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Invisibility and Labour in the Human Sciences

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Table of contents

Editorial: (In)visibility and Labour in the Human Sciences

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> It came suddenly, splendid and complete, into my mind. I was alone, the laboratory was still, with the tall lights burning brightly and silently….'One could make an animal—a tissue—transparent! One could make it invisible! All except the pigments. I could be invisible,' I said. […] 'To do such a thing would be to transcend magic. And I beheld, unclouded by doubt, a magnificent vision of all that Invisibility might mean to a man. The mystery, the power, the freedom.[2]

Science-fiction writer H.G. Wells put invisibility at the heart of a psychological thriller. A young scientist discovers how to alter the human body's refractive index so that it neither absorbs nor reflects light, making it transparent. But after testing the technique on himself, and finding himself unable to reverse the effects of his experiment, his optimism about the prospect of invisibility soon fades. Despite attempting to hide his predicament by covering himself in (visible) clothes, he appears as a phantom to those he encounters. The terror that ensues turns the scientist into an agent of mad, murderous, desperate destruction as he attempts to find a secluded place where he can hide his affliction and search for an antidote. His story offers two takes on invisibility: one of power and autonomy, and another of violence and desperation. Inhabiting the roles of both subject and researcher, the narrator shows us how invisibility permits a privileged kind of observation. He also draws our attention to the labour that goes in to producing and maintaining invisibility, the tension between protective and subversive aspects of concealment, and the ubiquitous threat of ghosts.

Wells captured the fact that invisibility can be both virtue and vice. Through informal conversations in and around the Max Planck Institute for the History of Science, volume contributors contemplated this double resonance as we discussed the ways in which certain people and questions haunt our work without taking shape in published form. In local reading groups we shared concerns about those people who are absent from our archives, the ethics of protecting and acknowledging sources, and the moral and political valences implicit in our work. To think

1 As well as those cited in this editorial, we want to sincerely thank: Mirjam Brusius, Eric Llavaria Caselles, Lorraine Daston, Josephine Fenger, Petter Hellström, Myriam Klapi, Nina Lerman, Veronika Lipphardt, Nina Ludwig, Birgitta von Mallinckrodt, Johanna Gonçalves Martin, Helga Satzinger, and Alexandra Widmer. The workshop was funded by Department II, directed by Lorraine Daston, and the Independent research group 'Histories of Knowledge about Human Variation', directed by Veronika Lipphardt.

2 From *The Invisible Man: A Grotesque Romance* by H.G. Wells (1898 [1897]: 169). These words are uttered by the protagonist as he recalls the circumstances of his momentous scientific discovery.

through these themes in a more formal setting, we convened a workshop in June 2015 for a group of colleagues whose interests converged on the history of the human sciences. For one of the organizers (Kaplan), a member of the working group 'Historicizing Big Data', it was an opportunity to investigate how the use of particular classifications, the selection of certain characters, and the marshalling of various scribes create shadows in the historical record. For the other organizer (Bangham), then working in the research group 'Histories of Knowledge about Human Variation', two questions seemed particularly pressing: What are the everyday self-assessments and prejudices that become incorporated into the wide range of sciences of human variation; and how do our own political positions, as working historians and sociologists, shape the stories that we tell.

Following Steven Shapin, Roger Sanjek and others, we began by talking about the politics of representation.[3] For these authors invisibility connotes a lack of recognition or credit, so that the social and political marginalization of certain people and processes pose barriers to understanding how science operates.[4] But we also wanted to attend closely to the risks, investments, values and motivations of those participating in science, and the circumstances in which these are made visible, or not. In order to highlight these stakes (political, moral, embodied, epistemological) we began to think specifically about labour.[5] The conjunction of invisibility and labour allowed us to align diverse practices, from tissue generation (Waldby and Cooper, 2014), diary writing (Moran, 2015) and dancing (Laemmli) to tabulation (von Oertzen), translation (Vongsathorn) and census making (Surdu). It produced an expanded category of the human sciences, including biomedicine alongside disciplines such as sociology, linguistics, psychiatry and demography.[6] As such, the stories gathered here are a contingent, local snapshot of work at the MPIWG. They emphasize European traditions in the history of science even as they depend on the insights and methodologies of global and postcolonial science studies.[7]

The workshop papers converged on three overlapping domains in which visibility and invisibility operate: people, power, process (corresponding to the three headings of this volume). The first of these attends to the set of enterprises that engaged 'ordinary' citizens in scientific projects. Laura Stark weighs up theoretical frameworks for understanding why United States citizens freely chose to volunteer as biomedical research subjects for the 1950s National Institutes for Health. Stark asks how historians might recover the structural and social conditions of participation without necessarily resorting to notions of exploitation. This led into discussion of those enterprises that are today labelled 'citizen science'. Analyses of citizen science often focus on what it can tell us about formal scientific practice; instead, several of the essays in this volume

3 For two foundational accounts see Shapin (1989) and Sanjek (1993). The conceptual ramifications of these have been further elaborated in the STS scholarship; useful landmarks and overviews can be found in Biagioli (1999) and Hackett et al. (2008).

4 Particularly rich approaches have been developed in Feminist Science and Technology Studies. For a captivating set of reflections on these topics, see interviews with Geoffrey Bowker, Sandra Harding, Annemarie Mol, Susan Leigh Star and Banu Subramaniam in Bauchspies and de la Bellacasa (2009).

5 The term 'labour' is has a rich and heavily theorized history (e.g. Arendt, 1958). We use it because we want to evoke the corporeal dimensions of human activity as well as the technical and intellectual.

6 On the difficulty of designating those sciences devoted to the study of humans, see Porter and Ross (2003: 1–3).

7 For a very partial (but exemplary) list of such scholarship, see Sandra Harding (2008) Gabriela Soto Laveaga (2009), Mary Louise Pratt (1992) and Warwick Anderson (2008).

engage critically with the very term 'citizen'. Who qualified as 'normal' subjects for the postwar NIH (Stark)? What kinds of citizens were produced by the British 1930s sociological enterprise Mass-Observation (Jardine)?[8] Complicating the democratic claims made on behalf of citizen science, Elena Aronova described how Soviet authorities implemented a top-down model of public participation in earthquake detection in her workshop presentation.[9] Not only do these examples unpack the term 'citizen', but they also suggest that different modes of citizenship might be constructed through participation in scientific research. Josh Berson's essay—which deals with today's 'quantified self' movement—highlights the different ways of knowing that people bring to such projects.

The essays in the second section of the volume tie participation in science to the themes of credibility and power. Rosanna Dent, in her contribution, describes how Xavante people in Brazil have systematically trained and enculturated visiting researchers—herself included—to secure political visibility in struggles for land, healthcare and education since the late 1950s.[10] Taking an ethnographic approach, Caitlin Wylie discusses how technicians use certain forms of invisibility to maintain power and autonomy in the paleontological laboratory. Other essays examine how certain experiences undermine or bolster the capacity to witness in the eyes of political authorities—even, as Mihai Surdu describes, one's own ethnic identity.[11] Whereas Susan Lindee previously showed how the embodied experience of a medical condition can sometimes be a source of valuable knowledge (Lindee, 2005), other instances reveal that such experiences can also discredit one's capacity to testify. In her essay for this volume, Lindee focuses on the challenges experienced by non-scientist residents of Fukushima, Japan, as they carry out their own environmental radiation monitoring programmes. Pointing to the epistemological privilege of certain kinds of observers—as well as the barriers to achieving credibility—workshop participant Sarah Blacker (not in this volume) gave examples of the kinds of extensive political and epistemological work that have been required to transform Indigenous knowledge into data that can be recognized by state actors and policymakers in Canada.

Reflecting on the papers presented at the workshop, several participants asserted that invisibility is not fixed but is instead the outcome of multiple and often conflicting processes—the theme of the last section of the volume. Abstracting and effacing relations are part of such work as collecting specimens, transcribing interviews, translating questionnaires and assembling archives. Several of the essays attend to how 'abstraction work' (von Oertzen) has been used to link individuals to collectivities defined by class, gender, language, and nationality (Bowker and Star, 1999; Kaplan, Laemmli, Surdu).[12] In his contribution to our roundtable discussion

8 For informative published examples, we have Alexandra Widmer's (2010) account of forms of biomedical citizenship in the colonial New Hebrides; Joe Moran's (2015) history of diary-writing practices in Britain, and Helen Macdonald's (2002) work on citizenship and birdwatching.

9 Aronova (not in this volume) was speaking of postwar Soviet Russia; her full paper 'Citizen Seismology, Stalinist Science, and Vladimir Mannar's Cold Wars' is forthcoming in *Science, Technology & Human Values*.

10 In another story of science and political visibility, Gabriela Soto Laveaga describes how Mexican peasant farmers achieved visibility in president Luis Echeverria's populist political programme via their farming of the wild yam Barbasco for steroid-based pharmaceuticals (Laveaga, 2009).

11 For more on 'situated knowledges' see Donna Haraway (1988); for 'standpoint theory'— how social identities and structures of domination determine what a person can see and know—see Alison Wylie (2003, 2012).

12 This builds on a robust literature devoted to scientific inscription (Coopmans et al., 2014; Delbourgo and

at HSS in San Francisco (November 2015), Dan Bouk showed us how this worked in practice: He described how doctors and actuaries of early twentieth-century life insurance companies created and used standardized 'blank' forms to make individuals and aggregates move in and out of view. Similarly, Lara Keuck writes about early twentieth-century psychiatrists' notebooks, and reflects on how their material properties (the flexibility to tear out sheets or add new pages) shape the historical record. Complementing Keuck's example (although part of an earlier section of the volume), Boris Jardine's essay about Mass Observation analyses epistemic and literary effects of the ways that its organizers abstracted and managed their observers in archives and in print.

In their materiality and particularity, archives seem to resist abstraction.[13] An ongoing investigation of the 'sciences of the archive' at the MPIWG directly informed much of our conversation about the contributions of collectors, translators and other kinds of scientific brokers.[14] Joanna Radin describes the labours of people and technologies that produce and maintain the value of archives (in her case, of frozen blood), while Donatella Germanese and other essayists draw attention to the work of moving objects of cultural heritage (songs, dances, skulls) into the realm of science, and out again (through repatriation, creative reuse). In some cases we see that the logic of an archive indelibly marks its meanings and possible uses; while in others, changing casts of characters give new meanings to materials over time (Laemmli, Jardine). Recent works also point to the challenges of reading disciplinary, colonial and vernacular archives (Roque and Wagner, 2012; Brusius, 2015)—and reflect on how the narratives we tell are shaped by the archivists who assemble those resources (Keuck), the legal frameworks that maintain them (Radin and Kowal, 2016), and the changing ethical regulations associated with their continued existence (Kowal, 2013).[15] These authors prompt us to think about how our institutions and professional identities condition access to resources (Dirks, 2015), and how our archival practices might be made explicit in our work (Stoler, 2009). We hope that the reading list at the end of this volume will extend discussion of these topics, and provide a resource for students.

Many of these readings drew attention back to our own work, and offered an opportunity to reflect critically on the sources, institutions, and systemic consequences of that research. While such questions chart familiar (if unstable) ground for sociologists and anthropologists, they are less routinely taken up by historians.[16] As commentator Sally Kohlstedt observed at the workshop, historians and STS scholars are often motivated by the 'thrill of the hunt'—a desire to discover that which has not been seen before. But we seldom discuss the tacit moral and political implications involved, and often set aside questions about where such recovery projects can lead. Some of our contributors tackled these issues head on. In her commentary at the workshop

Müller-Wille, 2012; Lenoir, 1998).

13 See the recent special issue of *Limn*, 'The Total Archive', http://limn.it/issue/06/.

14 For the wide range of 'sciences of the archives' projects at the MPIWG, see http://www.mpiwg-berlin.mpg.de/en/research/projects/deptii_daston-sciencesofthearchives. The rich literature on the important roles of brokers includes work by Wenzel Geissler (2005); Simon Schaffer et al. (2009); Lyn Schumaker (2001) and Alexandra Widmer (2010).

15 In these articles, Radin and Kowal are concerned with archives of frozen blood, but their insights on the temporalities folded into practices of storage are amply suggestive for thinking about paper archives.

16 Although note the rich analytic traditions of psychoanalysis as therapy and as critical approach.

Minakshi Menon described her own experiences of working with translators who desired to remain invisible, arguing that there are cases in which academics must resist the temptation to turn individuals into subjects of analysis. Underlining this point, commentator Jenny Reardon reiterated that taking 'visibility' for granted risks missing the ways that technologies and cultures of the visible concentrate power in unequal ways.

The final discussion also turned to the meanings and consequences of our work as historians and STS scholars. Several workshop participants raised the question of how we could become more visible as interpreters and negotiators of new technologies, new kinds of scientific knowledge, and their legal frameworks (Berson, Dent, Lindee, Radin). This might require us to make our own political stakes more explicit. It might require us to publish in different kinds of media and outlets. It might mean engaging more closely with archivists, curators and preservationists as they fashion the tools of future historical research (e.g. Aicardi and García-Sancho, 2016). And it could mean helping to forge new kinds of spaces and institutions for engaging scientists, humanists, engineers and other stakeholders (Reardon, 2013). As we look forward to such challenges, we are grateful for the spirit of openness and cooperation that went in to the preparation of this volume.

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People

The Bureaucratic Ethic and the Spirit of Bio-capitalism

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Past as palimpsest: Bureaucracy and the production of 'invisibility'

Since the social turn of the 1980s, the organizing question of the history of science has been, How is knowledge made? The premise of the field is that matters of fact and scientific truths are not pre-existing entities, nor the sum total of all possible certified truths. Rather, formal knowledge is an achievement. The task of scholars has been to study science as a practice—as an everyday activity—and the conditions of possibility for making knowledge, conditions that are at once social, material, institutional, and conceptual. For good reason, many scholars start with formal knowledge-makers themselves: Scientists, experts, and other authoritative knowers. When scholars invoke additional actors (which they do regularly, in a robust, relatively recent body of work) they find that activists, amateurs, underlings, and publics at times fight against, and at times join with, the established, the certified, the powerful, and the elite makers of formal knowledge in the craft of science (Epstein, 1996; Moore, 2009; Wisnioski, 2012). This early staple question of the field begat a second organizing question of a more recent vintage, namely, the question of 'justice', which explores the assumptions built into scientific products and their patterned consequences. Whereas 'ethics' marks an interest in understanding and, possibly, adjudicating historical actors' proper treatment of people and things in the past, 'justice' designates a focus on the ultimate shape that science takes—given that the content of science could take a variety of forms—and the systemic effects of its ultimate form. Questions of justice ask: Which social values have been embedded in knowledge-making enterprises? How? And with what broad-scale consequences? (Mamo & Fishman, 2013; Reardon, 2013; Reardon, Metcalf, Kenney, & Barad, 2015; for excellent case studies in justice, see Kowal, Radin, & Reardon, 2013; Reardon, 2004; TallBear, 2013).

I work on the history of morality (or 'moral kinds' as Ian Hacking would have it), that is, how certain practices come into being as right or wrong, good or bad (Hacking, 1990). My current project is on the modern market for 'human subjects' of medical experiment, which came into being sometime after World War II in its current, previously unprecedented form as an anonymous, large-scale system in which civilians are exchanged for money. This market stands in contrast to the smaller, intimate transactions in which scientists experimented on family members, their students or themselves, and also in contrast to large-scale markets that used, not civilians, but people with an obligation to the state, such as soldiers and prisoners. Since 2010, I have collected more than 100 oral histories of Anabaptist 'voluntary service' workers and other people who were 'normal controls' at the Clinical Center of the US National Institutes of Health, as well as oral histories of former program administrators and NIH scientists who used the Normals in their research. I have been comparing these vernacular accounts with NIH's archived record of its Normal Volunteer Patient Program.[1]

My aim has not been to find scandals, though there is plenty of unflattering information about NIH ready to be whipped into a media froth. My aim, instead, has been to explore the questions that organize our field: How was clinical knowledge made in practice, given that scientists' research materials were rights-bearing civilians who may have had thoughts, interests, and inclinations of their own; and What were the patterned consequences of these practices? The first step, though, was to create the very archive required to answer these questions, namely, the vernacular archive of former human subjects and the scientists who used them, which would complement existing historical collections. In creating this archive, these human subjects in the past became my own human subjects in the present.

It goes without saying that it was hard to find the people I aspired to include in the vernacular archive, but this difficulty pointed directly at an answer to my historical question. How was knowledge made? The answer: Through practices of erasure made possible within bureaucracies. These erasures—in the name of protection, privacy, and consent—in turn made clinical knowledge possible and, at the same time, made my historicist ambitions nearly hopeless.

In creating the vernacular archive, I had anticipated that I would find records of force and (soft) violence, and though I certainly found some cases (prisoners and laid-off mine workers were 'normal controls', too), I also found willing, enthusiastic civilians who participated for no financial reward, save a token stipend. In finding this politically uncomfortable evidence, I also located the source of a paradox, namely, willing human subjects of medical experimentation enthusiastically participating in a system that itself created forms of discrimination and violence they opposed. This paradox is a legacy in part of the historical record and the emphasis on archiving evidence of classic research abuses. To be sure, the Tuskegee Syphilis Experiments and Nazi doctors' abuses were real and remain important to study. But it is also a legacy of how previous scholars have interpreted the historical record.

My aim in this essay is to consider the strengths and weaknesses of two theoretical approaches to studying the experiences of people *within systems of exchange* in light of the structural consequences *of these systems of exchange*—scholarship in the Marxist tradition, and work in the Weberian tradition. Each of these approaches offers a way to align seemingly contradictory evidence. My own research yielded, on the one hand, situational evidence of (some) people's happy experiences as clinical materials, and on the other hand structural evidence that the market for human subjects sustained scientific racism and economic inequality that was either counter to their interests or that they actively opposed. Whereas work in the Marxist tradition aligns situational and structural evidence with recourse to an unsatisfying psychological concept, 'false conscious-

1 I have done more than 100 oral history interviews with former Normals (from a range of source organizations) as well as former NIH scientists and program administrators. For an example, see this YouTube video from my oral history with Wilmer Wedel, a Mennonite volunteer, who married another young Mennonite whom he met at the Clinical Center, Martha, is also audible in this video: http://youtu.be/36O9MfmLvVA.

ness', work in the Weberian tradition shows how the mechanism through which modern science operates—namely through bureaucracy—naturally produces the paradox I found: Happy human subjects of medical experimentation within a broader system that itself yields forms of discrimination and violence that they were actively trying to redress.

Marxist approaches

Histories of human experiment tend to place science in an agonistic field that pits scientists against other, less powerful actors in their immediate surroundings. This literature commonly draws on Marxist traditions in which actions are evidence of power struggles between workers and capital—embodied in various human dyads, including in the form of the scientist and human subject. The punch line in this body of work is that knowledge is made through exploitation. At a structural level, this claim is persuasive. There are striking patterns in social and economic inequality among the people who are used as human subjects (Fisher, 2007; Reverby, 2009; Reverby, 2011).

The Marxist tradition might seem to be a sensible framework for making sense of structural inequality, and yet it is weaker for thinking through situational evidence of people's experiences in the modern economy, including their experiences of science in the age of capitalism. As a result, the literature on the history of human experiment tends to deal with counterevidence in an unsatisfying way. The historical record is replete with examples of human subjects who themselves claimed to know precisely what they were doing. The happy, clear-headed worker under capitalism confounds the Marxist tradition. To explain how such people could exist, Marx and followers attributed their perspectives to false consciousness, which is the inability of (Marx's version of) people to appreciate that they are participating in their own exploitation (e.g., Abadie, 2010; Comfort, 2009; Dwyer, Ellen, 2012). The concept implies a universal human nature and experience of capitalism, and it undergirds scholarship created in Marx's debt.

Marx was cut from the same cloth and working in the same historical moment as Freud. Among late nineteenth-century German-speaking intellectuals, he was "like a fish in water," to use Foucault's felicitous phrase (Foucault, 1994: 262). Marx's concept of false consciousness, which was the mental veil that hid people's own best interests from themselves, had great affinity with Freud's unconscious, which was the puppet master working behind the scenes in all of us and pulling our strings. A century later, it came to seem, in Foucault's reckoning, that "the whole of modern thought is imbued with the necessity of thinking the unthought…of making explicit the horizon that provides experience with its background of immediate and disarmed proof, of lifting the veil of the Unconscious, of becoming absorbed in its silence, or of straining to catch its endless murmur" (372). Foucault's damning description of the liberal enlightenment political project of the human sciences is also a harsh criticism of historians who purport to know people better than they knew themselves. "[I]t is reflection, the act of consciousness, the elucidation of what is silent, language restored to what is mute, the illumination of the element of darkness that cuts man off from himself, the reanimation of the inert—it is all this and this alone that constituted the content and form of the ethical" (328). In his theory of capitalism, Marx treated with condescension the same people he wanted to rescue through his politics. Historians who follow in lockstep risk doing the same and adopting the liberal enlightenment project that Foucault warns against.

My point is not to suggest there is or was a world without exploitation. My point, instead, is to suggest that false consciousness is an unsatisfying explanation for historical evidence of action (Halley, 2008). The concept of false consciousness undermines the credibility of historical evidence itself, and turns historians' task of source criticism into a dangerous game. Evidence of the happy human subjects of experiment I sometimes found in my own work is an awkward fit with twenty-first century sensibilities. Politically, it would be easier to rely on the concept of false consciousness than to accept responsibility for explaining uncomfortable evidence. The concept of false consciousness also pushes historians into a contradiction. Through the condescension of posterity (Thompson, 1963), adherents of Marxist approaches recreate the very paternalism that they claim to want to subvert (Anderson, 1991).

How can scholars align structural findings about markets (they are exploitative) with situational findings (people often experience markets as satisfying)? The field of economic sociology has been developing ways of explaining this seeming contradiction between people's situational experience of markets and the structural patterns of inequality that markets create. The key has been to appreciate that markets trade simultaneously in symbolic values and in financial value.

Through the process of 'valuation', markets—including markets for human subjects—*come to appear* as objects outside of and prior to human activity (Fourcade & Healy, 2007; Lamont, 2012; Zelizer, 2009). In actuality, markets are social products that must be created and then sustained through ongoing work. The key insight of economic sociology is that symbolic practices—the ways in which people make meaning—affect market processes. When evidence of the injustices created by markets at a structural level butt up against evidence of the satisfaction of people involved in market transactions at an experiential level, they can be brought into smooth alignment with the help of another social theorist, namely Max Weber.

Weberian approaches

Weber has helped me to explore how bureaucratic arrangements, including those endemic to modern science, intentionally embed practices of erasure. Weber is useful for understanding the processes through which people are made invisible, both in the past of science and in the present of the historian's craft. At this reflexive level, Weber's approach also resonates with Donna Haraway's concept of "situated knowledges" (Haraway, 1988). Haraway's concept reminded scholars that all knowledge comes from specific positions (biographical bodies, physical locations). But more radically for its time, the concept of situated knowledges also described how the bits of insight that are authorized as formal knowledge are necessarily incomplete (a logical extension of the truism that people-in-contexts make facts) and that the bits of insight allowed to count as authorized formal knowledge reflect collective priorities about what *kinds* of insights can count as scientific. Haraway, like Weber, worries about the limits of Marxism. The Weberian approach to bureaucracy takes as its premise that invisibility is an historical achievement, and cautions that scholars who posit 'invisibility' rather than ask how invisibility is accomplished implicitly adopt a vantage point—an unacknowledged structural and moral position themselves.

For Weber, one defining feature of the modern Western world is the arrangement of all facets of life within bureaucracies. Put briefly, in *The Protestant Ethic and the Spirit of Capitalism* (Weber, 2008 [1930 trans]), Weber showed that saving (in the spiritual sense) morphed into saving (in the financial sense), which begat the modern Western variant of capitalism that, for Weber, resulted in the organization of the world into bureaucracies. This final move—the organization of everything into bureaucracy—is Weber's signature observation. He accepted the motivating claims of Marxist theory, then played them out, and ultimately and persuasively undermined them. The (successful) revolution that Marx forecasted did not come in Weber's lifetime because, he argued, moderns were ultimately happy living, and making knowledge, in the chains of bureaucracy.

As a set of historical claims, this account has problems. As a set of theoretical claims, however, this is a cunning insight into the conditions of modern knowledge production.[2] Weber invites historians of science to consider the conditions under which bureaucracies obscure individuals. By exploring the case of NIH's market for human subjects, I aim to suggest not that Weber is right, but that he is unusually helpful.

The bureaucratic ethic and the market for 'human subjects'

In 1953, the US federal government opened a research hospital at the new location of its main campus in Bethesda, Maryland. For scientist–administrators, one of the first orders of business was to sign contracts with suppliers of materials needed for clinical research. One kind of material they needed was human subjects for medical experiments. Their supply of sick people was secure: Physicians throughout the US and the world notified Clinical Center researchers of interesting patients and sent curious cases to the Clinical Center: Dwarfs, Native American children, schizophrenic quadruplets, for example.[3]

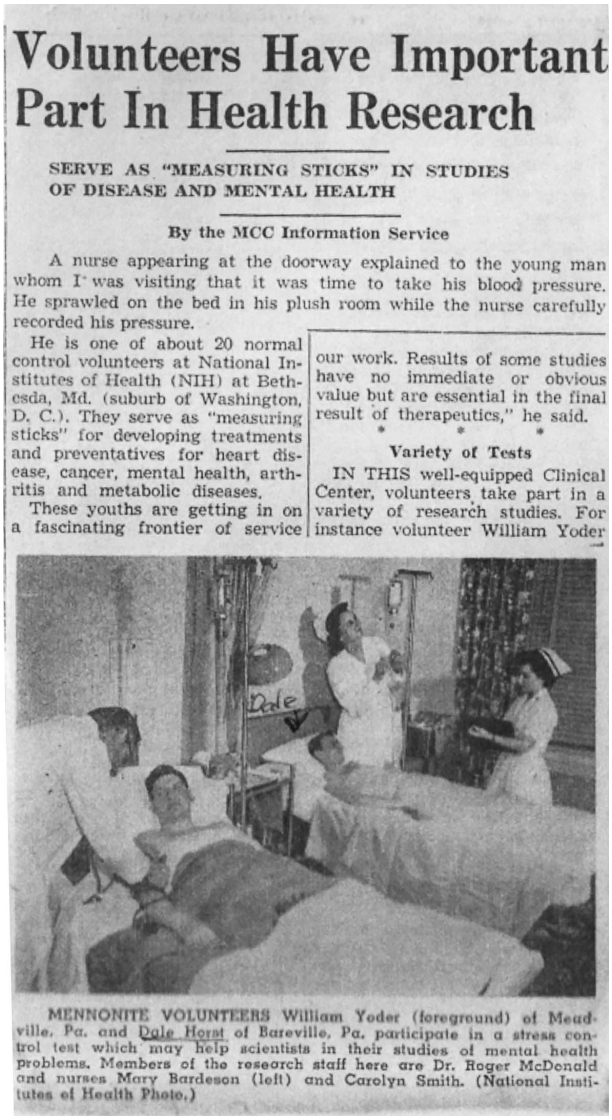
The supply of healthy people was a different matter. Prior to the middle of the twentieth century, medical researchers commonly ran experiments related to immediate wartime needs and ran them on specific kinds of people. (I am setting aside psychological research.) The research tended to be intentional infection studies or studies of wartime prophylactics (pesticides and materials tests, for example). Most important, the studies were done on people who had a debt to the state, such as soldiers, prisoners, or orphans (Aronowitz, 2014; Bateman-House, 2009; Comfort, 2009; Lederer, 1995; Moreno, 2001; Welsome, 1999).

At the Clinical Center in 1953, scientist–administrators reactivated an arrangement they found quite amenable during World War II that fit this model of 'procuring' subjects. NIH signed contracts with two Anabaptist churches, negotiated with the help of the Selective Service Administration, to use Religious Objectors as healthy experimental subjects. (The Korean War was

2 For a recent, instructive example of the use of Weber in science studies, see Shapin (2010). For elaborations of my own claims about Weber for science studies, see Stark (2014).

3 Evidence and further explanation is included in my book manuscript, *The Normals*.

a boon for the scientists, who needed an active draft and conscripts to justify the contract.) As soon as they had the legal-organizational infrastructure set, NIH science-administrators opened the program to any members of the Mennonite Church and the Church of the Brethren, who were channeled through formal programs already put in place by these churches' national organizations for missionary and voluntary service (Figure 1). It was 1954; NIH needed female bodies; and the churches needed service placements for Anabaptist women.



Volunteers Have Important Part In Health Research

SERVE AS "MEASURING STICKS" IN STUDIES OF DISEASE AND MENTAL HEALTH

By the MCC Information Service

A nurse appearing at the doorway explained to the young man whom I was visiting that it was time to take his blood pressure. He sprawled on the bed in his plush room while the nurse carefully recorded his pressure.

He is one of about 20 normal control volunteers at National Institutes of Health (NIH) at Bethesda, Md. (suburb of Washington, D. C.). They serve as "measuring sticks" for developing treatments and preventatives for heart disease, cancer, mental health, arthritis and metabolic diseases.

These youths are getting in on a fascinating frontier of service our work. Results of some studies have no immediate or obvious value but are essential in the final result of therapeutics," he said.

* * *

Variety of Tests

IN THIS well-equipped Clinical Center, volunteers take part in a variety of research studies. For instance volunteer William Yoder

MENNONITE VOLUNTEERS William Yoder (foreground) of Meadville, Pa. and Dale Horst of Bareville, Pa. participate in a stress control test which may help scientists in their studies of mental health problems. Members of the research staff here are Dr. Roger McDonald and nurses Mary Bardeson (left) and Carolyn Smith. (National Institutes of Health Photo.)

Figure 1: A Mennonite Central Committee news service article describes the service program at NIH. Dale Horst, on the right in bed is quoted: "…One doesn't mind feeling uncomfortable a day or so considering how much patients are suffering…" According to another Mennonite volunteer: "Since Christ spent so much time healing the sick, it is only natural that we serve." Article: "Volunteers have important part in health research," [unknown source] [circa 1956] Bertsche collection. VANV: Vernacular Archive of Normal Volunteers, Countway Library of Medicine, digital collections.

This was the start of the NIH's Normal Volunteer Patient Program. Its unprecedented character can be easy to miss: Healthy human subjects of medical experiments no longer had a debt to the state, but could be everyday citizens. In 1959, NIH science-administrators signed the agency's first contract with a college, which served as a source organization, and then, in the 1960s, with labour unions, civic organizations, and (more traditionally) the federal bureau of prisons—all the while renewing its original contracts with the churches.

At the origin of the Normal Volunteer Patient Program were young people who volunteered through their Anabaptist churches, which had a tradition of encouraging members to witness the teachings of Jesus through voluntary service. The idea of getting credit (much less compensation) for their voluntary service undermined the spirit of humility and the aim of self-sacrifice that voluntary service was supposed to accomplish.

One article in a church newsletter written by a Normal explained: "Progress in medical research requires many things from many people. At the Clinical Center of the National Institutes of Health in Bethesda Maryland, Brethren Volunteer Service sponsors a group of people who wish to do something positive to help bring the blessing of good health to others. The participants literally give themselves to science, and this sharing of themselves to further research is their way of [relieving] human need."[4] The church, as much as the hospital, was the source of the institutional logic that organized their experience of the Clinical Center. I take seriously the institutional logic of 'voluntary service', which celebrated self-abnegation—or at least promoted a lack of concern with credit, profit and worldly rewards (Figure 2).

Figure 2: The Brethren Service News describes church members' 'voluntary service' in the summer of 1961: "Normal control patients are vitally important to further the betterment of mankind in the conquest of disease." Brethren Service News, vol 16 no 9, Nov 1961. Judd Collection. VANV: Vernacular Archive of Normal Volunteers, Countway Library of Medicine, digital collections.

4 1962 April 14. Article in the newsletter *Gospel Messenger*. "BVSers Serve in the interest of science and for the good of humanity in the normal volunteer program of the NIH Clinical Center" by Brenda Schnepp.

The conscience of these Normals became a collective conscience through the bureaucratic organization of the Anabaptist churches. The Mennonite Central Committee and the Church of the Brethren were formal organizations empowered to sign legal contracts and had bank accounts for financial transactions. The creation of this first formal exchange of civilians for money was predicated on the churches' bureaucratic organization. This is not to say that their system was necessarily just, but simply that people recognized—and in many cases seemed to appreciate and actively uphold—a system of authority and submission. In short, their masters were freely chosen. To think otherwise is to commit the Marxist infraction of imposing present-day political sensibilities on circumstances in the past in the form of presumed exploitation and false consciousness.

For its part, the NIH was part of the US bureaucratic state. The tools of the state were designed to promote what today would be called transparency and privacy. In doing so, the NIH's bureaucracy intentionally concealed information, which simultaneously served the function of erasing personal accountability and individual credit. Consider the words of the Clinical Center's longtime director, Jack Masur. As "part of the research team", Masur told hospital staff that "the patient is contributing so much he should not be asked or encouraged to allow his picture to be taken or his individual story to be told". Masur believed that sick and healthy individuals were indispensable to research, and by the same logic they should get no credit for it. "If the study on that patient yielded information that helped others, it could be told in an impersonal scientific article or in a medical conference setting", one colleague observed. "In vain, writers for the public media would protest that big stories are told in terms of little people", this NIH insider continued. "Dr. Masur had a profound conviction that a patient has a moral right to privacy."[5] Lo, the face of privacy hides the tail of secrecy.

By the 1960s, many of the human subjects of postwar medical experiment at NIH were healthy White Christians living in relative privilege, and to insist on their status as victims like any other deflects questions about the cause of systematic differences among the subjects of medical experiment and about the organizational arrangements that made service as a normal control possible, even necessary. To obscure these intricacies is to risk diminishing the true horror of instances when obedience was not a choice, but was the only possible response, short of death or displacement, to a command enforced through violence. At the same moment that Anabaptists were under study at the NIH Clinical Center, Public Health Service scientists in Alabama were in the second decade of the Tuskegee Study of Untreated Syphilis in the Negro Male, even though the mass-manufacture of penicillin had been a landmark achievement of American wartime medicine. At the Clinical Center, it went without saying that 'normal controls'—people who functioned as a biomedical standard—were exclusively White middle-class people. In part, these circumstances could remain implicit because practices of racial exclusion were built into medical models of disease (Braun, 2014; Crenner, 2014; Jones, 1993; Proctor, 2000; Schiebinger, 2004). Moreover, and more important than abstract models and more consequential than distant debates, legal segregation in the postwar period was apparent in the immediate space of American hospitals, as well as in people's daily drives around Washington DC. The race of

5 Anon. "Dr. Jack Masur, a Giant in Size Gentle in Spirit—He Will Be Missed at NIH." *NIH Record*. March 18, 1968, vol XXI no 6 edition: 7.

Normals at the Clinical Center could be left unspoken not because discrimination was absent but because the place was predicated on it. This eventuality and its silence in the historical record are both functions of the bureaucracy, which shaped clinical knowledge production in the past and the possibilities of historical knowledge in the present day.

Bio-labour and bureaucracy: An agenda

Capitalism is successful in large part because it is flexible (Rajan, 2006). As a product of capitalism, bureaucracy shares this same trait. As a result, science within bureaucracies simultaneously creates and obscures the patterned and broad-scale consequences of formal knowledge—both good and ill (Cooper & Waldby, 2014; Epstein, 1996; Hecht, 2012; Heimer, 2012; Kowal et al., 2013; Reardon, 2004; TallBear, 2013). Taken together, studies of scientific practice and justice mark out a terrain of study of the ways in which social privilege is built into the practice of science, the mechanisms through which it operates, and the consequences beyond the laboratory. Building on Weber's theory of bureaucracy and the case of the NIH exchange for human subjects, I argue that clinical knowledge under capitalism has been made possible by the ability of bureaucracies to erase individual involvement. This observation is a complement, not a rebuttal, to scholars' efforts to rehabilitate historically and recognize politically the labour of previously invisible individuals. Weber's approach offers a compelling description of knowledge-making in late capitalism, a useful way to account for a wide range of unintuitive actions and experiences in market transactions and, importantly, a warrant for historians to read historical evidence that might otherwise appear to be contradictory as entirely compatible.

Where Marx was a revolutionary, Weber was a reformer and as such the politics that Weber's theory implies for the historian can feel like apathy. Yet Weber's interest in process and perspective suggests how we can tweak our way to greater justice; to create formal knowledge in order to point out the instability and incompleteness of formal knowledge itself. Weber's theory of bureaucracy raises important questions about how to interpret—and to create—the historical record:

- How can bureaucracy be used to better think about the process of bio-labour; and how can bio-labour refine theories of bureaucracy?
- What are the circumstances under which individuals derive power or lose power through processes to create invisibility that are endemic to bureaucracies?
- (How) should historians define the meaning of actions? Using actors' categories or analytic categories? In making these choices, what problems of evidence and interpretation do historians face?

- What are the political stakes in scholarly claims that actors participated in the erasure of their individual contributions to science? How do these processes contradict or substantiate current scholarship on credit and credibility, for example, in the history of authorship?
- As knowledge makers in the present day, do historians' conceptual frameworks afford some understandings of labour and obscure others? In what ways do our theories make legible particular conceptions of labour, while obscuring others?

As a starting point, I have offered the case of NIH's market for human subjects. There has been an insistence on violence, force, coercion, and exploitation as the exclusive action of capitalism. These are central functions of capitalism, no doubt, but they are not its exclusive mode. The defining feature of modern political economy is bureaucracy, and attention to its slow grinding gears can accommodate a fuller, intricate account that makes sense of seeming contradictions, like the happy human subjects I studied as historical actors and as objects of my own research, the apparently willing victims I worked not to find.

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Mechanical Subjectivity: Mass-Observation and the Scientific Citizen in Interwar Britain

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> The trained Observer is ideally a camera with no distortion. Mass-Observation has always assumed that its untrained observers would be subjective cameras, each with his or her own individual distortion. They tell us not what society is like, but what it looks like to them. (Madge and Harrisson, 1938: 66)

> I am a camera with its shutter open, quite passive, recording, not thinking. (Isherwood, 1940: 31)

Through the winter of 1936/7, readers of the letters pages in the left-leaning *New Statesman* were treated to the spectacle of a new social movement being born. 'Mass-Observation' was formed in a series of communications initiated by the educationalist Geoffrey Pyke, and taken up by poet and journalist Charles Madge, ornithologist-turned-anthropologist Tom Harrisson, and the artist, documentary film-maker and poet Humphrey Jennings. Pyke had originally called for an "anthropological study of our own civilization" (Pyke, 1936). By January 30th, 1937, Harrisson, Jennings and Madge responded by entreating the *New Statesman*'s readers to join a collective inquiry into a baffling list of subjects:

> Behaviour of people at war memorials.
> Shouts and gestures of motorists.
> The aspidistra cult.
> Anthropology of football pools.
> Bathroom behaviour.
> Beards, armpits, eyebrows.
> Anti-semitism.
> Distribution, diffusion and significance of the dirty joke.
> Funerals and undertakers.
> Female taboos about eating.
> The private lives of midwives.[1]

But the joke was a serious one: those who offered to help were soon instructed, for the 12th of each month, to "describe briefly and factually the events of your day, giving times; report any conversations, if any, with different types of people" (Madge and Jennings, 1937: 350–1). Madge and Jennings collected the results of these 'Day Surveys' in London; they would culminate in an

1 Harrison, Jennings and Madge (1937: 155).

elaborate account of the coronation of King George VI on May 12th, 1937. In the northern town of Bolton, meanwhile, Tom Harrisson began a very different part of the Mass-Observation project, in which he and other locals he had enlisted 'went native', immersing themselves in the life of the town, and gradually gathering material for a series of books on working-class life, most of which never appeared.

There is now an extensive literature on Mass-Observation (MO)—yet only a handful of studies that discuss MO as an episode in the history of science, and none that frame it in terms of the visibility of its members.[2] Yet MO's contemporaries judged it to be first and foremost a scientific endeavour, praising and criticizing it on those grounds. The Mass-Observers themselves, too, thought that they were contributing to a sociological study.[3] Of the three founding members, Harrisson and Madge saw it in this way—that Jennings did not is an important fact in its own right, to which I return below.

The idea underlying the project was "to make the invisible forces [of custom and agreement] visible" (Harrison and Madge, 1938: 8). In other words, MO was conceived as an instrument of social consciousness: How does a society come to know itself? Does the democratic system reflect the real wishes of the people? And so on. In this respect, MO was just one of a number of interwar studies that aimed precisely to bring the varieties of everyday life and labour into the open. The high-priest of this movement was H.G. Wells, whose visionary machine the "World Brain" would do just this (Rayward, 1999). As the double-edged term 'observation' implies, MO was scientific insofar as it drew on the techniques of fieldwork and promised to bring hidden elements of society to light—the *practice of observation* and the *making of observations*.

The 'scientific attitude'

For the many scientists who embraced mass publishing in the interwar years, communicating the findings of research was less important than imparting the 'scientific attitude' to the general public. Though somewhat nebulous, this attitude was consistently said to entail self-discipline, disinterestedness and the communal pursuit of truth.[4] The political project of much popular science writing is easier to pin down: Many of the era's most successful works were written by the group called by historian Gary Werskey the 'Visible College': Left-wing British scientists

2 For an excellent introduction to MO see (Hinton, 2013). Hinton's focus is on Harrisson, Madge (Jennings is largely ignored) and the wider circle of 'Mass Observers', so he is inevitably concerned with recovering lost voices, and does an excellent job in this respect. However, as evidenced by his short treatment of Jennings, he is not overly concerned with aesthetics, and the category of 'observation' itself does not play a major part in the book. So even here we have an account that the misses the richness of MO's practices of seeing and occluding. Kohlmann (2014) deals closely with the aesthetics of MO, specifically *May the Twelfth*. Macdonald (2002) gives a fascinating account of strategies of observation in the period, with brief mention of MO.

3 The 'science' of MO has typically been evaluated rather than analyzed or set in its historical context. For a summary see (Pollen, 2013).

4 The canonical statement was made a little later, during World War II, by C.H. Waddington, in his *The Scientific Attitude* (1948 [1941]). For an early critique that did (and does) much to solidify the concept, see (Russell, 1931).

who saw themselves as the inheritors of the spirit of the early Royal Society, and as agents for a new socially conscious interpretation of science's 'social relations' (Werskey, 1988 [1978]).

Although professing novelty, this dual conception—of science as a virtuous and socially responsible practice—was just as much a Victorian and Edwardian as it was an interwar phenomenon. For example, in his influential 1892 book *The Grammar of Science*, Karl Pearson had argued that the scientist's necessary "self-elimination in his judgments" was "an essential of good citizenship" (Pearson, 1900 [1892]: 6–7). Indeed this particular virtue, of self-elimination—in particular in the conduct of experiment—has a lineage that goes back all the way to the original *In*visible College, which demanded "docility" of temperament (Yeo, 2014: xvi).

But if scientific restraint and good character had long been linked, writers of the 1930s added a number of truly new ideals. First, to varying degrees Communism was seen as a model for, or necessary counterpart of, science. "In its endeavour," J.D. Bernal wrote in 1939, "science *is* communism" (Bernal, 1940 [1939]: 415). Second, and more practically, the reform of science, not least as advocated in Bernal's *Social Function*, was part of the broader 'politics of planning' movement that dominated British political discourse through the 1930s (Ritschel, 1997). Third, in addition to being lectured to via the BBC and mass-market paperbacks, the modern scientific citizen was effectively brought into being through a range of highly co-ordinated data-gathering exercises. Public intellectuals and enterprising scientists enlisted the public in bird-watching, plane-spotting, collective dream-recording and in meteorological and anthropological surveys—but the information they provided was secondary to the instillation of the scientific character through "corporate observation", a term implying communal effort and explicitly drawing on the language of planning (Macdonald, 2002).

While these three tendencies give a coherent outline to one version of the popular science project, they also generate some paradoxes: Corporatism in planning was associated with the self-organization of capitalist interest groups, and yet this served as a model for the socialist interpretation of science; the Left was increasingly unified through the 1930s in its opposition to fascism, and yet the 'scientific attitude' was said to be one in which a form of technocratic totalitarianism was welcome; planning was derived from economics and was primarily concerned with industrial organization, and yet scientific planners saw social problems in biological terms—the distinctions between industrial organization, technocratic governance, environmental management and human ecology were in flux.

Corporate observations

The mix of professional identities in early MO (painter, ornithologist, poet, film-maker, anthropologist…) has often led commentators to portray it as a doomed enterprise—a "rag-bag" of ideologies, aesthetic doctrines, literary and social theories.[5] Yet through the cultivation of the scientifically-minded *observer*—the modern citizen submissive to the demands of science—MO

5 For "rag-bag" see Cunningham (1989: 334).

appears not only cogent, but also typical of a range of interwar projects in all but the scale of its ambitions. We have seen the scientific heritage of the ideal of 'self-elimination', but this was brought to bear on other ways of knowing, for example through the work of the literary critic I.A. Richards, whose 1926 *Science and Poetry* praised T.S. Eliot for the "complete severance between his poetry and *all* beliefs" (Richards, 1926: 64–5).[6] Eliot himself, in his 1919 essay 'Tradition and the Individual Talent,' had argued that creativity was rooted in the host of predecessors who lived "immortally" in new works. Drawing on these ideas MO sought at once to denigrate the role of individual creativity in the arts while simultaneously bringing the scientific citizen into being.

Where MO *did* equivocate was over the instillation of these virtues in its observers *versus* the scientific value of their reports. But, to MO's founders, this problem was trivial when set against the burdensome administrative task of managing observers, and the intoxicating possibility of a new kind of social organization. MO was to be nothing less than the basis of a native, anti-fascist, social democratic unity, recording and preserving community traditions while (and thereby) helping eliminating superstition.

In practice, as outlined above, MO constituted a series of intensive studies conducted by Harrisson's team in Bolton, and the management of a 'National Panel' by Madge in London. Together these two parts of MO combined in the accumulation of 'corporate observations', a term first used in a distinctly Pearsonian manner by the ornithologist and planning advocate Max Nicholson. For Nicholson, corporate observations like those generated by MO combined three key 'essentials'. First: "Concentration of aim. Instead of every man working haphazard at whatever strikes his fancy the team of observers agrees to concentrate a large part of its attention on a problem or set of problems which are tackled by the light of a long-term plan." Second: "Expert direction. The formation of a team postulates leadership." Third: "Training of observers. The idea of corporate observation demands the setting of common standards much higher than the common denominator of the observers joining in the work."[7]

Arranging observations, organizing observers

MO's corporate observations were first brought together in the book *May the Twelfth*, which presented the material gathered for the coronation of King George VI on that date in 1937 (Madge and Jennings, 1937). Published that September, *May the Twelfth* drew almost entirely on the London branch of MO overseen by Madge and Jennings. Both men were active supporters of and contributors to surrealism, and although it put them at odds with the hard-headed

6 For a detailed discussion of Richards' and Eliot's position on the question of 'belief' see Constable (1990).

7 E.M. Nicholson, 'A Scheme of Corporate Observation', British Trust for Ornithology Archives, Oxford Bird Census, Box A/B2. Thanks to Helen Macdonald for passing on this information. On 'corporate observations' see Macdonald (2002) and Toogood (2010).

Harrisson, they saw MO as both a social and a literary exercise. Drawing on the notion of literature *as* social theory, and on literary creativity as an impersonal act, they argued that observation reports would be just as much social science as they would be 'popular poetry':[8]

> [the statements of Mass Observers] produce a poetry which is not, as at present, restricted to a handful of esoteric performers. The immediate effect of Mass-Observation is to de-value considerably the status of the 'poet'. It makes the term 'poet' apply, not to his performance, but to his profession, like 'footballer'. (Madge, 1937: 3)

Clearly then, effacement was not just a virtue for budding scientists, but was also central to the concerns of modernist writers in the 1930s. The documentary turn in literature is the broad context for MO's move—Christopher Isherwood's famous line, quoted above in my epigraph, is its most eloquent (not to mention ambiguous) statement.

Jennings compiled the bulk of the book, organizing the material gathered on the day of the coronation itself. Like Isherwood, Jennings employed the metaphor of the camera to describe his approach, which combined multiple observation techniques (survey, participant observation, diary report) in order to achieve multiple "kinds of focus": "Close-up and long shot, detail and ensemble" (Madge and Jennings, 1937: 90).

Strikingly, but in line with the theory of literature-as-self-effacement, Jennings not only edited the text but also participated in the observations themselves. He is present in the text as 'CM.1', first amongst the 'Mobile Squad' of Observers chosen from MO's inner circle. The dual role of editor-Observer gave Jennings enormous freedom: He could use his own material to fill in gaps that he perceived in the other reports. For Jennings the day was characterized by a sense of confusion, of never quite seeing the main event, of detritus and waste paper, of strange noises and comic interjections. Jennings' 'camera' moves freely amongst the celebrations. There are no individuals in his frame—but nor is there simply an undifferentiated mass. Rather there are a series of events, an invisible king, and a visible and highly variegated crowd. Jennings' text takes its reader through a sprawling survey, reaching out from London to the corners of Britain and back further into the unconscious of the dreaming masses, at which point the visual metaphors break down and the individual Observers re-assert themselves as if waking from a reverie. This is corporate observation as high literature (Figure 1a).[9]

Madge's section, meanwhile, tucked away at the end of the book, was intended as a point of comparison with the heightened emotions of the coronation. Entitled 'Normal Day Survey', it presents results from the same set of Observers but on ordinary days unconnected with the ceremony. Where Jennings presented an organic crowd, Madge gives us individuals, with their reports given separately. For Jennings, individual Observers were referred to by codes, only occasionally supplemented by gender, age, location, occupation. For Madge, the background of the individual is given first: Age, gender, marital status, politics and religion. These are then

8 On literature as social theory see Lepenies (1988); on 'popular poetry' see Hubble (2006: 61ff.).

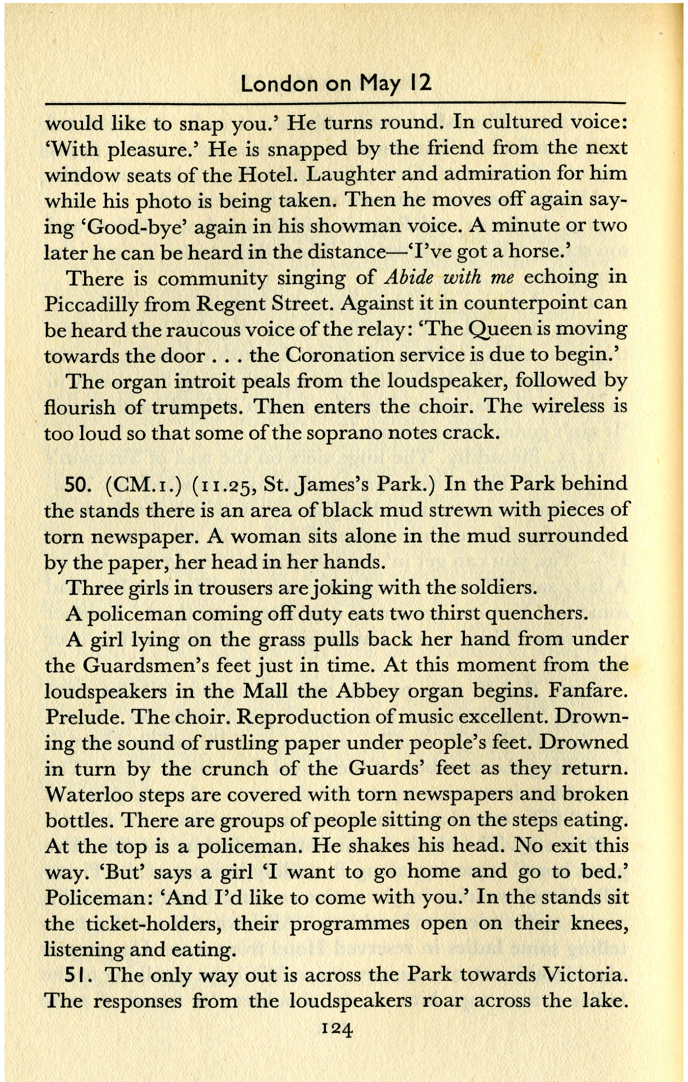
9 For a full analysis of Jennings' auto-editorial work in *May the Twelfth*, see Kohlmann (2014: Chapter 4).

followed by an account of activities on March 12 (the 'normal day') (Figure 1b). Madge instructed his Observers that "however ordinary the events may seem to you, they are of interest in this inquiry", and told them to "keep your feelings out [of your account of health, weather, local events, and the day itself]. Then describe your feelings during the day, if possible, in a final section" (Madge and Jennings, 1937: 351).

Although no Observers are named in *May the Twelfth*, it is possible to tally up the day surveys—which are kept in more or less their original form at the Mass Observation Archive (Sussex)—with the final published version. For Jennings this serves to show just how careful he was in selecting text that would present an image of the day without allowing the Observers' own opinions or judgments to emerge. Again, Jennings shows himself to have been concerned not with faithful reporting of the Observers' work but with providing a narrative coherent in its own right. Madge is far less subtle as an editor, and yet the archive shows him to have been actively involved in the organization and disciplining of his Observers—that is, in the expert management of corporate observations.

One of the more controversial aspects of MO, and in particular *May the Twelfth*, was the introduction by Madge of a classificatory scheme of 'three social areas' used to organize the observation statements in his section. The three areas correspond to (1) the Observer's close acquaintances, (2) people with whom the Observer might meet, (3) public figures and other people the Observer knows of and has opinions about but with whom a meeting is unlikely or impossible (Figure 2). The niceties and criticisms of the scheme do not concern me here—others have taken it to be either an important contribution to social theory or a negligible, even foolish piece of pseudo-science, but this is to misunderstand the importance of the device.[10] Perhaps Madge did think of it as a kind of sociological skeleton key, which would provide access to the structure of British society; perhaps, too, it turned out to be an ill-conceived analytical tool. But it was also a means of providing order to the digressive and confusing reports that MO received, and it had the effect, for good or ill, of ordering the daily lives of the Observers themselves as they attempted to provide useful information to Madge and his colleagues.

10 For a positive assessment see Hubble, 2006: 128ff.); for a recent critique see (Hinton, 2013: 71ff., and in particular p. 72, fn. 46).



London on May 12

would like to snap you.' He turns round. In cultured voice: 'With pleasure.' He is snapped by the friend from the next window seats of the Hotel. Laughter and admiration for him while his photo is being taken. Then he moves off again saying 'Good-bye' again in his showman voice. A minute or two later he can be heard in the distance—'I've got a horse.'

There is community singing of *Abide with me* echoing in Piccadilly from Regent Street. Against it in counterpoint can be heard the raucous voice of the relay: 'The Queen is moving towards the door . . . the Coronation service is due to begin.'

The organ introit peals from the loudspeaker, followed by flourish of trumpets. Then enters the choir. The wireless is too loud so that some of the soprano notes crack.

50. (CM.1.) (11.25, St. James's Park.) In the Park behind the stands there is an area of black mud strewn with pieces of torn newspaper. A woman sits alone in the mud surrounded by the paper, her head in her hands.

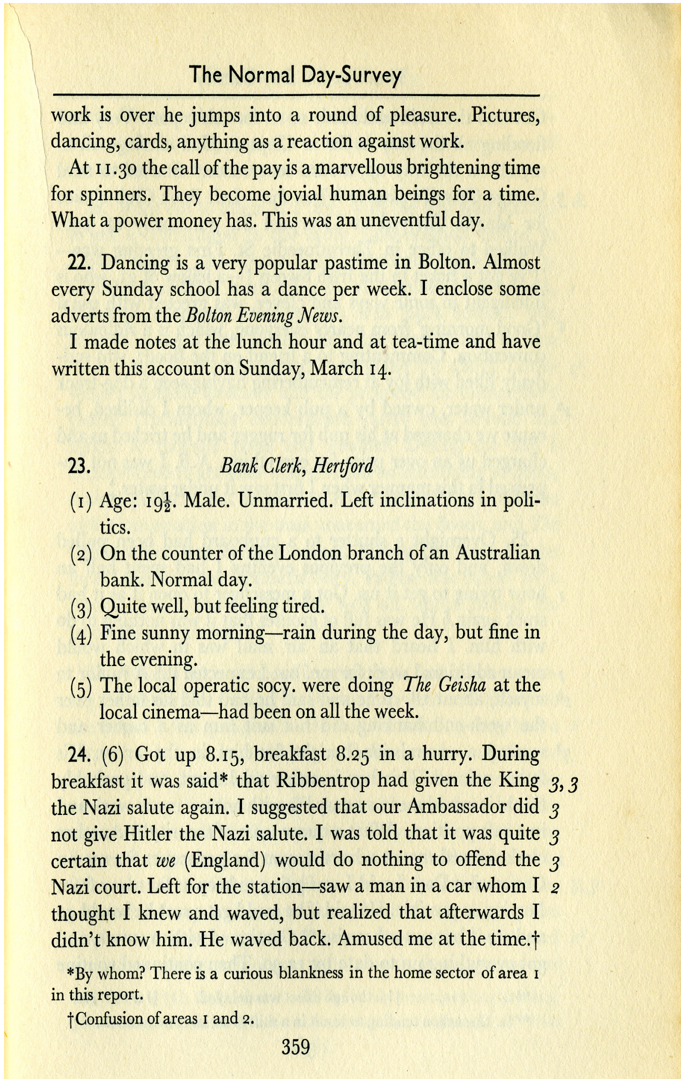
Three girls in trousers are joking with the soldiers.

A policeman coming off duty eats two thirst quenchers.

A girl lying on the grass pulls back her hand from under the Guardsmen's feet just in time. At this moment from the loudspeakers in the Mall the Abbey organ begins. Fanfare. Prelude. The choir. Reproduction of music excellent. Drowning the sound of rustling paper under people's feet. Drowned in turn by the crunch of the Guards' feet as they return. Waterloo steps are covered with torn newspapers and broken bottles. There are groups of people sitting on the steps eating. At the top is a policeman. He shakes his head. No exit this way. 'But' says a girl 'I want to go home and go to bed.' Policeman: 'And I'd like to come with you.' In the stands sit the ticket-holders, their programmes open on their knees, listening and eating.

51. The only way out is across the Park towards Victoria. The responses from the loudspeakers roar across the lake.

124



The Normal Day-Survey

work is over he jumps into a round of pleasure. Pictures, dancing, cards, anything as a reaction against work.

At 11.30 the call of the pay is a marvellous brightening time for spinners. They become jovial human beings for a time. What a power money has. This was an uneventful day.

22. Dancing is a very popular pastime in Bolton. Almost every Sunday school has a dance per week. I enclose some adverts from the *Bolton Evening News*.

I made notes at the lunch hour and at tea-time and have written this account on Sunday, March 14.

23. *Bank Clerk, Hertford*

(1) Age: 19½. Male. Unmarried. Left inclinations in politics.

(2) On the counter of the London branch of an Australian bank. Normal day.

(3) Quite well, but feeling tired.

(4) Fine sunny morning—rain during the day, but fine in the evening.

(5) The local operatic socy. were doing *The Geisha* at the local cinema—had been on all the week.

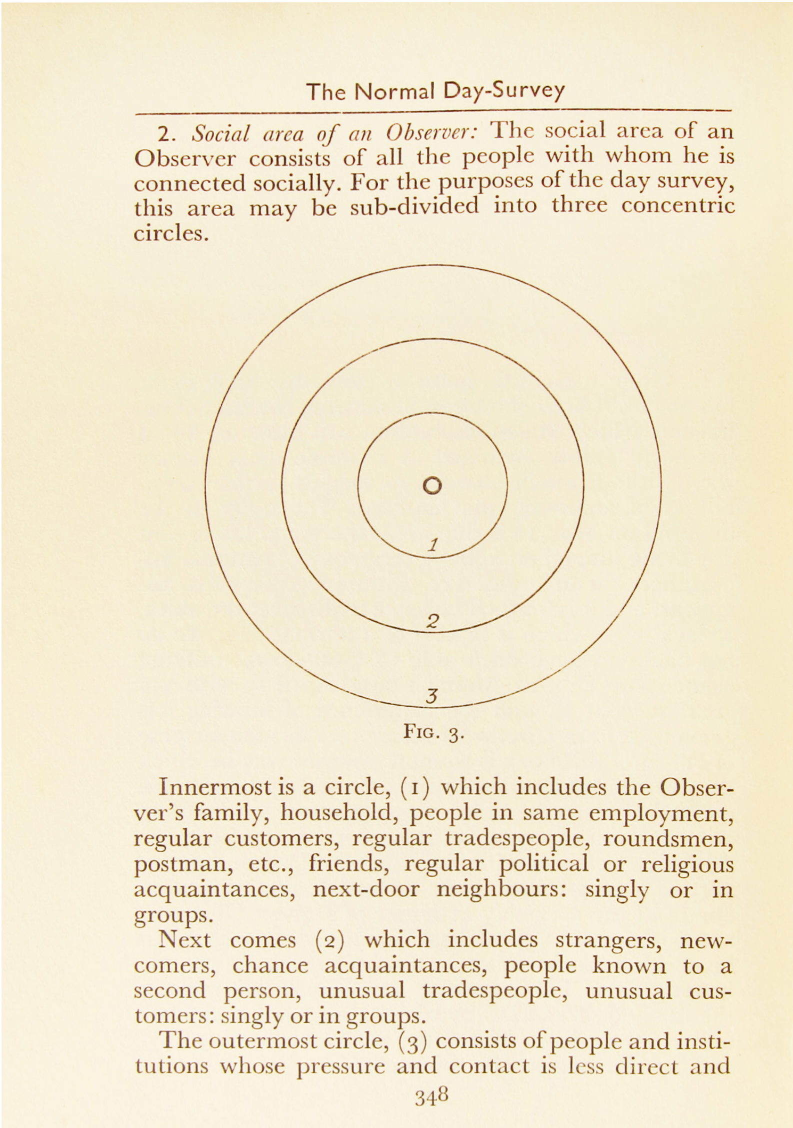
24. (6) Got up 8.15, breakfast 8.25 in a hurry. During
breakfast it was said* that Ribbentrop had given the King *3, 3*
the Nazi salute again. I suggested that our Ambassador did *3*
not give Hitler the Nazi salute. I was told that it was quite *3*
certain that *we* (England) would do nothing to offend the *3*
Nazi court. Left for the station—saw a man in a car whom I *2*
thought I knew and waved, but realized that afterwards I
didn't know him. He waved back. Amused me at the time.†

*By whom? There is a curious blankness in the home sector of area 1 in this report.

†Confusion of areas 1 and 2.

359

Figures 1a and 1b: Sections of *May the Twelfth* compiled by Jennings (left) and Madge (right). Here Jennings surreptitiously presents his own observations ("CM.1"). Where Jennings is concerned to create an impression of the day by juxtaposing reports, Madge concentrates on the demographic details of his Observers, presents them singly and deploys his notation system in the margins. Reproduced with permission of Curtis Brown Group Ltd, London on behalf of The Trustees of the Mass Observation Archive.



The Normal Day-Survey

2. *Social area of an Observer:* The social area of an Observer consists of all the people with whom he is connected socially. For the purposes of the day survey, this area may be sub-divided into three concentric circles.

FIG. 3.

Innermost is a circle, (1) which includes the Observer's family, household, people in same employment, regular customers, regular tradespeople, roundsmen, postman, etc., friends, regular political or religious acquaintances, next-door neighbours: singly or in groups.

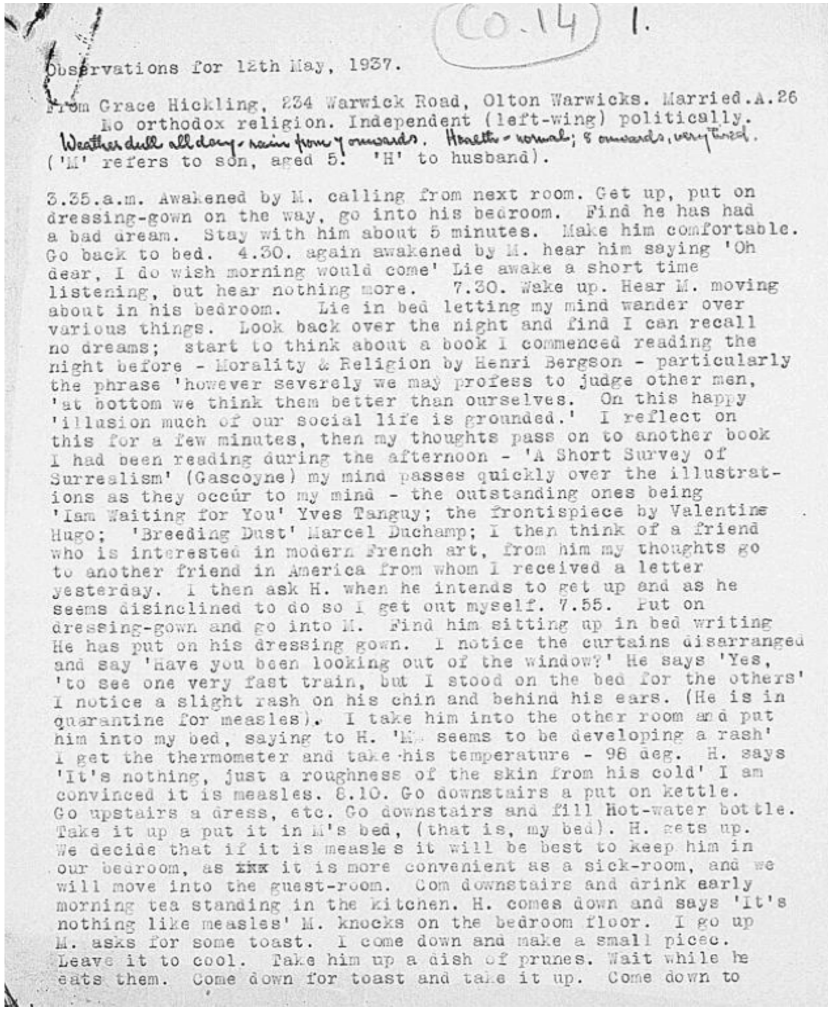
Next comes (2) which includes strangers, newcomers, chance acquaintances, people known to a second person, unusual tradespeople, unusual customers: singly or in groups.

The outermost circle, (3) consists of people and institutions whose pressure and contact is less direct and

348

Figure 2: Diagram representing Madge's three-area system of notation for his section of *May the Twelfth*, which was subsequently used by Observers in ordering their reports. Reproduced with permission of Curtis Brown Group Ltd, London on behalf of The Trustees of the Mass Observation Archive.

In *May the Twelfth,* the analytic system was appended by Madge to the reports by means of a marginal notation. But for the first surveys after *May the Twelfth* Madge attempted to shift the work to the Observers themselves, instructing them to send reports under their own Observer number and divide their observations for June 12 not by activity but by interaction as defined by the three-area system. This can be seen most clearly by comparing observation reports by the same person before and after the implementation of Madge's system (Figures 3a and b).

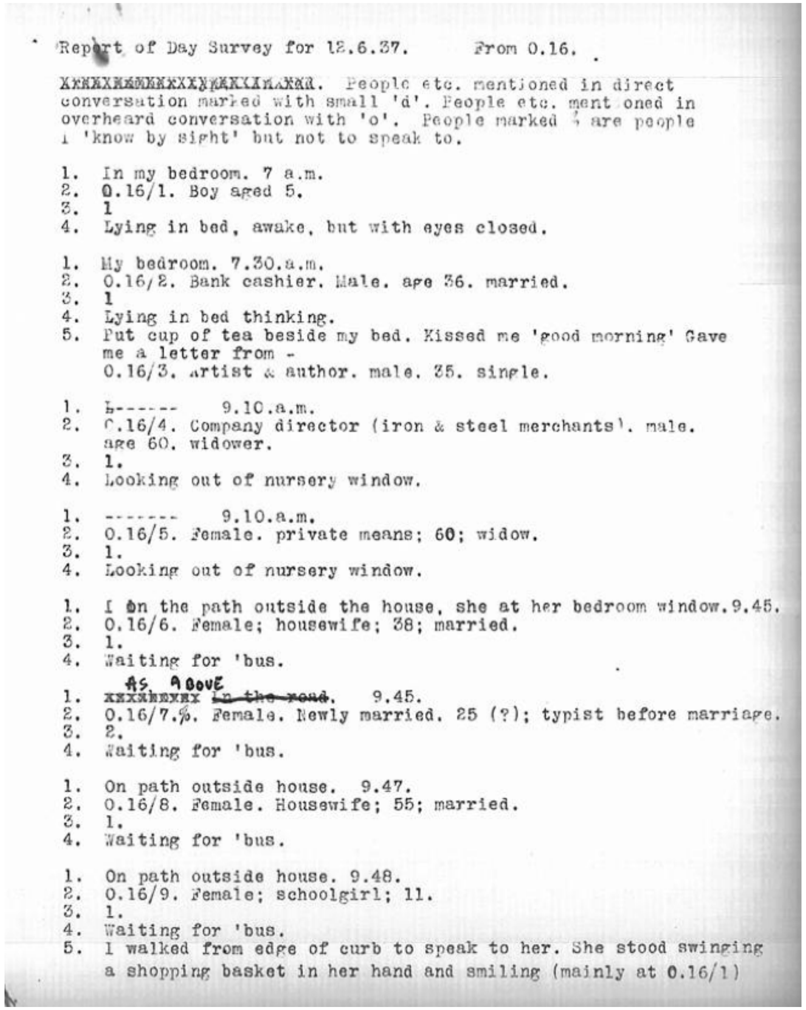


(CO.14) 1.

Observations for 12th May, 1937.

From Grace Hickling, 234 Warwick Road, Olton Warwicks. Married.A.26
No orthodox religion. Independent (left-wing) politically.
Weather dull all day, rain from 7 onwards. Health - normal; 8 onwards, very tired.
('M' refers to son, aged 5. 'H' to husband).

3.35.a.m. Awakened by M. calling from next room. Get up, put on dressing-gown on the way, go into his bedroom. Find he has had a bad dream. Stay with him about 5 minutes. Make him comfortable. Go back to bed. 4.30. again awakened by M. hear him saying 'Oh dear, I do wish morning would come' Lie awake a short time listening, but hear nothing more. 7.30. Wake up. Hear M. moving about in his bedroom. Lie in bed letting my mind wander over various things. Look back over the night and find I can recall no dreams; start to think about a book I commenced reading the night before - Morality & Religion by Henri Bergson - particularly the phrase 'however severely we may profess to judge other men, 'at bottom we think them better than ourselves. On this happy 'illusion much of our social life is grounded.' I reflect on this for a few minutes, then my thoughts pass on to another book I had been reading during the afternoon - 'A Short Survey of Surrealism' (Gascoyne) my mind passes quickly over the illustrations as they occur to my mind - the outstanding ones being 'Iam Waiting for You' Yves Tanguy; the frontispiece by Valentine Hugo; 'Breeding Dust' Marcel Duchamp; I then think of a friend who is interested in modern French art, from him my thoughts go to another friend in America from whom I received a letter yesterday. I then ask H. when he intends to get up and as he seems disinclined to do so I get out myself. 7.55. Put on dressing-gown and go into M. Find him sitting up in bed writing He has put on his dressing gown. I notice the curtains disarranged and say 'Have you been looking out of the window?' He says 'Yes, 'to see one very fast train, but I stood on the bed for the others' I notice a slight rash on his chin and behind his ears. (He is in quarantine for measles). I take him into the other room and put him into my bed, saying to H. 'M. seems to be developing a rash' I get the thermometer and take his temperature - 98 deg. H. says 'It's nothing, just a roughness of the skin from his cold' I am convinced it is measles. 8.10. Go downstairs a put on kettle. Go upstairs a dress, etc. Go downstairs and fill Hot-water bottle. Take it up a put it in M's bed, (that is, my bed). H. gets up. We decide that if it is measles it will be best to keep him in our bedroom, as ~~xxx~~ it is more convenient as a sick-room, and we will move into the guest-room. Com downstairs and drink early morning tea standing in the kitchen. H. comes down and says 'It's nothing like measles' M. knocks on the bedroom floor. I go up M. asks for some toast. I come down and make a small piece. Leave it to cool. Take him up a dish of prunes. Wait while he eats them. Come down for toast and take it up. Come down to



Report of Day Survey for 12.6.37. From 0.16.

XXXXXXXXXXXXXXXXXXXX. People etc. mentioned in direct
conversation marked with small 'd'. People etc. mentioned in
overheard conversation with 'o'. People marked ? are people
I 'know by sight' but not to speak to.

1. In my bedroom. 7 a.m.
2. 0.16/1. Boy aged 5.
3. 1
4. Lying in bed, awake, but with eyes closed.

1. My bedroom. 7.30.a.m.
2. 0.16/2. Bank cashier. Male. age 36. married.
3. 1
4. Lying in bed thinking.
5. Put cup of tea beside my bed. Kissed me 'good morning' Gave
me a letter from -
0.16/3. artist & author. male. 35. single.

1. L------ 9.10.a.m.
2. 0.16/4. Company director (iron & steel merchants). male.
age 60. widower.
3. 1.
4. Looking out of nursery window.

1. ------- 9.10.a.m.
2. 0.16/5. Female. private means; 60; widow.
3. 1.
4. Looking out of nursery window.

1. I on the path outside the house, she at her bedroom window.9.45.
2. 0.16/6. Female; housewife; 38; married.
3. 1.
4. Waiting for 'bus.

1. AS ABOVE ~~xxxxxxxxx in the road~~. 9.45.
2. 0.16/7.%. Female. Newly married. 25 (?); typist before marriage.
3. 2.
4. Waiting for 'bus.

1. On path outside house. 9.47.
2. 0.16/8. Female. Housewife; 55; married.
3. 1.
4. Waiting for 'bus.

1. On path outside house. 9.48.
2. 0.16/9. Female; schoolgirl; 11.
3. 1.
4. Waiting for 'bus.
5. I walked from edge of curb to speak to her. She stood swinging
a shopping basket in her hand and smiling (mainly at 0.16/1)

Figures 3a and b: Comparison of two reports by Observer 80, one dated 12 May 1937, the other dated 12 June 1937 and showing her faithful use of Madge's classificatory system. Mass-Observation Archive, University of Sussex, DS80. Reproduced with permission of Curtis Brown Group Ltd, London on behalf of The Trustees of the Mass Observation Archive.

If we take just two of the Observers whose reports had been used by Madge in his 'Normal Day Survey' we can see the effect this change had on them. For example, a 26-year-old schoolmaster from Northern Ireland ('Observer 467'), whose reports had been used by both Jennings and Madge, wrote following the June 12th survey that it was:

> Easily the hardest yet. When a meeting took place, I found myself time and again carried away by the ordinary rules of intercourse, and forgetting to record in my mind what was happening. The result was that afterwards I was able to remember what was said and the atmosphere, but never what was done.[11]

A housewife from Olton, Warwickshire ('Observer 80'), had a similar experience:

> This has been the most exhausting survey I have made. I typed part of it on Sunday afternoon (after making three false starts) and half way through felt so exhausted (mentally) that I had to go to bed for two hours.[12]

For the next month, presumably in response to the overwork of the Observers, Madge requested information only on those contacts in 'Area 2', and this duly presented a more manageable task. But unruly Observers were perhaps even more damaging than overworked Observers. The two just quoted complained that the system seemed at odds with their experiences: At the heart of Mass-Observation's technique was the overheard conversation, and the Observers found that they were being asked to observe in a way that specifically precluded recording conversations. The Northern Irish schoolmaster (Observer 467) also worried about the numbering system: "If every person met were to be numbered, surely it is going to make a very cumbersome form of reference?"[13] And why, he asked, give anonymous codes to people in Area 3, who were defined by their being known only through their name?

Meanwhile Observer 80, the housewife from Olton, found that there was no place in the scheme for people with whom she had only a corresponding relationship—and note that this is precisely the relationship she had with Mass-Observation itself (specifically Madge).

Although we might simply find this an amusing aside, it is also indicative of MO's inability to factor in its own relationship with the Observers, who often included reports of the difficulties they had had observing, hoping these were of more interest than they in fact were. In response to Observer 80's complaint Madge requested a full list of Area 1 contacts, which were duly sent in, numbering some 145 individuals.

In addition to these strictures on the technique and form of self-reporting, Madge went further in his attempts to control the Observers, even trying to manage the interaction *between* members. The following notice ran in the April 1938 'Mass-Observation Bulletin' setting out the rules of conduct:

11 Mass-Observation Archive, University of Sussex (MOA), DS467, Day Survey for June 12th 1937.
12 MOA, DS80, Day Survey for June 12th 1937.
13 MOA, DS467, Day Survey for June 12th 1937.

THERE MUST BE A CLOSE LINK between the organisers and the scattered Observers. We want to avoid, however, the setting up of local groups, except in the case of local intensive surveys where Observers are working in a team—and even then Observers refrain from discussing their results or comparing notes until the work is finished. Otherwise, one set of results tends to influence another, and part of the value of Mass-Observation is lost. To establish closer contact, the organisers intend during the next few months to see personally as many active Observers as possible. As the movement has grown, it has been increasingly difficult to keep up personal contact by correspondence.

Figure 4: Excerpt from the April 1938 'Mass-Observation Bulletin'. MOA, University of Sussex, FR A8 (13). Reproduced with permission of Curtis Brown Group Ltd, London on behalf of The Trustees of the MOA.

The technique of planning

At first we might wonder why a 'science of ourselves', as MO styled itself, disregarded the unruly behaviour of its Observers and sought to distance them from each other. Yet at the heart of corporate observation, implicit in the politics of planning, and openly called for in the 'Social Relations of Science' movement was the figure of the organizer—the "technician" to the dismissive Bertrand Russell (1931)—who would manage and control an enterprise only ostensibly devolved to its participants.

In MO, the question of how and why the Observers had become Observers in the first place was relegated to a line in one of their surveys, rather than becoming a thoroughgoing object of investigation in its own right—into, for instance, how the Observers behaved and responded, how their observations changed over time. Moreover the survey that MO *did* run, into why its members had joined, revealed that wishing "to take part in scientific work for its own sake" was the primary motivation—not the raising of their powers of observation or the invention of a new form of democracy (Madge and Harrisson, 1938: 67). The Mass-Observers took their role as scientific citizens seriously, even if MO itself did not.

Corporate observations were designed specifically to hide the expertise of the planners by making the labour of the participants visible. The metaphor of the camera, used frequently in MO's publications (and, as we saw at the outset, in other socially and politically conscious literature of the period), enabled MO's organizers to place a mechanical surrogate between themselves and the Observers, between the Observers and the world they saw. Explaining the editorial technique of *May the Twelfth*, Jennings had written of "close-up and long shot, detail and ensemble", referring not to any one group of Observers, but to the collective text that is, to complete the quote from Isherwood used above, "developed, carefully printed, fixed" (Jennings and Madge, 1937: 90; Isherwood, 1940: 31). The more closely managed the observations, the more useful they would be—in the end, for better or worse, to government and big business.[14] In MO we can see a kind of modernist Fabianism: The management of popular poetry and the 'anthropology of ourselves' pragmatically required a board of overseers, and the result was a carefully nurtured anti-fascist movement that matured into an information gathering service during the War.

In this MO was aligned with other modernist planning organizations: architects praised the virtue of stylistic, modernist 'anonymity'; visual artists were encouraged to sacrifice their pursuit of 'uniqueness' in favour of 'design'; Bernal and other members of the Social Relations of Science movement explained that, in a planned economy, scientists would have to embrace the 'freedom of necessity'. Though not (yet) a state concern, MO was pioneering in one dubious respect: it enacted what Bronislaw Malinowski astutely described as a "Nationwide Surveillance Network" (Harrisson and Madge, 1938: [81]ff.). Again, the technocratic 'scientific attitude' was in no way at odds with this. As C.H. Waddington wrote in the book of that title:

> The continual intrusion into what used to be considered the sacred privacy of the individual is again primarily a technological and only secondarily a political change. (Waddington, 1948 [1941]: 22)

Inspired by developments in mass publishing, documentary film, in radio and television broadcasting, and through the technique of corporate observations, MO sought to break down not just individual privacy but the notion of the individual *per se*.

MO has typically been read as a movement that started out with a blend of literature and social science, and ended with only the latter. This is true, but for the Observers the bigger problem was that MO changed from an organization that was specifically interested in observation for its own sake to an organization that instrumentalized those observations and turned its attention to market research, war-time opinion polling, statistical surveys and, as James Hinton emphasizes, the compilation of an authentic record of everyday life during WWII.

For all that MO was a science of visibility, of observation, of seeing and inscribing and bringing to wider legibility, it remained intentionally blind to the work that its Observers did in disciplining themselves, preparing their reports and even altering their behaviour. For writers like Hinton and the many other social historians who now use the Mass-Observation Archive, the recovery of the voices and opinions of people from a wide range of backgrounds on a bewildering array of topics is virtuous in and of itself—it is the means of creating an authentic historic record.[15] But the origins of MO itself were far messier than these straightforward narratives and archival reincarnations imply—not because MO was an unwieldy mix of literature and science, anthropology, social survey and journalism, but because it traded on the virtue of self-effacement while requiring Observers to act as 'subjective cameras' at the same time. This is the paradox of MO's

14 During the War MO was co-opted by the Ministry of Information; after the War it was run by an advertising agency.

15 Indeed Hinton goes as far as to say that MO was, "i*n its prime*," an organisation that "designed to construct an archive appropriate to the creation of a people's history of the [second world] war" (Hinton, 2013: vii, emphasis added).

peculiar 'mechanical subjectivity'. The Observers understood themselves to be learning how to observe—how to become modern, scientifically literate citizens. But in the end, MO abandoned these reforming 'corporate' ambitions in favour of the instrumentalized observations that characterise its work during and after World War II. The point is not that there is no such thing as a perfectly objective archival source with which to reconstruct the past. Rather it is that archives, far from being passive repositories of information, are active agents of self-formation.

Conclusions

Questions of the visibility and invisibility of labour in the human sciences, it seems to me, are always in fact questions of *sources*. As the Mass-Observers knew only too well, there are only indirect means of rendering visible everyday life, human labour, play, scientific work, artistic creativity. Rather than propose just one in a line of noble theories of human action, MO enacted the collection of material. That is, rather than engaging in classical sociology and attempting to solve the epistemological problems of the human sciences, MO engaged in a practice that would be so vast (in fact, all-encompassing) as to negate the desire for (and the possibility of) theory. Hence, for its founders, MO was "not a party, a religion or a philosophy, but an elementary piece of human organisation and adaptation" (Harrisson and Madge, 1937: 47–8). The various kinds of data used in the human sciences would, in MO, be superseded by the construction of a wholly new archive.

As our critical and historical interest turns from the succession of *ideas* about the human sciences to the range of *practices* they have employed, this kind of archive building will, I suspect, increasingly become the focus of attention. Reconstructing the way that the Mass-Observers were marshalled involves a laborious trudge through the Mass Observation Archive—a beautifully run institution that applies modern archival standards to a collection of papers that were badly mistreated from the very beginning (Hinton, 2013: 86–7). My intention here has not been to 'recover' the lost story of the Mass-Observers themselves—that would be to become entangled again in questions of reflexivity and objectivity. Rather, the archive itself is the means by which the interwar politics of visibility can be seen in action.

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Making Things Incommensurable

Josh Berson

Hubbub, The Hub at Wellcome Collection

In February 2013, I gave a talk to the Munich chapter of the Interaction Design Association. My theme was 'Circadian Selves'. The talk represented an early sketch for what became a big chunk of my forthcoming book on "instrumented life" (Berson, 2015). Things had been moving quickly. In October I'd come across the Valkee, a bright-light stimulation device designed to resemble an iPod. The Valkee led me to the burgeoning field of activity-tracking devices—the Nike Fuelband, the Jawbone Up. The same week, a friend mentioned in passing a movement called the Quantified Self. Four weeks later I was at a seminar in Berlin, holding up the Valkee as an exemplary artifact of the Anthropocene. Now, in February, I was talking about 'chrono-plasticity' and 'activity rhythms as a focal object of self-care' in front of a room full of people who designed things like bright-light therapy devices and accelerometer bracelets for a living.

Not quite full. Some weeks before the event the organizers wrote to say the local Quantified Self community—the first in Germany, with just one previous event to its credit—was interested in cosponsoring the event. In fact, the Quantified Self people had the venue, so their participation was essential. Would I mind?

At the talk, modest confusion arose as participants from the two camps, interaction designers on one side, self-trackers on the other, wondered who all these other people were. During the question-and-answer period, a difference in worldviews emerged. The designers had questions about historical developments in rhythms of activity and rest—the emergence of consolidated nighttime sleep, changes in diagnostic criteria for bipolar phenomena in kids. The Quantified Self contingent had questions of a different type: Would I switch on my lightbox so we could see how it glowed blue? How long did I use it each day? At what hour and at what brightness setting? Had the Valkee worked for me?

As I got to know people in the Quantified Self—those who gave show-and-tell talks at regular meetups, those who kept a blog detailing the devices they'd tried, those who simply came to meetups and listened—I found that for most people, the aims of the exercise were modest and practical: How can I stand up straighter, get in a little more walking each day, better avoid distraction when I'm working? Indeed, participants in the movement, and in cognate movements such as the one devoted to polyphasic sleeping (Berson, 2015: Chapter 6) are generally the first to frame their interests in practical terms, even when they draw inspiration from a loftier vision of revolutionizing everyday habits of bodily upkeep—spending the entire workday in 'flow state' or abolishing the habit of sleeping in an extended nighttime sleep episode.

It was not long before people started asking me what I thought of one device or another and which vital signs one should monitor to achieve 'flow state'. I felt uncomfortable responding, even when I had something useful to share: these questions ran orthogonal, if not counter, to the ones that interested me. The questions that interested me were epistemological: How does 'flow state' take form as an object of desire? How do we come to value certain rubrics of self-reflection over others? How do certain kinds of behavior modification come to seem virtuous? (On the unintended consequences of treating flow states, states of boundaryless immersion in a task, as aspirational states, see Schüll 2012.)

The bemusement I'd felt during the Q&A session that evening in Munich turned to disquiet and frustration. I wanted, I suppose, my interlocutors to see the devices they were so invested in something like the way I saw them: As boundary objects marking a shift in the register they—and the broader we for whom, like it or not, the release of wristwatches with built-in ulnar photoplethysmographic (approximately, heart rate) sensors has become a major news event—use to talk about how they care for their bodies, not to mention how they think others should care for *their* own bodies.

Rather than asking how the information provided by new sensor montages fit—or failed to fit—with other ways of experiencing one's presence in the world, some of the most enthusiastic self-trackers seemed content to let instrumentation define a new register of experience all its own. This new register bore the imprimatur of objectivity, of a theory-neutral 'ground truth,' the constellation of value-laden decisions implicated in the design of activity tracking devices and classifier algorithms having been rendered invisible by the private concerns that marketed them. More disturbing, this register traced out a world that ended at the edge of the individual's perisomatic space. The Quantified Self was a more focally autonomous self, a self that took on a more saturated hue, becoming more distinct from its surroundings, more self-like. It was, I came to realize about year after that first meeting, a distinctly *liberal* self, a self that was uniquely responsible for its fate. The best thing, perhaps the only thing that, say, government could do to improve the health of its citizens was to provide behavioral 'nudges,' immediate incentives to good behavior that served to correct fallible human beings' tendency to depreciate future returns on present investments too steeply. Indeed, the nudge model was taking off hand-in-hand with self-tracking, two aspects of a rising tide of *behavioral design* (Bennhold, 2013; Hendriks and Hansen, 2014).

Let me be clear: My concerns have little to do with quantification itself. You don't have to spend much time with the STS literature on self-tracking before you find yourself tripping over hand-wringing claims about the pernicious effects of 'reducing people to numbers'. As if the effort to operationalize high-dimensional aspects of human behavior tended, ineluctably, to exclusion, hierarchy, and all the other evils of our social world. As if simply to ask, say, "Is there a relationship between the speed of the music people are listening to and the degree to which their full-body movements become synchronized?", to take an example from one project I'm involved in at the moment, were to place oneself in the camp of those who seek to erase difference from our world. This is not my position. If anything, I think we need more of this kind of operationalization—a *critical* practice operationalization, one that recognizes just how difficult it is to characterize human action with precision and fidelity.

It is this critical spirit that is missing not just from so much contemporary cognitive science but from the new register of self-care embodied in the work of enthusiast movements such as the Quantified Self. It depends on a confidence about what I came to call *ontological commitment* (Berson, 2015: 55), that is, a willingness to commit to a particular model of the world, a set of indicators and a range of values for each of those indicators, and to exclude from consideration phenomena that are not captured by those indicators. Historians Fred Turner (2008) and Adrian Johns (2010) have documented how an epistemic stance characterized by a strong degree of comfort with ontological commitment became hegemonic in the community most closely associated with the explosion of computing technologies, body sensing among them, that underlie self-tracking enthusiast movements (cf. Coleman, 2012 on hacker liberalism). A world-remaking ethos, together with a strong degree of perceived control over the course of events in one's life and the world at large, has been central to this community since its inception.

The way body sensing technologies have been used to emphasise the agency of the self has to do not with tendencies immanent in the technologies themselves—thirty years before the debut of Fitbit and its ilk, researchers at the U.S. National Institute of Mental Health had demonstrated it was possible to incorporate body-borne accelerometers into a sophisticated and remarkably patient-centered research practice (Wehr et al., 1982). Nor is it for want of evidence that the greater part of what determines our chances in life is beyond the scope of individual behavior change or nudges (Costello et al. 2010). It is, rather the product of a worldview in which the coarse-grained operationalization of complex social phenomena is understood to be integral to social reform—and to progress more broadly. This is a worldview increasingly common in the social sciences (Xygalatas et al., 2011; Morin, 2015). It encourages a style of research that embraces epistemological foreclosure. What Morin writes in a breezy defense of null hypothesis significance testing could apply equally to body sensor montages as they are being used in a broad swath of well-received research: Measurement "is not its function. It serves to terminate certain disputes. Scholars (when they play the game honestly, which as we know is not always the case) agree in advance to give up on some hypotheses that fail to pass the test. Here again, the use of one crude and arbitrary standard of success (instead of many subtle ones) is instrumental: a tally should not have too many degrees of freedom, as any ambiguity in the tally may be used to save one's pet hypothesis and keep the dispute going" (Morin, 2015: 244).

At the same time as I was talking to self-trackers, I was also talking to people in the polyphasic sleeping community. Indeed, it was polyphasic sleepers, those who experiment with giving up sleeping in a consolidated nighttime sleep episode in favor of one of a variety of precisely timed naps spaced over the course of the twenty-four-hour day, whose self-experimentation spoke more directly to my original theme: Changes in the ecology of circadian time. And of course there is a certain overlap in worldview between the two communities. Many in the polyphasic sleeping community get interested in the practice as way to increase the time available to them in the day to 'be productive' and 'achieve their goals'. For my main interlocutor in the community, who had literally written the book on how to transition to the "nap-based lifestyle" (Puredoxyk, 2013), this was misguided:

> Josh: This came up on one of the polyphasic discussion lists just the other day: "I have a deadline, I have some play in my schedule, I've been meaning to try polypha-

> sic anyway, could I transition partway, just to give me the extra working hours I need for this project?" Do you get this a lot?
>
> Puredoxyk: I do. That's a really common question. I compare it, sloppily but validly I think, to "I don't have time to eat healthily; maybe if I switched to being vegan real quick it'd be easier?" Kind of, if you don't have time to do it the easy way, why would doing it a harder way fix that?"

What is at stake here is the way that techniques of fine-grained time-series capture of bodily activity have been used to erase body-to-body differences in the experience of phenomena such as 'activity' and 'rest'. What architect Laura Kurgan says of remote sensing is equally true of body sensing: "[W]e do not stand at a distance from these technologies, but are addressed by and embedded within them … Only through a certain intimacy with these technologies—an encounter with their opacities, their assumptions, their intended aims—can we begin to assess their full ethical and political stakes" (Kurgan 2013: 14). Not long after I started working on body sensing technologies, I had the opportunity to stage exactly this type of 'encounter with their opacities'.

About half a year after that first encounter with the Quantified Self, I was asked to join the pitch for a new grant being offered by the Wellcome Trust. The Hub Award aspired to inject the sex appeal of the tech startup world and the energy of user-centered design into research on health and wellness. Wellcome would construct a new space, the Hub, kind of a cross between a startup incubator and an institute for advanced study. They wanted to see proposals for projects that were too interdisciplinary to get funded through other grant programs. The would-be core investigators for Hubbub, the team I was recruited to, had a very specific request: Would I design a 'Quantified Self'–style study, something that used consumer-grade self-tracking technology to generate new kinds of evidence about people's habits of busyness and rest.

With the help of the design studio LUSTlab, I set out to design the opposite of a 'Quantified Self' study, a study that renders body sensor data, along with whatever kind of self-report information you can squeeze out of a mobile phone notification, as idiosyncratic and incommensurable as possible and provide an opportunity for study participants to elaborate their own epistemologies of busyness and rest. If the interpellation by sensor technology that Kurgan describes is inevitable, we can at least use it as an opportunity to broaden the range of actors who have a say in the design of sensor-based indicators—and, in fact, to make visible the role of individual participants in small-scale pilot studies in shaping measures that go on to assume a life of their own in larger-scale studies.

At this writing (October 2015) we've completed one pilot study and we're in the process of planning a second (Figures 1 and 2). In September we had the chance to exhibit visualizations from the first pilot at a one-night show at Wellcome Collection. The design of these visualizations was strongly influenced by the conversation at the Invisible Labour workshop (Figure 3). In the explanatory text made available to gallery visitors we wrote,

> Often, self-tracking studies expect data to 'speak for itself'. Rarely do researchers ask how the tracking process affects the people being tracked. Rarer still do they ask participants how the self-tracking measures compare with their own understanding of what's being studied. This erasure of participants' 'ways of knowing' gets repeated in data visualization. Designers work to exclude extraneous detail—in the process they end up draining the events described of emotional coloring. The public is treated to eye-popping graphics that hide the messy debates about what the data mean—what relationship they bear to people's lived experience. We've tried to produce visualizations that tell a story about how participating in the study changed participants' experience of moving through space—and keep viewers in mind of the fact that tracking data offer but a thin and distorted slice of participants' lives. (Berson and Nieuwenhuizen, 2015)

Indeed, the installation we produced to summarize the results of this pilot features none of the characteristic indicia of data visualization—no timelines, no scatter plots, no heat maps. Instead it features the faces of the participants, recorded in video self-portraits in which they were asked to reflect on the study, overlaid with animated phase transition diagrams illustrating selected elements of their responses to automated prompts for self-reflection during the study.

I'm not holding my breath for the day when making research participation visible in this way is standard practice in data visualization. But it does suggest that sensor-supported studies need not contribute to epistemological foreclosure and the erasure of bodily difference.

Figure 1: Pilot participants reviewing data at the close of the study, June 2015.

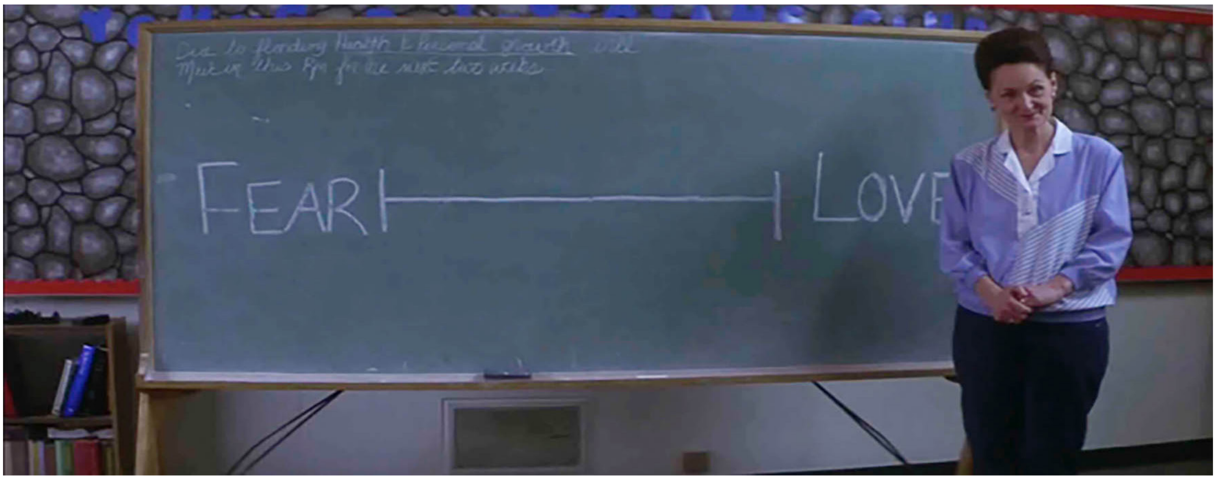

Figure 2: “You can’t just lump things into two categories … life isn’t that simple.” Still from the “Fear/Love Lifeline” scene from *Donnie Darko* (Kelly 2001), which we used in the initial discussion with pilot participants to illustrate the inanity of Likert scales.

Figure 3: Still from installation video used to communicate the first pilot, showing animated phase space diagram overlaid over participant’s image.

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Hidden Helpers: Gender, Skill, and the Politics of Workforce Management for Census Compilation in Late Nineteenth-Century Prussia

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Over the course of the twentieth century, the social sciences have become big data operations.[1] The gathering of mass data and their use for research purposes figure prominently in many of this collection's essays, as do questions about who was acknowledged for the work involved in such efforts – and who was not. My contribution adds a nineteenth-century perspective from the beginning of the modern information age, and also one that explicitly addresses questions of gender.[2] By focusing on the many hands involved in the manual processing of Prussian census data, I will discuss the mechanisms that made invisible a crucial part of the labour force engaged in this work: middle-class women engaged in at-home abstraction work for census statistics. The practices and politics that evolved around recruiting and managing these hidden helpers allow me to analyze why and how their work was black-boxed, while at the same time, their skills and resources became indispensable for the prompt delivery of ever more complex census statistics.

Almost everywhere in Europe, census and population statistics expanded into complex operations during the second half of the nineteenth century, driven by efforts to render them 'scientific', i.e. turning them into standardized procedures that would yield more than just population counts and lead to verifiable, 'objective' descriptions of the populous in question (Porter, 1995). To meet these standards, Prussia's statisticians quite radically transformed their practices of census taking. By the mid-nineteenth century, standardized enumeration procedures reshaped collection efforts, while a new paper tool—the so-called counting card—turned the abstraction of the gathered information into a centralized operation. As the Prussians were proud to emphasize, the new technology would allow them to compile tables displaying a rich combination of variables, which no other nation was able to achieve.[3]

With the centralization of abstraction work in 1871, it fell to the Prussian census bureau to create and direct workflows dictated by unprecedented waves of incoming data, strict budget constrains, and tight schedules. Their goal was to produce ever more refined census tables. The implementation of the new counting card technology became embedded in a circular system of piece-rate sorting and tabulating unique to Prussia, one intimately intertwined with core social and political structures of the state. To begin with, centralized census compilation increased the bureau's demand for seasonal labour quite dramatically. The census bureau hired extra workers

1 See for example Lemov (2015) on Cold War social science.

2 The historiography on gender and science is rich in examples of women's invisible work, see first and foremost Rossiter (1982) and Schiebinger (1989); a more recent example is Lykknes, Opitz and van Tiggelen (2012).

3 On the introduction of this new technology and a more in-depth analysis of the gendered compiling effort see von Oertzen (2017, under review).

to check the incoming data and handle the analysis of the cards (Engel, 1873: 349). However, the main work of sorting and counting the cards, which required space to spread out the material in piles, was not done on the bureau's premises. Rather, it was distributed for homework. Keeping the masses of paper cards in constant motion between the bureau and different homes was considered crucial to handling the material. Homework emerged as a key component in the processing of census data over the following decades, with its volume growing considerably each year. A peak was reached in 1895, when the census bureau employed 1000 wage-workers and about 3000 piece-rate workers at home to compile the commerce, trade, and agricultural statistics of that year, the most comprehensive statistical investigation the agency had ever undertaken (Blenck, 1897).

The Prussian circular system of outsourcing census work depended crucially on well-trained and perfectly reliable personnel. How did the census bureau build up such a trustworthy work force? As elsewhere in the Prussian civil service, low rank positions such as piece-rate abstraction work for the census bureau were formally reserved for those who had served in the army. To keep pension burdens to a minimum, lower rank officials were entitled to state employment upon completion of a dozen years of military service. The army offered educational and social prospects in exchange for lifelong service, while at the same time infusing the civil service—and to a considerable measure the society at large – with military values of duty, rank, and obedience. These were key elements of what became known as Prussian militarism (Stübig, 1987). However, for the Prussian census bureau (and many other state agencies), this constellation proved problematic. Bound on the one hand to comply with the state's social policy measures, and on the other to produce high-end statistical tables as speedily and as cost-efficiently as possible, the bureau's directors developed a subtle strategy. Publicly, they committed to the bureau's mandate of providing work and income for retired soldiers. Behind the scenes, however, they did what they could to operate with the most efficient, skilled, and steady workforce available.

Over the years, the census bureau painstakingly built up a faithful workforce toiling on site and from home, tailored to the bureau's fluctuating demand. The bureau used the seasonal character of census compilation to optimize its male in-house workforce, and to expand its reserve of home workers. The bureau's employment ledgers indicate that only married men were hired for on-site wage work—and that it was through these married male workers that the census bureau created a flexible female workforce ready at its disposal. While the bureau's male workers performed abstraction work on site, their wives, children, and extended family members undertook the same or related work at home on an as-needed basis. The bureau counted on family ties for having trustworthy hands available to work remotely: control was imposed through the heads of household employed in the bureau itself. These workers were held responsible for impeccable results of their own, as well as for their dependents' work. All income, whether earned on site or at home, was paid to the respective male workers alone, and rigorous wage-cuts were in place to discourage slipshod abstraction work and to keep revisions at a minimum.

Overall, the Prussian census bureau relied much more heavily on the professional expertise of men who had never served in the army, and the skills of their wives, widows, and unmarried middle-class female staff working from home to secure the functioning of its data processing machinery than on social policy regulations granting preferred employment to retired servicemen. At peak times, mobilization efforts were explicitly extended to include the spouses of the bureau's higher-rank office clerks. The bureau's registers reveal that some of their households came to resemble busy office floors, providing extra work and income not only for the officials' wives, but also to sisters, neighbouring widows, and unmarried female acquaintances. The work of these groups was deemed so excellent that the bureau insisted to forgo all set limits for maximum income. Demand was so high that wives had to hire maids to free themselves for abstraction work. The female kin of the bureau's staff proved especially profitable, because once trained and experienced with abstraction home work they would always remain available for this kind of temporary work—other means for additional income compatible with their social status did not exist.[4] In addition, the bureau considered the women to be much better suited for abstraction work than veterans in need. For one, they were better educated, but also "meticulous, assiduous, mentally adept, undiscouraged by the cumbersome abstraction work, and, most importantly, able to handle the precious original data with the necessary care, because they have an orderly home at their disposal".[5] Homebound middle-class women, rather then impoverished veterans, proved indispensable for abstraction work in Prussia. Embittered petitions by dismissed servicemen accused the bureau of privileging "the fair sex.".[6] Suspecting favouritism and corruption, many veterans voiced their anger, feeling betrayed by the bureau's policy. It is because of such charges that the bureau's strategies were made explicit, leaving traces for historians thereby. In defending their practice, the bureau's directors stated repeatedly that the agency's statistical endeavours should not be mistaken for a welfare program. Efficiency rather than charity was to rule the bureau's employment strategies—otherwise, the value of the work would suffer and its completion would be delayed, resulting in cost overruns. The best help for the bureau's efforts was therefore often provided by the female dependents of agency officials, "enjoying their guidance and supervision".[7]

Home-based census abstraction work mainly done by educated middle-class women proved to be the most reliable, fastest and cheapest way to produce numbers and tables for the Prussian state. Neither bound to eight-hour work shifts common in civil service jobs, nor to Sunday and public holiday rest, women produced numbers and tables for twelve to sixteen hours a day at home, invisible to the public at large. The bureau's hidden helpers did their work so well that Prussia's bureaucrats were able to publish fine-grained results of their population surveys faster than anywhere else in Europe.

As much as Prussian officials valued the women's work and acknowledged their skills internally, they were careful to conceal their appreciation publicly, as veterans began to organize and to join forces with social democratic radicals, castigating civil service employment policies as

4 Similar mechanisms can be observed in many fields of seasonal work from the canning and textile industries to metalworking to office work, see Canning (2002), von Oertzen (2007).

5 Blenck to Ministry of the Interior, 10 December 1908, Secret Prussian State Archive (GStA) I HA, Rep. 77, Tit. 536, No. 30, vol. 2.

6 Letter from Blenck to the ministry of the interior, April 21, 1898, regarding the petition of Militäranwärter Wilhelm Mittelstädt, Preußisches Geheimes Staatsarchiv (GStA), I HA, Rep. 77, Tit. 536, No. 30, vol. 1.

7 Petersilie (one of Blenck's speakers) to Ministry of the Interior, 27 January 1909, Secret Prussian State Archive (GStA), I HA, Rep. 77, Tit. 536, No. 30, vol 2.

well as the state's pension system for leaving retired soldiers in far worse condition than factory workers. With socialism on the rise, the veterans' unrest raised fears that Prussia's most loyal supporters might radicalize and join the labour movement (Vogel, 2001). For these reasons, Prussia's statistical office remained firmly committed to manual data processing, paying lip service to state paternalism while black-boxing the actual practices of at-home data compilation. As a consequence, the mechanical processing of data via punch cards, tabulating machines, and other electric sorting devices, would remain unexplored in Prussia's census compilation until after the First World War.

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Power

Invisible/Visible radiation: Skin in the Game at Hiroshima and Fukushima

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> The zoning has been revised a few times, resulting in the division of the City into five areas; the difficult-to-return-to area; the not permitted to live area; the ready-to-be-lifted evacuated area; the specific spot recommended-for-evacuation area; and the no-restriction area.[1]

The spaces described in terms of human presence or absence in the quote above are all in Minamisoma City, a town formerly of about 70,000 people, now reduced in size, on the coast of Fukushima Prefecture in Japan, 14 km north of the Daiichi Nuclear Power Plant. The terrain of this small coastal town has been parsed into spaces defined by human risk—by bodily absence or presence, future presence, recommended presence—based on levels of radiation that have been officially detected and seemingly stabilized, despite wind and rain. The area categories map how the radioactive materials fell in March 2011and how they have been reconfigured. Across the region, radioactive soil is also being moved and rearranged by human labour: Landowners, contracted workers and activist volunteers power-wash sidewalks (to protect their children walking to school) and scrape off topsoil (for safe vegetable gardens). In many places trees and bushes rise hopefully out of the tops of massive stored bags of radioactive topsoil, green life working its way through the plastic and into the sun, again redistributing what has been temporarily contained. Radiation is both a visible and invisible actor in the region. It is made manifest in thyroid nodules, monitors on children's backpacks, lists of radioactivity readings for produce at the local co-op—and made to disappear in official pronouncements from medical and scientific authorities about risk. The region of Japan affected by the meltdown is now in a state of slow violence, the evocative term proposed by Rob Nixon in his 2011 book to describe the ways that environmental damage unfolds and in the process performs the violence of inequality (Nixon, 2011).

The Fukushima nuclear disaster, which began in March 2011, has had wide-ranging technical, political and social effects. Official reports of the devastation wrought by the earthquake and tsunami document 15,954 people dead and 3,155 missing. Hundreds of thousands of structures were destroyed or damaged; at least 400,000 people were displaced. Many regions, like the coast near Sendai, were still incompletely cleared four years after the disaster. The Fukushima event

1 From the online report of the International Committee on Radiation Protection Eighth Dialogue Meeting, May 10–11, 2014, 'The Situation and Challenges of Minamisoma-Working Together in the Affected Areas', Posted at http://ethos-fukushima.blogspot.jp/p/icrp-dialogue.html.

(the entire complex disaster including earthquake, tsunami, nuclear meltdown, and the resulting contamination and dislocation) has been widely assessed as the most economically devastating disaster in world history (UNEP, 2012). Galvanizing awareness of nuclear power risks, it undermined public trust of key institutions in Japan and elsewhere. Government pronouncements about the accident were misleading and incomplete; human migrations during the emergency were chaotic and disastrous. More than two million people have been and continue to be directly affected by the 3/11 event.

The accident also generated a new alliance between the survivors of the atomic bombings and the populations exposed to radiation as a result of the nuclear power accident. Characterized as the "third atomic bombing" of Japan by Hiroshima atomic bomb survivor Keijiro Matsushima, the disaster led atomic bomb survivors groups to become, for the first time, vocal in their opposition to Japan's nuclear energy policies.[2] "Is it Japan's fate to repeatedly serve as a warning to the world about the dangers of radiation? I wish we had found the courage to speak out earlier against nuclear power", survivor Masahito Hirose told the *New York Times* in August of 2011 (Fackler, 2011). As residents in the region began to question what they were being told about the accident, they were supported by the atomic bomb survivors and by a national and even international network of citizen scientists (most importantly, the global data collection movement Safecast). These groups challenged official statements about biomedical risks, and undertook their own, do-it-yourself clean-up and monitoring programs with minimal governmental or scientific support.

The Fukushima disaster thus marks a site where various kinds of labour and various kinds of invisibility coalesce. Non-scientists presenting themselves as uniquely unbiased have become a powerful force in the public debate about radiation risk, in some cases prodding governments to act or to reveal their data. Mistrusting official pronouncements, they have taken up the fundamental work of science, data collection. While scientists characterized the risks as very low, the elevated radiation was experienced by residents as *visible* on a daily, intimate basis. It could been seen in the lists of measured radiation levels for beets or carrots posted weekly at the local co-op, or in the still-occupied corrugated metal housing in parking lots filled with displaced residents, or the children with monitors clipped to their backpacks on their way to school. Radiation could not be seen directly, literally, but its presence was manifest in social practice every day. It was impossible not to see it. At the same time, those affected by the meltdown have become, in the cogent perspective of historian Robert Jacobs, invisible. He proposes that "radiation makes people invisible" and that irradiated populations around the world become "second class citizens" now "expendable" because their experiences and needs challenge powerful economic and political interests (Jacobs, 2014).

2 "Instantly, I felt, oh, Japan has suffered from the third A-bombing." Interview with Matsushima, National Public Radio, Frank Langfitt, August 8 2011, "Japan Rethinks Its Relationship With the Atom." http://www.npr.org/2011/08/08/139080238/japan-rethinks-its-relationship-with-the-atom (accessed May 10, 2016). But many others linked Fukushima and Hiroshima too, comparing scenes of devastated towns to photographs of Hiroshima after the bombing; see http://www.hiroshimapeacemedia.jp/?p=23991 Accessed January 3, 2015. See also an interview with Matsushima with CNN, Eve Bower, March 15 2011, "Nuclear Crisis Recalls Painful Memories in Hiroshima." http://www.cnn.com/2011/WORLD/asiapcf/03/14/japan.hiroshima/

As I attended meetings and talked with people in Hiroshima and Fukushima Prefecture during the fall of 2014, several mentioned an idea, "skin in the game", drawn from the work of a philosopher at New York University engineering school, Nicholas Taleb.[3] I had not read Taleb and was not familiar with his work, though he is rather famous. He is the best-selling author of *The Black Swan* (2007) and is a former hedge fund manager. His notion of 'skin in the game' proposes that those who build risky systems (such as in this case nuclear power engineers) should face some danger and/or damage if the worst occurs. "We propose a global and morally mandatory heuristic that anyone involved in an action which can possibly generate harm for others, even probabilistically, should be required to be exposed to some damage, regardless of context" (Taleb and Sandis, 2014: 1).

His ideas were invoked at formal international meetings in Hiroshima and Tokyo and in my discussions with scientists at the Radiation Effects Research Foundation (RERF) on Hijiyama Hill (Figure 1), the park-like site overlooking Hiroshima where the scientific agency studying the atomic bomb survivors has been located since 1950. The idea of 'skin in the game' suggests an implicit social theory of technoscientific risk: The problem is that risk is distributed irrationally across social networks. The people who generate risk do not necessarily experience the consequences that their own labour brings into being.

Figure 1: Photo of Radiation Effects Research Foundation (RERF) on Hijiyama Hill in Hiroshima used with permission of the RERF.

3 See Nassim, Taleb and Sandis (2014). Also, online presentations of his idea at: http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2298292 and Taleb on this issue in a Youtube lecture: https://www.youtube.com/watch?v=0Uc4DI-BF28.

Residents of Fukushima who met with me, who had real skin in the game, did not mention this theory and might not have read Taleb, but knew perfectly well that they had been disadvantaged by the accident in ways that were morally, politically, and economically out of sync: They had not profited from the nuclear plant and had not caused any part of the accident. But they had been forced to leave their communities, give up their homes and farms, and continue to face confusing and uncertain biomedical risks. I would suggest that scientists engaged with radiation risk had a different kind of skin in the game. They sought to assert and reinforce the reliability and credibility of results on radiation risk drawn from the studies of the survivors at Hiroshima and Nagasaki—often referred to as the 'gold standard' in the field. This sort of professional investment is not the kind of skin in the game invoked by Taleb, but it is one integral to the legitimacy of scientific knowledge. And the results from Hiroshima and Nagasaki suggested that no one in the Fukushima area (outside of highly contaminated zones) was at risk because exposures to radiation were too low. Experts at Fukushima Medical University and at the Radiation Effects Research Foundation in Hiroshima said that the elevated radiation levels in the region had no *biological* significance. They were 'barely above background'. International experts at the World Health Organization (2012) and the International Commission on Radiological Protection (2013) concurred: No long-term health effects could be expected (González, Boice, Chino et al, 2013; WHO, 2012; WHO, 2013). Experts acknowledged that residents faced profound social, economic, political and emotional disruption, but declared that local fears about long-term health consequences were overblown; emerging evidence of high rates of thyroid changes in children were interpreted as the result of increased surveillance rather than increased risks; and measurements showing hot spots along sidewalks and near schools might be the result of inappropriate, incompetent, or non-expert use of radiation monitoring technologies.[4] Residents doubted and disbelieved the official (governmental and scientific) accounts and data; technical experts doubted and disbelieved the accounts and data of residents—technologies could be blamed and so could emotions, ignorance, kickbacks, industry pressures and politics.

Those on both sides wrestled with a knowledge problem (Balogh, 1991). It is a classic problem of authority and trust that resonates with many other stories in the history of technical knowledge, with the deep quandaries posed by what seems true and what seems false, and to whom. At the centre of these debates is an invisible actor—radiation—that can be seen and known only at a remove, through statistics, epidemiology, or weekly vegetable lists (Figure 2). Radiation can make its presence known through the human body (manifest in radiation sickness, cataract, leukaemia, thyroid nodules, cancer); it can be detected with many different kinds of monitoring devices; and calculated based on engineering protocols for a particular source (a given nuclear energy plant that exploded in a given way); or reconstructed historically and in staged re-enactments (as at Hiroshima, Nagasaki, and Nevada; see Auxier, 1965). But the exact level of ambient radiation is not directly visible to anyone—not to residents, not to scientists. And it does not hold still. It changes over time, moving through leaves and then falling to the trails when the weather cools; flowing with rain and settling in creeks, ditches, ravines; or collecting where winds deposit it. It shifts and moves—so that any claim made about its presence or absence must depend on time as well, and a measurement on Tuesday could be different from one on Thursday. Radiation might stabilize in a policy document but not in a neighbourhood.

4 Ian Thomas Ash's award-winning documentary, "A2-B-C", about families in the region trying to measure radiation and assess thyroid cancer risk, provides critical perspectives on these issues of expertise and measurement: Who knows how exactly to hold a Geiger counter? See http://www.a2documentary.com/synopsis/, Accessed March 6, 2016).

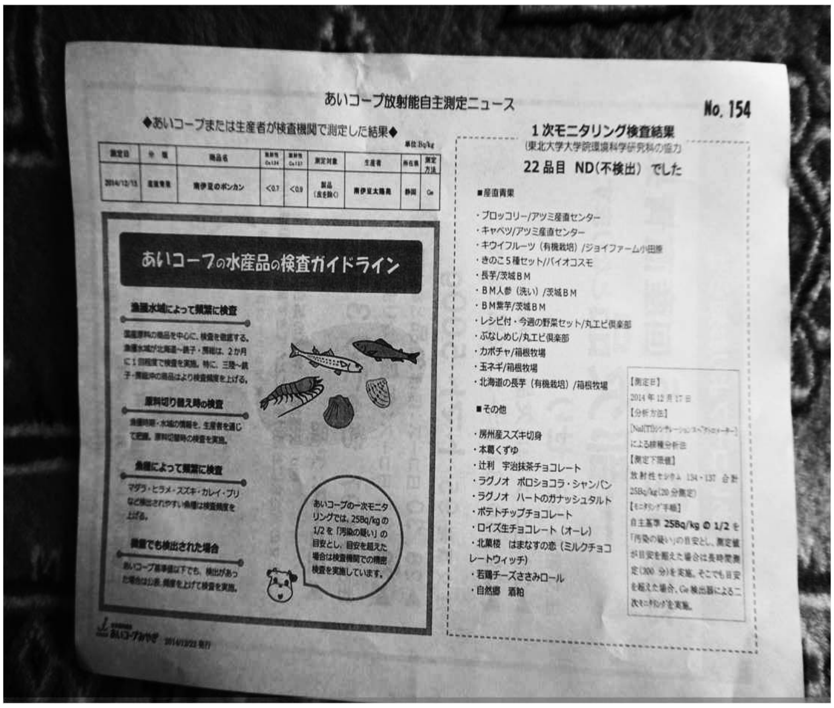

Figure 2: Historian of Medicine Suenaga Keiko's weekly list of radiation readings for produce in Sendai. Photo by author, January 2015.

Measuring its presence, then, has taken on a particular resonance in the region, and indeed, across Japan. One of the most ambitious citizen science programs ever developed, Safecast, is devoted to the 'unbiased' measurement of radiation in a repudiation of the official pronouncements. It began on Twitter as a call to produce knowledge that did not depend on elite (newly mistrusted) institutions in Japan. This 'pro-data' movement promised to make knowledge of radiation that would be characterized by "independence, transparency, and openness", all "key to credibility."[5] Safecast thus claimed credibility not through elite systems of training, consensus and professional prestige, but through the hard, committed labour of dispersed and personally invested people all over Japan (and eventually all over the world) who were taught to build and read radiation monitors and download what they found to an online database freely accessible to all:

> Unfortunately, it has been difficult until now to find radiation data which truly has been free of bias, or of the perception of bias in favour of one ideological position or another. From the outset, Safecast has not sided with either the pro- or anti-nuclear camps, and has striven to demonstrate the advantages to science and to the public of having an independent organization devoted solely to providing the most accurate and credible data possible.[6]

5 http://blog.safecast.org/ "Safecast is Pro-Data" The comments are taken from the description of the project's purpose on the website. Accessed May 10, 2016.

6 http://blog.safecast.org/ "Safecast is Pro-Data" The comments are taken from the description of the project's

Safecast began just after the accident when Los Angeles-based blogger/artist/hacker Sean Bonner queried friends in Japan about what the radiation levels were like in their area.[7] When they did not know—and it quickly became clear that neither the utility nor the government was releasing much information—he and several friends, including Joichi Ito, Director of the Massachusetts Institute of Technology Media Lab, set out to buy Geiger counters to begin a people's survey of affected areas. Unfortunately the disaster had increased global demand for Geiger counters, and they could only buy ten. In order to collect more data, more quickly, the group and its volunteers, who began scanning in Japan, attached the counters to a laptop with a GPS device and started driving around the Fukushima region collecting measurements (Pierre-Louis, 2014: 52; Edwards, 2015). Eventually Safecast's technologically-oriented volunteers developed a new machine—the bGeigie, a combination of a Geiger counter, GPS receiver and data recorder, in a small, easily transported form. Available for $450, it was compared favourably by Safecast's leadership to top-of-line $15,000 radiation monitoring machines. When these counters were sent in with residents allowed to return to exclusion zones to collect belongings, some of the results were disturbing: Levels were lower in some parts of the exclusion zone than in some areas outside the zone. After Safecast published these data, the government released maps which acknowledged the disparities. "There's no way to speculate what [the Japanese government] would or wouldn't have done", Bonner told Pierre-Louis, "but they had the information for months and didn't publish their data till days after we published ours. Only when we published our data did they admit that they had information that confirmed it and started changing evacuation zones" (Pierre-Louis, 2014: 53). By 2015, there were 600 bGeigies in use around the world, and about 30 million measurements had been collected, including measurements from both inside the Fukushima plant, and in the Chernobyl compound (Edwards, 2015). Safecast has now turned its attention to other kinds of civilian monitoring devices that could be used to track air and water quality and to create crowdsourced science.

Kendra Pierre-Louis, in her 2014 *Newsweek* article about Safecast, highlighted the inadequacies of environmental regulation (in the United States) by quoting an environmental agency official who said regulation is so dysfunctional that "we basically need citizens to sue in order for us to do our job" (Pierre-Louis, 2014). The comment suggests how a critical feedback loop in environmental risk has begun to function. Citizens are an active part of the scientific and regulatory landscape—as data collectors but also plaintiffs. "The rise of citizen science is one of the few bright spots that have emerged following the Fukushima disaster. The citizen science genie is out of the bottle, and cannot be forced back in" (Safecast, 2011).

The disaster also animated more local activist citizen movements, like '3a' (anshin:security, anzen:safety, and action) in the city of Koriyama (Yasushito, 2015). 3a, run by Suzuki Yohei, a construction contractor, makes equipment and training available to residents in the area who want to test for radiation, decontaminate their property or assess their soil (Figures 3 and 4). He also power-washes sidewalks, routinely, and has developed a certain expertise in the proper methods. There have also been four Citizen–Scientist International Symposia on Radiation Protection since the disaster, at which scholars, scientists and citizens discuss questions of laws

purpose on the website. Accessed May 10, 2016.

7 http://blog.safecast.org/ "Safecast is Pro-Data" Also, on the origins of Safecast, http://blog.seanbonner.com/about/ is now the Director of Safecast and holds an appointment as visiting researcher with the MIT Media Lab's Center for Civic Media.

and rights, public information, emergency responses and so on.[8] Most importantly, activist residents and atomic bomb survivors called immediately for the consistent and comprehensive surveillance of thyroid abnormalities in children in the region, and what began as a project of reassurance (at Fukushima Medical University in October 2011) became something else. In late 2015, a startling scientific paper drew on these thyroid tests to suggest a 50-fold rise in thyroid cancer for children in the area. The paper, by Tsuda Toshihide, Tokinobu Akiko, Yamamoto Eiji and Suzuki Etsuji and published in *Epidemiology*, concluded that this excess was unlikely to be explained by the screening surge. The group drew on the data from the first round of screening with 298,577 examinees, on the second round begun in April 2014, and on overall Japanese annual incidence (Tsuda, Tokinobu, Yamamoto and Szuki, 2015).

Figure 3: Suzuki Yohei at 3a in Koriyama, photo by author.

8 See http://csrp.jp/symposium2014, accessed December 12, 2014. See also the Citizen Science Initiative Japan (What's CSIJ, 2013), which began before the disaster but which has developed programs on low level radiation risk.

Figure 4: Koriyama Citizen Science map of radiation levels, December 2014, courtesy of 3a.

Of course, these results can be contested. One common claim, by defenders of nuclear energy, is that the high rate of thyroid nodules is only apparent—that rates of thyroid nodules in general are unknown, and that many of the nodules identified as cancer would never have health consequences for the children.[9] In addition, as the paper acknowledged, individual doses for Fukushima area residents are essentially unknown, and not likely ever to be known. People who study the health effects of disasters spend a great deal of time reliving them. They try to reconstruct as much as possible the details of the chaos: The timing, sequence of events and where everything and everyone was. At Hiroshima and Nagasaki at the moment of detonation—the critical moment of exposure, in comparison with which all other exposures were trivial—the types and amounts of radiation released and absorbed were not stable, measured, known facts about the world. Rather, they were frequently recalculated estimations, grounded in technical and social uncertainties and shaped by Cold War secrecy (you can get a sense of this by looking at the December 2015 workshop report on atomic bomb dosimetry in *Health Physics*; Kerr et al, 2015). At Fukushima, the complex nature of the disaster, with an earthquake, tsunami and meltdown, and the movements of people in the immediate aftermath, make it unlikely that individual doses will ever be stable, known, reliable facts. Like the doses in individual survivors at Hiroshima and Nagasaki, which have been calculated and recalculated for the last 70 years, they are likely to always pose a technical problem. Tsuda and colleagues had to rely on proxies: "Because there is

9 A pro-nuclear energy website thebreakthrough.org called the results of the 2015 paper on thyroid cancer flawed: "So *is* the cancer surge real? Nope. The Tsuda study's conclusions are the product of bad methodology, flawed reasoning and egregious obfuscation of evidence." (Accessed May 10, 2016).

no precise measurement of external and internal radiation exposure in Fukushima, we used the residential addresses of the subjects in March 2011 categorized into each administrative district as a surrogate for individual radiation exposure measurement" (Tsuda et al, 2015).

Conclusions

The survivors of both the bombings and the meltdowns have been exposed to a form of environmental contamination that supposedly can be known to them only through certified technical expertise. As Kuchinskaya has observed:

> […] radiation is not directly perceptible to the unaided human senses. People cannot see, hear, or feel radiation. Their senses register nothing. As a result, *formal representations become doubly important* in defining the scope of what is considered dangerous contamination. (Kuchinskaya, 2013: 78, my emphasis)

Those formal representations are generally technical products, research papers, international reports, charts of dose-response curves.

As an invisible actor, subject to many levels of negotiation and uncertainty, radiation puts extreme stress on systems of inscription and analysis. It also animates public and citizen responses that then feed back in to technical inscription. Citizens demanded thyroid screening because of data from Chernobyl, and the resulting screening data became crucial to a contentious scientific paper in a respected journal, *Epidemiology*; Safecast published its own, citizen-gathered radiation readings, and shortly thereafter the Government of Japan confirmed that they had collected similar readings, and revised the exclusion zones. The push and pull of public negotiation turns on quantitative results that are virtually qualitative. The numbers do not speak for themselves.

Radiation in my story is a particularly provocative actor, because whether it acts or not is precisely the technical and social issue. Radioactive soils and materials can in some ways be seen as doing a form of 'invisible labour'. The scientific process of making its labour visible—making it something that can be reliably tracked and managed—is the core technical problem of radiation biology. Fukushima Prefecture is thus a site of scientific uncertainty and citizen activism, a site where the knowledge of the streets, amateur expertise, and local knowledge intersect with the knowledge of international bodies and national governments that collate information and reach conclusions about policies and risk.

One of my agendas in thinking through these issues in this essay is to assess what science studies and the history of science have to contribute to our understanding of complex disaster events. What tools, what insights: What kind of skin, and what kind of game? So this is a reflexive discussion—situated, and perhaps activist. I suggest that canonical themes in the history of science are directly relevant to technoscientific crisis (Clancey, 2006; Knowles, 2011). Scholars in science studies broadly conceived have long been concerned with the social negotiation of credibility—and at Fukushima, credibility is in short supply. Mistrust is a dominant theme, for people in every position in this game. Now alongside Hiroshima, Bikini, Hanford, Chelyabinsk, Semipalatinsk, Sellafield, and Chernobyl, Fukushima has joined a different kind of 'nuclear club', characterized not by military power but by contamination and biomedical uncertainty. It is also now more than local. What happens in this site provides a potential model for other sites, other nuclear energy disasters. Many kinds of experts are studying the region to develop guidelines for the management of future disasters. Many scholars have characterized a new modern era in terms of a 'radical break' that occurred around the early 1970s, produced by scientific and technological activities "for which the planet has become a vast laboratory" (Boudia and Jas, 2007: 319; Centemeri, 2010).[10] Fukushima is a 'model', a test, a laboratory.

In her presentation at a meeting about Fukushima held in Hiroshima in the fall of 2014, Kim Fortun outlined scales of analysis relevant to the immediate management of disasters. These include, she proposed, the public image of radiation (a meta scale: 'Godzilla'), the political domain (a macro scale), the social disruption (a meso scale: evacuation), the intimate medical experiences of each person (a micro scale), and the technical, ecological and digital analysis of risk and damage.[11] Disaster management now must operate with all these levels in play and to do so often involves complex collaborations rather than one-way statements about the natural facts. There are many kinds of labour, and many kinds of invisibility, in these networks.

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Invisible Infrastructures: Xavante Strategies to Enrol and Manage *Warazú* Researchers

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A Xavante field seminar

When Fabrício Rodrigues dos Santos and the members of his National Genographic field team arrived in the Xavante Indigenous Territory of Pimentel Barbosa in 2010, it was almost as if they were entering a ready-made training system for scientists. The researchers were working on a large-scale initiative to track pre-historic human migration through the genetic sampling of Indigenous groups, and had extensive experience with other Indigenous communities.[2] However, their short stay in Central Brazil was exceptional. It was more of a crash course in Xavante culture than a routine collection of cheek swabs.[3]

Residents of the village of Etênhiritipá stipulated that even to study Y-chromosomes and mitochondrial DNA, the researchers had to understand the Xavante way of life. "For you to understand us, you have to live with us", leader Jurandir Siridiwë Xavante told Santos and his colleagues.[4] Upon their arrival, the *warazú* (non-Xavante) visitors were installed in an old schoolhouse. They were assigned two village residents as guides and guards to help with daily tasks, and to protect them and their equipment from overly curious children. Rather than days filled with collecting genealogical data and genetic samples, Santos's narrative of his time in the village centred on hunting and fishing trips, a movie night, and incredible star filled skies. "And not only that", Santos told me, "We participated in rituals with them. Not the rituals they put on for tourists, ones that they were really doing." He went on later in our interview to describe the ceremonies, "One was a baptism. […] In that ceremony I was baptized too." Pedro Paulo Vieira, another researcher in the field team, echoed this sense of engagement and inclusion, saying, "The Genographic [Project] was adopted by the Xavante in Brazil. […] Fabrício, my-

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2 The National Genographic Project is one in a long line of transnational research programs that have emphasized the scientific value of biosamples from Indigenous peoples. See Tallbear (2013); Reardon (2004); Radin (2013).

3 Fabrício Rodrigues dos Santos, interview with the author, 6 March 2014, Belo Horizonte, MG. The National Genographic Project was carried out in a number of different villages in the Indigenous Territory (T.I.) of Pimentel Barbosa, coordinated through the village of Etênhiritipá with the help of Jurandir. (Figure 2). This piece discusses research carried out exclusively in this territory, primarily in the villages of Pimentel Barbosa and Etênhiritipá, which constituted a single village until they split in 2006. Here "the Xavante" refers to the Xavante of T.I. Pimentel Barbosa. I discuss evidence that other Xavante communities also seek engagement with researchers elsewhere.

4 Ibid.

self, and some other members of our team were even assigned to clans within the village. […] We became part of the Xavante community" (Figure 1).[5] As they tell it, the researchers' time in Pimentel Barbosa was a consuming, immersive experience of Xavante culture. It is a story of how the Xavante enchanted them.

Figure 1: Following his first visit to Etênhiritipá, Pedro Paulo Vieira went back on three additional visits. He was given a Xavante name 'Serenhi'õmo' and assigned to an age grade (hötörã) in addition to being 'baptized' öwawe. Young men in the village made this uniform for Vieira. Copyright Pedro Paulo Vieira, used with permission.

Santos and Vieira are two in a long line of researchers that the Xavante have received, welcomed, and trained. The experiences of these geneticists, and my own 2015 experience collaborating on a digital archive project in Pimentel Barbosa village, were fundamentally shaped by the communities' long histories of interactions with *warazú* researchers. Over time, Xavante communities in *Terra Indígena* (*T.I.*) *Pimentel Barbosa* (Figures 2a and 2b) have systematized their approach to enrolling, managing, and training researchers.[6] Although invisible to readers who delve into the resulting scientific papers and ethnographic monographs—and at times even to those researchers visiting Xavante villages—a great deal of labour is involved in hosting us. Xavante leaders have created infrastructures to mediate and distribute the work and material

5 Pedro Paulo Vieira, interview with the author, 7 May 2014, Rio de Janeiro.

6 In 2010 the Xavante Territory of Pimentel Barbosa was home to approximately 1,200 people who self-identify as *A'uwe* or Xavante, distributed in nine villages (Welch et al., 2013; Figure 2).

compensation associated with research. Furthermore, this invisible labour of enchanting and enrolling researchers is part of community members' larger projects of political visibility in their ongoing struggles for land, healthcare, and education.

An enduring site of research

The first researchers arrived shortly after the Xavante established diplomatic relations with the Brazilian government in the mid 1940s. Anthropologist David Maybury-Lewis, and his family completed the first major ethnographic study in 1958 with the community that would later settle in T.I. Pimentel Barbosa (Maybury-Lewis, 1967). Building on this early work and Maybury-Lewis's 1962 collaboration with human geneticists Francisco M. Salzano and James V. Neel (Neel et al., 1964), academic visitors followed from all walks of the human sciences: public health, dentistry, human genetics, social and physical anthropology, human ecology, linguistics, and even ethnomathematics.

Initially Maybury-Lewis struggled to carry out his research. He commented that village residents "were little inclined for the tedium of instructing a foreigner in their tongue," and that when asked for help to understand "they were not usually of much assistance, for they had no experience at that time either of translation or of paraphrase" (Maybury-Lewis, 1967: xxi).

The six-month visit that Maybury-Lewis made in 1958 was foundational in a number of senses, but at the most basic level, it established a precedent for inscription activities. Doctoral student and cultural anthropologist Nancy Flowers perceived as much during her fourteen months of fieldwork in the mid-1970s. It was only her third afternoon in the field when the effects of Maybury-Lewis's earlier stay became quite clear to her. As Flowers sat in the shade for a moment an elderly woman took it upon herself to teach the new *warazú* a lesson in social organization. And yet, even as Flowers carefully repeated back the names of the age sets, the woman scolded her. Flowers noted in her field journal, "She said I should write them down right away like David always did, but I had my cameras with me and not a notebook. Very bad – one should always carry a notebook".[7] Likewise, Flowers noted how "everyone" now took an interest "correcting my Xavante pronunciation and grammar".[8] The elders had specific ideas about what interested anthropologists and strong opinions about what anthropologists should do.

But the Xavante have not limited their engagement to tutoring individual researchers; they developed infrastructures that go beyond those that were so visible for Flowers. Academic visitors represented an important source of material, social, and political capital. In response to this potential, community members and leaders developed techniques to manage the influx of material wealth and the substantial labour required to supervise the presence of researchers.

7 Nancy M. Flowers, "Field Diaries, 1976-1977," Papers of Nancy M. Flowers, Museu do Índio Archive, Rio de Janeiro.
8 Ibid.

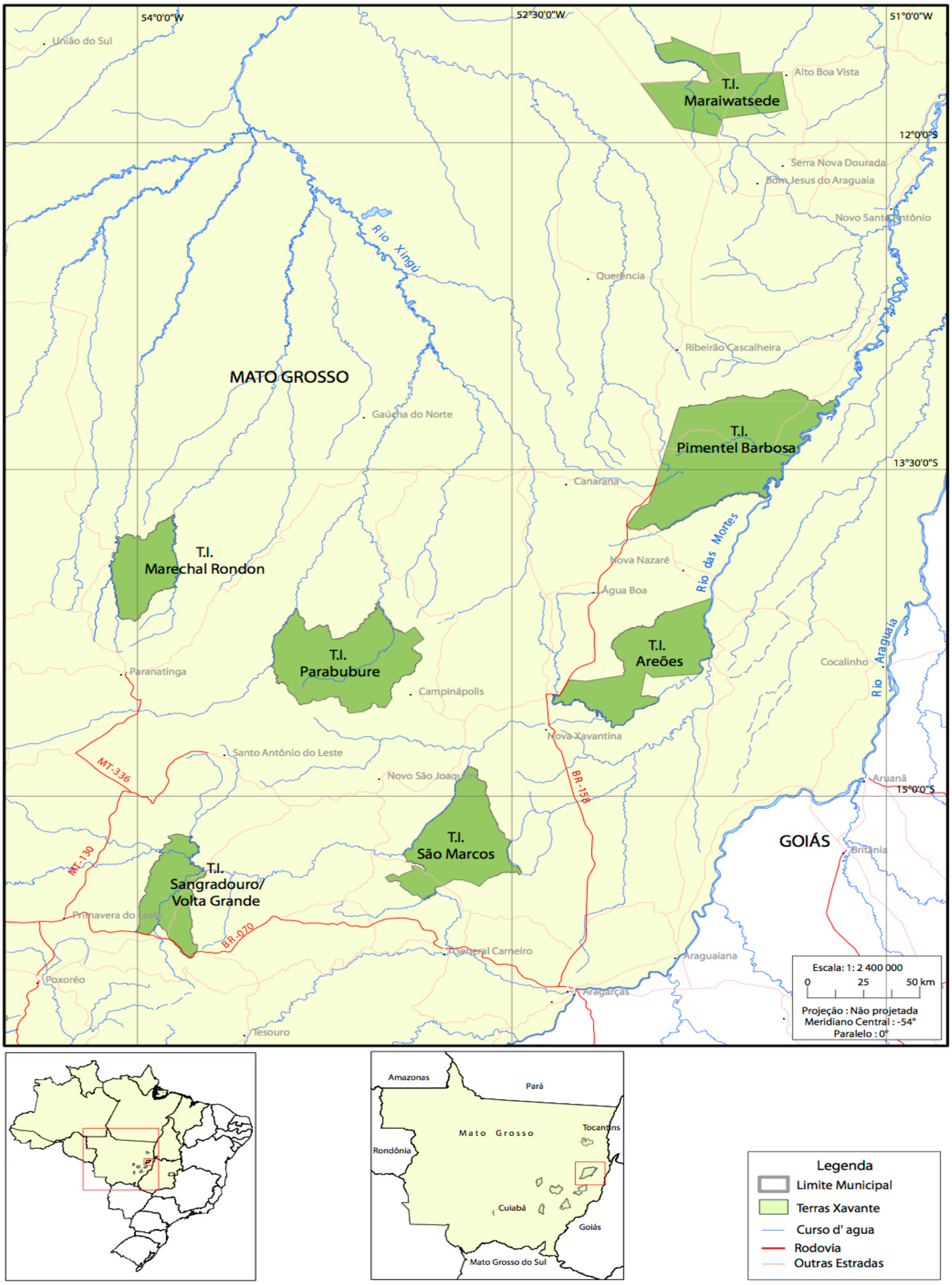

Figure 2a: Map of legally recognized Xavante territories in the state of Mato Grosso as of 2010. Pimentel Barbosa is one of ten legally recognized Xavante territories. Copyright Welch et al. 2013, used with permission.

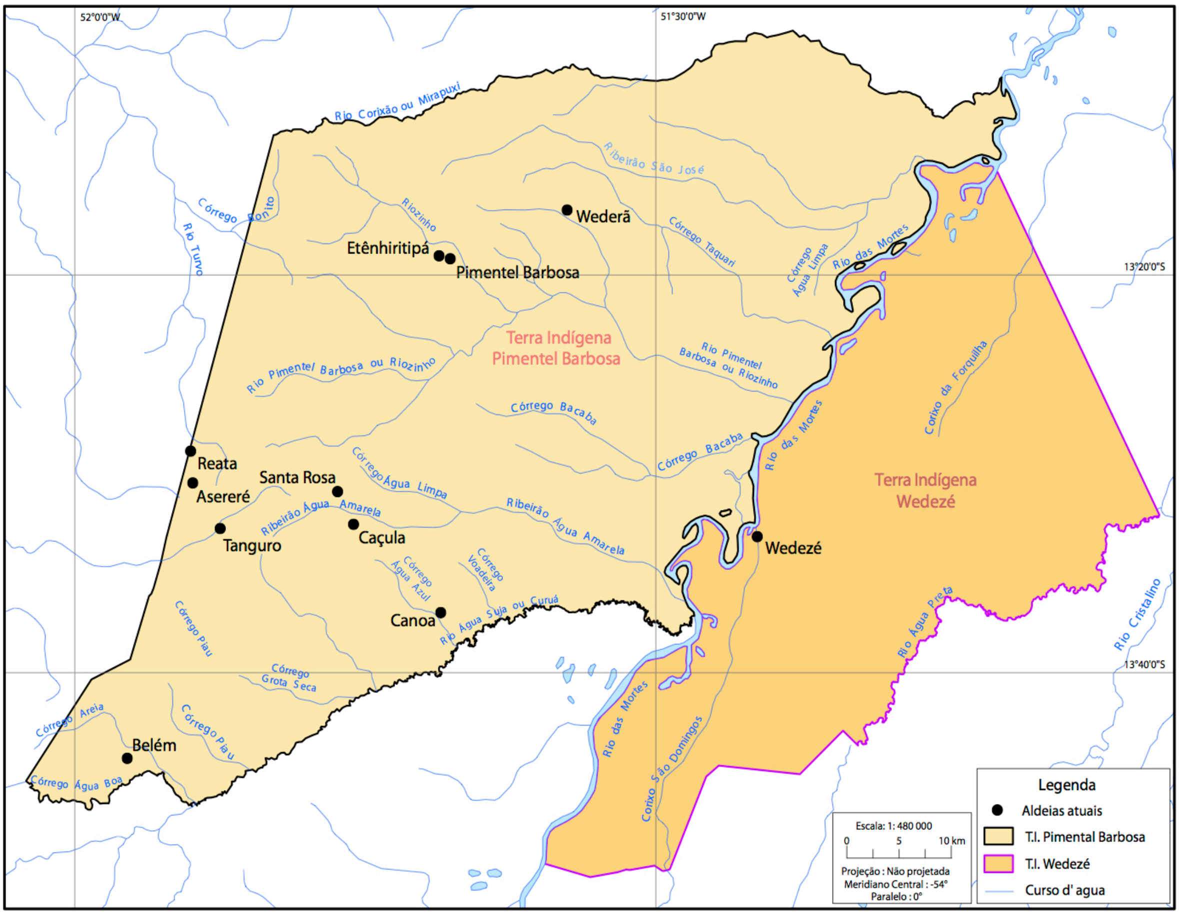

Figure 2b: Map of old Xavante villages in the region of T.I. Wedezé and T.I. Pimentel Barbosa. Researchers Welch, Ventura Santos, and Coimbra Jr. produced this map as part of their delimitation study, completed at the behest of the villages of T.I. Pimentel Barbosa. Copyright Welch et al. 2013, used with permission.

Authority and exchange in research relationships

When Maybury-Lewis arrived, members of the village had been in contact with Brazilian society for over ten years. Prior their 'pacification' of the *warazú*, the Xavante had been at constant war with settlers attempting to invade their land (Garfield, 2001; Graham, 1995). But by 1958 they were accustomed to agents stationed at government posts and received fairly regular visits from officials, journalists, and filmmakers for photo opportunities. Each visit was mediated through extensive gifts. Effectively, material goods became the basis for most interactions with *warazú*, whether researchers or not (Garfield, 2001:77).

The expectation of material gifts was challenging to researchers, who had to think carefully about how and when to offer material compensation for hospitality, food, shelter, tutoring, and time (Maybury-Lewis, 1965, 1967; Neel, 1990). Influxes of material goods were also complicated for the Xavante. Whether delivered by government officials or curious academics, distribution of the newfound wealth created competition and jealousy along political lines within the village. Extensive pressure from land-invading settlers and outbreaks of disease exacerbated this political conflict, and in the late 1950s and early 1960s the village fractioned a number of times, primarily along moiety lines (Maybury-Lewis, 1967).

Anthropologists have long described Xavante society as a moiety system, where each individual belongs to one of two groups: Öwawe or poreza'õno. Belonging in these groups passes from father to child, and individuals of one moiety may only marry a member of the other moiety. This institution is one of a number that structure both familial political power and ritual relations in village life. Leaders able to ally themselves with researchers have benefitted from the political prestige accompanying the resulting availability of goods. These alliances have at times both reflected and fuelled internal political competition, with leaders from opposing factions resisting what they consider unfair distribution of wealth and attention (Garfield, 2001: 76–78). With the arrival of outside researchers since the mid-twentieth century, balancing tensions over incoming material wealth was an issue that the Xavante had to learn to manage in order to both maintain community cohesion and continue to host the anthropologists, geneticists, and public health researchers of the future.

The National Genographic researchers' experience provides insight into how the Xavante have adapted existing social institutions. In the case of Santos's field research, two individuals participated in each activity. As he explained, "With the Xavante, it's always in twos. One öwawe and one poreza'õno."[9] That is to say, one member of each moiety is assigned to each task, as was the case for the two men assigned to 'guard' the researchers. Including researchers in the moiety system also has helped share the labour and rewards of interactions with scientists. Half of Santos' team was 'baptized' öwawe, while the other half was 'baptized' poreza'õno. Many researchers who spend more than a few days in the village are adopted into one of the moieties.

Cultural anthropologist James Welch described his initial days of fieldwork in 2004 as a whirlwind of activity:

> At least at the beginning, it was just busy, busy, busy. You think, 'it's going to be peaceful and calm, […but] one person would say 'I'll take you to my garden,' and then the next, 'I'll take you here, I'll take you there.'[10]

In addition to training the anthropologist in Xavante ways, these efforts served to build personal relationships and establish Welch's place in the kinship system. Once he was located in a family, age-set, and moiety, this social position informed who would help with his research. Members of his adoptive family were more likely to answer questions or help with language. Members of his age-set explained the ritual practices that they performed. But even once situated in a specific social role, Welch was still expected—and explicitly reminded—to share his attention and gifts

9 Santos (cit. n. 3).
10 James R. Welch, interview with the author, 27 March 2014, Rio de Janeiro.

broadly.[11] This distribution of the work of training, teaching, and guiding researchers has also resulted in a distribution of the material and immaterial benefits of the researchers' presence.

The Xavante are deft at finding common ground with researchers, motivated by the hope that these scientists might wield their authority and political clout in favour of community interests. Beyond material benefit, within the context of constant threats to community autonomy, health, and land, engagement with researchers has become a key political and social strategy for the communities of Pimentel Barbosa.

Political pressures, political demands

In Pimentel Barbosa, community members think creatively about the future when they choose to invest in researchers. Asking a geneticist to live with them, even if only for a few days, in order to 'understand them' is not a simple mechanism to invite gifts or material benefit. Over time, the presence of *warazú* has become a source of political potential. Demands for political engagement date back at least to Nancy Flowers's research in the mid 1970s, and have become increasingly nuanced and strategic over time.

Flowers first arrived at a precarious moment for the village. Under unofficial siege during the developmentalist ravages of the military dictatorship, page after page of Flowers's field notes document the encroachment of *fazendeiros* (ranchers), and waves of epidemics. Flowers described how village elders asked for help:

> The Indians thought that I had a lot more power than I had, of course. Because [one of the leaders] would say, 'well when you get back to the United States you tell your president…'.[12]

Flowers left the field deeply frustrated by her inability to intervene in the political and economic systems that were stripping the Xavante of their land, health, and livelihood. But although she initially despaired over her lack of usefulness, her return to the village twelve years later would lay a path to help fulfil some of the demands she first thought impossible.

Flowers returned in collaboration with Ricardo Ventura Santos and Carlos E.A. Coimbra Jr., two young scholars from the public health research institute *Fundação Oswaldo Cruz* (FIOCRUZ). Building on earlier genetic research from 1962 and Flowers's doctoral research, their study on health and nutrition in Pimentel Barbosa became one of the first comprehensive diachronic studies of Indigenous health in Brazil (Coimbra et al., 2004). The initial project also inadvertently grew into a long-term research program that, over the course of twenty-five years, has addressed some key Xavante political concerns. Perhaps most significantly, after James Welch

11 Ibid.
12 Nancy Flowers, interview with the author, 22 August 2013, New York City.

joined the FIOCRUZ team, they conducted a land study for Wedezé, a large area adjacent to Pimentel Barbosa. Drawing on Flowers's field notes and hundreds of other sources, their 2011 report led to the delimitation of 150,000 hectares of Xavante land at a time when few new Indigenous territories were being recognized (Welch et al., 2013; Figure 2). Xavante political demands, which began in the 1970s with somewhat naïve requests for Flowers to talk to her president about the land conflicts, had developed into strategic engagement with and investment in researchers.

Conclusion

Over the past sixty years, various government institutions in Brazil have been responsible for land demarcation, health care, and education in Indigenous territories, and yet, they have consistently left needs and rights unattended. The Xavante have responded to this abandonment with a wide variety of political tactics. One important strategy has been to engage with researchers in creative, didactic, and deliberate ways. Thinking back to the beginning of his fieldwork, Welch described a vision that his hosts had for him, well beyond what he thought he could offer:

> I found out later on that one particular person, when he supported my coming, was hoping that one day I could help with the land fight for Wedezé. […They] had learned that when you invest in a relationship with someone, there is a possibility that it will turn out to be a long-term relationship.[13]

The implications of this description were not far from my mind in June of 2014 when I first sat down in Água Boa, Mato Grosso with four leaders from the village of Pimentel Barbosa. Above the persistent growl of the air conditioning, I briefly described my research. Within an hour of discussion mediated by translations from Xavante to Portuguese, the elders surprised me by professing their interest in the history of science. "We want to know," they told me, "what was said? What was written in those books, in that research?"[14] Their enthusiasm – along with the support of colleagues in Rio de Janeiro – meant I arrived in the village a year later with a hard drive of salvaged images, scanned from researchers' papers. I had been enrolled to help create a digital repository. The leaders hope this vernacular archive might provide evidence in future debates over territory, or meet other unforeseen village needs. However, beyond my role as historian and data transporter, within days of my arrival I also became an adoptive daughter, *poreza'õno*, and a favoured source of coffee and entertainment. Although not uncomplicated or uniformly pleasant, I felt deeply compelled by these new relationships. I was imbricated almost immediately in conflicting priorities of memory, history, and archive. I too have been enchanted.

When Xavante community members draw a new researcher like me or Fabrício dos Santos into their social and political circle, they are imagining possible futures for engagement that go

13 Welch (cit. n. 10).

14 Tsuptó Buprewên Wa'iri Xavante, Barbosa Sidowe Xavante, Luiz Hipru Xavante, Agostinho Seseru Xavante, interview with the author, 4 June 2014, Água Boa MT.

beyond our narrow research plans. In a certain sense, the Xavante become the subjects of our research in exchange for the advocacy we might one day offer. They are made visible, knowable, and 'protectable' to governmental and non-governmental organizations through their scientific interlocutors. Ultimately, it is a precarious technique as it depends on the consistency, dedication, and resources of researchers who may or may not meet Xavante community members' expectations. It remains to be seen whether Fabrício Rodrigues dos Santos or I will stay 'enchanted' long enough to fulfil the hopeful futures our subjects have likely imagined for us.

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Assembled Objectivity: Categorizing Roma in Censuses, Surveys and Expert Estimates

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Production of Roma-related numbers

Roma are believed to be the largest minority in Europe with a current population of about 10–12 million people who share a distinct social profile: A low socio-economic status and level of education, and a marginal place on the social ladder. Academic, journalistic and political accounts describe the Roma as a population that migrated from India some one thousand years ago and characterize the population as having high spatial mobility and close social ties around traditional cultural practices that are often seen as deviant and opposed to those of mainstream society. But how are the numerical figures of Roma determined in this well-established narrative? By questioning Roma data production in what follows I am not claiming that Roma people do not exist; rather, I aim to demonstrate how the Roma group is made through the visible and invisible work of various experts. As I argue further, through repeated academic, administrative and police-led investigations carried out in different political regimes, people have learned how to identify either themselves or others as Gypsies or Roma. Counting and categorizing Roma are two strongly interrelated aspects of expertise that have so far not served those being studied—quite the contrary. Expertise on 'Roma issues' has developed over time to sustain the reification of the Roma category, leading to further corrective measures of repression and paternalistic management of people seen as deviating from the norm.

Counting Roma is a practice that includes or excludes individuals on the basis of arbitrary criteria that undermine their numerical assemblages. As a highly diverse group Roma do not share a common language, religion, territory, lifestyle, or physical appearance. Yet, since the eighteenth century, experts have counted Roma populations continuously through four types of quantitative methods: Expert estimates, police-led Gypsy/Roma censuses, regular general censuses and policy surveys. In this short piece, I examine the period after 1990 most closely but I look back for hints in the history of quantifying Roma (and other) populations, which involved a range of different types of invisible work: Persuasion in censuses and surveys, sampling, adjustment and post-coding of census questions.

1 In producing this paper, I benefited of the feedback and support of several people and institutions. My deepest thanks go to Veronika Lipphardt who constantly encouraged me, engaged with my work, and opened my interest for the fields of science studies and history of science. I am truly indebted for the feedback and careful editorial work of Judith Kaplan, Jenny Bangham, and Alexandra Widmer. While working on this paper, I enjoyed being a post-doctoral research fellow of Max Planck Institute for the History of Science, Berlin, and a senior fellow of the Institute of Advanced Study of Central European University, Budapest: My warmest thanks to staff and colleagues.

Fieldwork in a policy survey in the '90s

As a Sociology student in the early 1990s, I was a field worker in a countrywide research project on the social situation of Roma in Romania. Together with fellow students, I received a half-day training on sample strategy and interview techniques for the approximately thirty questionnaires that each of us had to carry out. My motivation was twofold: To gain an initiation into sociological fieldwork and to supplement my student income. To administer the rather long questionnaire (it took about forty-five minutes), I had to decide whether the interviewee was a Roma person or not. Our task was made easier by the fact that towns in which we had to search for Roma subjects were earmarked in the first population census after the fall of the communist regime as zones with a higher Roma population. The task of selecting interviewees was very much in the hands of individual field workers like me, who had to decide whether or not to record his/her interlocutor as Roma. With a polite greeting and a short formula explaining the aim of the survey, followed by ensuring the subjects about confidentiality and anonymity, I asked potential subjects to accept participation in this Roma-related survey. As I have since learned, this is what polling agencies and research institutes in their Roma-related surveys consider to be 'implicit consent', which means that if someone agrees to be interviewed in a survey pertaining to Roma identity, he or she implicitly accepts the designation of Roma ethnicity.

As the interview progressed, I asked about the language spoken in the family to double-check the subject's ethnicity. As a supplementary check, field workers had to assess whether the clothes of the subjects were coloured in Romani-like fashion or their houses were vividly painted in Romani style. Regardless, in the final section of the questionnaire, devoted to socio-demographic variables, we had to straightforwardly ask: 'What is your ethnicity?' For this question I was instructed to wait for the spontaneous answer of the subject, and if it did not readily come, to help by showing or reading the list with predetermined categories: 'Romanian', 'Hungarian', 'Roma/Gypsy', 'German', 'Other'. Subjects were expected to choose a single designation. Except for a few cases when the answer was directly 'Romanian' or 'Gypsy', most of my subjects did not sort themselves into a category as envisaged by the lead researchers. Hence, I asked, 'What is your ethnicity?' and then waited for some long and embarrassing moments. In several cases I was told, 'Tick whatever you like, what you think is best to be noted.' I felt it was incorrect to note my perception, and not that of the subject, in the questionnaire. Nevertheless, I had to circle a category. So, I insisted on having the subject's answer and I reframed the question as such: 'To which ethnic group do you belong?' Some of people answered, 'It's your choice. Put the right thing, write what you think is good.' Trying to clarify with people in front of me what 'good' means for them, I was told to circle the answer that would best serve the survey, or even what might have been good for myself, having made a long and tiring trip to visit them. This dialogue sometimes lasted for a few minutes without helping me any further to circle a single ethnic category as the questionnaire requested. In order to finalize the survey and my task of filling out the thirty questionnaires, I had to decide myself who was a Roma and who was not.

Since the 1990s, hundreds of Roma-related surveys and studies (on a variety of topics such as education, health, employment, discrimination, migration and social welfare) have been carried out in Romania and all over Europe and their findings have been broadly disseminated. Certain assumptions related to a Roma population profile, as for example marginality and poverty, have usually been set in surveys well before data gathering begins. By using various forms of external identification, social scientists have adapted their epistemic object to fit a policy target. As some researchers acknowledge, samples in social studies of Roma after 1990 occur exclusively in homogenous poor neighbourhoods contributing this way to the reification and stigmatization of the group (Rughinis, 2011; Prieto-Flores et al, 2012; Surdu, forthcoming).

Expert estimates and police led censuses of Roma

Early estimates of 'Gypsy' numbers in Europe were first assembled at the end of the eighteenth century (Grellman, 1783; Kogalnitchan, 1837; Vaillant, 1857; Serboianu, 1930). The sources and methods these pioneering scholars used to calculate the number of Roma in Europe are not documented. Similarly, late twentieth-century scholars have compiled "a rough estimate of the total number of Gypsies in Europe" (Liègeois ,1986: 47) with no sources to rely on. Yet their compilations have been assimilated and widely disseminated by the international organisation Council of Europe (CoE). For those countries in Europe considered to have the highest number of Roma, surprisingly, CoE estimates of Roma numbers remained the same between 1994 and 2007: Numbers of 'Gypsies and Travellers' published by CoE were frozen for more than a decade. This is unexpected: Within an interval of thirteen years, one would expect the numbers, if reflecting a real population, would either increase or decrease.

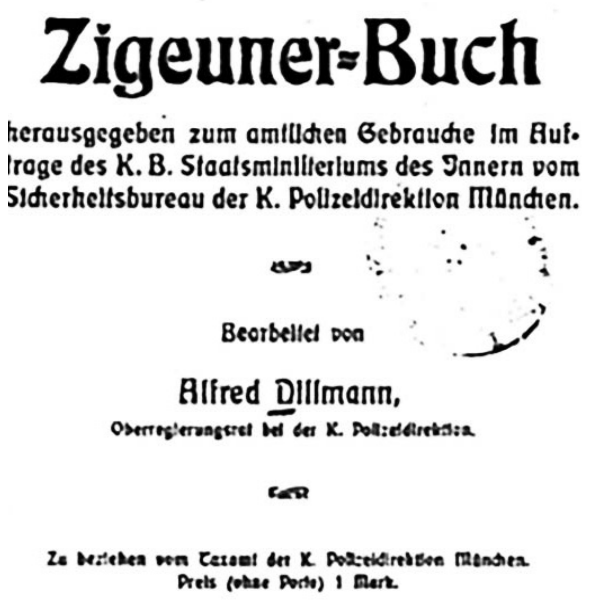
In addition to scholars, police have compiled Roma numbers for centuries by carrying out Gypsy-only censuses based on invisible, though systematic, categorization and surveillance work. In eighteenth-century France, for example, the police approached the work of defining and controlling 'dangerous classes' in a manner resembling that of bookkeeping: Police duties included establishing legal employment, recruitment, hiring and firing protocols, checking certifications, surveying changes in patronage and the division of labour, as well as the causes for refusing and leaving work. Police classed those not employed by a master or without valid unemployment certificates as vagabonds, 'masterless' or *'gens sans aveu'* (Kaplan, 1979). From the second half of the nineteenth century and much later, police would identify Gypsies by checking their papers, conducting interviews, compiling lists of suspects, and documenting family histories. Like expert surveys, police-led censuses were based on external identification and pre-determined definitions and categories while ignoring self-ascription.

Diverse types of expertise were brought to bear on the making of these numbers: The 1893 census in the Kingdom of Hungary, for example, was a matter of cooperation between police (the Ministry of Interior), professional ethnographers, and statisticians. As for the definition and registration of Gypsies, the 1893 census instructions mentioned:

> The observation of Gypsy descent and origin normally does not run into especially great difficulties. The public opinion, the folk-consciousness [Volkbewusstsein], keep a reliable, current record of those with Gypsy heritage; the anthropological character is a sure enough identification, surer than language, which is the only criteria of Gypsydom which appears in the general census. (Cited by Johnson 1998:103)

According to census instructions, all that was needed for someone to be counted as a Gypsy was to be seen as such by ‘public opinion’ or deduced as such by visual inspection. While the calculation of percentages and the making of moral statistics was the work of statisticians, police were responsible for on-the-ground collection work.

Defining a Gypsy population and counting it was also a police venture in the 1895 census of ‘nomads, Bohemians and vagabonds’ in France (Kaluszynski, 2001; Filhol, 2007; Asséo, 2007; About, 2012) and in the 1905 census of Zigeuner in Bavaria (Lucassen, 1998) (Figure 1). Police-led Gypsy censuses have continued ever since. Under communist regimes in Romania and Bulgaria, for example, official censuses were considered unreliable, and again the task of counting Roma was assigned to the police[2]. Although Roma were not part of public discourse or academic inquiry in these countries during communism, they continued to be subject to police surveillance.



Zigeuner-Buch

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Figure 1: Cover of the Zigeuner Buch (1905) written by the chief of Bavarian police Alfred Dillmann. The census of Gypsies initiated by Munich police was the first step for the total registration of Roma and Sinti in Germany and later, during National Socialism, to deportation and extermination of many of them in labour camps.

2 Romanian census documented in National Archives of Romania: Document 25/336, Fond C.C. al P.C.R secţia organizatorică, nr. inventar 3292, 1978. Police census in Bulgaria mentioned in Liègeois, 1994, 2007; World Bank, 2000 and UNDP, 2002.

Persuasion and propaganda labour in interwar censuses

New statistics about Gypsies became available in the interwar period, with regular censuses proposing to measure ethnicity according to a procedure in which the task of identification, at least in theory, was left directly to subjects. The census registered a Gypsy ethnicity, and created a way for the subject to choose his or her own identity. For census numbers to be collected and assembled, an enormous feat of persuasion had to be practically accomplished, work that has received less attention from scholars analysing censuses as a form of identity production. Human sciences, as Igo (2011) put it, are "solicitous sciences". In the case of censuses and surveys, field workers, census takers, and statistical agents must consistently undertake persuasion work to create subjects of inquiry. Censuses not only involve the work of definition, assemblage and data processing, but above all, they require people's cooperation. As one of the experts working on the first modern Romanian census in 1930 stated, this cooperation had to be ensured: "[…] by means of active and massive propaganda timely executed so as to enlighten the people about the aim and utility of the census". (Colescu, 1930: 807)[3]

As a means of propaganda, experts recommended that the state use printed media, leaflets, posters, cinematography, radio, as well as school and church networks. As one of the documents about the organization of the 1930 census attests, statisticians also calculated propaganda tools in numbers: Romanian state authorities printed and distributed 240,000 colour wall calendars, 250,000 leaflets for census takers, 20,000 posters for exhibition in trains and buses and about 5,000,000 postage stamps; in addition, they broadcasted 8,000 meters of propaganda movies, introduced millions of stickers in cigarette packs, and published thousands of articles in popular media (Figure 2).[4]

Figure 2: Post stamps issued and distributed with the occasion of the first modern census in Romania in 1930.

Beyond mass-media propaganda, face-to-face meetings were also considered important tools for convincing people of the utility and benefits of the census. Teachers and priests were seen as significant state agents for census-taking given the trust they enjoyed and their ease in approaching

3 Quotes from Romanian language publications are author's translations.
4 The document *Populaţiunea actuală a României*, published by the General Direction of Population Census, Bucuresti, 1931.

individuals in professional and private settings.[5] One of the census experts explained the role of teachers and priests for census propaganda:

> [...] propaganda through schools and churches is required to accomplish great services. In schools, by awakening on the one hand the curiosity and interest of the pupils, the teacher will guide them to disseminate at home, in their families, the words of light and faith about the necessity and benefits of the census. The same great work could be easily and effectively done by priests from the church pulpit as well as in their private discussions (Colescu, 1930: 819).

Coupled with this propaganda, seen as popular enlightenment, experts advised that it would help to remind people that census participation was compulsory by law and that a lack of co-operation would be punished. The census takers were seen as state agents on duty rather than paid field workers, and the whole census process was compared to that of a well-prepared military campaign in which the census takers were the soldiers and the central statistical office was designated as the commandment of the army (Colescu,1930).

Post-coding of ethnicity question in 2011 census in Romania

Post-coding has had an essential function in producing groups quantitatively from free-response questions on the Romanian census. Without post-coding, the numbers simply do not stick together. The procedure of post-coding is usually not explained to the wider public, as it is considered too technical and difficult to be understood by non-experts, for whom such processes are accordingly irrelevant. Moreover, explaining this complex procedure does not lend more credibility to the data, on the contrary, it threatens to shed light on the constructed character of ethnic numbers. Side-stepping the description of statistical work that goes in to censuses (and surveys) makes ethnic categories appear to be natural kinds rather than statistical working tools.

To exemplify the invisible work in coding of ethnic census categories, I will turn now to the last Romanian census, in 2011. In this census, ethnicity was an open and optional question, an item that subjects freely decided whether to answer or not. Accordingly, a rather large number of people preferred not to assign themselves to any particular ethnicity. Social scientists and experts unanimously interpreted this preference, which seems a reasonable choice, as the 'greatest anomaly' in the history of modern census taking in Romania since 1930. The number of non-answers, reflecting a refusal to identify with a specific ethnic category, was so big that it defied all expectations. The expert conclusion was, strangely, that people of an undeclared ethnicity would be in fact Roma unwilling to disclose their true ethnicity.

In the 2011 census in Romania, 621,573 people were found to be of Roma ethnicity despite the fact that not all of these people declared themselves as such. As could be found in the

5 See for example Colescu (1930) and Sanielevici (1931).

census manuals, the category 'Roma' was a highly composite one, uniting no less than 19 different sub-categories under the umbrella term, 'Roma'[6]. Most of these sub-categories were names of occupations considered by some anthropologists to be specific to Gypsies/Roma: brick maker ('cărămidar'), musician ('lăutar'), bear trainer ('ursar'), flower seller ('boldean'), tinsmith ('spoitor') or tinker ('căldărar'). While 19 sub-categories linked to Roma were formed, the Hungarian minority had three sub-categories and the German minority accounted for just seven. As disaggregated data for sub-categories were not provided, it is in fact unknown how many people chose the label 'Roma' which was one of the nineteen sub-categories. If people would have liked to record themselves by the name 'Roma' they would have done so and not given other answers to the open question related to ethnicity. As Latour has put it:

> *Numbers* are one of the many ways to sum up, to summarise, to totalise as the name 'total' indicates - to bring together elements which are, nevertheless, not there. (Latour, 1987: 234)

Just before the 2011 Romanian census, people were consistently encouraged to declare themselves as Roma by NGOs advocating for Roma rights but also by the chief of the Romanian Institute of Statistics. There were financial stakes behind this mobilization: the more Roma the country had, the more European funds would be received for their social inclusion. It is hard to estimate how much these public appeals contributed to the making of official ethnic statistics, yet they were part of a work of persuasion and assemblage that has contributed to the making of Roma numbers.

Conclusions

Scholars, international organizations, governmental representatives and policemen, past and present, led the categorization and counting of Roma. These different actors and institutions assembled a Roma population by shaping the group according to an institutional perspective in line with their positions and interests than those of the people being categorized. The knowledge derived from expert estimates, whether they were based on police or census data, was used to give contour to an object of knowledge, and, moreover, an object of political action.

Less essentialist notions of Roma ethnicity can be realized through the use of non-exclusive ethnic categories—by giving subjects the possibility, in other words, to choose more than one ethnic category (a procedure used for example in the 2011 Hungarian census). Nevertheless, so far, most of the counting methods advanced in censuses and surveys have imagined ethnicity as an essential data point, ignoring the fact that ethnic categories are not exclusive, but intertwined.

6 In National Institute of Statistics (NIS) document "Nomenclatorul etniilor şi limbilor materne" (Nomenclator of ethnicities and mother tongues) are given 19 sub-categories. Even more sub-categories for Roma (32) are presented in NIS (2011, p.98) document "Manualul Personalului de Recensământ" (The Manual of Census Personnel).

The processes by which statistical knowledge about Gypsy/Roma people is produced rely largely on expert or external presuppositions, rather than self-identification. The numbers, once launched in public discourse, seem to take on a life of their own that is independent of where they are produced, of their rationale and methods of calculation. Roma population estimates are conveniently imported from one field to another by the virtue of their purported objectivity as numbers. High numbers of Roma produced by experts serve political discourses (in the past combating the Gypsy nomadism and a deviant life style, currently alleviating Roma poverty, unemployment and social exclusion), which promote the perception of emergency, moral panic and risk.

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Invisibility as a Mechanism of Social Ordering: Defining Groups among Laboratory Workers

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It is easy to assume that invisibility indicates systemic oppression. Often it does, such as the unrecognized work of slaves who built the physical and social infrastructure of the United States, or the women whose contributions as 'computers', bubble chamber operators, scribes, and researchers in their own right were long omitted from histories of science. It is true that invisibility reflects a society's divisions of power. However, invisibility can also empower. Freedom from scrutiny, surveillance, and documentation can allow people to structure their own behavior, work, and knowledge, and thereby to thrive. Laura Stark (this volume) illustrates a fascinating case of medical research volunteers who strove for invisibility, as a way to legitimate their service. Furthermore, invisibility—like all power relations—is dynamic. John Law reminds us that a single stable "social order" does not exist; there is only the ongoing, shifting process of "social ordering" (Law, 1994: 1–2). Workers negotiate their labour relations in every interaction; sometimes the invisible are in charge, while in the next conversation they may be oppressed. Invisibility is therefore not always a social problem, though it is always a revealing sign of how people construct and maintain their relationships and hierarchies. As such, it is worthy of study and (sometimes) activism. Ways to investigate invisibility without assuming oppression include asking crucial questions such as who is invisible, to whom, in what ways, and in what contexts. These questions will remind us, as researchers, whose perspective we are perhaps inadvertently taking—most likely that of the powerful visible.

I began my PhD thesis as a study of what seemed to be classic "invisible technicians" in science laboratories (Shapin, 1989, 1994). Fossil preparators chip the rock off fossils, literally shaping the specimens that are the basis of our knowledge about evolution and Earth history (Figure 1). But how a fossil is prepared for study—and by whom—is rarely described in scientific publications. Preparators' names are not listed as authors, though they are sometimes thanked in the acknowledgements. There are no protocols, no authoritative manuals, and few publications about fossil preparation practices; instead, preparators use a variety of methods and tools to reveal fossils and to repair and reconstruct them (Wylie, 2009; 2013; 2015). It seemed to me, based on experiences and observations gained from my undergraduate job as a fossil preparator, that preparators' work, expertise, and identities were being left out of science, to the detriment of preparators as well as current knowledge about fossils. I rushed to the rescue, to observe preparators in their labs, interview them, and tell the world what they do.

But fieldwork held surprises, as it so often does. I saw no researcher–tyrants screaming instructions at cowering, docile preparators, but the opposite: preparators were controlling the physical space of the lab and the practical decision-making space of fossil preparation. As an example of a typical interaction, I witnessed researcher Henry bringing a slab of rock containing tiny mammal fossils to the preparation lab. He asked preparator Kevin about the "feasibility" of "breaking this apart … to get little bones out."[1] This discussion could be interpreted as an expert scientist informing a less-qualified, lower-status technician of the important aspects of a specimen while assigning the technician to do less-skilled, lower-status work of specimen preparation. However, Henry asked Kevin about "feasibility"—he wanted Kevin's opinion. Only when Kevin said that it was possible did Henry's question turn into a work request. Then Kevin said thoughtfully, "A little acid?" and went to try dissolving the rock with acid. He was thinking out loud rather than asking permission, and Henry gave no instructions. Deciding how to prepare the specimen—what tools and materials to use, which tasks to do first, and even crucial judgments of what is fossil and what is rock—is the domain of the preparator. As a result of preparators' claim to the tasks of choosing and applying preparation techniques, researchers tend to act more like visitors to the lab and seekers of information than overbearing micromanagers.

Figure 1: A preparator removes the crumbling rock around a fossil with a small metal pick. The fossil—at his tool tip—is dark grey, surrounded by lighter-coloured rock. Distinguishing the fossil from the similarly-coloured rock depends on experience and skill, as does removing the surrounding rock without damaging the fragile fossil. (Author's photograph)

In addition to controlling their methods and work practices, preparators organize their labs by deciding who will work on which fossils; they organize their field by training novices, often selecting new hires, and defining desired skills through their recently established Association for Materials and Methods in Paleontology (Brown et al., 2010; Jabo et al., 2010; Wylie, 2013:

1 All names are pseudonyms.

chapter 2; AMMP, 2016). They thus have "craft control", unlike many technicians who must follow strict directions and who are hired and managed by people who are not technicians (Keefe and Potosky, 1997: 78–9). For example, though they are excluded from researchers' publications, many preparators build their own community by giving conference talks about best practices (both at scientific societies and their own preparation-specific meetings), and by visiting other labs to observe different ways of working or to give training workshops. A few preparators even publish papers about methods and training strategies in extensive online forums (e.g., the PrepList, the Society of Vertebrate Paleontology's Preparators' Resources page), self-organized conference proceedings, and scientific journals, such as The Geological Curator and even, though rarely, the Journal of Vertebrate Paleontology. These workers were not oppressed, I realized (Wylie, 2013; 2015). Actually, they were thriving under their own leadership, both in the lab and as a field.

I argue that it is a strong sense of division of labour that creates a sheltered space for preparators to define their own methods and identity. From the outside, it seems that researchers are more powerful, with their higher status and salaries, their impressive academic degrees, and their published knowledge claims. And it is true that researchers are the founders and funders of fossil preparation labs. But from the perspective of everyday work, as well as the physical shape of the fossils, preparators are in charge. Seen from their perspective, the lab is not a place of oppression but rather of divided labour, with researchers most often writing papers and grants in their offices while preparators work with specimens and tools in the lab. Andrew Abbott (1988) describes this construction of divided labour as a process of groups claiming 'jurisdiction' over different problems, tasks, and skills, and thereby defining each group's domain of power as well as their professional identity.

The perceived divide between the jurisdictions of researchers and preparators is deepened by each group's ignorance about how to do the other's tasks. Researchers and preparators are not high- and low-status members of the same group, therefore; instead, they see themselves as separate work communities. In Pierre Bourdieu's terms, because these groups have distinct capital (skills and expertise), doxa (knowledge and beliefs), and habitus (practices), they are separate fields (1993: 72–73; Wylie, 2013: chapter 3). This perceived distinction is both a cause and a result of severe conflicts that I witnessed in rare situations of jurisdiction violations, i.e., when preparators did research and researchers prepared fossils. One preparator described research as high-status work that was reserved for—and ruthlessly protected by—researchers:

> We started doing experimentation on adhesives and consolidants, … comparing things. We started setting experiments and that was too scientific, too much like research for our boss and he put the kibosh on it. In fact he stole our glues! … It was basically anything that was not simply removing rock from bones and sticking [fossils] back together and putting them in a shelf was frowned upon by our boss.

This preparator suspected that this researcher–boss considered preparators' research a threat to his own status. Likewise, preparators are offended when researchers prepare fossils, because that is not their jurisdiction. For example, when a researcher tried to prepare a fossil and broke it in the process, preparator Brent criticized him both for offending the fossil's preparator, Jane, and for interfering in the preparators' domain: "So he broke [Jane's] specimen. God … He just has no respect for anyone else. This is [Jane's] effort … He should just let her do the prep. Back away from the fossil" (Figure 2). Tellingly, Brent ascribed ownership of both the fossil and the work of preparing it to the preparator, not to the researcher who had collected the fossil and would publish about it and whose grants funded the preparators' jobs. However, because researchers have higher status, they are likely to have the power to confiscate glues and dismiss preparators to prevent boundary crossing. In comparison, preparators can only complain and resist, such as, in one case, installing locks on the specimen cabinets to prevent a rogue researcher from stealthily preparing the fossils. The divisions between these groups are thus locally obvious and fiercely enforced, despite workers' seemingly closely related tasks.

Figure 2: A fish fossil that a researcher broke, shown after repairs by the preparator. As the glue reuniting the two halves of the specimen dried, the preparator tried to protect the specimen with warning notes, signaling her power over the fossil and the lab space. (Author's photograph)

The strictness of these boundaries both explains and is enacted by preparators' absence from researchers' papers. Omitting preparators from publications serves to set them aside from the research community, but this omission is also justified by the belief that preparators and researchers belong to separate fields. Invisibility is thus a dynamic and continuous method of defining groups. For the same reasons, researchers are invisible in preparators' everyday work.

Both groups rely on each other, but they see their work as complementary rather than overlapping. Law (1994) explains this 'deletion' of groups as necessary for social ordering—creating and representing a certain social structure within an organization. He ascribes the concept of deletion to symbolic interactionists, sociologists such as Susan Leigh Star (1992) who study local, microsocial interactions as the basis of social order. While ordering a society, Law argues, people and things must be deleted, meaning excluded and omitted, to reduce reality to manageable patterns and to create social meaning and structures. Deletion, as a way of simplifying and focusing to represent a certain social order, isn't necessarily bad. But in his ethnography of a synchrotron laboratory, Law observes how workers 'delete' each other based on group identity but also on status: "Outsiders tend to delete the work—and particularly the heroism—that is involved in the efforts of others. And they tend, in particular, to delete the work of subordinates" (Law, 1994: 131). Thus, problems arise when ordering includes "ranking" people and things by status and value, and accordingly deleting the unvalued as a form of "disenfranchisement" and "silence" (Law, 1994: 113, 116, 132). The power of deciding who and what to delete is the site of inequality and unfairness. Law observed that, like preparators, synchrotron technicians felt underappreciated but also enjoyed moments of craft control allowed by their deletion: "[Technicians] tell of autonomy, of being left to get on with a responsible job like running the machine overnight. They don't necessarily mind being ignored" (1994: 133). We see here the striking complexities of the multiple effects and perspectives of social ordering, which workers are always simultaneously enacting, resisting, negotiating, and constructing.

Without the sense of their fields' separateness and the resulting invisibilities of outsider groups, I surmise, the fossil laboratory community would look very different. If researchers had to describe how a fossil was prepared in a publication, then they would pay more attention to preparators' decisions and actions. They would stop deleting the work and the workers. They would probably become more involved in the everyday life of the lab, by supervising preparators' work and by suggesting or perhaps even ordering methods, despite their usually limited knowledge of preparation techniques. They might begin hiring preparators themselves, selecting for scientific credentials more than experience or the skills that preparators value. Because preparators' work would be published in their papers, researchers would be more invested and interested in it. This could detract from preparators' autonomy, by limiting their power over their work and their community. Susan Star and Anselm Strauss observed that making workers and their work visible can cause "the eradication of discretion from skilled workers" as a result of increased supervision or newly standardized tasks (1999: 20–21). Thus preparators might be less powerful when visible, if they become lost in the background of scientific papers instead of designing methods, training new preparators, and managing a workplace.

This case shows the complexities and multidimensionality of invisibility. Social order is not always as it appears. Workers may be oppressed, overlooked, and unappreciated for reasons of social status, class, gender, race, and many other factors. On the other hand, they may be empowered craft workers whom we are viewing from the perspective of a separate field that does not include them and from which they distinguish themselves. Workers also oscillate between these extremes and most often operate somewhere on a spectrum between 'oppressed' and 'empowered'. The concept of invisibility is not static or universal, though it is a widespread way of differentiating groups and creating a social order based on local priorities. Invisibility is selective and purposefully so, for reasons such as those described by Star and Strauss: "Visibility can mean legitimacy, rescue from obscurity or other aspects of exploitation. On the other [hand], visibility can create reification of work, opportunities for surveillance, or come to increase group communication and process burdens" (1999: 9–10). It is our mission as researchers to understand the ongoing construction of social relations, and therefore the multiple roles that invisibility can play. It is also our duty to investigate the functions and implications of these social systems, to understand why actors order their worlds in the ways that they do.

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Process

Self-inscription and (In)visibility: The Oneida Language and Folklore Project

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Andrew Beechtree, a member of the Oneida Nation of Wisconsin, committed the following words in pencil to a spiral-bound notebook in 1941:

> During these lean years of the Great Depression, the whole nation was hard pressed to furnish employment to its millions of unemployed inhabitants. The result was the national, state, local government officials and every other capable organization was thinking up work or occupations of many types and descriptions. It will take no great stretch of imagination to be convinced that the Indian early became part of this great army of unoccupation […]. It was thus then that the University of Wisconsin sponsored what is called a social research project. By a streak of fate (some Oneidas say it was through the intervention of their native God, Dehaluhyawá.gu), the once eminent but now humble Oneida Indians, a former member of the great Iroquois Confederacy, were given consideration. The project affecting us, the Oneidas, had for its object the recording, for the first time, of the language…of the Oneidas, in a methodical or scientific manner…The results from this undertaking were so satisfactory and interesting that a correlated research project, the historical study, was immediately sponsored and approved. This embraces the writing of biography, autobiography, and consulting newspaper and other records. (Beechtree cited in Lewis, 2005: xvii)

As a member of the great army of unoccupation himself, Beechtree was paid for his writing as a participant in the historical study to which he referred: The Oneida Ethnological Study, sponsored by the Works Projects Administration (WPA), a New Deal initiative that put millions of unemployed Americans to work on public improvement projects after the Great Depression.[1] This historical study was successor to the Oneida Language and Folklore Project, which was initiated in 1938 with a mandate to record the unwritten Oneida language before its complete extinction. This project sought to impart a novel 'scientific' alphabet to Oneida students during a two-week intensive course, preparing them to interview elders still in full command of the language. Participants were paid $45 per month for their labours, which organizers hoped would ultimately contribute to the production of an Oneida dictionary, hymnal, chronicle, and folklore collection—source materials for linguistic revitalization among younger members of the community.

Beechtree's notebook, along with 166 others like it, was brought back to the University of Wisconsin-Madison, where it was eventually consigned to a vast basement storeroom in the Social

1 The initial proposal was funded under the aegis of the Federal Writers Project of the Works Progress Administration.

Sciences Building overlooking Lake Mendota. Forgotten there for some sixty years, the notebooks were only recovered in 1998 by dint of some "anthropological sleuthing" on the part of Professor Emeritus, Herb Lewis (University of Wisconsin, 1999).[2] Though Lewis had originally been looking for something else—biographical materials on the eminent linguist and University of Wisconsin alum, Floyd Lounsbury (1914–1998)—he immediately recognized how significant the Oneida notebooks would be for cultural anthropology and revitalization efforts. The collection was swiftly processed and transferred to the Wisconsin State Historical Society: Copies were "donated" to the Oneida Nation.[3] Beechtree's words were then transcribed by Elizabeth Hassemer (who, in toto, prepared more than 18,000 pages of handwritten text for editing and processing), and printed as an epigraph in the 2005 volume *Oneida Lives: Long-Lost Voices of the Wisconsin Oneidas* (Lewis, 2005: xiv).

The Oneida Ethnological Study, like the Language and Folklore Project that preceded it, provides an opportunity to reflect on (in)visibility as a process in the history of the human sciences—in this case, linguistic anthropology. Those members of the Oneida Nation, like Beechtree, who gave their voices and stories to the projects, were explicitly recognized; Their very individuality being part of the 'methodical or scientific manner' of language documentation in question. Indeed, visitors to the Language Archives of the Oneida Nation webpages find those collections organized by contributors' names.[4] Similarly, intermediaries like Hassemer have been acknowledged publicly for their labours and legacies—*Oneida Lives* features an especially lengthy preface in this regard, one that opens with a personal reflection on the collection by Gerald Hill, the Oneida lawyer who brokered the copies' return (Lewis, 2005: ix-xii). These publications, furthermore, are filled with thanks for the successive interventions of university researchers and their federal grantors. With such copious acknowledgment, what labour could possibly remain to be seen?

Posing Invisible Labour as a question in this way can promote historical understanding of research practices in the human sciences. Thinking with this analytic in the case of the WPA-sponsored Oneida research, my mind shifts from participants and the politics of representation to the processes of language documentation and archival preservation. As scholarship on the history of archival and data practices has lately pointed out, scientific collections are often remarkably contingent and fragile in nature, despite claims as to their stability and permanence (Lemov, 2015). We see this in the material history of the Oneida Ethnological Study notebooks, the misplacement of which likely happened when Lounsbury, who served as director of the project for two years, was abruptly called to serve in World War II.[5] Their initial loss can be explained by the fact that they were associated with the university researcher rather than the Oneida speakers and writers involved. Such contingency and fragility points to an even more fundamental problem for the language historian and/or the historian of linguistics: The relative conservatism of oral versus written forms of language transmission. In a sense, the basic commitment of these

2 "Rediscovered native history notebooks donated to Oneida", University of Wisconsin Press Release, 6/1/99. University of Wisconsin Archives, Morris Swadesh Faculty File.

3 Ibid.

4 See https://oneida-nsn.gov/Language/Archives/ accessed 1/9/2015.

5 "Rediscovered native history notebooks donated to Oneida," University of Wisconsin Press Release, 6/1/99. University of Wisconsin Archives, Morris Swadesh Faculty File.

studies—reflecting 'salvage anthropology' more generally—was that inscriptions (whether in print, audio, or lately video media) stood a better chance of surviving over time than did oral tradition alone. The archival impulse here was to preserve the ephemeral present for the enduring future by 'scientifically' writing it down. And the stakes were undoubtedly high: According to UNESCO standards, the Wisconsin dialect of the Oneida language was "critically endangered" throughout the twentieth century, with just three native speakers still living in 2011, all of them over 90 years old.[6]

The move from spoken to textual language is a process that conceals some content in order to reveal other features. As scholar and theologian Walter J. Ong pointed out years ago, both modes of communication are interpretive and temporal events—textuality, according to Ong, does not have a unique claim on hermeneutics (Ong, 1988). For him, "meaning is not assigned but negotiated, and out of a holistic situation in the human life world". That situation—full of non-verbal cues and information—is to varying degrees "shared by speaker and hearer in oral communication, but in written communication is generally not shared" (Ong, 1988: 267). Thus, we lose perspective on the 'interpersonal negotiation' with inscription, while perhaps gaining the ability to see formal characteristics and categories of language instead. Writing obscures communicative action, in other words, but reveals stabilized objects fit for comparative analysis.

How do these general (and perhaps obvious) reflections correspond to the history of documentary practice? Morris Swadesh (1909–1967), who launched the first Oneida WPA project in 1938, was well known as an innovator in linguistic method—work he laid out explicitly in correspondence and published papers. A primary consideration, as he made clear in private exchange with colleague Dell Hymes, was the choice of interlocutor. For instance, in support of Hymes' graduate research on Sinslaw, an indigenous language of the Pacific Northwest, Swadesh gave the names of potential 'informants', noting their ages alongside other socio-linguistic observations. One of them, Billy Dick, age 71, "last summer was still working regularly on the logging boom", Swadesh told Hymes. "He sometimes drinks heavily. His fault is that he tries too hard to form expressions to meet your request".[7] Though full consideration of the pathways and infrastructures that led Swadesh to the identification of suitable informants is beyond the scope of the present essay, this is one potentially shadowed corner of research practice that that might be illuminated by paying attention to the visibility of constituent labour practices.[8]

But what of the nature of the linguist's research query—that which Billy Dick might have struggled to answer? Such 'elicitation' attempts—in which items on a protocol list are presented for translation—have typically depended on the presence of a translator or an intermediate language held in common by interviewer and interviewee. Here, the very conditions that threatened to extinguish the Oneida language—forced acculturation to English in government-run schools—ironically made it possible for participants on all sides to talk to each other.[9] Common knowledge

6 See http://uwdc.library.wisc.edu/collections/Oneida/about accessed 1/9/2015.

7 Swadesh to Hymes, 1/2/1954. APS Ms. Coll. Dell H. Hymes Papers No. 55 Series I, Morris Swadesh correspondence.

8 The role of the Bible translation organization, SIL International, is particularly salient in this regard. See the special issue of *Language* (September 2009) on this topic.

9 As the University of Wisconsin (1999) reported in its press release on the subject, "One woman recounted that

of English, reflecting this violent history, was essential to the design of the study. Moreover, the project was unique among WPA-sponsored cultural heritage initiatives in that it depended on Oneida speakers trained to write their own language interviewing each other directly without the intervention of academic researchers.[10] As the *Milwaukee Journal* reported in February 1939, this made the Wisconsin study "the only white collar WPA project among the Indians".

That said, in the tradition of Franz Boas`, Swadesh and his supporters blended activist pedagogy with theoretical interests in the Oneida language as such (Darnell, 2001). In this connection, Swadesh gave Lounsbury the following instructions on data collection: "Be sure that the native word corresponds with the meaning intended. Use the semantic key, and watch out for ambiguities in the English like these. <u>back</u> body part rather than direction, <u>bark</u> of tree, <u>blow</u> with mouth, <u>burn</u> intransitive, <u>child</u> young person rather than kinship term, <u>day</u> opposite of night rather than abstract measure...", and so on.[11] These directives illustrate Ong's claim about negotiated oral meaning quite well, even if such ambiguities are ultimately concealed from readers of the final published text.

Though my sense is that Swadesh and other like-minded researchers shared a genuine commitment to linguistic and epistemological pluralism, there is no doubt that external pressures—material, professional, comparative—biased their research and textual outputs towards the cultural context of the dominant language (English). In a paper for *American Anthropologist*, Swadesh urged novice fieldworkers, for example, to: "*Record a few hundred short utterances*…It saves time to have a list of English words made out in advance. Any list will do. If the informant cannot give you the equivalent of one of the words in your list without long thinking, skip it" (Swadesh, 1937: 728-32). It was on the basis of such one-sided negotiations that he and Lounsbury developed the novel Oneida orthography. While their efforts to revitalize the Oneida language through literacy training have paid significant dividends to that community, it is worth asking (a) to what extent the media that they engaged bear invisible structural imprints of the dominant language, and (b) to what extent an Oneida language thus revitalized challenges tacit assumptions about the clear-cut division between 'native' and 'non-native' speakers (Swadesh, 1960: 39-42).[12] How would one label, for instance, a speaker who gained fluency through inter-medial, rather than inter-generational, transfer?

What absolutely *was* clear at the time was the extent to which the Oneida linguists and historians were in fact labouring. This understanding stemmed from a New Deal brand of politics that once characterized the state of Wisconsin. For example, Swadesh made his progressive commitments clear to project member John A. Skenandore in a 1939 letter (Figure 1). Swadesh

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she had been reprimanded for speaking Oneida in school by having 'a rag tied around my mouth all one day.'"

10 Lewis here reports brass tacks: "Bilingual and literate Oneidas were invited to apply to work on the project, and about twenty were selected for preliminary instruction and testing. Eventually, two women and nine men were employed on the project for eighteen months" (Lewis, 2005: xxxiii).

11 APS Ms. Coll. Floyd Lounsbury Papers No. 95, Folder 15: Morris Swadesh correspondence.

12 For a more technical discussion of the question of structural imprints, see Swadesh's 1960 paper "On the unit of translation". Fiona McLaughlin and Thierno Seydou Sall provide an excellent case study, presenting the perspectives of both linguist and 'informant' in their essay, 'The give and take of fieldwork…' Significantly, McLaughlin concludes that the concept of 'native' speaker hinders, rather than helps, her research.

was leaving the project, and was writing to show his appreciation for Skenadore's involvement, emphasizing its value as labour:

> […] even if I'm not there I still want the project to succeed and I appreciate what you are doing to keep it going in the best way. I particularly realize that the Workers Alliance is necessary to keep the WPA. I know that if it wasn't for the Workers Alliance there would be a big cut in the WPA and the Oneida language project never would have got started. I want to show my appreciation to the members of the project who belong to the Workers Alliance [...] I want to give you as a present a two-month subscription to the Daily Record…It has news about the labor movement and tells the true facts about what is going on in the world from the worker's point of view […] Many newspapers tell things about the WPA to make it seem like the people are only killing time, loafing, wasting money. They try to make people believe that WPA workers are lazy, dumb, useless. They want people to believe that labor unions are vicious and radical and destruction [*sic*] and dishonest. People ought to know the true facts so they can form correct opinions. That's why we ought to read a newspaper that shows the other side.[13]

Not only did Swadesh reveal a keen fundraising acumen with these words, he also defined cultural expression and stewardship as labour in explicit terms. Stadler King, who conducted several interviews as a WPA labourer, gave proud voice to 'the other side', shedding light on the Oneida contribution while decentering the role of university researchers and government patrons:

> We have made words for [an] Indian dictionary. We have written stories which we solicited from many of the Oneidas [...] We have translated all the Indian stories and the words for the dictionary into English. We have written biographies of some of the older people. We have written in Indian and translated some old Indian medicines which they used long ago. All the work has been done by Oneidas under the supervision of some university students. (King, cited in Lewis 2005: xxxiv)

On this telling, the administrative labour of some unnamed university students was pushed to the background, offsetting the more visible documentary labour of the Oneidas.

Historical understanding of these WPA projects may well have been inverted if the notebooks' pencilled pages had faded or never been found. Part of the significance of *Oneida Lives* therefore lies in its demonstration of the processes that variously render research labour visible or invisible. Still, questions of process remain: How may historians interpret the tight bond between endangerment, salvage, and inscription; what negotiations gave rise to the 'methodical or scientific' writing system employed in these studies; and what power inheres in the structures of English as a medium of translation and access?

13 Swadesh to Skenandore, 5/4/1939. APS Ms. Coll. Floyd Lounsbury Papers No. 95, Folder 15: Morris Swadesh correspondence.

Darnell, R. (2001) *Invisible Genealogies: A History of Americanist Anthropology*. Lincoln and London: University of Nebraska Press.

Lemov, R. (2015) *Database of Dreams: The Lost Quest to Catalogue Humanity.* New Haven and London: Yale University Press.

Lewis, H. (2005) *Oneida lives: Long-lost voices of the Wisconsin Oneidas.* Lincoln and London: University of Nebraska Press.

McLaughlin, F. and Sall, T. S. (2001) The give and take of Fieldwork: Noun classes and other concerns in Fatick, Senegal. In Newmann, P. and Ratliff, M. (eds.) *Linguistic fieldwork.* Cambridge: Cambridge University Press, 189–210.

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An Uneasy Archive: Alan Lomax, Labanotation, and the Disappearing Body

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In 1981, the American folklorist Alan Lomax wrote that the globe's growing store of filmed footage was "by far the most valuable data extant for the study of human beings, far more meaningful than written or printed documents".[1] He then described his dream of:

> […] a great library of the visual arts, where all important cinematic documents would be stored, catalogued, and analyzed. Such a temple of knowledge would cost no more than an atomic submarine, but its influence would far outrun the famed library of Alexandria or, indeed, all the libraries that ever existed, since it would preserve a living, moving record of all human behavior.[2]

This archive, Lomax mused, could serve as a basis for entirely new forms of scholarly analysis, approaches that made visible the too-often ignored experiences of embodied daily life. This quixotic library was, of course, never more than a fantasy, but Lomax did achieve a measure of success through his own work on a project that came to be known as 'Choreometrics'.

Between approximately 1965 and 1985, Lomax and his collaborators—dance experts Irmgard Bartenieff and Forestine Paulay—collected and analyzed filmed samples of dance from nearly two thousand cultural groups (consisting of 250,000 feet of raw footage), arguing that dance was an untapped resource for understanding humanity. It was dance, Lomax contended, that held the key to adaptive human flourishing; the patterns of movement observed in dance both reflected and helped ingrain the everyday kinetic behavior needed to work, remain healthy, and thrive in a particular setting. The stooped posture and "deep shoulder rotation" characteristic of West African dance, for example, mirrored the widespread use of the short-handled grubbing hoe in agriculture.[3] Similarly, Eskimo dance prominently featured explosive elements, as such movements represented "one effective way to generate heat in the extreme cold".[4]

Whether interviewing ordinary Americans in the wake of Pearl Harbor or recording the songs of southern sharecroppers, Lomax's folkloric career was dedicated to the deeply political work

1 Alan Lomax, "The Treasury of Dance on Film," from unpublished manuscript for *Dance and Human Culture*, 1981, Box 4/18-01, The Alan Lomax Collection at the American Folklife Center, The Library of Congress. (Hereafter referred to as 'Alan Lomax Collection.')

2 Ibid.

3 Alan Lomax, "Dance and the Everyday," from unpublished manuscript for *Dance and Human Culture*, 1981, Box 4/18-01, Alan Lomax Collection.

4 Ibid.

of making the unseen seen, the silent heard, the intangible felt (see Szwed, 2011). This paper considers Choreometrics as an imperfect episode in this broader quest. First, it examines why Lomax believed that capturing, analyzing, and publicizing the seemingly ephemeral experience of dance was so profoundly important. Second, it explores the invisibilities Choreometrics created and depended upon, showing how and why the work required to produce Choreometrics—from the creation of the films Lomax drew upon to the physical labour of the dancers they depicted—was often hidden in its public presentation.

The story begins, however, with that tantalizing promise of total visibility. As already noted, Lomax believed that paying attention to dance would allow anthropologists to gain new insight into long-standing questions in the field. Answering these questions was not, however, the only—or perhaps even primary—goal of Choreometrics. Fundamentally, Lomax wanted to convince both anthropologists and the public at large that cultural practices like dance were not mere window-dressing on the human experience, but were, in fact, crucial to human survival. While non-human animals depended on genetic change to produce new adaptive behaviors, humans, Lomax argued, passed on knowledge about how to thrive in varied environments through symbolic cultural codes. Community dances in tropical climates, for example, taught gardeners the "graduated and flowing" habitus necessary for agriculture, just as dances in northern Europe and Asia schooled mountain hunters in the rigid body postures they assumed when poised to strike.

Such enculturated labour, Lomax contended, was absolutely necessary, but was also frequently ignored. As dance scholar Lynn Brooks Matluck (2001) has written, dance as a field long suffered from the curse of 'double invisibility', overlooked first because it was relatively difficult to record, and, second, because it was so often associated with women. Lomax hoped that studying dance closely would alter this state of affairs—not only transforming the picture of *how* culture worked, but of *whose* labour propelled it.[5]

Lomax also sought to combat the comparative invisibility of minority cultures in the increasingly Western-dominated global media environment. He bemoaned the way in which television "impose[d] its US mainstream cultural tyrannies everywhere", and warned of an insidious process of worldwide homogenization he called the "greying of culture". "With every passing month", he warned, "we are being moulded and remade by what we are allowed to see and hear".[6] In fact, Lomax argued that the world had *already* seen the negative effects of a specifically *somatic* standardization. The globalization of markets, he argued, "imposed the confining and stiff-waisted European costume almost everywhere. The head-back, chest-out, erect posture of the North European elite is held up for universal admiration as the only way for a *real* human being to carry himself. Schoolchildren and soldiers in every clime were drilled in this carriage, often with ridiculous and unfortunate consequences."[7]

5 Lomax's aims, therefore, can be compared to those of the feminist anthropologists chronicled by Alison Wylie (Wylie, 2001).

6 Alan Lomax, "The Treasury of Dance on Film," from unpublished manuscript for *Dance and Human Culture*, 1981, Box 4/18-01, Alan Lomax Collection.

7 Alan Lomax, "The Oldest Art," from unpublished manuscript for *Dance and Human Culture*, 1981, Box 4/18-01, Alan Lomax Collection.

Lomax's solution was to use existing forms of media in new ways and, in doing so, provoke a "recalibration" of the human perceptual apparatus.[8] First, he promoted film screenings, both on American public television and in places where "traditional" cultures seemed threatened. Second, he planned to publish a detailed handbook that would allow any reader to analyze movement in same way the Choreometric specialists did. These expert-lay viewers would be able to easily "spot the false, the fake and the oversold", and value instead the natural, the beautiful, and the adaptive.[9] Moreover, once an individual was trained to analyze movement style in dance, Lomax hypothesized that his or her day-to-day experience of non-dance movement would *also* change, new scientific understandings demolishing old prejudices.[10] No longer would the "shuffling" movements of African-Americans connote laziness; instead, they would tell a story about climatic adaptation, agricultural technologies, and dogged persistence. Indeed, as Choreometrics-trained observers moved through a city, they would encounter hard evidence about the long course of human history in the body of every person they passed. A trip to the grocery store might teach as much as an afternoon at a natural history museum.

Studying human interaction in this way was, Lomax promised, "like looking through a microscope or underwater for the first time",[11] and he fervently contended that:

> […] once these cultural traditions of movement style become visible, members of all the varied human traditions, whether they be viewers or program makers, film-makers or film goers, can no longer easily be shamed or enticed out of their birthright. They can build upon their inherited visible culture, can cope creatively with the media and participate in developing the multi-channel, multi-cultural civilization that a healthy human future demands.[12]

But if this were the dream, the reality was significantly more complicated. In part, this was a function of the byzantine negotiations required to obtain the film upon which Choreometric analysis would rest. Though Lomax praised the increasingly large store of global film, it was in no way centralized or easily accessible. Over the course of nearly a decade, Lomax, Bartenieff, and Paulay reached out to anyone and everyone that might have filmed dance—from anthropologist Margaret Mead to kuru-researcher Carleton Gadjusek to a retired vascular surgeon and the U.S. military.[13] The process was long and sometimes contentious. While some institutions—such as the Institute for Scientific Film in Göttingen—were eager to participate, others—including the Russian cultural ministry—required more delicate diplomatic negotiations. Wealthy adventurers, unsurprisingly, were often the most difficult to work with. As sometime collaborator Margaret Bach wrote in a letter to Lomax, "Some of these fellows do lecture cir-

8 Forrestine Paulay, Irmgard Bartenieff, and Alan Lomax, "The Choreometric Coding System," Box 4/18-03, Alan Lomax Collection.

9 Alan Lomax, "The Treasury of Dance on Film," from unpublished manuscript for *Dance and Human Culture*, 1981, Box 4/18-01, Alan Lomax Collection.

10 "Choreometrics—Groundwork. Progress Report—Undated," Box 9.1-1/02, Alan Lomax Collection.

11 Alan Lomax, "Universals of Human Movement," from unpublished manuscript for *Dance and Human Culture*, 1981, Box 4/18-01, Alan Lomax Collection.

12 Ibid.

13 Margaret Bach, "Letter to Alan Lomax," September 4, 1972, Box 9.2-03/01, Alan Lomax Collection.

cuits, thus realizing the 'cash' value of their traveling and filming. They must be handled with kid gloves."[14] The team even unsuccessfully attempted to locate fabled footage made by Roy Disney and H.G. Wells.

Lomax recognized that many of the films were not produced for the purposes of scholarly analysis, but he remained sanguine about their value. While an inexperienced or biased cameraman might shape the data in minor ways, Lomax held that movement style was so profoundly entrenched in the body that—even in the most poorly made films—its basic structure and elements could not help but emerge.[15] This methodological naiveté was not necessarily uncommon for the era, but it is striking in the context of Lomax's deep, principled belief in the moral bankruptcy of most commercial filmmaking. While publicly decrying the work of Disney, U.S. military filmmakers, and the gentleman adventurers who disturbed the peace of remote tribes, Lomax relied upon their labour—and, in fact, upon their directorial judgment.

To draw attention to this irony is not necessarily to condemn Lomax; in many ways, this methodology was a clever one: Undermining global media hegemony from within, finding value in footage likely otherwise abandoned on the cutting-room floor. It does, however, speak to the complicated territory Choreometrics—and other projects like it—had to negotiate. As Jonathan Sterne pointed out in the case of nineteenth century recordings of Native American music, "the work of anthropological cultural stewardship coincided with the decimation that necessitated the stewardship in the first place" (Sterne, 2003: 332).

Attending to the hidden labour of filmographers is important not only for the purposes of historical accuracy, but because it gives contemporary scholars the tools to engage critically with the astounding archive of film and coding sheets Choreometrics did produce. When I visited the Lomax collections at the Library of Congress two years ago, the archivist told me that not a single person had touched the Choreometrics collections in the many years he had overseen them. In the past year, however, there has been a renaissance of interest in Choreometrics. Working alongside Library of Congress archivists, a group of dance scholars from the University of Maryland has embarked upon an effort to digitize Lomax's films and make them available online for both academic study and popular enjoyment.

Lomax originally had similar plans for Choreometrics, though his ambitions were frustrated by a lack of funding and by the technological capacities of the 1970s. In fact, even *Dance and Human History*—the strange chimera of academic tome and coffee-table book that Lomax hoped to widely distribute—was never published. Crushed under the weight of its own ambition—and

14 Margaret Bach, "Letter to Alan Lomax," May 15, 1972, 9.2-03/01, Alan Lomax Collection.

15 This view may not have been unique to Lomax, as Anna Grimshaw has commented on a certain methodological naiveté that plagued early many ethnographic filmmakers (Grimshaw, 2008). Lomax himself notes that "Sorenson and Gajdusek indicate, however, an assurance that subjectivity in film footage is less likely to interfere with later research than are other more conventional methods of recording, and remembering events. If such a film is preserved, catalogued and made available, it can then be of use to investigators from more than one field. They also recognize something which I found to be quite commonly conceded by anthropologists—namely that the camera can record material which went unnoticed by the cameraman, or which may not, at the time, have been seen for what it was." Alan Lomax, "Report: Sources of Films," Box 9.1/01-07, Alan Lomax Collection.

more than a thousand pages of data, photographs, and do-it-yourself coding sheets—it simply grew too large for any publisher to take on.

In many ways, this new venture, which the project's leaders are calling "re-imagining Choreometrics", represents a fulfillment of Lomax's long-ago vision.[16] At the same time, however, it makes the question of the films' context newly pressing. The danger, of course, is that the project's leaders will let the work of the filmmakers *remain* invisible, allowing the films to be read as transparent representations of static cultures, rather than as documents produced through dialogic interactions at particular historical moments. There are indications that the project may, in fact, provide *some* information about the circumstances of filming, though exactly what kind or how much remains unclear. Highlighting the circumstances of production of these films is, however, fundamental to creating a useful scholarly repository—as well as an archive that is respectful of the varied and complicated relationships that the individuals depicted may have had with those who filmed them.

I turn now to just that—the labour of the dancers who made Lomax's analysis possible. Ironically, these individuals are both the *most* and *least* visible actors in the Choreometrics story. They are undeniably present when watching one of Lomax's films—they compel the viewer's gaze and their actions are the object of study. Information about their lives, motivations, and even the aesthetic traits of their work, however, is almost totally unavailable. Why did they allow themselves to be filmed? What did they hope to get out of the process? These are questions that are very difficult to answer.

In part, this absence is a result of Lomax's desire to elucidate the basic movement patterns of large *groups,* rather than the particularities of individual styles. Lomax held fast to a belief that the field of folklore properly dealt with the art and wisdom produced by the community, and criticized American folklorists' reticence to focus on this collective knowledge, suggesting that it stemmed from a "built-in prejudice of our psychology-oriented, individualist, anti-communalist age".[17] In fact, this orientation—toward the group, away from the individual—was firmly embedded in the system of recording and analysis the Choreometrics team chose to utilize.

Initially, Lomax, Bartenieff, and Paulay were interested in using a system called Labanotation to record the dances they witnessed. Developed by the German choreographer Rudolf Laban in the 1920s, Labanotation was initially used to preserve choreography and coordinate mass dance spectacles. This system is quite complicated: It symbolically records on paper every movement a dancer makes and ultimately produces something akin to an intricate musical score.

But because the Choreometrics team was more interested in the overall *characteristics* of movement than in specific choreography, they decided to retain the basic framework that underpinned Labanotation, eschewing its attempt to capture complete, individual dances. A large cadre of trained raters would record the movement qualities Labanotation coded for—like

16 "Re-Imaging and Re-Imagining Choreometrics," Accessed July 9, 2015. http://www.reimaginechoreometrics.com/

17 Alan Lomax, "Notes," n.d., Series 1, Box 13/01-02, Alan Lomax Collection.

rhythm, forcefulness of limb movement, and bodily coordination—but specific choreographic elements would be ignored.

As a result, at least in the printed texts Lomax produced, there is very little sense of what these dances—or dancers—looked like. As Lomax himself admitted, excluded from the study's purview were "the sequences of movements, the gestures, the costumes, the dramas, the themes, the functions, [and] the contexts in which particular dance sequences acquire their meanings".[18] Considering Lomax's scholarly aims, this makes some sense: to produce knowledge, it is often necessary to highlight one phenomenon at the expense of others.

The invisibility of particular dancers in the written record does, however, threatens to further efface their contributions to Choreometrics. Ironically, as Lomax himself recognized, it is already too easy to ignore dance, to see it as an unthinking, pleasurable pastime, rather than as true labour, both physical and intellectual. In fact, a tension lies at the heart of Choreometrics between two different notions of the nature of dance: One that held it up as a form of purposeful, individual expression with an infinite degree of variation, and another that suggested it was merely a collection of basic cultural vernaculars, mimicked with varying degrees of perfection by largely anonymous practitioners. In the end, Lomax's view seemed to tend toward the latter.

Tellingly, Lomax publicly thanked the "explorer film-makers of many countries [for] generously sharing their hard-won findings with us", but he did not thank the dancers whose groaning muscles, sweating brows, and calloused feet made those films possible.[19] The reasons for the absence are manifold and perhaps not uncommon: Gender, race, political and economic power, as well as the prevailing popular tendency to denigrate bodily labour. Lomax's inability to recognize these contradictions speaks to how deeply embedded some of these prejudices are, but it also attests to the ongoing importance of our collective endeavor, particularly at a moment when new forms of representation, inscription, and archive-production are proliferating. Making dance visible, but not its dancers, is a step in the right direction, but not the final one.

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18 Alan Lomax, Unpublished manuscript for Dance and Human Culture, 1981, Box 4/18-01, Alan Lomax Collection.
19 Alan Lomax, "The Treasury of Dance on Film," from unpublished manuscript for Dance and Human Culture, 1981, Box 4/18-01, Alan Lomax Collection.

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Thinking with Gatekeepers: An Essay on Psychiatric Sources

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This paper grew out of an unpublished essay that I wrote in September 2014 after a research trip to Southern Germany. I was searching for records to compare and contextualize the diagnostic procedures that were used to identify the peculiarity of cases of 'Alzheimer's disease'. My goal was to paint a more nuanced picture of the establishment of this psychiatric category in the 1900s. The sources that I was able to access, including patient files, diagnosis books, and unpublished correspondence and typescripts, were indeed helpful in giving me a better understanding of the making and use of the diagnosis of Alzheimer's disease. However, it also led me to think about the practices that historians use in archives. In this essay, I do not focus on the results of my historical scrutiny. Rather, I respond to the call for "reflection on our *own* scholarly practices" by the organizers of the workshop *(In)visible Labour*. The paper should be read as a self-reflexive piece on the search for sources. It documents my attempt to find a way to write history that acknowledges the many layers of meaning that the sources reflect. It poses meta-questions such as: what makes something a source? What does it imply to treat something or someone as a source?[2]

I have called this piece 'Thinking with gatekeepers' to stress that we gain more than the bureaucratic access to historical materials through our interactions with archivists. Their very approaches to the sources that they are responsible for can provide windows through which we can see the meanings and functions that these materials can have. The multiplicity of possible readings gives rise to a double task: rendering given constructions and uses of perceived histories visible, and finding our own stance, our own approach to historical materials and history writing. This paper is, in the literal sense, an essay about the social and intellectual interactions that precede a scholarly piece of writing, and then often become invisible or encoded in acknowledgement sections and footnotes. It is an account of how gatekeepers of historical sources (be they professional archivists or not), and the ways in which they treat their archives, impact the

1 I want to thank all the people, named and unnamed, dead and alive, that I am referring to in this essay. Special thanks go to Sabrina Zinke, Norbert Müller, Konrad Maurer, Ulrike Maurer, and the staff of the *Heidelberger Universitätsarchiv* and of the *Klinik für Psychiatrie und Psychotherapie der Ludwig-Maximilians-Universität München*. Earlier versions of this essay have been presented at a colloquium of the Max Planck Research Group on "The construction of norms in 17th to 19th century Europe and United States" in October 2014 and at the "(In)visible Labour" workshop in June 2015. I would like to thank the organizers, discussants and participants for many helpful comments. Research for this paper funded by the mentioned Max Planck Research Group, and "Society in Science - The Branco Weiss Fellowship", administered by ETH Zurich.

2 Susan Lindee (2005) recounts in her "Essay on Sources" in the appendix of her book *Moments of Truth in Genetic Medicine* her struggle with these questions. She is particularly concerned with how to treat grey literature and first-hand accounts of the history of human genetics. Her critical but appreciating approach to gatekeepers and their sources and narratives has inspired my research and this essay.

ways in which historical research is done. Against this backdrop, the essay touches on the roles of authorities and authority in governing knowledge, the memorialising functions of archives and museums, and the materiality of paper work. More narrowly, this paper reflects on how the making, storing, and (re-)use of paper work in psychiatric clinics has not only shaped medical ways of knowing in past times, but also how historians of science and medicine approach these objects of inquiry today.[3]

Sources and gatekeepers

In Munich, Heidelberg, Frankfurt, and the small village Marktbreit, I was looking for sources on and from Alois Alzheimer (1864–1915), the psychiatrist after whom the now famous 'Alzheimer's disease' was named. In November 1901, the German psychiatrist Alzheimer encountered a severely confused 51-year-old female patient, Auguste Deter. After her death five years later, he examined her brain and presented his clinical and histopathological analysis under the heading of "a peculiar disease" on a conference in Tübingen (Alzheimer, 1907). In 1910, Emil Kraepelin, the director of the *Königlich Psychiatrische Universitätsklinik* in Munich where Alzheimer then worked, recounted Alzheimer's case story in the eighth edition of his influential psychiatry textbook, where he labelled the disease with the term "Alzheimers Krankheit" (Kraepelin, 1910, 624-629). Kraepelin's textbook and Alzheimer's conference paper are published sources; the patient file is kept in the public archive of the city of Frankfurt (Main). Excerpts of the file, including a photograph of the woman that was taken in the psychiatric clinic, can nowadays even be found in Wikipedia under the patient's full name, Auguste Deter.[4] However, other sources that could enrich the history of Alzheimer's disease are less easily accessible. Some are kept in private estates. Some are lost. Some are available but reproduction is forbidden.

For instance, the patient files of the Munich psychiatric clinic where Emil Kraepelin and Alois Alzheimer worked from 1904 onwards are missing. However, the archive holds the original diagnosis books (*Diagnosenbücher*) in which the hospital staff recorded the names, ages, incoming and outgoing dates, and clinical diagnoses of all patients. Additionally, there are books in which doctors summarised patient profiles with a one-page clinical record per person, so-called 'epicritical reports' (*Epikrisenblätter*). Visiting the Munich clinic, I was allowed to view the diagnosis books and epicritical reports for my historical research, but when I asked if I could scan them I was informed that medical confidentiality continued to exist after the death of a patient. Since the documents contained both personal data as well as diagnoses, their reproduction was not possible.[5] This influ-

3 For histories of medicine and psychiatry that have emphasised the importance of paper work, see e.g., Engstrom (2005), Rosenberg (2005), Hess & Mendelsohn (2010), Löwy (2011).

4 The Wikipedia entry for 'Auguste D' was created on March 14, 2007, and was immediately discussed as a candidate for 'speedy deletion as an article about a real person that does not credibly indicate the importance or significance of the subject.' Yet, it remained in the Wikipedia and expanded quickly. The photograph was added ten days after creation of the website; the heading of the entry was changed to include the woman's full last name in May 2010 (https://en.wikipedia.org/w/index.php?title=Auguste_Deter&action=history).

5 E-mail from Norbert Müller (August 26, 2014). However, the museum of the psychiatric hospital in Munich displays the epicritical report (including biographical information about the patient) of one of the early cases

enced my work with the sources in several respects: since I could not visually reproduce the documents that I studied, I described them in as much detail as possible in my field notes. This made me very attentive to material details: which coloured pens were used, who produced the paper, how diagnoses were corrected and amended. In fact, the limitation turned out to be epistemologically productive for me. The said sensitivity of information about psychiatric patients provided me with questions about my own practices, such as whether to anonymise my sources, and if so, how. And these considerations resonated with a series of historiographic reflections that are central to my research on the history of Alzheimer's disease: from which standpoint do I write this history? How would I treat the documents about people that were the medico-scientific objects of more or less well-known psychiatrists or biomedical researchers? How could I tell a history of knowledge that, at the same time, did not forget that this was also a history of suffering people?

I encountered a very different situation in Heidelberg, where Kraepelin and Alzheimer had worked in the psychiatric clinic in 1903. Whereas in Munich the historical medical documents have remained in the psychiatric clinic, in Heidelberg the old patient files were re-located to the university archive in the 1990s. Here, I could ask for copies of everything, even of complete patient files. Moreover, the archivist explained to me that to correctly quote a patient file in my work, I had to include the last name and first name of the respective patient (or alternatively, a reference to the file number). When I asked whether it was sensitive information to provide the full name, the archivist replied that she did not want to impede historical research and that even the strictest regulations of the federal archive allow for publication of personal information 110 years after birth.[6] To this archivist, the fact that the patient files were kept and carefully maintained made them, besides all other possible meanings, historical sources, embedded in the institutional setting of an university archive. If I wanted to work with these sources, I had to treat them according to given disciplinary standards and legal and institutional regulations concerning historical research in German archives. With the transfer of the patient records from the clinic to the university archive, their status apparently changed from being primarily confidential medical documents to becoming primarily research sources for historians. When the institutional setting and authority (clinic or archive) that was responsible for regulating the use of the documents changed so did the rules according to which historians of medicine were allowed to access and work with patient records.

The more I was faced with different stances towards access and reproduction in my search for sources, the more I thought about how the archivists and other gatekeepers needed to be part of a story of the history of a disease.[7] In the following, I discuss three possible approaches to treat historical psychiatric materials: as documents of human lives, as research materials, and as treasured relics. All of these relate to different reasons why historical psychiatric objects have been kept and protected.

of 'Alzheimer's disease' (see Hippius et al., 2005, see also Graeber et al., 1997, 78).

6 Personal communication with Sabrina Zinke (September 2, 2014); the quotation rules of the federal archive are statutorily regulated, cf. http://www.bundesarchiv.de/bundesarchiv/rechtsgrundlagen/bundesarchivgesetz/index.html.de, §5 (2), accessed January 12, 2016.

7 In a related vein, historian of biomedicine Nathanial Comfort (2011) has argued that face-to-face-interactions with 'sources that talk back' provide not only information about scientists' biographies and social networks, but also opportunities for historiographical reflection.

Human lives

When I searched the archives for patient files and epicritical reports, I searched them with this question in mind: What writing and diagnostic practices were applied to single out relevant from irrelevant facts? I found many traces in the files that I will analyse in more detail in the next section. But I also found myself constantly replaying a related question about my own methodology: What do I include as relevant information and what do I leave out?

Many epicritical reports contain information about disrupted families, suicide attempts, and precarious life situations. These might be presented as causes of or symptoms for a mental disease, but they also mirror human tragedies. This becomes particularly evident in the patient files. The files comprise a variety of different documents, including transcripts of doctor-patient-interviews, intelligence inquiry forms, photographs, medical examinations, requests of other hospitals or asylums to lend the file, and letters, drawings and sometimes poems of the patient. The dozens of letters belonging to a female patient may have been kept in her diagnostic record as evidence for hypergraphia (defined as excessive writing as a symptom of a manic disorder), but they also tell a diagnostic story from a patient's perspective. This patient, for example, documents the shame of her illness, even though she recounts that the doctors insisted it was not her fault.[8] The diagnostic purpose of extensive patient interviews and the storage of letters at the time of their recording and writing is why these documents have been preserved. The question is now: how to read these documents today? And why consider them at all? This relates to the methodological challenges of writing history of medicine from below, but it also pertains to the epistemological double task that I set out at the beginning of this essay: to understand the diagnostic reasoning of the time I am studying, and to find my own stance in approaching the patient records and describing how they reflect the making and working of psychiatric knowledge.[9]

This starts with the question of quoting the names of the patients or not doing so. In most medical reports and histories of medicine, patient names are abbreviated or pseudonymised. In this essay, I have used the full name of the patient, Auguste Deter, who became known as the first case of Alzheimer's disease. I decided to do so, because her name was already publicly known, and because I think that giving her full name instead of only writing about 'the first case' or abbreviating her name, puts more emphasis on the fact that she was not only the 'material' for Alzheimer's discovery, but actually performed her own agency in the diagnostic process. A cursory Google search confirms that quotes from an interview in her patient file (translated and partially published in Maurer et al., 1997, and in Maurer and Maurer, 2003)—most prominently "Ich habe mich sozusagen verloren" ("I have lost myself")—are recurrently used to characterize the disease in diverse popular and academic contexts. Against this background, it might even be considered as a question of authorship to give the credentials to Auguste Deter.

8 Universitätsarchiv der Ruprecht-Karls-Universität Heidelberg (UAH) L-III 03/100 ♀.
9 On the challenges of writing history of medicine from below, see: Porter (1985), Risse and Warner (1992), Condrau (2007), and Ralser (2007).

The practice of abbreviating the patient's name ensures medical confidentiality. But confidentiality not only protects sensitive data; it also defines what should remain hidden from the public world in the first place. Anonymisation practices may cover the patient's identity beyond his or her being a patient, and his or her being a case. On the other hand, I do not consider the unmasking of a patient's identity as a surplus *per se*. Leaving coherence in citation practices (i.e. rules of conduct as a pragmatic solution to a deeper epistemological problem) aside, I could not give a good reason for what might have been gained from naming the woman whose letters I briefly referred to above. Maybe my struggle in finding a good way to deal with the names of patients, but also of doctors, hospital staff, and gatekeepers, is most of all a reflection of how difficult it is to find an appropriate way to write not only about the making and working of psychiatric knowledge, but about the humans involved in and affected by this endeavour.

Research materials

One approach to the patient files is to regard them as a collated, fragmented archive of (an episode in) a patient's life. Their archive-like function is reflected in their materiality: The Heidelberg files contain papers, documents, letters, tables, and requests that were perforated with a small hole in the upper left corner. A piece of twine was pulled through them and the paperboard document folder to hold the file together. Different colours and layouts of the document folder covers indicate that archived files were sometimes transferred into new folders if the patient was admitted again in later years. Some of the folders bear several archival signatures, indicating that the re-identification of a once admitted person was an issue. An archivist alerted me to the fact that the continuous updating of files was facilitated by the '*Badische Bindheftung*' technique. You can easily loosen the twine to add more pages or to browse the file like a book. The small hole in the upper corner allows paper of very different formats and margins to be added. This binding technique is known from notary documents: a twine is pulled through the pages that compromise the legal contract and is then sealed. In the case of the patients' file, the sealing never took place. Requests and replies were continuously added to the file, not only if the patient had long left the Heidelberg psychiatric clinic, but also if the patient had died long ago. In a way, the files continued a life of their own as research materials for psychiatrists and historians.[10]

When I searched the patient files and read the diagnosis books, I found evidence indicating that my practices of historical inquiry and some of the practices of the psychiatric clinic were very similar. The patient files contained underlining in red and blue coloured pencil, acclamation marks and dashes on the margins that signal the different doctors' evaluation of important facts, and reflect the collective authorship of a patient record. Also, the Munich diagnosis books in-

10 Sophie Ledebur (2011) provides a closer analysis of the use of patient files as research materials in and for psychiatry; see also Brändli et al. (2009). On the use of historical records as scientific resources in other disciplines (such as geology, botany, meteorology or astronomy) since the early modern period, see, for an overview Daston (2012).

cluded notes and signs in different handwritings and colours: circles in blue and red and orange coloured pencil, small hooks in violet coloured pencil, corrected clinical diagnoses and crosses in black ink. I could identify patterns: red circles indicated cases of dementia praecox, orange circles cases of manic depressive confusion, blue circles cases of paralysis, violet hooks cases of alcoholism, a reversed hook in pencil indicated cases of epilepsy.[11] I noticed this while I was myself counting and grouping diagnoses. For my historical analysis of the establishment and use of the diagnostic category of Alzheimer's disease, I searched for entries of this diagnosis as well as of related ones such as arteriosclerosis, organic brain disease or senile dementia. The doctors' practice of compiling statistics and re-evaluating diagnoses a century ago, and my practice of reconstructing their diagnostic and classificatory reasoning, looked quite alike. We were both re-using archived paper work as research materials. The role of archiving and re-assessing diagnoses is but one example for the various uses of history in psychiatry. Another one is memorialising founder figures.

Treasured Relics

In 1995, the pharmaceutical company Eli Lilly bought a house in the old part of Marktbreit, a small village in Franconia on the river Main. Konrad and Ulrike Maurer had identified the house as Alois Alzheimer's birthplace, had initiated its transformation into a museum, and the sponsorship of that process by Eli Lilly (see also Whitehouse, Maurer & Ballenger 2000, xii-xiv). A dedicated local tour guide who showed me the museum recounted how Mr. and Mrs. Maurer got in touch with the heirs of Alzheimer and collected all sorts of items that related to him, ranging from his old microscope to the embroidery work of his sister. Why were these objects deemed relevant to preservation and exhibition? In the context of the museum, this question is framed against the background of the aims and scope of exhibiting and memorialising the life and work of Alzheimer. For the Maurers, Alzheimer's work was important for psychiatry but was, moreover, a cultural achievement that needed to be recognized.[12] Their long-term vision for the museum was to have it nominated as an UNESCO cultural heritage site.

Konrad Maurer had worked as professor of psychiatry first in Würzburg, a city near Marktbreit, and then in Frankfurt from 1993 until his retirement in 2009. In Frankfurt, he continued his search for the remains of Alzheimer's work and Auguste Deter's life. In 1997, Maurer and two colleagues published in the 'medical history' section of *The Lancet* that they found the original patient file of the first case of Alzheimer's disease (Maurer, Volk and Gerbaldo, 1997). In 1998, Maurer published together with his wife Ulrike a popular science book on Alzheimer and the patient file of 'Auguste D'—to whom they never refer fully by name, but for whom they give many biographical details that extend the medical case report (translated in English: Maurer and Maurer, 2003). When I asked Mr and Mrs Maurer why they thought that the case of Auguste D was so important, they replied that this was self-evident: Alzheimer's disease had

11 Psychiatrische Klinik der Ludwig-Maximilians-Universität München (LMU-P) Diagnosenbücher Männer / Frauen, 1904–1912.
12 Interview with Konrad and Ulrike Maurer in Frankfurt/Main on July 30, 2014.

become the infamous disease of our time ("more famous than Elvis" [my translation]), and this was the first case. Konrad Maurer referred to his most cherished material—Alzheimer's original histological slices of Auguste Deter's brain, given as a gift—as a *relic*.[13] He made clear that he did not use the religious term as a metaphor. Rather, he explained to me that these slides hold the "embodied" disease.[14] Maurer's approach to sources as treasured relics provided me with yet another perspective on historical psychiatric materials. The idea that a histopathological slide is a relic provides a reading-glass to scrutinize the ways in which the factuality of bodily remains has been used as evidence for a continuous history of a real disease.

The story narrated by my visits to the Maurers and the museum in Marktbreit is very much about the exceptionalist role of Alzheimer and of the first case of the disease that was named after him. The exceptionalist status of Alzheimer is framed by the conception of the gatekeepers for whom their collection, museum, and publications on Alzheimer is a life task. I met and interviewed the Maurers at their home in Frankfurt, where Konrad Maurer is keeping a private collection of historical psychiatric resources. These resources and the ones kept in Marktbreit are not freely accessible. They are (literally) treasured. At first, I thought that the most interesting part of my meeting with the Maurers would be to look at their private archive. But while interviewing them, I realized that more than the historical material itself, I could gain from their view on the material. The reasons why the Maurers kept and treasured these objects could tell me something about the re-making of the foundational role of Alzheimer's first case after its rediscovery in the mid-1990s. My research trip to Frankfurt that had begun as a search for archival material, ended with a piece of oral history being my main source. This brings me back to the question that I started this essay with: What makes something or someone a source? For one, it is the historical question with which historians approach their subjects and objects of inquiry. For another, archival gatekeepers, the objectives of collections, and the contexts of private estates provide specific frameworks of why and how to keep and treat something. Gatekeepers do not only regulate accessibility to historical sources; their views on the material they govern can be sources of inspiration of their own.

13 Interview with Konrad Maurer on July 30, 2014. Maurer's comparison of the histological specimen with a religious relic echoes nicely with historian of art and science Angela Matyssek's thesis that Rudolf Virchow treated his collection of pathological preparations as 'secular relics' (Matyssek 2001, 153).

14 Konrad Maurer used the English term embodiment in our German interview. The cherishment of the material remains of the founder case might compare to the prominence given to type specimens in botany. Maurer also used the word embodiment to underscore that Alzheimer's disease provided insights into the fleshly basis of the human mind. Finally, 'embodiment' has also a religious connotation.

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Translators as Informers, Mediators, and Producers of Knowledge: Reflections from Medical History Interviews in Uganda

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My doctoral and post-doctoral research projects concern leprosy, and the role of women in the spread and adaptation of biomedical knowledge in Ugandan history, respectively. In the course of these investigations, I have interviewed more than 100 people in eight different languages and four different countries. The bulk of these interviews have taken place in Uganda, mediated by translators. Most frequently the translators are medical professionals with a connection to the research I am undertaking, sometimes they are researchers with experience conducting interviews themselves, and occasionally they are family members of the interviewee or local community notables. These translators, each of whom brings his own experience, expertise, expectations, abilities, and relationships to our interviews, shape my research profoundly. They mediate the questions that I ask, often proposing new possibilities or closing down certain avenues of inquiry. They choose how to translate responses to the questions asked, and sometimes pose questions of their own. Many have been instrumental in connecting me to informants, thereby shaping whose stories I have access to. In the course of conducting interviews and attempting to improve my research methodologies, I have reflected on the role of these translators as mediators and co-producers of knowledge, but as these reflections have never made it onto a page—and rarely arise in conversations—the labour of these translators remains relatively invisible in my research output. In this piece, I will reflect on the (in)visibility of translators, exploring the disciplinary and research practices that shape that visibility; examine the consequences of those practices; and question what value there might be in making the labour of translators more visible.

The interviews

In this brief case study, I will focus on the translators who mediated my interviews with elderly, former leprosy patients in Uganda. Although I have interviewed a wide range of medical professionals, Ugandan and European, who worked in the fields of leprosy, nursing, and midwifery in Uganda, almost all of these interviews were conducted in English, which was the language of colonialism, and is today the language of secondary schooling and higher education. By contrast, very few of the former leprosy patients I interviewed felt comfortable or able to speak with me in English. Leprosy has a long association with poverty, and people affected by leprosy therefore have limited access to education. Those who had entered a mission leprosy settlement as children usually learned English, but they used the language infrequently after the hospitals were turned over to Ugandan staff in the 1980s. Accordingly, they felt more comfortable speaking in a language that they used daily, such as Lusoga. This was not necessarily their first language; each leprosy settlement had patients from a mixture of different ethnic groups, and the language shared by the largest number of patients—usually the ethnic group among whom the settlement was situated—became the lingua franca for staff and patients, wherever they had travelled from.

I conducted interviews between 2009 and 2014 in the proximity of seven former leprosy hospitals, in seven languages (Lusoga, Luganda, Rukiga, Ateso, Labwor, Acholi, and Lugbara), with about sixty former leprosy patients, assisted by a variety of different translators. These translators have a wide variety of backgrounds and professional experience: They included (1) a part-time university student, part-time researcher with a cultural research centre run by the Catholic church to study Basoga culture; (2) a town council-member; (3) two district government Leprosy/Tuberculosis officers, one trained in the 1970s and one trained more recently; (4) a leprosy assistant and shoemaker who has lived and worked at one leprosy hospital for the last fifty years; (5) a hospital leprosy supervisor; and (6) a clergyman posted to a leprosy hospital for several decades. Others, usually staff affiliated with the hospitals, have also acted as occasional translators, and I have also had separate translators who have listened to recordings of interviews and retranslated them, or who have translated archival documents into English.

I undertook most of these interviews during my doctoral research on colonial leprosy settlements in Uganda, seeking to find voices from the Ugandans who lived in the leprosy settlements when the majority of sources available to me were documents written by missionaries and colonial government officials. I asked questions about the social and medical experience of leprosy and its treatment; local attitudes towards leprosy and towards the hospitals; how they came to enter a leprosy settlement; what life was like while they were living there; and more.

Given the diverse expertise and goals of all the men who translated for me, and their differing relationships with and expectations of me, it is difficult to generalize about the role that they played in the production of knowledge through these interviews—except to say that without exception, that role was substantial. Instead, I will offer several very specific examples of my interactions with translators, which are representative of a wider range of interview experiences, and of the different priorities and motivations that my translators and I have brought to interviews.

The first translator I worked with in Uganda was also the only translator who had professional interview experience—though this was primarily as a research assistant conducting his own interviews under the guidance of Ugandan-led cultural research projects, and not strictly as a translator. He translated more than twenty interviews in two different languages, and we worked primarily from a questionnaire that we created together. Sometimes, an interviewee would talk for several minutes, and he would then offer a far briefer account of what they had said. I tried to discover what was filling those minutes, first by pressing him further for details, and then by having the recordings of some of those interviews re-translated. Some gaps were, of course, due to the strains of long translation sessions and the challenge of remembering several minutes' worth of speech without an opportunity to stop and translate in the midst. Some gaps, however, were a choice on his part. For example, when I pressed to hear more about one woman's long explanation of how she had contracted leprosy, he told me that she believed she had contracted leprosy through witchcraft, because a neighbour was jealous of her farm's prosperity. He prefaced this by telling me that I did not want to hear about such things. Though I had not explicitly told him so, I wanted very much to hear about alternatives to the biomedical construction of disease causation. I knew from my studies and from experience in the field that biomedicine was very unlikely to be the only framework through which individuals perceived leprosy, and moreover I hoped that I would find continuities between contemporary stories about the supernatural or social causes of leprosy, and pre-colonial ideas about the disease. I hoped to broaden my awareness of how Ugandans have perceived and experienced disease, and interviews were the best way to seek that knowledge, because the disapproval of biomedical professionals has long stifled written discussions of alternative medical therapies and systems.

In the historical context of colonialism, it is easy to understand why my translator might assume that, as a European, I would not want to hear about 'superstition', which was always something that missionaries and colonizers fought against.[1] We were, after all, conducting the interviews in a former mission hospital, presently run by a Ugandan religious order, the Little Sisters of St. Francis. Or perhaps, given his own religious affiliation and his employment in a Catholic organization, he was embarrassed to transmit such stories to an outsider.[2] Given that his regular job was to interview people about cultural practices for a research centre aimed at preserving local heritage, the incongruity of his unwillingness to translate such a practice suggests that his expectations of my goals, his personal feelings, his perception of his employer's beliefs, or perhaps all of the above, affected the way that he chose to translate interviews.

Many of the individuals who translated for me were professional leprosy workers, working either for the government or for former leprosy hospitals. Interviews with these translators were among the smoothest of all my interview experiences. With their expertise in leprosy, as well as their experience working with missionaries and more recently, NGO workers, they often understood the questions I was trying to ask, and the answers that interviewees were trying to give, much more readily than someone outside the field. I obtained more of the information that I sought, more quickly and easily, and found that interviewees' answers were less likely to be altered or excerpted in translation as a result of the agendas or misunderstandings of the translator. Moreover, their expertise and experiences have been invaluable to me in another way: Quite often they have been my connection to former leprosy patients who are no longer living in hospital compounds. Thus, in addition to translation, I am indebted to these men for helping me to find informants.

One of my smoothest translation experiences was with a government leprosy/tuberculosis officer who had been trained relatively recently, after leprosy had been eliminated as a public health threat in Uganda, and after tuberculosis had been grafted on to the leprosy control service.[3] Tuberculosis, which has a far higher incidence rate than leprosy, was the primary focus of his training and his work, and so he did not have a personal relationship with any of the former

1 Lyn Schumaker makes a similar point, noting how historical memory and context affects local perceptions of the foreign interviewer and their goals and intentions (Schumaker, 2001: 16).

2 Lyn Schumaker notes a similar example (Schumaker, 2001: 196).

3 According to the World Health Organization, a disease has been eliminated as a public health threat when the annual incidence of new cases drops below 1 in 10,000.

patients we talked to. However, he understood leprosy and many of the social issues surrounding it well enough that he was able to ask questions and translate answers without my having to rephrase or redirect very frequently. He was not very invested in leprosy, and so he did not appear to insert very much of himself into the interview. By this I mean both that there were no apparent gaps in the translations that he offered, that he did not answer questions himself (instead of asking them), and that if he generated questions of his own for the interviewees, he did not translate their answers for me. As far as I could ascertain, for him the day of interviews provided a break from his normal duties. At the same time, they fulfilled his technical responsibility to conduct regular check-ups on former leprosy patients in the district to see if their disabilities had worsened or if they were suffering belated drug reactions. My money paid for petrol, and provided a little bit of extra income for him besides. Our interests, expertise, and experience were complementary, and this influenced the fluency of our communication—and my perception of the accuracy of the translation—far more than did our linguistic abilities or the translator's previous interviewing experience.

Interviews with leprosy workers who had been working in the field for decades were sometimes quite different. One leprosy assistant, who was still working at the hospital, said I need not offer him any financial recompense for translating, because the work was part of his job at the hospital. His normal work was making orthotics for former leprosy patients and others in need of special shoes, but as one of the longest working staff members in this former mission leprosy hospital, his sense of responsibility, vocation, and interest extended beyond this remit. When I paid him regardless, he was pleased but surprised at the amount. His expertise was valuable for my research in its own right, and facilitated interviews with men and women who had been treated at the hospital in earlier decades. On the other hand, sometimes when I would ask a question to a patient interviewee, he would say, but I know that! And he would offer the answer himself. I had wanted to compare different patients' perceptions, for example of how leprosy was contracted, but his expectation was that I needed the accurate information, not variation, and his own expertise offered that information. In this case, the expertise of the translator both added to and detracted from the process of gaining knowledge through interview.

You may have noted, in reading through these cases, that many are examples of mistranslations, or questions or answers that were not translated—in other words, an interview where something went 'wrong'. This is a common theme in the history of research assistants, as exemplified by Steven Shapin's discussion of Boyle's laboratory:

> Technicians' work was transparent when the apparatus was working as it should and the results were as they ought to be. In contrast, the role of technicians was continually pointed to when matters did not proceed as expected. In such circumstances, technicians' labor (or, rather, the incompetence of their labor) became highly visible (Shapin, 1989: 558).

In my own interviews, it is easier for me to see where my research assistants, or translators, are producing results that are not as I expect or desire. Their labour becomes more visible to me through its problems than its rewards, though of course the rewards far outweigh any challenges that my translators and I might experience in trying to understand each other and our interviewees, or in negotiating between our priorities and expectations of each other's motivations, goals and expertise. This notion of the translator as a mediator—and a potentially problematic mediator because their own expertise, perceptions, and priorities influence that mediation—is, I believe, one of the reasons that the labour of translators is not more visible in research outputs.

The invisibility of translators

The labour of research assistants has not often been visible in anthropological and historical studies (Sanjek, 1993: 13–18). One of the many reasons for that invisibility lies in the search for authentic, unmediated access to the voices and knowledge of informants in the 'field'. In the late nineteenth century, intellectual credibility in the generation of scientific knowledge shifted away from 'armchair scholars', who based their conclusions on evidence gathered by others, to scientists who collected their data in the 'field' (Kuklick, 2011: 3). For anthropologists, this idea became enshrined in the method of participant observation, whereby anthropologists immersed themselves in a local culture, gaining understanding and empathy through language learning, proximity, and local relationships. This immersion lent authority to individual anthropologists, whose bodies were themselves instruments of inquiry: An idea stemming in part from the Victorian construction of heroic fieldwork, in which the rigours of fieldwork built enough character in the scientist to make his or her findings reliable (Kuklick, 2011: 12–13, 28–9).

The idyll of the lone researcher, gaining 'authentic' knowledge through fieldwork, cultural immersion, and vernacular language learning, has persisted among those in the social sciences and humanities who study Africa today. This persistence represents a continuation of a cultural ideal that looks to arduous, lone fieldwork as a means to build character, understanding, and academic rigour. It appears arrogant indeed to assume that one could build any adequate understanding of another place or group of people without seeing it personally, seeking for sources that were not produced by colonizers and foreigners, and gaining insight and input from those at the heart of the research in question. Moreover, any study relying only on externally produced sources would in all likelihood reproduce imperial power dynamics by denying the former colonized any voice in the telling of their own stories. Similar arguments have been made for the importance of learning a vernacular African language.[4] European languages were introduced by colonizers and missionaries, and tend to be spoken by those who have been privileged enough to receive a formal education. Not to speak an African language means that all research must be mediated through the language of the colonizer, thereby leaving out or distorting the voices of less privileged Africans—i.e. the majority. The Africanist who does not speak an Af-

4 The importance of learning an African language is emphasized both by scholars of Africa (from within and outside the African continent), and by scholars whose work does not relate to Africa. For example, in the American academy, the ability to speak an African language is regularly queried in African history job interviews, and increasingly it is mentioned specifically in job advertisements. However, as long as one has undertaken extensive fieldwork within Africa, it is possible to be taken seriously as a scholar of Africa without familiarity with an African language. Ultimately, the ability of cultural translation precedes linguistic translation.

rican language is in danger of reproducing the colonial power dynamics of language learning and speaking. The Africanist who can speak an African language can conduct interviews in that language, thereby gaining direct access to the stories of individuals and communities who have been invisible to the historical record. Even if she or he needs translators in other languages, learning at least one vernacular language demonstrates commitment to and solidarity with Africans. This is both a purported necessity in the endeavour to avoid reproducing imperial power relationships, and a means of augmenting the researcher's ability to undertake cultural translation. Translation is not only a linguistic endeavour, but also a cultural one.

These are powerful arguments in favour of sensitive fieldwork and vernacular language learning, but the emphasis on the lone researcher gaining authentic knowledge through close engagement with local people has a drawback: There is little space to acknowledge the labour of the research assistants, translators, friends, and casual acquaintances who contribute to the production of this knowledge. Ironically, the disciplinary imperative to access more authentic voices of and knowledge about Africans by limiting mediation in the process of knowledge collection, often comes at the cost of acknowledging the role of African facilitators and experts who contribute to research. The conventional ways to recognize the labour of research assistants and translators in the production of knowledge are either in the acknowledgements at the beginning of a publication, or in methodological discussions—most frequently in analysis of the bias that their mediation may have produced in the results. The value of direct engagement and immersion can imply that a translator is a necessary evil, whose mediation will alter the voice of the interviewee, thereby reducing the authenticity of the knowledge gained. There are various solutions to this 'problem': To learn one vernacular language and to focus one's research solely on the people who speak that language; to undertake research that only necessitates speaking with people who know the language of the colonizer[5]; or recognizing the bias inherent in translation, but providing intellectual support for its necessity, and limiting written analysis of that bias, so as not make a wider opening for criticisms of the authenticity or authority of one's conclusions.

The researcher is left with a choice—whose labour or perspectives should be (in)visible, or rather, whose voices are the priority? The 'authentic' unmediated voices of one group of people? The mediated voices of a wider range of people? The voice of the translator, who undeniably shaped the results of each interview? The latter choice is very rarely made, but it begs the question: Are the intellectual contributions of the translator a worthy absence in the name of bolstering the perceived authenticity of the voices of the interviewees? Or is there a place or a need to recognize the pivotal role that translators play in the research process? Note that I have, in part, judged the success of my own interview experiences by the presence of the translator's influence and expertise, but also by the absence of their intervention into the content of the interview itself. Even in seeking to make the contribution of my translators visible, my valuation of their invisibility is apparent.

The idea of the lone fieldworker as a privileged knowledge-maker has been criticized, both because it makes fieldwork—and thus the source of knowledge—invisible to anyone but the fieldworker; and because scholars have pointed out that fieldwork has never been a lone endeavour

5 Many Africanists pursuing such projects still learn a vernacular language, even if they do not use it in interviews.

(Schumaker, 2001: 253). In her study of the Rhodes-Livingstone Research Institute for social science research in central Africa, Lyn Schumaker examines the 'field' as a space where a coordinated network of individuals negotiated and shaped the production of knowledge. It was not a space where lone anthropologists simply extracted data; rather, knowledge was co-produced (Schumaker, 2001: 227). Research assistants and translators were and are inevitably a part of the field research process, and studies of knowledge production that recognize the role of these individuals are one answer to the issue of translators' invisibility. But how might those who are not writing histories of knowledge production, and who might therefore have less space to reflect upon the co-production of knowledge in research, recognize the contributions of research assistants in their written work?

Conclusion

Schumaker's discussion of the Rhodes-Livingstone Institute's history as one of the co-production of knowledge offers an intriguing possibility for re-writing the role of the translator into the methodologies of oral history (Schumaker, 2001: 3). Rather than explaining any potential bias that translators have brought into my research—which is how I was trained to approach oral histories as a doctoral student—I might frame the consequences of my interactions with translators in terms of the co-production of knowledge.[6] The interviewee, the translator, and I might *all* be understood as authors. The dual narratives of fieldwork in Senegal by linguists Fiona McLaughlin and Thierno Seydou Sall, from the perspective of researcher and informant, multiply authorship of the experience and results of fieldwork quite literally (McLaughlin and Sall, 2001). Such hybrid authorship would by no means be possible in every case, but it raises the possibility of multiple authenticities in the experience of fieldwork. The nature of scholarship is that we seek to write our findings with one authentic voice—but perhaps we should consider that fact that there are multiple authentic voices and forms of knowledge. What results might we obtain if the interviewee, translator, and interviewer are each recognized as authentic producers of knowledge rather than mediators of a single authentic voice?

As McLaughlin writes of their challenges in entering into a dialogue about and confronting issues of ethnographic representation, 'We have not solved them, we are merely more aware of them than ever' (McLaughlin and Sall, 2001: 189). I have no answer to the question of whether translators ought to be more visible in research, and have not even discussed the extent to which individual translators might wish for visibility or invisibility. But I hope that these reflections have made the labour of translators in my research a little bit more visible, and raised questions for further thought.[7]

6 The impetus to recognize and explain bias is one that is present in all forms of historical analysis, and has been particularly important in justifying oral history as a historical research tool. While most historians of Africa draw heavily upon oral histories, in some other geographical and thematic sub-specialties of history, oral histories are still looked upon with some skepticism as a source.

7 I recognize the irony that in an essay seeking to make the labour of translators more visible, I do not actually name them. This choice was a result of my desire not to tie an individual translator's name to an assessment that could be perceived by some as negative.

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Comments

The In/Visible Historian

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Since the 1980s, at least, historians of science have been attending to the kinds of labour that contribute to the production of technical knowledge. Take Steve Shapin's classic essay on invisible technicians, which made it impossible to ignore the forms of expertise required to construct (and to use) the most powerful experimental apparatuses of early modern science (Shapin, 1989). Take Naomi Oreskes' sensitivity to gender, which exposed how heroic 20th-century discourses of objectivity camouflaged forms of knowledge produced by women (Oreskes, 1996). Take Donna Haraway's work on the gendered labour of primatology researchers as well as the role played by non-human organisms in scientific research (Haraway, 1989; 1997).

More recently, historians and allied science studies scholars have 'reinvested' in the subject of labour, with compelling results. For instance, new work in the history and sociology of biomedicine has considered patients, research subjects and tissue donors as engaged in 'clinical labour' (e.g., Cooper and Waldby, 2014). The very terms 'patient', 'subject', and 'donor' are provocations when viewed in in terms of labour. What kinds of relationship to the production of value (economic, emotional, epistemic) have these categories come to signify and why? What kinds of relationships have they helped to privilege or, alternately, to efface and how? How does the historian interested in invisible labour deal with his or her own relationship to the knowledge production process? This last question is especially pressing as historians of science confront ethics, not as a set of normative or bureaucratic practices, but as a situated field of obligations. These obligations include responsibilities to all those we portray in our histories—scientists and other technical workers as well as *their* subjects and objects of analysis.

Along these lines, one theme that I would like to highlight involves the spatial and temporal dislocations that make labour sometimes visible and sometimes not—what STS scholar Astrid Schrader might call "phantomatic" (Schrader, 2010). My research has involved attention to the labour involved with accumulating blood collected from indigenous bodies and frozen such that it could serve as an open-ended resource for research in epidemiology and the life sciences. This blood has persisted for decades and, periodically, it is thawed for uses other than that for which it was originally extracted. Indeed, in the twenty-first century certain of these now decades-old, cold blood samples—representing dozens if not hundreds of indigenous populations from around the world—are being subjected to efforts to recover old microbes, such as malaria, also incidentally frozen along with other potentially valuable materials about which scientists can only speculate.

The process of prospecting old blood for new uses has intensified as the demand for unique human specimens has escalated. In the twenty-first century, the rise of personal genomics has made the biospecimen appear as the "center of the universe of molecular medicine" (Compton, 2010). In such studies, DNA samples from members of diverse human communities have become valued for research into targeted therapies to specific populations (Bliss, 2012; Lee, 2013). Blood samples collected from communities in remote regions have been seen as especially valuable for understanding the etiology and origins of infectious disease (Radin, 2014). Studies concerned with human evolution and migration emphasize the value of salvaging genetic material from communities thought to be isolated and in danger of disappearing due to the encroaching forces of modernity (Reardon and TallBear, 2012; Kowal, Radin and Reardon, 2013; TallBear, 2013). At the same time, members of certain indigenous communities have determined that they do not wish to continue participating in these recurrent cycles of value production. They have become visible as activists who have demanded, and, in some cases, succeeded in effecting the repatriation of frozen blood (Harmon, 2010; Kearns, 2015; Radin and Kowal, 2015).

In the long periods of time that separate the collection of this blood and its reuse is an invisible history of maintenance (Russell and Vinsel, 2016). Making the freezer 'work' as a technology for maintaining biological materials such that they can be reused—or returned—involves many types of labour including that of technicians, janitors, students, scientists, institutional review boards, activists, as well as beliefs about what it even is that freezers do. It makes sense to consider the various forms of human labour involved with making biomedical knowledge *as well as* the machines and non-human life forms that also play a crucial role in the enterprise.

Contributions to the workshop by Sarah Blacker, Rosanna Dent and Susan Lindee also help historians to bring into view the complex ways in which temporality, in particular, constructs fields of visibility and invisibility. Blacker (not in this volume), for example, called attention to the seemingly incommensurable timescales that complicate efforts to understand environmental risk in the territory occupied by First Nations people in Canada. The long-term, trans-generational knowledge of indigenous people who live on the land is often seen to be incommensurable with the forms of measuring risk familiar to scientists, who translate the results of their own short-term field trips into technoscientific terms that appear more legible to bureaucratic regulatory regimes. In such encounters, those who know and live on the land are interpreting shifts in the environment that are often ignored due to the structures of what gets to count as 'reliable knowledge.'

For Susan Lindee, the long-term horizons of radiation risk and equally distant horizons of remediation mutate historical and epidemiological knowledge, alike. Her 1994 history of the Atomic Bomb Casualty Commission's work with the survivors of the atomic bombing of Hiroshima and Nagasaki has acquired new relevance in the wake of the Fukushima meltdown (Lindee, 1994). The forms of inquiry offered by science studies scholars, which include attention to invisible labour, comprise an effort to respond to the uncertain horizons of risk that accompany large-scale technological systems, risks which are multiplied exponentially when the focus is on innovation as opposed to maintenance. Lindee's continued engagement in the activities of the RERF has led her to entertain the idea that science studies, more broadly, is a product of the bomb and plays a role in ensuring its aftermath receives continued attention. She considers what it means to be a part of maintaining this legacy, to have 'skin in the game'.

The scientists who worked on assessing fallout during the Cold War also trained their focus on members of communities that they perceived to be as yet untouched by modernity and its toxic byproducts (Salzano and Hurtado, 2004). Human geneticist James Neel, a key figure in the Atomic Bomb Casualty Commission, also conducted work with members of the Xavante population in Brazil, including efforts to collect and freeze their blood. In the 1960s, Neel regarded the Xavante as an isolated, 'stone age' population (Neel, 1970). Today, the Xavante are one of the most studied communities in the world. Over some sixty years, as Rosanna Dent is exploring, future visions of political and social advocacy on the part of Xavante people have become part of an exchange with scientist's shorter-term hopes of acquiring knowledge and research materials. For Xavante representatives, agreeing to participate in longitudinal studies has become a practice of maintenance, a way of extending the possible ways that a community is made visible and therefore capable of surviving. In a telling mutation of the role of the historian in such contexts, Dent's engagement with the Xavante people has involved constructing an archive to serve the community, in a way that is self-conscious about the longer history of researcher's regard for the community as a bodily archive from which to extract knowledge about human history.

In each of these cases, the labour of the historian is uniquely visible as our subjects—through time—make claims upon our expertise and we confront our reliance on access to their experiences.[1] We may not always be comfortable with making ourselves visible in the text, but the ethical questions prompted by discussions around labour make it impossible to ignore the extent to which historians may come to, or already have, 'skin in the game'. Historians of science, especially those who are interested in countering normative bioethical or legal prescriptions about informed consent, property, and harm are making important contributions to the history of science when they ask questions about labour, in particular the routine labour of maintaining research agendas, technologies, and commitments to justice. The clinic, the lab, and the field are amenable to historical interpretation as 'shop floors'—but they also complicate older ideas about how, where, and especially when labour exists to be employed, exploited, or organized in the service of knowledge.[2] The invisible labour of the historian should be an ethical *as well as* an epistemological consideration. We should be attentive to the ways our own knowledge production enterprise potentiates new forms of value latent in the labour of our subjects.

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Notes on Inscription and the Archive

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An inscription is a stabilized form of knowledge that can last over time and be repeatedly accessed. Roland Barthes points out that in "every society a certain number of techniques are developed in order to *fix* the floating chain of signifieds, to combat the terror of uncertain signs" (Barthes, 1986: 28).[1] Inscriptions engraved in clay around 3000 BC in Mesopotamia testify to the ways in which writing and arithmetic originated together out of the growing need for durable administrative tools (Damerow, 2012). In the following notes I discuss some modern examples of inscription presented in this volume that were made in an endeavour to capture human social and cultural life in words, images, or symbols via processes of observation, abstraction, and encoding.

In her essay, Whitney Laemmli describes and analyses the 'Choreometrics' project carried out by anthropologist and folklorist Alan Lomax and dance experts Irmgard Bartenieff and Forestine Paulay in the second half of the twentieth century. With the help of unnamed 'raters', these researchers and dance experts, transcribed key features of filmed folk dances into text and diagrams. In this way, they felt able to compare the dance patterns of different ethnic groups in statistically reliable ways. A further purpose of their project was to preserve human heritage and cultural diversity. Similarly, the Oneida Ethnological Study, started in the 1930s at the initiative of linguist Morris Swadesh, sought to document and preserve the language of the American Oneida Indians, as Judy Kaplan shows in her contribution to this volume. Boris Jardine presents another case study originating in the 1930s: 'Mass-Observation' was a social survey executed in the UK based on questionnaires and diaries. Such inscriptional endeavours—whether codifying on paper dance movements (Choreometrics) or endangered languages (Oneida) or the witnessing of everyday life (Mass-Observation)—aimed to archive the ephemeral in a permanently accessible, thus 'visible' form.

Processes of inscription pose a special challenge when it comes to the temporal dimension. As Roland Barthes explains in relation to photography, recording techniques have revolutionized the way humans experience reality:

> [T]he photograph institutes, in fact, not a consciousness of the thing's *being-there* (which any copy might provoke), but a consciousness of the thing's *having-been-there*. Hence, we are concerned with a new category of space-time: immediately spatial and anteriorly temporal. (Barthes, 1986: 33)

1 The quote is from the essay 'Rhetoric of the Image', first published in French in 1964.

These counterintuitive properties of things that are experienced in the immediate space (in front of the viewer), but expressly referring to a past time, pertain not only to photographs but also to archival objects in general. When we consider text as a particular kind of archival item and examine spoken language in relation to written language, as linguistic anthropologists do, a further kind of temporality inherent in archival artefacts becomes evident. Kaplan shares in this volume Walter J. Ong's view that both speech *and* text are 'events' taking place in a temporal dimension. This is a paradoxical condition for written language, which becomes on the one side "fixed, recuperable, manipulable" (Ong, 1988: 262) and on the other side moving "through time" as "sounded words" when read (Ong, 1988: 265). Recent developments in information technology have made the accessibility of past events—though in translation, i.e. with the loss of original elements and dimensions—a staple feature of contemporary life. But prior to achieving this degree of access, human scientists have implemented strategies for capturing a given moment, as we have seen in these particular case studies. Artistic performances, spoken language, thoughts and emotions had to undergo a process of abstraction in order to be codified and made permanently accessible.

Observation (or witnessing) is a precondition for writing an event down, either by human and/or mechanical means. In the natural sciences, methods of collecting observations and making them accessible to contemporary and future scholars gained momentum during the seventeenth and eighteenth centuries. Virtual communities were built beyond the boundaries of countries, families, and teacher-disciple relationships for sharing observations and exchanging specimens, in search of "general, constant features" (Daston, 2011: 105). As Lorraine Daston points out, observation achieved the status of an epistemic category during this period resulting from major innovations in the practice of observing and sharing the information with the help of new tools "like the telescope and microscope … the questionnaire [and] the synoptic map." All of this served to engender "new forms of reasoned experience" (Daston, 2011: 82).

With this historical and theoretical background in mind, we can easily find traits of scientific observation in Jardine's case study of the Mass-Observation project: it involved a network of observers who used standardized tools and methods; moreover, the project's initiators promised to seek after common elements from a multitude of observations on people's behaviour (Madge and Jennings, 1937). Such a social laboratory aimed initially to expand the boundaries of science—including the arts—in its scope, but, as Jardine describes, ended up in limiting itself to the practice of social science. He draws our attention also to the problematic utilization of statistics that resulted from the data collected for commercial and political uses. During the twentieth century, statistics have become one of the most powerful tools in the social and behavioural sciences. Mihai Surdu and Christine von Oertzen provide, further, two striking examples of 'making numbers out of people.' The Mass-Observation project can be considered an effort to reconcile the dehumanizing anonymity of statistical abstraction with the empowerment of ordinary people, who could become 'experts' in exercising observation and self-observation.

In the *Encyclopaedia Britannica,* sociologist Neil J. Smelser gives a behavioural definition of 'mass' that is particularly useful for understanding the socio-anthropological enterprise of the Mass-Observation project: "Member of a mass exhibit similar behaviour, simultaneously, but with a minimum of interaction" (Smelser, n.d.). Although the designers of the Mass-Observation project do not seem to have embraced any politically revolutionary ideology, the term 'mass' has revolutionary connotations, with the October Revolution of 1917 and other European revolutions following World War I forming part of the Mass-Observation project's immediate past. In his impressive 1919 theatre play *Masse-Mensch*, for example, German writer and revolutionary Ernst Toller cast dramatis personae with no personal names—with one exception, "Sonja Irene L." Rather than individuals, these characters were exclusively considered as types: 'the man (civil servant)', 'the nameless man', 'workers', 'imprisoned girls', 'army officer', 'priest'. This drama shows the unresolved conflict between the powerful, anonymous, and a-moral mass and the moral, sensitive individual, who bears a name.[2]

Other than propagating the mobilization of the masses, as it happened in continental totalitarian systems of their time, the designers of the Mass-Observation project promoted a kind of ironic patriotism. They let their observers record any, even insignificant or irreverent, event that took place during the coronation of King George VI on 12 May 1937. Their unpretentious attitude reverberated from the political to the scientific domain when Charles Madge, one of the initiators of Mass-Observation, noted in an essay published a couple of months before:

> The statements are useful also to scientists who can each utilize them in his [sic] own way. The number of scientific interpretations of a given body of material is only limited by the number of scientific interpreters. (Jennings and Madge, 1937)

Such an emphasis on plurality and incompleteness evokes the key feature of postmodern theory, namely its post-structuralist view of the discontinuity and heterogeneity of knowledge.[3]

The words, 'a given body of material,' lead us back to the main topic of this volume, namely, the manifold methods used by different agents in collecting material for scientific purposes, making it visible and accessible to others in the present and the future. I would like to highlight, at the end of this comment, that all of the contributions in this volume share an ethical concern as to the visibility of the knowledge producers involved, implying different aspects of fairness, individual rights, and public credit.

2 Ernst Toller wrote this play in 1919 in jail. It was published in 1921 and translated into English in 1923 by Vera Mendel as *Masses and Man,* and in 1924 by Louis Untermeyer as *Man and the Masses.*

3 See Foucault (1966) and Sontag (1966). See also Gumbrecht (1997), entitled *In 1926: Living at the Edge of Time*, in which the author puts together, in alphabetical order, fragments of life of the year 1926 as witnessed in different texts from different countries, without trying a master narrative but producing instead "an essay on historical simultaneity" (XIV). Anthropologist Jeremy MacClancy (1995) sees in Mass-Observation a precursor of the 1980s literary postmodernism.

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Accounting for Knowledge Production

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The thoughtful, evocative, and well-researched papers gathered in this collection reveal in tantalizing ways the topics and issues that expand our historical detective work. They also further an ongoing conversation about why it is important to consider what has previously been hidden or unexplored, and what methods of inquiry pry open previously obscured elements of our past. Considerable historical inquiry, as demonstrated in these papers, is changing the ledger book that records the labour put into new knowledge. The research here demonstrates that such work has been (and continues to be) produced through cumulative, competitive, and collaborative efforts. Explaining the previous invisibility of critical aspects of workplace dynamics is essential to the historical project of discovery and revelation. The papers assembled here provide specific examples of the ways in which the invisibility of individual women (and men) alongside gendered and hierarchical categories of labour, have built and intensified the patterns of historical obscurity. The juxtaposition of invisibility alongside specific scientific work makes evident the subtle patterns that connect them.

Making women and gender visible

Women are evident in nearly all of these papers, but discussion of how gender may have influenced the processes and outcomes of knowledge production reveals just how difficult it can be to identify and articulate that relationship. Many of the papers investigated projects that were situated in significantly masculine surroundings or dominated by men sure of their authority and expertise. How much did the masculinities of such settings, I wonder, help to explain how erasure, appropriation, dismissal, invisibility or even cautionary discretion regarding women occurred? What traditions, assumptions, and behaviours set up gender distinctions and categories? How can we best interrogate the settings, rather than taking them for granted as the backdrop for simply watching human behaviours without taking gender (or perhaps class and other social factors) into account? This is not a new concern, of course, because historians and other scholars regularly acknowledge that we cannot take our historical settings and sources for granted as we seek to understand how knowledge is produced and disseminated. Still, it was important to recognize, as Christine von Oertzen reminded us, that as scholars we can too easily re-inscribe gender dynamics in our own work. She provided an alternative in her detective work in the Prussian archives that pushed below the official description of census calculations in order to reveal women's efforts. Her account opened up a reality that was deliberately masked because, among other things, the obscuring of their data processing both protected the identity of the women census workers and sustained a representation of masculine work that coincided with bureaucratic criteria.

It is significant that nearly all of the authors seem quite deliberate in their inclusion of women, reflecting insights from an array of women's studies research. Susan Lindee's sources on the aftermath of the radiation catastrophe in Japan include reference to a woman who struggled to determine what foods were safe for her family. This provides a further important counterpoint to the abstract measurements of (largely male?) experts who often standardized and generalized radiation risk. Similarly, at the workshop (but not in this volume), Sarah Blacker offered an indigenous woman's voice elaborating on the ways that pollution from extensive mining in Canada affected daily living in her community. Local family life, as Elena Aronova noted, contributed to systematic meteorological and other data gathering that residents of the Soviet Union hoped might eventually allow them to predict the dangerous earthquakes that challenged their very existence.

But the kind of intimacy that could facilitate knowledge production and problem solving need not necessarily be local. If male gender affinities could sometimes work to the exclusion of women, female affinities could also play a positive role. While Rosanna Dent's essay acknowledges the somewhat ambiguous local status of one of the significant early women anthropologists among the Xavante, she personally formed a bond with a woman elder who improved her language and taught her cultural nuances. This offset the more formal engagement and less revealing interactions with male tribal members who initially gave permission for her work. In a very different way, Whitney Laemmi shows us that two women dancers (Irmgard Bartenieff and Forestine Paulay) were critical facilitators in Alan Lomax's ambitious project to record dance and movement around the globe. The familiarity of these women with dance provided them access that would have eluded Lomax despite his inclusive intentions.

In the information age begun in the twentieth century, gathering data and using it for research purposes in topics that loosely cohere as the social sciences has become central; more than half of the papers in this volume are on such topics. In these subject areas, understanding how gender and sexual identity operated in the shaping of questions, the acquisition of data, and the interpretation of survey outcomes is essential—though often overlooked in the initial studies. Boris Jardine's presentation of Mass-Observation included a clip from a television dramatization of the group's processes, which reminded us that many of the active participants were, in fact, women.[1] While men were the organizers, did they deliberately encourage women to volunteer and did they believe that women would be more effective in soliciting responses by telephone? Similarly the participation of 'self-trackers,' described by Josh Berson, opens the question of the distinctly gendered selves being marketed to by the Quantified Self movement. Laura Stark's illustrations of 'Normals' at the National Institutes of Health documents medical hierarchies (male administrators and physicians with female nurses) during the mid-twentieth century. Thus gender here also played a mediating role, with women having the most immediate and social relationships with (mostly male) patients and doing the essential and often defining

1 *Housewife, 49* (2006), directed by Gavin Miller, broadcast by the British ITV, captures a glimpse into the purpose of the project in the first minute. The clip can be viewed here: http://www.dailymotion.com/video/x2npydg.

record keeping, some routine and some subjective. Similarly Kathleen Vongsathorn accessed patients and health workers through primarily male translators. Her interrogation of what that translation actually meant is a reminder of cultural as well as linguistic dynamics, social sensibilities, and particularly gender when the investigators turned to the topic of sexual behaviour. At the workshop (but not in this volume), Myriam Klapi identified women in the interpretative work with deaf people, and suggested in discussion that more might be noted about the relative gender representation of pupils and teachers and intentions for their outcomes. Mihai Surdu's Roma populations were interviewed with 'male' and 'female' as standard categories. The question remains whether women or men were more likely to self-report their ethnic identity, if that information made them vulnerable. Lastly, Lara Keuck's investigation of the 'original' Alzheimer-identified patient prompted my recall of the 'feeble-minded females' who were disproportionately institutionalized in the United States in the late nineteenth century. Were such women presumed to be more or less available or compliant for medical studies, and, if so, what distortions did that introduce?

This deliberate and sometimes challenging commentary on women in these essays is not, of course, to add women and stir. Rather, my intention was to suggest at the workshop some of the ways in which a deliberately conscious look at gender dynamics may enable us to understand in a more sophisticated way just how gendered behaviours influence scientific participation and outcomes. These were exciting and evocative presentations and prompted further questions: What do we make of these gender tracings and what will be gained as we further investigate the nature of women's engagement with and as knowledge makers? Where did women's participation make a difference in specific situations and in what ways? Did gendered behaviour and assumptions in any way change the very nature of what was being accomplished? The collection reveals both gender and women in sometimes subtle but nonetheless significant ways as they operated in patterns of twentieth- and twenty-first century (social) science. The dynamics around knowledge and power are layered in these papers, offering the encouraging possibility that scholars will continue to provide ever more explicit commentary on these matters as we continue to move toward inclusive scholarship.

At a recent (June 4–6, 2015) conference in Prague sponsored by the Commission on the History of Women in Science, Technology, and Medicine, an emerging theme addressed techniques that facilitated collaboration among women—indeed, the extent of casual and systematic coordination suggested that this orientation forms an important norm among women in science. Given stories of achievement, an inference in some of the conference papers was that inter-connections and communication served to maintain visibility and acknowledgment of effort by and among women themselves. It raises the question: What is invisible and to whom? The angle of vision should be an important part of our attention to invisible labour. While some in positions of scientific authority were oblivious to the collaboration and contributions made by those around them—and that orientation influenced the historians who studied visible leaders—other peers and other collaborators often did recognize and acknowledge such labour. Rereading and expanding the record by attending to the voices of women is not always easy, but it is essential.

Reinstating the 'historical amateur' as a category

Several of the papers in this collection take up the recent history of citizen science, a clear acknowledgement of the layers of complexity in science and an implicit rethinking of the role of amateurs in the past. Until recently, historians of science have largely dismissed or ignored the term 'amateur' as either old-fashioned or stigmatizing. Originating in the Latin 'to love', the term originally suggested that, whether related to work or knowledge (typically in combination), the actor was doing something in relationship to expertise that was not in some way credentialed but yet vaguely or reluctantly acknowledged by those considered more expert. 'Amateur' in the eighteenth and early nineteenth century often was a self-attributed characterization and used to explain an individual's activity and to claim some proprietary rights in relationship to results. The term allowed for a more porous way of thinking about knowledge construction and dissemination, and, in particular, sometimes provided visibility to women and men in their own circles of knowledge engagement and beyond. Its diminution in the late nineteenth century meant that certain kinds of effort became less visible because being named an amateur was sufficient to keep women (and some men) out of the emerging professional, academic, and other institutional institutions where aspirations to 'scientific' status came to predominate and where credit accrued through various kinds of public recognition (publications, awards, participation in printed programs).

It may be worth thinking about the significance of the term, both in historical settings and by historians, whether or not it has been directly utilized or even explicitly ignored. 'Amateur' brought in elements of intellectual challenge and opportunity but also related to certain qualitative aspects of knowledge making. After all, 'to love' to do something implied an emotional or affective engagement. That might be a term among the upper classes who disdained any financial gain or among those of lesser rank who explained that they were not moving out of their status even if they participated in botanical collecting or astronomical observation. Perhaps most important for our discussion, amateurs were not 'invisible' to contemporaries—or at least to selected contemporaries. They were often viewed as part of knowledge making but, equally important, they were also involved in 'knowledge dissemination' or sharing (popular writings, education, illustration) as they worked with natural history, astronomy, physical science or chemistry. Although acceptance was uneven, amateur status created a space for participation in the circulation of knowledge.

By the twentieth century, amateurs were less visible in science. Accordingly, they were not discussed by historians of science who focused on those who were given authority, and thus visibility, through particular institutional affiliations. It is clear from papers in this collection that amateur levels of activity continued but under various other rubrics that included assistants, observers, adjuncts, and more specific names like 'calculators' in astronomy. As historians we want to name these active participants, acknowledging the particulars of their work and assigned occupational titles while pointing to the contributions masked by such factors. As the papers here demonstrate, additional categories where genuine contributions were made included students, post-docs, librarians, family members and others whose work could easily be absorbed into the credit of a well-positioned and well-credentialed expert. Whether or not we revive the vocabulary of 'amateur', some of the qualities it implied are worth recall in our discussion of knowledge work.

A final consideration in reflecting on labour, perhaps especially labour that is invisible, is that of motivation. Although the term labour carries multiple meanings, it most commonly implies work for remuneration of some kind. Yet accounts of intellectual labour suggest that, whether described as amateur or not, the direct engagement is often tied to something more than financial compensation as experts and amateurs alike comment on inspiration, engagement, enjoyment, and satisfaction as motives for their work. In fact, studies of patterns of partnerships in science make it clear that the incentives to engage in knowledge production often includes multiple and intangible rewards beyond money and recognition. Accounts of citizen science, from regular astronomical observers to those who participate in cloud sourcing projects, are a reminder of the reinvented ways in which collaboration is implicated in the fostering, disseminating, and genuine excitement of knowledge making. This is not to discount the importance of payment and individual recognition, which is also all-too-often unequally distributed, but the qualitative and less tangible rewards also deserve attention.

The papers in this volume, taken collectively, remind us that in accounting for labour involved in producing science, the calculations must be inclusive and identify work that has been obscured, ignored, or undervalued. The participants are part of a lively conversation to which they have made significant contributions through explication of their methods and outcomes. Our common goal is to reveal the categories and mechanisms that allow us to identify, make visible, and even calculate with specificity the layers of labour and networks of communication that contribute to knowledge production.

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