Discovering by Surrendering: for an Epistemology of Serendipity Against the Neoliberal Ethics of Accountability

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Abstract

In an essay on serendipity in relation to Peirce's notion of abduction, Ugo Fabietti (2012, p. 17) highlighted how, in spite of a vast literature on ethnographic authority, reflexivity, and positioning, little attention has so far been paid to the process of ethnographic discovery, that is, the identification of something that will allow the ethnographer "to alter their perspective on a given theme or problem and, naturally, enable them go forward in the knowledge of their object." Drawing on Fabietti's reflections, I discuss how an epistemology of serendipity can be used to counteract the neoliberal bureaucratic regimentation of ethical reasoning and practice. I contrast my 2002-2003 fieldwork experience in upland Indonesia with my 2015 encounter with IRB procedures in the U.S. to show how the heuristic potential of serendipity advocated by Fabietti might be obstructed by (and mobilized against) the demands of a bureaucratized ethics of accountability and the growing hegemony of the research protocols of audit cultures.

Keywords: Ethnography; Serendipity; Audit cultures; Upland Indonesia; U.S. Bureaucracy

Ethnographic Discoveries and Bureaucratic Procedures[1]

February 16, 2015, a new message appears in my electronic mailbox. I skim

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1 Acknowledgments. I dedicate this article to the memory of Ugo Fabietti, from whom I inherited my love for fieldwork and my dislike for bureaucratic paperwork. I thank all the people in Toraja who in so many ways enabled my serendipitous discoveries. My philosopher colleague Brom Anderson and two anonymous referees helped me sharpen my argument. Isaac Woodbury-High proofread my revised draft and offered precious editorial comments. During the past years, my colleague Bob Desjarlais has been a source of comfort and intellectual insights on the challenges and the craft of ethnography. My deepest gratitude goes to Ugo Fabietti for having taught me how to be an ethnographer and how to ward off what, with his graceful irreverence, he used to call "scartoffie" (i.e., bumf). I will always remember him in my daily alchemic struggle to prevent the gold of fieldwork from turning into the lead of academic life.

it quickly. A paragraph immediately catches my eye:

> […] We would need to know more specifically about the ethnographic interviewing—how will you recruit participants, what will you tell them about your work, what are the possible risks for participating […]

As I read the email with which I am advised to revise the research proposal I submitted to my University's newly established IRB (Institutional Review Board)[2], I can almost sense a stinging feeling in the pit of my stomach. That word, that unnerving word, *recruit*, sticks out on my computer's screen as a reminder of the difficult conversations that have accompanied the recent establishment in my New York academic institution of a review board aimed at ensuring that the research conducted by faculty, staff, and students is "ethically sound" and compliant with "the principles of respect for persons, beneficence, and justice set out in the Belmont Report and the principles set out in the Federal Policy for the Protection of Human Subjects, 45CFR 46 (known as the "Common Rule") […]"[3].

As I re-read that email now that I have almost completed the three-year NSF-funded research project in upland Indonesia for which, at the beginning of 2015, I had applied for IRB approval, I can still vividly recall the feeling of profound discomfort triggered by the request for additional details on my intended plans for *recruiting* my interlocutors. Why such a perfectly legitimate request elicited in me such a visceral—quite literally speaking—reaction? I argue that the answer to this question entails a broader reflection on the nature of what Ugo Fabietti (2012, p. 17) called "ethnographic discovery" vis-à-vis the bureaucratic procedures typical of the global spread of audit cultures and neoliberal values. In this article, I discuss my 2002-2003 doctoral fieldwork in the Toraja highlands of Indonesia and show how in order to conduct a follow-up research in 2015-2018 I had to navigate IRB procedures in the U.S.. My analysis highlights how being an ethnographer entails deliberate and methodical forms of surrendering to the

2 Originally established in the U.S. in the 1970s, as a result of national debates triggered by reported abuses in federally funded medical experimentation, IRBs are interdisciplinary committees aimed at reviewing research involving human subjects in order to ensure protection for human participants in biomedical and behavioral research. As Schrag (2009, 2010), Pels (1999, 2000), and Pels et. al. (2018) point out, since the late 1990s there has been an upsurge in ethics review board activities both in the U.S. and beyond.

3 https://my.slc.edu/ICS/Campus_Life/Departments/Institutional_Review_Board/, Sarah Lawrence College, Institutional Review Board page, accessed June 27, 2018. As the website further explains, these principles include: "minimizing risk, weighing benefits against risks, fully informing participants of the voluntary nature of their participation through a process of informed consent, and maintaining the confidentiality and safety of participants." Issued in 1979, the Belmont Report was supplemented in 1991 by a Federal Policy—called the "Common Rule"—aimed at ensuring ethical research on human subjects.

unpredictable and the unexpected. Such an apprenticeship in "the creative tension between structuration and event" (Pieke 2000, pp. 129-130) entertains a complicated relationship with the growing hegemony of the research protocols of audit cultures.

Originating from protocols of financial accountability and based on the idea that "transparency of operation" equals integrity and efficiency (Strathern 2000b, p. 2), audit practices have now become a natural (i.e., largely unnoticed and habitually unquestioned) ingredient of neoliberal management technologies and ethos. By neoliberalism, I mean not only (and not so much) a politico-economic doctrine grounded on free market fundamentalism and a set of policies aimed at promoting flexible accumulation, the erosion of the welfare state, and the privatization of the public sector. Rather – together with many other scholars of anthropology, sociology, and political theory (see, for example, Martin 2000, Ong 2006, Rose 1990, and Brown 2006) – I conceive neoliberalism as a moral project aimed at restructuring individual subjectivities through bookkeeping practices and moral technologies of self-monitoring. This Foucauldian approach (see Hilgers 2010 for a review) foregrounds the connection between neoliberalism and the technologies of the audit, which, as we will see, can be understood as sets of practices aimed at promoting transparent accountability through the segmentation of human conduct into bureaucratic procedures.

In this paper, I will engage these questions by drawing on my experiences with U.S. bureaucratic protocols aimed at "institutionalizing moral standards" (Pels 1999, p. 101) and on my fieldwork encounters in upland Indonesia. In so doing, I discuss how the structures of agency and temporality posed by the principles of informed consent and by the preemptive framework of research participants' recruitment occlude fundamental aspects of ethnographic research. More specifically, I argue that the methodical surrendering to the unexpected, which is essential for the ethnographic discoveries described in Fabietti's essay, collide with (1) the preemptive temporality and (2) the model of the sovereign moral agent underwriting the neoliberal ethics of accountability. The first aspect concerns a specific politics of time typical of the "anticipatory regimes" (Adams et al. 2009) of late capitalism, which is exemplified by the habit of treating the future as it were already present[4]. The second aspect pertains to the configuration of the ethnographic encounter as an interaction between a clearly accountable and maximally agentive researcher and a irretrievably passive – but well informed (and fully

4 Based on the proliferation of credit and insurance industries, financial and speculative capitalism has in fact generated new markets and fields of expansion centered on the management of risk, which has itself become a commodity. Not coincidentally, this preemptive framework and the focus on risk are well apparent in the request for additional details I received in February 2015.

consenting) – research subject. Before developing my analysis, a brief flashback is in order.

A few years ago, realizing that having their research proposals assessed by external ethics review boards was becoming too costly and impractical, some of my colleagues (mostly psychologists) pushed to institute an in-house IRB. Unsurprisingly, the decision caused some internal debate among the faculty. IRBs were originally developed in the U.S. to ensure ethical treatment of human participants in medical experimentation. The extension to social sciences research of regulations and terminologies originally developed in biomedical sciences has long triggered vehement criticism on behalf of sociologists and anthropologists[5]. When our IRB was instituted in 2015, a group of ethnographically minded colleagues and I expressed our reservations concerning the language and procedures that appeared too heavily based on biomedical protocols and/or on the experimental style of clinical settings.

In a series of somewhat inconclusive discussions, for example, we pointed out how the word *recruit* was unsuitable for describing what we do in the field. Aside from being enwrapped in distasteful military undertones, the idea of recruitment (of the research participants) suggests a preoccupation with informed (preferably written) consent understood as "a one-off process" (Bell 2014, p. 512)[6]. Some of these concerns informed the rewrite of the research proposal that I submitted in February 2015, where I outlined the two main reasons that made it difficult to get people in my upland Indonesian field site to sign off consent forms:

> While in the experimental style of research used in clinical settings, social psychology, qualitative sociology, and survey research it is often possible to demarcate clear beginnings and endings for the data-gathering event, ethnographic research is ongoing, dynamic, and it often takes place outside English-speaking countries. […] Due to the large amount of illiteracy and to the great suspicion (resulting from over three decades of authoritarian regime) that surrounds official forms requiring signatures in Toraja and in Indonesia, the application of the standard U.S. procedure to obtain informed consent in writing may undermine the scientific integrity of the project.

5 For additional critical reflections on the moral and epistemological assumptions underlying the informed consent protocols, see Bell (2014), Lederman (2006, 2007), Murphy and Dingwall (2007), Schrag (2009, 2010), Shannon (2007), Thorne (1980).

6 According to the definition provided in the recently revised American Anthropological Association Statement on Ethics (AAA 2012) informed consent "includes sharing with potential participants the research goals, methods, funding sources or sponsors, expected outcomes, anticipated impacts of the research, and the rights and responsibilities of research participants […]. Anthropologists have an obligation to ensure that research participants have freely granted consent, and must avoid conducting research in circumstances in which consent may not be truly voluntary or informed."

Drawing on these arguments, in the rewrite I demanded (and was later allowed) to be able to rely on oral assent in order to not be forced to continuously elicit my interlocutor's written approval:

> For this reason, I am requesting that the requirement of obtaining informed consent in writing may be waived and replaced by verbal consent. Instead of asking my interlocutors to sign a form, I will request their permissions to record our exchanges by writing or taping through straightforward verbal questions: 'Can I tape?' or 'Can I take notes?' and verbal reminders: 'You can ask me to stop taping/note taking any time you want.'

As it is well apparent in the excerpt quoted above, with this revised version of my IRB proposal I sought to comply with the request for more details on "participants' recruitment." But in so doing, I was being confronted with an alienated writing experience – one in which I was feeling disconnected from the epistemological and moral basis of my own writing. I realized I was dissatisfied with my own phrasing. To me – or, to be more precise, to my un-alienated writing self – the passage seemed at once superfluous and incomplete. In fact, if the commitment to requesting permission to tape and take notes seemed obvious and redundant, the lack of references to other important aspects of research ethics raised a number of questions. For example, what about the several layers of writing that follow the encounter?[7] How should consent be negotiated after the "interview" takes place, such as in the field notes that ethnographers are supposed to take at the end of the day? I was also perplexed with my own use of the term "interview," which in fact I rarely do, but had reluctantly resolved to insert (in response to the IRB request) as a way to refer to the casual conversations I have during fieldwork[8].

The pressure of time concerns combined with the risk of losing the prestigious grant I had been awarded – which, needless to say, was conditional upon my University's IRB approval – prevented me from developing these reflections or challenging the informed consent doctrine in its entirety. However, in describing the procedures I was going to use to ensure voluntary participation in my project, I sought to express my respectful disagreement with a narrow understanding of recruitment, as illustrated in another excerpt from the revised version of the IRB application I resubmitted in February 2015:

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7 Making a similar observation on the moral issues that arise not within the context of fieldwork but within the ethnographic texts, Bell (2014, p. 517) notes how, in agreement with a positivistic paradigm underlying IRB protocols, "the act of writing is generally understood to present a transparent 'writing up' of study results." See also Pels et al. (2018).

8 As I had explained in the my first draft of my proposal, my methodology in fact mostly revolved around audio-visual recording of spontaneous interaction and participant-observation.

> Due to the dynamic and emergent nature of ethnographic research, consent to be interviewed and to participate in the research is an ongoing process and not a bounded event marked by the signing of a release form. In order to ensure that my interlocutors will always be informed and consenting participants in this project, I will provide them with frequent reminders of the fact that my presence in the community is motivated by a research intent. […] With the term open-ended interviews, I do not refer to a formal interview setting in which research participants sit at a table and respond to a list of predetermined questions. Rather, I refer to a dynamic alternation of questions and answers that emerge organically form joint activities and that result in in-context conversations about daily life and practices. […]. Interlocutors will be to a large extent self-selected.

While the rewrite did produce the letter I needed to secure the grant that I had been offered pending the IRB approval, the discussions aimed at expunging the recruitment framework and language proved unsuccessful[9]. Contrary to the dynamic and complex nature of the roles and relations emerging in the field, the increased bureaucratization of research ethics in the U.S. and beyond entails clear conventions and consistent regulatory practices to ensure institutional and individual accountability. As Pels (2000, p. 135) points out, the regimentation of ethical reasoning and practice through "auditing and accounting" has become one of "the operational signs of the global spread of neoliberal values." By audit cultures and technologies I mean ubiquitous sets of moral ideologies, management practices, monitoring techniques, and regulating policies aimed at enhancing standards of accountability – a concept that, as Marilyn Strathern (2000b, p. 1) points out, links the two separate domains of moral reasoning and financial accounting.

Not limited to a specific type of institution or region of the world, audit cultures are distinctively crosscutting and multifaceted in nature. They range from the techniques used by transnational financial agencies to measure a country's economic performance, to the technologies for assessing individuals' skills both within educational and professional contexts; from the calculative practices to enhance academic productivity, to the protocols for regimenting research. In spite of their diversified application, the technologies of the audit are tied to the neoliberal reorganization that char-

9 For the sake of clarity, but at the risk of stating the obvious, let me underscore that criticizing "the embrace of informed consent as an […] anthropological virtue" (Bell 2014, p. 512) does not equal to advocating a *laisser faire, laisser passer* approach to research ethics. Rather, my reservations resonate with Bell's (2014, p. 512) point that anthropologists should not uncritically accept the paradigm of informed consent as "a meaningful way of conceptualizing and addressing the ethical issues involved in ethnographic research."

acterizes our contemporary moment[10]. They profoundly impact everyday life, refashioning working environments, political discussion, and human subjectivities. Interestingly, a significant prerogative of these protocols is, as Strathern (2000b, p. 3) notes, that: "the audit is almost impossible to criticize in principle – after all, it advances values that academics generally hold dear, such as responsibility, openness about outcomes and widening of access." This capacity of the audit to command moral assent is a fundamental factor in promoting the interwoven spread of neoliberal rationalities and bureaucratic procedures of accountability.

But if the interconnectedness of moral and financial accountability of the audit is almost impossible to criticize, how can we elude the moralizing bend underlying the contemporary bureaucratic regimentation of human life and conduct (Graeber 2015)? How can we effectively resist the imposition of research protocols of audit cultures over our fieldwork practice? In order to answer these questions, I draw on a recent essay in which Ugo Fabietti (2012) discussed the epistemology of serendipity and the structure of the ethnographic discovery in relation to Peirce's notion of abduction. I suggest that rather than seeking to contrast the bureaucratic standardization of our intellectual labor by underscoring the moral intricacies of fieldwork encounters and advocating for plural, organic, and context-based moral approaches – what Pels et al. (2018, p. 409) call the enactment of respect through the exercise of "contextual social-relational judgment" – we may try to recast the discussion through a different perspective. To be blunt: I propose to bracket the emphasis on morality in favor of an epistemological and semiotic approach. In so doing, I do not mean to dismiss the efforts of those who have warned us against the risks of privileging formal compli-ance with informed consent over the practice of "systemic self-reflexivity" (Stodulka 2015, p. 84); appealing to a higher and more sophisticated context-based morality does not seem, however, to be an effective response against the regimentation of academic labor that characterizes the current late capitalist moment.

Thus, instead of contrasting the sweeping morality of neoliberal accountability through a discourse of ethical complexity, I would like to go back to Peirce's semiotic reflections on abduction as discussed in Fabietti's (2012) essay on ethnographic discovery. Contrary to progressive entanglement of both everyday life and ethnographic research in the neoliberal protocols of financial and moral accountability, the "evidentiary process" through which – according to Fabietti (2012, p. 18) – theories develop from fieldwork en-

10 During the last two decades, following Strathern's (2000a) seminal edited volume and Power's (1994, 1997) path-breaking discussion, audit cultures have become a key domain of anthropological research. See for example, Brenneis et al. (2005), Cavanaugh (2016), DiGiacomo (2005), Graeber (2015), Power (2004), Shore (2008), Shore and Wright (1999).

counters entails, I argue, structures of agency and temporality that cannot be subsumed under the bureaucratic machinery of neoliberal audit cultures.

As I will argue in the following pages learning how to be an ethnographer calls for a surrendering to the unexpected. In order to illustrate this point, I will discuss the role that, during my fieldwork in upland Indonesia (Toraja, Sulawesi), unexpected events had in advancing my understanding of local social hierarchies and moral-political systems, reorienting the very way I conceived my ethnographic object. But what exactly is an ethnographic discovery? Let me discuss this in relation to Fabietti's epistemological reflections on serendipity.

Serendipity as Method

In an illuminating essay published five years prior to his premature death, Ugo Fabietti (2012) focused on the somewhat neglected topic of ethnographic discovery, that is, a mode of knowing that, drawing on unplanned encounters with events and "surprising facts" (Peirce 1931-1958, 5.173), may produce a new perspective on a certain issue and give rise to a new theory. In this sense, ethnographic discoveries may be understood as epiphanic revelations and sudden realizations that enable the ethnographer to "go forward" in the knowledge of her object.

According to Fabietti (2012, p. 19, my emphasis), serendipity – the faculty of discovering by "accident *and* sagacity" things that one has not been seeking – plays a key role in enabling these ethnographic insights. Contrary to a generic and commonsensical meaning of "serendipity" as "fortunate accidental discovery," Fabietti claims that the "and" that connects "accident" with "sagacity" should be emphasized, for the role of intuition based on inference is essential to the ethnographic discovery. Put differently, it may be argued that it is the enhanced inferential process stimulated by the state of wonder originated by these unanticipated and meaningful events that furthers anthropological knowledge, thus producing new theories.

These cognitive mechanisms can also be captured by the notion of abduction. The term was coined by Charles Sanders Peirce (1931-1958, 5.172) and used in reference to the logical process of "forming explanatory hypotheses." Different from both deduction and induction[11], abduction takes place in the context of discovery (i.e., the stage of inquiry where new

11 According to Peirce (1931-1958, 5.170-5.171), "induction consists in starting from a theory, deducing from it predictions of phenomena, and observing these phenomena in order to see how nearly they agree with the theory;" while deduction entails that "we set out from a hypothetical state of things, which we define in certain abstracted respects" (Peirce 1931-1958, 5.161).

theories are generated) and "is the only logical operation which introduces any new idea" (Peirce 1931-1958, 5.172). Abduction entails a specific type of semiotic interpretation, in which a sign is creatively and insightfully linked, through the selection of a probable hypothesis, to another phenomenon and/or a possible cause.

In order to fully appreciate Peirce's notion of abduction, we need to situate it within his larger theory of signs. As it is well known, Peircian semiotics is complex and not systematically organized; his theories are in fact distributed over numerous articles written over a period of several decades. Simply put, according to one of Peirce's (1931-1958, 4.536) semiotic trichotomies, a sign could be engaged with its object through three basic kinds of relationship: symbolic, iconic, or indexical. A symbol is a sign that stands for its object by virtue of arbitrary convention; an icon is a sign that reproduces some aspect of its object; an index is connected to its object through a relation of causality or contiguity[12]. To put it in other words, indexes are signs that have some kind of spatial or temporal relation with what they refer to as in "smoke indexes fire." The two phenomena entertain a relation of contiguity. The sign (smoke) is spatiotemporally and physically connected to the phenomenon it stands for (fire). Unlike the relation of resemblance connecting an icon with its object and unlike the conventional relation connecting a symbol with its referent, the semiotic relation between 'smoke' and 'fire' is established "by knowledge of a recurrent natural phenomenon" (Duranti 1997, p. 17). While the indexical relation between smoke and fire is well known, abduction is exerted on seemingly unrelated facts and takes place when the relation between indexes and their objects "is yet unknown" (Kruse 1986, p. 437).

Abduction can thus be understood as a semiotic interpretation that produces the discovery of new (i.e., previously unknown) links and connections between signs (or more precisely the category of signs that Pierce calls indexes) and between signs and their objects. Such process entails a form of "rational instinct" (Kruse 1986, p. 436) that may reveal connections between anomalies and regularities and between apparently unrelated phenomena. In this sense, abduction involves a mode of knowledge based both on reason and instinct, conjectures and observations, logic and guesswork. This type of reasoning is epitomized by the work of a "detective who establishes the author of a crime […] on the basis of clues that are not perceptible to most

12 It should be noted, however, that even icons and indexes entail a partial degree of conventionality and arbitrariness. The relation of resemblance and contiguity between an index (or an icon) and its referent undergoes a process of social construction. As Bruce Mannheim explains (2001, p. 102), "even the most 'natural' looking icon, […], is mediated through social convention and subject to the historically specific interpretative habits of its users. Consider the skull-and-crossbones, which used to mark poisons. Some children interpreted the icon through an alternate set of conventions to mean 'pirate food".

people" (Ginzburg 1979, p. 276). Drawing on Ginzburg's reflections on the epistemology of criminology, (art) history, medicine, and psychoanalysis, Fabietti (2012, p. 18) convincingly demonstrates how abduction is based on a larger "evidentiary process" by which subjects make discoveries through the inference of the underlying connection between a sign and its object. Like detectives, doctors, and hunters, ethnographers work with clues and traces. A clue may reveal a criminal's identity, a symptom may stand for an illness, a footprint may indicate a prey's passage.

There are two important characteristics in Fabietti's (2012) analysis of the cognitive mechanisms of ethnographic discoveries that are significant for my discussion. The first pertains to the oxymoronic combination of luck and wit, randomness and intentionality, reason and instinct, proactive perseverance and acquiescent abandonment to chance. If I understand Fabietti's point correctly, in order to make ethnographic discoveries, one does not need to be lucky to stumble across interesting facts. Luck *per se* is not essential and may be irrelevant or even counterproductive, for what really counts is the capacity—acquired by training and experience—to slip into a specific "structure of attention."[13] By this I mean a particular way of positioning oneself with respect to experience, the capacity of phenomenologically inhabiting a specific form of consciousness that may lead to ethnographic discovery.

The immersive and organic procedures of data gathering – generally subsumed under the category of participant observation – that characterize the ethnographers' work require learning how to be methodical and yet open to the unexpected. They involve designing meticulous research plans while being prepared to surrender to unanticipated events. This methodology entails using the researcher's body, as well as her sensorial and personal experience, as tools to understand cultural differences and local systems of practice and knowledge. For example, an unanticipated illness experienced by the researcher in the field may disclose important insights into local beliefs and practices, which otherwise would remain hidden. In a similar vein, the situated (i.e., through gender, age, body-size, linguistic proficiency, race, class, etc.) positionality of the researcher in the network of relations of her research context is an instrument of (not a hindrance to) understanding (see Fabietti 2012, p. 20-24). Based on a "systematic openness to contingency" and "disciplined relinquishment of control" (Lederman 2006, p. 485), the ethnographic structure of attention is inherently oxymoronic and therefore incompatible with the contractarian approach underlying IRB protocols,

13 I borrow the term from the phenomenological analyses (Berger 1999, Berger and Del Negro 2002) of the social organization of attention within music performances, where musicians arrange their experience of phenomena through foregrounding and back-grounding certain elements of their thoughts, bodies, perceptions, and memories.

which requires clearly defined roles and positions.

The second significant fact about the cognitive mechanisms underlying abduction (and evidentiary processes more in general) is that they seem to resist the logic of transparent accountability mandated by the regulatory systems of the audit. Rarely rigorous and generally occurring through split-second inferences, ethnographic insights proceed in a serendipitous way. Based on "coup d'oeil, instinct, and intuition," (Fabietti 2012, p. 19), they are difficult to parse and undo according to the documentary regime of the audit.

Linguistic anthropologists working on the relation between language and neoliberalism have showed the role played by specific textual artifacts and discursive genres in crafting forms of moral subjectivity and political rationality congenial to the neoliberal project (see Cavanaugh 2016 and Donzelli 2019a). Pivoting on notions of transparency, traceability, and accountability, logbooks, checklists, and informed consent forms partake in the "documentary practices" (Cavanaugh 2016) of neoliberal capitalism. Modeled on the preemptive logic of risk management, standardized consent forms aspire in fact at rendering the complexities of the ethnographic encounter commensurable with neoliberal discourse of choice and personal accountability – a model that is incompatible with the abductive inferences underlying ethnographic discoveries based on chance. Let me illustrate these claims with an ethnographic anecdote from the fieldwork I conducted in Indonesia in the early 2000s. Through this discussion, I seek to demonstrate how chance played a key part in directing the focus of my ethnographic research.

Discovering by Surrendering

When in 2002 I settled in the village of Marinding in the Toraja highlands of Sulawesi, I was excited at the prospect of devoting my doctoral fieldwork to the study of village politics. Local electoral campaigns were in full swing, which promised to offer a precious chance to attend to my primary research interest: political speechmaking. However, as is often the case with fieldwork, things did not go exactly as I had planned. Due to a series of fortuitous circumstances I gradually became incorporated in the domestic routine of the house where I was hosted and found myself unwittingly in charge of a number of household chores, which, among other things, entailed attending to numerous guests. My host was in fact the village chief and an expert in the local customary law and his house was the target of a constant flux of daily visits.

At first, I did not consider these domestic duties as worthy of ethnographic attention. While surrendering to the events that had turned me into a vicarious host of my host's guests[14], I initially considered my object of research as strictly limited to ritual and political speechmaking and I tended to dismiss the more intimate sphere of domestic interaction as not relevant to my project. My life appeared to me as divided between what I saw as "my ethnographic tasks" and "my domestic chores." For months, I would spend my days trying to juggle two very different sets of activities. On the one hand, I had to attend a wide array of events and political meetings, record and transcribe speeches, and keep track of my fieldwork notes. On the other hand, I was busy preparing meals or coffee for my host and his guests and making trips to the market to get food and other supplies.

As time went by, however, I gradually became aware of some drastic discrepancies between the implicit patterns that ruled my treatment of guests and Toraja tacit rules of polite behavior. My initial attitude in handling the customary offering of coffee or food was based on the idea that I should always make some enquiries concerning the preferences and the desires of the people I was in charge of taking care of. Therefore, whenever somebody paid us a visit, before preparing anything, I would first ask whether they wanted to drink something. Addressing guests either in Indonesian (I) or Toraja (T), I would use a variety of offering formulas aimed at unearthing visitors' preferences and inclinations:

Apakah Bapak ingin minum sesuatu?
Would you like to drink something, sir? (I)

Mau minum kopi atau teh?
[Do you] want to drink coffee or tea? (I)

La mangiru' apa komi? Kopi raka? Teh raka?
What [can I get] you to drink? Coffee? Tea? (T)

My offering formulas in both Indonesian and Toraja were modeled on the pragmatic norms that I had learnt in Italian and Northern American environments, where leaving the possibility of verbal refusal and eliciting the interlocutor's preferences and desires was considered a standard pragmatic procedure (Brown and Levinson 1987; Lakoff 1973)[15]. However, my efforts to apply the principle of non-imposition proved not very successful. People seemed embarrassed by my asking what and if they would like to drink, generally avoiding any direct reply.

Despite their consistency with grammatical norms, my offering formulas

14 For an extended discussion of this anecdote and its implications for a theory of ethnographic rapport, see Donzelli (2019b).

15 For a critique of the Eurocentric bias underlying classical sociolinguistic models of cross-cultural and universal norms of politeness, see Agha (1994).

were pragmatically off. Being, however, unable to understand what it was that I was missing, I began to experiment with different honorific levels, adding and removing terms of address and respect pronouns. But regardless of how I would frame my offers, asking the guests what they would like to be served unavoidably produced embarrassed smiles or silences. I thus adopted another offering strategy, and I presented my offering in a "yes or no question format", which did not entail expressing a preference on the offered item:

Apakah Bapak ingin minum kopi?
Would you like to drink coffee, sir? (I)

Lamoraikomiraka mangiru' kaa, ambe?
Would you like to drink coffee, sir? (T)

Moraikomi raka mangiru' kaa, ambe'ku?
Could I make you some coffee, sir? (T)

But even this "yes or no offering format" was either met with the negative answer 'tidak usah!' (I: 'It is not necessary!') or 'tae'namatumba' (T: 'There is no need!'), or, more often, with a no uptake on my interlocutor's part.

After several weeks of embarrassments and awkward exchanges, an unexpected joke casually made by my host confirmed my suspicions that my way of handling guests did not match the local standards. One evening, coming back home, I found that my host had a guest. I rushed to the kitchen to light the fireplace and boil water for the coffee. While I was walking towards the kitchen, I overheard my host jokingly asking the guest what he would like to drink, and then smiling at me, he commented with the guest: "we are now using the Western system." Baffled by the comment, I asked my host what he meant for 'sistim Barat.' Laughing my host explained that "sistim Barat" ("Western system") meant: "always asking the guest what he wants before preparing it."

As it is often the case with ethnographic discoveries, "details usually considered unimportant or even trivial" (Ginzburg 1979, p. 280) can provide important cues for understanding larger processes and develop new theories. In the case in question, upon hearing my host's amused gloss to his own joke it dawned on me that the misunderstandings and embarrassed responses I had observed during the previous months of domestic engagement represented important ethnographic facts. My host's casual joke reoriented my structure of attention and prompted me to compare with greater care my own technique for handling guests with that employed by other Toraja women with whom I shared daily activities. I thus realized something that had been hiding in plain sight for me for quite some time: that the most common pattern employed for offering was that of commands. Contrary to the interrogative structures with which I had tried to communicate deference, offers and invitations were made both in Indonesian and in Toraja using exhortative forms and imperatives:

Mangiru'!
Drink! (T)

Kumande komi sidi'
Have something to eat! (T)
Makan!
Eat! (I)

Minum kopi dulu!
Go ahead and drink [your] coffee! (I)

Once I finally noticed the systematic use of highly directive offering formulas and the equally systematic discomfort for my attempts at eliciting individual preferences through questions, I realized that the scripts that ruled food-giving in rural Toraja could be connected both with the local structures of land tenure and with the delicate position occupied—within the local moral-political economy—by the exposure of one's needs and desires. Let me clarify this important point through a brief overview of the Toraja socio-economic context.

The vast extensions of paddy fields (T: *uma*) that characterize Toraja landscape have historically been concentrated in the hands of the aristocracy (Bigalke 2005, pp. 231-236; Santos and Donzelli 2010). As a result of this uneven structure of land tenure, rice cultivation has always been based on sharecropping arrangements, a system locally referred to as *ma'tesan* or *mendulu*. These land tenure arrangements are conducive to complex patterns of food-mediated mutual dependence. Landlords rely on their clients to farm their lands, while clients depend on their patrons for a vast array of needs, such as help in paying school fees or medical expenses. In a context characterized by subsistence farming, high demographic pressure, risk of food shortage, and by a marginal presence of cash and market economy, "donations" in kind and the sharing of food have historically played a fundamental role in structuring the local forms of human sociality. In this system, the basic idea is that the landholder gives access to the land in exchange for a share of the harvest[16]. As famously described by Scott (1972, p. 92), the patron-client relation, typical of but not limited to Southeast Asia, involves a complex cluster of informal relations between a "power figure who is in a

16 In the past, the common division was one-third for the tenant (*to ma'tesan*) and two-thirds for the landholder. In more recent times, the tenant's share has increased to half of the product, in most cases.

position to give security, inducements, or both, and his personal followers who, in return for such benefits, contribute their loyalty and personal assistance to the patron's designs." In such a context, food-mediated hospitality is a fundamental means of gaining symbolic capital and social influence.

Rooted in the interplay of mutual dependence and enveloped within a powerful redistributive ideology, the relation between landowner and clients still unfolds in contemporary Toraja through practices of commensality. Landholder families and households, like the one where I was hosted, are expected to provide rice to a network of clients and distantly related lower-ranking relatives. While at first opaque to me, the abrupt commands whereby my Toraja interlocutors performed commensality (i.e., "eat!", "drink!") are thus to be related to a social environment marked by a combination of asymmetrical relations and social reciprocity. My "ethnographic discovery" concerned establishing an interpretative link between a casual joke made by host, the "subsistence ethics" of Southeast Asian agrarian societies (Scott 1972, 1976), the imperative structures with which my interlocutors went about the sharing of food and drink, and the awkward reactions triggered by the interrogative structures with which I would instead phrase my offers (i.e., "would you like some coffee?").

In a context where – to paraphrase Scott (1972, p. vii) – people have been historically standing with their neck up in water in fear that "even a ripple might drown [them]," the open inquiry into one's needs and desires has to be avoided as potentially face-threatening. As I discuss more in detail elsewhere (Donzelli 2019a, 2019b), the centrality of subsistence farming and the fear of rice shortage (T: *karorian*), that, according to Scott (1976, p. 3), shaped for centuries the "moral universe" of agrarian Southeast Asia, suffused the (to me) puzzling scripts of host-guest interactions. In this sense, the pragmatics of food-giving and the avoidance of direct enquiries into one's preferences and desires can be seen as related to a politics of generosity driven by a pervasive attempt at avoiding the degrading potential of exposing the patron's needs for services and labor and the clients' desires for food and protection.

The practice of compensating the extraction of unremunerated labor through "generous free" meals has long been described as deeply ingrained in the "subsistence ethics" of Southeast Asian agrarian societies (Scott 1976). My ethnographic discovery allowed me to see how, this large-scale "moral economy" depends, in large part, on daily communicative interactions through which food is offered and shared through careful conversational negotiations. The embarrassed smiles and awkward silences with which people were responding to my interrogative offers could be thus related to a local intersubjective tactic for the concealment of personal desires and mutual dependence between landowners and small farmers. Contrary to my efforts to be kind by offering options and granting my interlocutor freedom from imposition, the Toraja pragmatics of politeness requires using highly directive offering formulas. By offering food and performing commensality through imperatives and commands, my Toraja acquaintances could avoid the direct exposure of the interlocutor's needs and desires for food or services, thus concealing the precarious balance underlying relations of mutual dependence. My host's casual joke thus unveiled to me a fundamental aspect of the local political economy of desires, allowing me to establish an interpretative connection between the everyday patterns of commensality and the larger social structures underlying a great part of rural Southeast Asia.

Put differently, contrary to the models, pivoting on notions of choice and freedom, of polite behaviors described by Brown and Levinson (1987) and Lakoff (1973), the intersubjective risks that infuse host-guest interactions in Toraja do not derive from concerns for possible limitations on the hearer's freedom to refuse. Instead, what is intrinsically problematic within the handling of these categories of situations is that they inevitably involve the assignment of desires and the potential revelation of one's wants. In a context based on subsistence farming, the desire to be fed and to control others by feeding them plays a crucial role in the social reproduction of power relations and undergoes elaborate processes of cultural and linguistic elaboration. I realized that in order to understand Toraja political economy, I had to explore the cultural grammars of desires (i.e., the moral and affective ideologies surrounding local notions of longing and yearning) and the linguistic management of needs, preferences, and wishes. The cultural idioms of longing and the linguistic scripts used to express and conceal desires both within political discourse and domestic interactions thus became important aspects of my subsequent ethnographic work.

The abductive links that I originally drew one evening during the fall of 2003 when my host, amused by my inadequate style of entertaining guests, decided to openly tease me re-directed the focus of my attention and reshaped my research object. I became more interested in the patterns of domestic interaction and started to draw unexpected links between political oratory and the politics of the household. As a result of serendipitous events, I began to explore the specular relation between the highly directive structures of hospitality and the elaborately indirect grammars of political speeches. My linguistic and ethnographic work thus became primarily concerned with this apparent contrast, bringing me to study the Toraja politics and aesthetics of persuasion, that is, the semiotic labor performed around the desire to influence others through the spoken word (in the public realm of political speechmaking) and through the offering of food (in the domestic sphere). My research focus became the study of how the local structures of desire are being affected by impact of the major institutional changes that have refashioned Indonesia's political economy since the end of the Suharto regime. This process led me to conceptualize these ongoing transformations as a shift from a moral regime centered on the expectation that desires should remain hidden to a novel social legitimacy of individual desires and an emerging rhetoric of personal aspirations (see Donzelli 2019a). I could not anticipate any of these developments when I left for a year of continuous fieldwork in 2002.

Conclusions

The request for additional details on my recruitment plans that I received in February 2015 is an experience common to many ethnographers. Lederman (quoted in Anonymous 2006) says that "ethnographic proposals look incomplete to IRBs, lacking details in such areas as when research will start and end, exactly who will participate and what questions will be asked."

In the follow up "Checking In On Your Research Study" emails I receive from my University's IRB, I am periodically asked to fill in the "Continuing Review of Ongoing Research Form," which contains a set of simple and straightforward questions such as:

> Have you started recruiting participants? If so, detail how many.
>
> Have any participants withdrawn from the study? If so, detail how many and reason for withdrawal, if known.
>
> Have there been any changes to your protocol? If so, re-submit the protocol with changes indicated, and any modified informed consent and/or assent forms.
>
> Have there been any complaints, unexpected events, or protocol deviations related to the research? If so, detail them here.

When I get these emails, I cannot but think that three years ago we (my ethnographically-minded colleagues and I) should have tried harder to have our perspectives heard, so that our IRB could formulate protocols whose actual language might sound more congruent with what we actually do in the field. But back then I was overwhelmed with a full-time teaching load and the NSF pressure to get the IRB approval letter, and I was exhausted after months of difficult negotiations over the exorbitant (65%) share of indirect costs that my grant office wanted to apply to the research award. So I gave in. I surrendered, but not in the deliberate and methodical way I do while in the field. This time I simply succumbed to bureaucracy-driven frustration and exhaustion.

Responding (and being held accountable for my answers) to questions that presuppose methods that do not belong in what I consider sound ethnographic practice has required making difficult moral and scientific compromises. Some of these painful experiences, however, did seep into my most recent fieldwork experience, redirecting once more the focus of my attention. As a result of my 2015 encounter with IRB protocols, I became interested in exploring how neoliberal rationalities spread globally. Once again, in a rather serendipitous way, an unanticipated (and unwanted) occurrence affected (and partially disrupted) my research plans, making me realize how a similar logic of accountability underwrites both the context of U.S. research protocols and contemporary Toraja village politics. During these last three years of intermittent fieldwork in Toraja, my ethnographic attention has thus been primarily focused on the local uptake of the rhetoric of auditing and accounting. My encounter with IRB bureaucratic procedures has thus informed my current ethnographic project, prompting me examine how new genres of discourse drawn from the entrepreneurial ethos of corporate culture (i.e., logbooks, customer satisfaction surveys, electoral mission statements, cheering chants, flowcharts, and workflow diagrams) are being absorbed in a relatively remote and peripheral area such as Toraja, partially transforming local forms of moral and political rationality and partly being transformed by them.

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