be given at one point, only to regret it later. Making de-identified field observations available may introduce errors and misinterpretation of data, and there will remain a gap between the interpretative understanding of the ethnographer in the field and somebody who reads a text years later. Genuine replication may only be achieved by conducting research using new data to reproduce a previous study (Abramson & Sánchez-Jankowski 2020, p. 79). This would amount to engaging in cumulative social science, as done by a number of scholars who have sought to test Gambetta's (1993) model of the Sicilian mafia on Russia (Varese 2001), Hong Kong (Chu 2002) and Japan (Hill 2003).

### 3. CONCLUSIONS

In the section on what ethnography is, I define ethnography as a particular method of data collection that involves spending an extensive time in the field, conducting prolonged observations and interviews with the subjects of the research. Modern ethnography in sociology is also associated with conducting a project that is independent of authorities, a commitment to protect participants and, for some authors, the observation of interactions. In the section on rigorous ethnography, I outline the epistemology, attitudes towards theory and causation, data collection and anonymity of RE. I also discuss generalization, the small-*N* problem and replication. This approach subscribes to the Western rationalist tradition, believing that there is a mind-independent external reality, that language refers to objects and events and does not just communicate meaning, and that empirical support can lend a degree of truth-value to sentences about the world. Furthermore, RE posits that explanations can be found and testing theories is a key objective. The chapter shows that RE has been able to produce both 'morphological' as well as 'generative' explanations (Ermakoff 2019). Rigorous ethnographers have also tested the empirical implications of their theoretical predications, enlisting other methods, such as formal modeling and simulations. RE plays a key role in the 'context of justification'. Concern for the validity and the reliability of the data collected are a crucial part of the RE. The scholar must explain why he or she chose the field site, whether that choice might impact on the argument made, discuss his or her position in the field and the role of informants, and follow procedures that reduce distortion due to time lags and ethical repulsion. Finally, RE recognizes that it is a good practice to share, when possible, field notes and encourages transparency and restudy. A key aspect of data collection, to be found in many studies, is the anonymization of subjects and localities. I have argued for a limited use of anonymization, which at times might prove to be futile and hinder external scrutiny.

Several challenges exist to the classic form of ethnography. It would be impossible to address them all here. Yet it is worth mentioning one. Recently, scholars have elaborated on the method of 'rapid ethnography.' Baines & Cunningham (2013) argue that rapid ethnographies, involving short, compressed time in the field and ideally teaming the outside researcher with an insider, can generate important insights into organizations (e.g. hospitals) and, most crucially, allow international qualitative comparisons at a

relatively low cost. Yet this research design departs from ethnography as I have defined it, and it becomes more akin to short field trips. While it is possible to collect valid data, it would be next to impossible to stumble by chance on unexpected yet revealing events during a short trip.

In conclusion, why should scholars spend their time observing patterns of interaction and decision making in natural settings? This particular method of data collection should be chosen when there is no other way to gather the information one needs to test arguments. Ethnography is particularly suitable for the study of hidden populations, such as Whyte's gang or the mafia, and the collection of personal, sensitive data. Since data are hard to come by, time spent in the field is a signal of commitment on the part of the scholar in the eyes of the people he or she studies.

The chapter has highlighted some criteria for the production of RE. Yet it should be clear that following the precepts discussed above does not ensure that the final product is interesting or worth reading. It hardly needs reminding the reader that Tolstoy's *War and Peace* and le Carré's *The Spy Who Came in from the Cold* (to name just two books) contain profound insights into the human condition. Yet, for those of us who are not Tolstoy or le Carré and still want to leave the office desk and study the social world, it is advisable to be rigorous, systematic and transparent.

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