2014 | *Hau: Journal of Ethnographic Theory* 4 (1): 211–236

SPECIAL SECTION

After objectivity

An historical approach to the intersubjective in ethnography

Peter Pels, *University of Leiden*

Intersubjectivity in anthropology has rarely been studied against the broader background of the place of intersubjective exchanges in the long-term history of anthropology. This article attempts to do so by setting the history of anthropology against the history of objectivity since the Enlightenment as outlined by Lorraine Daston and Peter Galison's seminal *Objectivity* (2007). On that basis, it concludes that the currently popular romantic ideal of a dyad of interchanges between researcher and researched does not give a proper impression of what anthropology is about. Instead, it argues that the mission of anthropology is based on an expertise about cultural classification that cannot be divorced from the asymmetrical breaks with everyday perceptions provided by ethnographic methodology.

Keywords: intersubjectivity, objectivity, history of anthropology, classification, methodology

For anyone acquainted with the history of anthropology and ethnography, there is often something not quite right—something "Whiggish" (Stocking 1968: 3–4)—about anthropological representations of intersubjectivity. Many attempts to grapple with a legacy of colonial inequalities try to make anthropological research relationships less asymmetrical by reframing them in terms of advocacy, collaboration, dialogue, and positive engagement between researcher and researched (Dwyer 1977, 1979; Hastrup and Elsass 1990; Lamphere 2003; Lassiter 2004; Rappaport 2008; Tedlock 1979). This is historically ironic, for it relies on a dyadic reduction of the multiplex social relationships of anthropological research that was first formulated under late colonial circumstances in order to safeguard the asymmetrical, superior position of the anthropological expert—at the time when, for example, Bronislaw Malinowski tried to erase colonial administrators, traders, and missionaries (as mere "practical men," that is, amateurs) from contemporary

ISSN 2049-1115 (Online). DOI: http://dx.doi.org/10.14318/hau4.1.009

conceptions of anthropological work (see Malinowski 1922; Pels 2011). Unless we subscribe to the conceit—refuted by the anthropology of colonialism (Pels 1997, 2008)—that we can leave the colonial past behind without a trace, we cannot assume that ethnography is sufficiently characterized by an intersubjective dyad. Instead, we should study how intersubjective epistemological conditions were interwoven with asymmetrical standards of objectivity that, more often than not, grounded a hierarchical anthropological claim to expertise in the field of classifying and understanding human difference(s). In this article, I try to show that the intersubjective only became part of ethnographic authority *after* "objectivity" emerged as a historical construct.[1] It did so, I propose, in two steps: one, around 1900, that introduced the need for taking native categories as a starting point of ethnographic method; and a second, in the 1970s, that made the reflexive scrutiny of Western classifications of others into a necessary condition of research. This should support my conclusion that, even today, a methodology of making asymmetrical reflexive breaks with the subjectivities, classifications, and categories encountered during research is necessary to avoid turning an, at best partial, representation of the epistemology of anthropological research into an ethical fantasy of intersubjective harmony.

Toward a history of the objective and subjective in anthropology

If the dyadic imagination of the ethnographic relationship became hegemonic after World War II, this should not persuade us that it thereby also became an adequate representation of anthropological practice (see Pels and Salemink 1999; Stocking 1991). By this, I do not mean to deny that the intersubjective is both a positive and a necessary epistemological condition of ethnography. Well before Malinowski focused his professional claims primarily on the relationship between "the Ethnographer" and "his" natives, his teachers brought (as we shall see in the fourth section of this article) the intersubjective forward as a method of ethnography from within a more complex colonial epistemological hierarchy, and before them, the intersubjective was at least a practical condition of field sciences.

More recently, one of the first (and to me still one of the most profound) pleas for an intersubjective epistemology of social science was itself immersed in a dialogue with such asymmetrical standards of objectivity: Johannes Fabian told us not to describe the social and scientific process of learning the communicative competences of the environment we study, as many social scientists still do, in terms of "observation" and "data"—as a way of "looking on" from a distance—if we want to avoid misleading our students into thinking that they can study people without communicating with them. Calling it "observation" may impose visual standards of "objective" science from a laboratory physics that have a limited applicability to research on humans who, after all, talk back (Fabian 1971). Two years later, Pierre Bourdieu similarly sketched the "objective limits of objectivism" by returning

1. "Objectivity" is here treated as a discursive object that may signal little more than a certain aspiration to knowledge. I use the word throughout this article in this sense.

researchers to their subject position, when he argued that, after their break with empiricism, they had to make a second break with the prejudices introduced into their descriptions by being situated in the academy (1977: 2). Note that neither argument for the necessity of the intersubjective in ethnographic research includes the claim that is also sufficient. Even more, Bourdieu explicitly hinges his claim to expertise on a *double* asymmetrical break with situated subject positions. Fabian argued, thirty years after his initial and pioneering essay, that even if the ethnographic condition of intersubjectivity prevents us from assuming an initial hierarchical relationship between knower and known, it should also "counteract the anodyne, apolitical, conciliatory aura that surrounds 'communication' (or for that matter, 'dialogue')" (2001: 25). In the following attempt to outline a history of the entanglement of the intersubjective in anthropology with its contemporary claims to objectivity, I am inspired by both authors to argue that "objectivity," while originating in a historical situation that has now passed, reminds us of the necessity of such asymmetrical breaks for the constitution of anthropological expertise. Indeed, the constitution of expertise seems fundamentally dependent on a social practice of articulating its difference from lay knowledge (for better or worse). If, then, the "antisocial anthropology" of making such breaks is an inevitable moment in anthropological epistemology, we may also be in a better position to see how it can be complemented by another intersubjective moment of "objection" (both steps suggested by Mosse 2006).

Both anthropologists and their historians have so far neglected the history of such constitutive aspects of their work, the epistemology of objectivity in particular (Fabian 2001: 17). Standard histories of schools and theories do not regard objectivity and intersubjectivity as topics for discussion, although, as we shall see, George Stocking's work remains an invaluable treasure trove. More recent studies of the history of anthropology as a field science come closer, but do not systematically address these topics (Bravo 1996; Kuklick and Kohler 1996; Pels 2011; Schumaker 2001). A few have discussed how anthropology's qualitative standards related to mathematical or statistical models of scientific objectivity, but not with the depth and rigor one finds in social histories or science written by the likes of Steven Shapin, Ian Hacking, or Mary Poovey (Asad 1994; Pels 1999; see Shapin 1994; Hacking 1990; Poovey 1998). Fortunately, we can now juxtapose existing histories of ethnology and anthropology with the massive study by Lorraine Daston and Peter Galison of the rise (and fall) of objectivity since the early nineteenth century (Daston and Galison 2007). Their seminal book is both readable and brilliant, and makes a compelling argument that objectivity occasioned an irreversible shift in our thinking about science and the world. When studied from the point of view of the history of anthropology, *Objectivity* confronts us, firstly, with an embarrassing history of racial (and racist) classification that may seem diametrically opposed to today's conceptions of the intersubjective, but will nevertheless (as I hope to show) turn out to be relevant for the present. Moreover, *Objectivity* suggests that anthropology continued to employ the older epistemic virtue of the classification of types even after "objectivity" introduced new standards. Finally, while *Objectivity* could, perhaps, have taken more account the *practice* of contemporary field sciences—where, interestingly, naturalists, ethnographers, and geographers produced "objective" knowledge by making themselves dependent on collaborators who

"talked back" (cf. Jones 2012: 28)[2]—the book also helps explain why and how these intersubjective moments were subordinated to a hierarchy of epistemic values in the metropoles of science. Taken together, these insights from the history of science will help us pinpoint two crucial moments in the history of anthropology—the first when the subjectivity of people researched became a *sine qua non* of ethnographic research (around 1900); and the second when the subject position of the researcher was itself turned into an object of study in the 1970s and 1980s—and reflect on the significance of these moments for the present.

I will first outline Daston and Galison's argument and the advantages and limitations of their focus on the visual culture of science. The next section will discuss the early history of anthropology in terms of Daston and Galison's notion of "mechanical objectivity" by zooming in on anthropology's focus on language, race, and material culture, but complicate that picture by juxtaposing the field sciences to metropolitan expertise, using the history of natural history and its reliance on an earlier typological epistemology, as a counterpoint. The third section will sketch how Daston and Galison's conception of the late-nineteenth-century critique of "mechanical objectivity" by "structural objectivity" and "trained judgment" may apply to late imperial anthropology. Daston and Galison suggest that many contemporary natural scientists saw their "trained judgment" as comparable to how the observation of physical features led to the classification of race. In contemporary anthropology, however, racial classifications became suspect and "primitive classification" became a topic of research at more or less the same time that "trained judgment" in anthropology came to refer to the incorporation of the subjectivity of the people researched (while maintaining a hierarchy of so-called objective values in theory). The fourth section sketches how, after decolonization, the sociocultural and political process of global classification itself became one of anthropology's core objects of study in the work of Pierre Bourdieu, Edward Said, and Johannes Fabian. This leads to a conclusion that reconsiders the value of asymmetrical epistemological breaks in the present, along the lines already suggested above.

Objectivity as a historical construct

Lorraine Daston's and Peter Galison's *Objectivity* (2007) is perhaps best summarized by saying that it tries to chart a succession of dominant "epistemic virtues" in the European scientific tradition since the Enlightenment by means of a study of scientific atlases. The visual representations in atlases were meant to give apprentice scientists quick access to a reality often obscured by the bewildering variety of empirical manifestations of specimens, organs, or celestial bodies (and much else). Daston and Galison expected this material to show what scientists from widely differing disciplines thought would be the best way to represent the natural world, and found that these epistemic virtues could be summarized in three phases: the effort to be "true-to-nature" of Enlightenment taxonomy, the "mechanical objectivity" that emerged since the 1830s, and a critique of the latter from the 1880s

2. Jones's essay is part of a useful symposium on Daston and Galison's book (Dear et al. 2012). See also Kim (2009).

onward that can be characterized by the coexistence of two principles: "structural objectivity" and "trained judgment." "Mechanical objectivity"—the ideal of depicting individual "facts of nature" (or specimens) without interference by human subjects—appears quite late in the development of science (ca. 1830). It overlays, but never completely replaces, the Enlightenment ideal to depict representative types (or species) through the collaboration of savant and artist (or "four-eyed sight"). Instead, the blind sight of a machine becomes preferable to the subjective intervention of an artist or even a savant. Some of these core assumptions are already under siege by the 1880s, when some say that the only real reality are the structures of "relational invariants" that are only accessible by (for example) mathematical and statistical means, and others that the subject's judgment needs to be trained to perceive the "family resemblances" that define the objective. The book concludes with the suggestion that the period when scientists thought of themselves primarily as representing (rather than intervening in) the world may be at an end.

The book is important because of the main thrust of its argument: that objectivity, when treated as a recent historical construct with a brief lifespan, cannot at the same time remain the standard of true knowledge against which a secular and modern society can measure eternal progress toward scientific superiority. Yet, as history, the introduction of mechanical objectivity also marks an *irreversible* development: whereas the Enlightenment taxonomic tradition of classifying types, for example, did not disappear, it would never be the same again. This will, as we shall see, affect our understanding of the history of ethnology and anthropology profoundly.

Daston and Galison's focus on the atlases and their visual images also has important historiographical consequences: it counters the classical interest of historians and philosophers of science in the succession of theories and discoveries of new facts or technologies, and, as a result, the interest in breaks with previous ideas, or, in a more relativist mode, a succession of incommensurable paradigms. Put differently: the history of visual and material methods in the history of science seems much less discontinuous than the succession of theories or paradigms. The focus on the atlases also reveals how ethical and epistemological values merge in ways that standard histories of science have marginalized (Daston and Galison 2012: 35). Perhaps most important, however, is that it is less the distinction of these "epistemic virtues" than their historical layering that is important:

> Truth-to-nature (types) is positioned against mechanical objectivity (individuals), but then mechanical objectivity is addressed by structural objectivity (relational invariants), and trained judgment (families of objects). This division does not mean that each replaced the former in sequence: on the contrary, *each new regimen of sight supplements rather than supplants the others*. (Daston and Galison 2007: 318, emphasis mine)

Thus, different ways of making sense of the variety of phenomena coexisted in historical layers, even if the previous layer's assumptions about scientific virtue explicitly contradicted the later one's. These epistemic virtues (unlike the scientific theories or paradigms presumed by the logical positivists, Karl Popper, or Thomas Kuhn) do not refute or displace each other: they cohabit and continue to interfere with each other even if the assumptions that belonged with a previously hegemonic

epistemic virtue lost credibility. The study of the visual and material culture of "hard science," therefore, shows that it is built on "hierarchies of credibility" just like history (Stoler 2009: 181) rather than on the exclusion of previous insights by later ones. It is a palimpsest overwritten more than once; or, put differently, it is characterized by a "bush of variations" rather than a linear succession of tradition and modernity expounded by so many modernists (cf. Ferguson 1999).

The focus on the visual materialization of epistemic virtues combines ontological, epistemological, and ethical assumptions: "truth-to-nature" combines the scholar's multitude of observations with the artist's skillful drawing. The process of determining "species" from individual "specimens" of Linnean botany can be taken as exemplifying such four-eyed sight. From approximately 1830 onward, however, the rise of mechanical objectivity makes any subjective intervention in the depiction of nature—by both artist and scholar—suspect. By restraining their will and desires, scientists tried to let blind sight determine the depiction of individual phenomena, a process for which photography, while it did not cause the epistemic shift, became emblematic (Daston and Galison 2007: 187).[3] From 1880 onward, structural objectivity criticized mechanical objectivity's promise of a direct access to nature. Mistrusting "any kind of seeing" on the basis of advances in sensory physiology, this notion shifted the ontology of the objective to the kinds of relational invariants presumed by a new breed of mathematicians who locked their scientific selves away "from nature and other minds alike" (2007: 256–57). That attitude, however influential, would not do for more empirical sciences, where mechanical objectivity was instead criticized by emphasizing the "trained judgment" necessary to complement the machines used in the depiction of nature. Trained judgment was often consciously compared to the capacity to visually distinguish members of "races" on the basis of "family resemblances" ("physiognomic sight": 2007: 314). Anthropology is, of course, an important part of this history since the anthropological critique of "race" as an explanatory category seems to coincide with the rise of Daston and Galison's "physiognomic sight."

Despite the critiques of mechanical objectivity of the later nineteenth and twentieth centuries, however, the ideology of blind sight was a powerful intervention that irreversibly changed the scientific landscape, if not in the depiction of specimens then at least in the conception of the proper morality of the scientist. As Thomas Huxley, at the height of the reign of mechanical objectivity and less than a decade before he cofounded the Anthropological Institute, told his interlocutor Charles Kingsley in 1863,

> I know nothing of Necessity, abominate the word Law. I don't know whether Matter is anything distinct from Force. I don't know that atoms are anything but pure myths. Cogito, ergo sum is to my mind a ridiculous piece of bad logic[,] all I can say at any time being "Cogito." The Latin form I hold to be preferable to the English "I think" because the latter asserts the existence of an Ego—about which the bundle of phenomena at present addressing you knows nothing. (quoted in Desmond 1994: 319)

3. Instead, Daston and Galison reduce the possibility for a techno-fetishist interpretation by arguing that mechanical objectivity helped the invention of photography as much as vice versa (2007: 161–73).

This lyrical and passionate mantra of agnosticism—rejecting materialism ("Matter," "Force," "atoms") and idealism ("Cogito") on the basis of objectivity ("bundle of phenomena")—perfectly illustrates the subject position of mechanical objectivity. It also shows that the popular form of this opposition that many continue to use today—that true ethnographic knowledge is only possible if the subjectivity of the observer is erased as much as possible from the process of knowledge production—derives directly from this nineteenth-century cultural precedent. The "objective" ethics of the absence of the observer—the fact that the ethnographer should ideally try "not to be noticed"—is still mentioned in recent popular textbooks (Eriksen 1995: 26). The interpretation of objectivity and subjectivity as a single nineteenth-century cultural construct rather than a mutually exclusive opposition is another major contribution of Daston and Galison's work.

However, their focus on the visual and material culture of science and its methods is incomplete without an understanding of natural history *as a field science*. Natural history, whether in its Enlightenment, true-to-nature version, or its Victorian, objective guises, was fundamentally dependent on centuries of collecting the material items on which metropolitan epistemic virtues could be unleashed (Jardine, Secord, and Spary 1996). The labor of collecting in the field proceeded in relative autonomy from the ideals formulated in the making of atlases and the encyclopedic desires they objectified, and despite paradigmatic shifts in scientific doctrines. The emergence of objectivity, therefore, was conditioned by a collecting practice long before discourses of depicting individual objects emerged in the early nineteenth century. The asymmetrical practice of collecting objects *as objects* produced or collected elsewhere—that is, as things abstracted from their contexts of production and consumption—preceded the epistemic regime later defined as mechanical objectivity by Daston and Galison, first as "curiosities," then as "specimens" (see Daston 1994; Koerner 1996; Whitaker 1996).[4]

The practice of collecting rarities, specimens, or objects in the field, however, brought specific conditions of knowledge-production with it: the objects collected had to be translated and appropriated into the discourses and institutions of metropolitan science. This required metropolitan scientists to negotiate such asymmetrical relations of knowledge, power, and identity with collectors and their assistants (Kuklick and Kohler 1996). For example, when comparative anatomist Georges Cuvier reviewed a report written by that undisputed master of field science, Alexander von Humboldt, he challenged the latter's expertise by arguing that "the traveller can only travel one road" while the museum observer in his own *cabinet* "can roam freely through the universe" (quoted in Outram 1996: 261). If such elite members of the scientific establishment already disagreed with each other, it is unlikely that intersubjective collaborations of the field collector with the people observed, the assistants in the field, or the metropolitan scholars on whom the former relied for the translation of knowledge into a scientific canon could be openly acknowledged. The fundamental condition of the validation of knowledge collected in the field was that a network of reliable and trustworthy persons who connected the field with "home" authoritatively endorsed the classification

4. Lorraine Daston outlined the role that collecting such objects played in early modern science elsewhere (1994).

of the objects acquired abroad (Camerini 1996; McCook 1996). As science was institutionalized primarily in museums and voluntary societies, the latter largely controlled not only the data produced but also the criteria for what was to be considered an object of study, who could count as a proper scientist, and who as a mere collector. In other words, field science, and the intersubjective practices that went with it, played a negligible role in the process of *imagining* scientific development, at least from a metropolitan perspective. Yet, Daston and Galison's seminal study of scientific atlases would remain parochial (even somewhat "internalist") if it were not contextualized as part of a more global consciousness pertaining to the worldwide practice of collecting and the development of field science. Without it, the "science" historicized would seem to have happened in a world in which colonialism had no impact on modern markets and statecraft.

Language, race, and material culture: The reign of objectivity in anthropology

Histories of anthropology commonly discussed the period of mechanical objectivity in terms of the development of racial doctrines of monogenism and polygenism, or the emergence of the questionnaires by which metropolitan intellectuals tried to discipline colonial travellers who collected objects and specimens in the field. The period is rarely commemorated with pride: many anthropologists regard the early nineteenth century with embarrassment, as the "dark ages" of the history of anthropology (Stocking 1973: xii). It is, however, a poor science that does not look its own history squarely in the eye, and I believe that tacit avoidance of anthropology's colonial history may prove to be more detrimental to our knowledge claims than any confrontation with its genealogy of racism and colonialism.

Daston and Galison's view of this period in terms of the succession from truth-to-nature to mechanical objectivity, and the palimpsest that both formed by the 1840s is particularly useful for understanding anthropology: in the "natural history of mankind," the preoccupation with the classification of types was never completely overruled by the depiction of individual specimens. However, it did change shape, and radically so: the "system of races" presumed by William Edwards in 1829 synthesized the naturalists' focus on biology and the historians' intellectual, moral, and political concerns to formulate, in the words of his disciple Paul Broca, the first "complete idea of race" (cited in Blanckaert 1988: 19). The idea of race rounded off (according to Martin Thom) a transfer of political value in the history of ideas from classical texts on the *polis* to "the barbarian tribe"—a shift that resulted in a description of European history, in the words of Thomas Hodgkin, as "disgracing" itself in (revolutionary) 1848 by its "wars of races" (Thom 1995: 1; Hodgkin cited in Stocking 1973: x). The monogenist theory presumed a single origin of humanity that conformed to Biblical chronology: it tended to emphasize the classificatory criterion of language over race. It remained hegemonic among most British ethnologists at least until 1863, when the Anthropological Society of London split off from the Ethnological Society of London founded by Hodgkin and James Cowles Prichard in 1844 (Bravo 1996: 339; Stocking 1971). Hodgkin, however, had himself inspired his polygenist friend William Edwards to found the *Société Ethnologique*

de Paris in 1839. Because Edwards, in the absence of colonial aborigines to protect, could not follow the example of Hodgkin's Aborigines' Protection Society he turned with his associates to scientific ethnology instead (Rosenfeld 1993: 193–94). Apparently, the intellectual and moral differences between both medical doctors, one a Jamaican-born French polygenist and the other a monogenist and devout Quaker, did not prevent them from agreeing about the need for ethnological classification. Such a consensus about the object of study is also suggested by the questionnaire for the use of travellers and Navy personnel produced by a committee instituted by the British Association for the Advancement of Science in 1839. Consisting of the young Charles Darwin together with Hodgkin and Prichard, the committee listed nine major headings, *starting* with physical characters, to be followed by language, individual and family life, buildings and monuments, works of art, domestic animals, government and laws, social relations, and "Religion, Superstitions, etc." (Stocking 2001: 170). Clearly, one did not need a polygenist to prioritize the physical differences between humans.

Of course, the question of the plural or singular origin of humanity did become a bone of contention during the struggle between a more plebeian contingent of "anthropologicals" and the more establishmentarian "ethnologicals" from 1863 until 1871, when Thomas Huxley rejoined the two groups in the Anthropological Institute (Stocking 1971). However, the intellectual landscape in which objectivity materialized in anthropology had by that time also changed: the discovery of human remains alongside extinct animals in Brixham Cave in Kent in 1858, and the publication of the theory of natural selection by Darwin and Alfred Wallace a year later, made it easier to think racial differences as an outcome of a much longer process, helped by the increasingly plausible thesis of sociocultural developmentalism. Polygenism and monogenism could be reconciled by the emerging three-stage conception of evolution in terms of savagery, barbarism, and civilization, which was easily color-coded as black, brown/yellow, and white (Stocking 1988: 7–8). While this did not exactly lessen the emphasis on the objectivity of biological differences, another way to come to terms with the discussion about plural or singular origins of humanity was to shift attention from differences in biological makeup to differences in material culture, epitomized by Edward Tylor, who, as a result of his position as Keeper of the University Museum (and the Pitt-Rivers collection) in Oxford, became the first Reader in Anthropology in the United Kingdom (Chapman 1985: 32, 36). The serialization of objects and materials in an evolutionary sequence became one of the main "material" ways to objectify theories about human differences in this period.

If, however, the emphasis of objectivity in the metropole often lay on collections of skulls and material culture, we should not forget that "language"—the means of communication in the field—was also a major object category in the 1841 questionnaire (subsumed, in the first edition of the *Notes and queries on anthropology* in 1874, under Tylor's "culture" [Stocking 2001: 174]). Moreover, one should not exaggerate the extent to which the actual division of scientific labor corresponded to the prescription of the 1874 *Notes and queries*, that those in "the field" were "not anthropologists themselves" and solely responsible for supplying "the information which is wanted for the scientific study of anthropology at home" (quoted in Stocking 2001: 176). Indeed, metropolitan scientists (whether specialized in

language, human biology, or material culture) tried to control the process of object classification, often successfully. Processes of professionalization, however, also took place in the field, and not least in the gathering of intelligence for (colonial) government. The methods of ethnography and the social survey as an emerging technology were developed "from the margins," by travellers who were increasingly professionalized through the questionnaires they brought with them into the field (Stagl 1995). As Justin Stagl has shown, those were the contexts in which the concepts of *Statistik* and *Ethnographie* were first coined around 1770, and they were further developed into a systematically elaborated instrument of rule by colonial administrators, especially in India (Pels 1999). The transformation from travelogue to survey took place in the career of (for example) Francis Buchanan, whose earlier *Journey through Mysore* of 1807 was to be replaced by the topography and the tabulated enumerations of populations of his descriptions of Bengal (Vicziany 1986). If the main object of Enlightenment methods for the observation of "savage peoples" was the "thought" of these peoples (Dégerando [1800] 2007: 37), by 1841 this had changed, as we have seen, to the primacy of reading "physical characters" to support ethnic and racial classifications (cf. Stocking 2001: 170). Likewise, the slow professionalization of Indian ethnology from Buchanan's orientalist predecessors to the racialization of caste practiced by Sir George Campbell—the leading ethnologist of the Asiatic Society of Bengal in the 1860s—implied a shift from (Sanskrit) texts to (Indian) bodies (Pels 1999: 84).[5]

The classification of human bodies was, of course, a main preoccupation of the emancipating class of medical doctors that started ethnological societies in the metropoles, but this methodological focus was always complemented by alternative sources of classification—language, material culture, religion. The primary answer to the question of what objectivity did to the history of anthropology, then, is that for most of the nineteenth century, the classification of human beings by means of individual specimens of heads (in phrenology) and skulls (in comparative anatomy) was paradigmatic—*but it never overruled the primacy of human classification in "species" or "types" as such*. Anthropology, despite mechanical objectivity's emphasis on the blind depiction of individual specimens, could not do away with the ontological primacy of representing "types" analogous to species, inherited from the Enlightenment.

This diagnosis—that Victorian anthropology continued to be based on true-to-nature typology rather than on the objective representation of specimens—is reinforced by the kind of evidence that Daston and Galison also prefer, but that (despite pioneers like Mead and Bateson or Rouch) is unfortunately still marginal in thinking about the history of anthropology: ethnological visualization. A few examples will have to suffice to make my point: the sober skull drawings of James Cowles Prichard's earlier and authoritative *Researches in the physical history of mankind* (1813) contrast starkly with the colorful plates of "types" that accompanied his more popular *The natural history of man* (1848)—but *both* were drawings

5. In fact, this shows a possible relationship of anthropology to broader social changes: here, what Michel Foucault famously designated as the "anatomo-politics of the human body" and the "bio-politics of the population" first became the object of formal experimentation (Foucault 1980: 139).

of types. David Livingstone, one of the first to commission both photographs and paintings of ethnological types during the Zambezi Expedition (1858–1864) instructed his brother Charles (his photographer) to depict "the better class of natives who are believed to be characteristic of the race," but found (in a way that recalls the four-eyed sight that supposedly characterized the Enlightenment) the drawings of Thomas Baines (his painter) wanting since he exaggerated the subjects' features (Barringer 1996: 187). In India, Benjamin Simpson's collection of photographs, informally commissioned by the governor general, was turned into a project of the Political and Secret Department by the Indian Mutiny of 1857 and only published from 1868 by the Indian Museum as *The people of India* (Pinney 1990: 262). Herbert Risley, the administrative ethnographer who was to become director of the Ethnographic Survey of India and one of the originators of the methodology of fieldwork (see below) supported the increasingly racial typology of caste (by ignoring its occupational classifications), not least because of his unwavering faith in the classification of "castes and tribes" by the "nasal index" (ibid.: 265). Such "objective" anthropometry was sometimes a mere asymmetrical performance of classification from power—when, for example, E. H. Man used the anthropometric ruler in a photograph more as a stage prop than for actually measuring his Andamanese types (ibid.: 272).

Such performances of the so-called objectivity of racial classification show that it was an ideal, not a practice. Just as in India, researching and collecting texts and proverbs was never completely displaced by biological classification and the racialization of tribes and castes (Raheja 1999), just so subaltern scientific fieldworkers could not avoid more intersubjective engagements: Alfred Russel Wallace could present a new species of Paradise Bird because his assistant Ali collected and described it, and Wallace trusted his testimony. Nevertheless, Ali's authority was gradually erased from the publication of the results in England (Camerini 1996: 61). One is tempted to read some of this trust back into the concluding chapter of *The Malay Archipelago*, where Wallace, while starting with the classification of its races into Malay and Papua, ends with an appreciation of the "perfect social state" in which he found communities at "a very low stage of civilization" in stark contrast to the moral "barbarism" of "the mass of our population" (Wallace [1869] 1962: 456–57). By that time, however, Wallace was engaged in a more disreputable form of intersubjective exchange, with "Mohamedan and Hindoo" spirits during Spiritualist séances (Wallace (1874) 1896: 226). He is therefore less representative of mid-Victorian attitudes to the "native point of view" than Richard Burton or Francis Galton, who had few qualms about dissimulating to the people they studied. Yet even Burton, when writing freely about his adventures on the pilgrimage to Mecca, was also upbraided for betraying "his character as a gentleman, let alone that of a Christian" when disguising himself as a Muslim (Burton [1893] 1964: xxi; Fancher 1983: 74). It seems, then, that the asymmetry implicit in the primacy of objective classification precluded the possibility of any explicit recognition of the intersubjective, and of communicative exchange between researcher and researched during the heyday of Victorian anthropology.

And yet, the fact that Thomas Hodgkin invited Native Americans and Africans to his home, Alfred Wallace relied on his servant Ali, and Richard Burton needed to become a Muslim to enter Mecca, shows that intersubjective exchanges

conditioned the Victorian's drive toward an asymmetrical and objective classification. This may explain why the first communicative methodologies arose from the Victorian experience of colonialism. In the 1892 edition of *Notes and queries*, editor Charles Hercules Read argued for an Imperial Bureau of Ethnology that would help in "understanding the motives which influence the peoples with whom we are constantly dealing, and thus be able to avoid the disagreements arising from the ignorance of their cherished prejudices and beliefs" (quoted in Stocking 2001: 178–79). This British Museum curator thereby echoed pleas by administrative ethnographers of India for "sympathy" with natives, coupled to a critique of the armchair naivety of metropolitan scholars (Risley 1890). Reiterated by a coalition of Indian civil servants and a new generation of anthropologists, the model of the Ethnographic Survey of India would help to transform British anthropology (Haddon 1921; Risley 1911; Rivers 1913, 1917; Temple 1914).[6] Putting some of these new insights to work in the 1899 Torres Straits Expedition and later fieldwork among the Todas and Veddas of India, Alfred Haddon, William Rivers, and Charles Seligman came up with the conception of intensive field research that Malinowski appropriated in the 1920s (Stocking 1983). That, however, marks a critique of objectivity that recalls Daston and Galison's "trained judgment," defined as directed at the "users" of knowledge—in this case, the colonizers. Tylor warned observers in the first edition of the *Notes and queries* not to leave their own category assumptions unexamined, but this advice should, I think, be read as the ethical-cum-epistemological questioning of scientific subjectivity characterizing mechanical objectivity. Instead, the (initially quite pragmatic) exhortation toward sympathy with colonized natives that arose from some of the intersubjective experiences of colonial practice was to transform objectivity in anthropology and prepare it for the twentieth century (Stocking 2001: 174, 180–83)—without, at first, challenging the hegemony of racial typology.

Structural objectivity and trained judgment in anthropology

The above focus on the history of method and visual representation in anthropology shows a watershed, not in the theoretical innovations of the 1920s (when most histories of anthropology see it emerging from the work of Boas and Malinowski), but—corresponding with Daston and Galison's shift from mechanical objectivity to trained judgment and structural objectivity—in the late colonial circumstances of the 1890s and 1900s. Moreover, this seems to coincide with a first explicit acknowledgment of the importance of *native* subjectivity in the concept of "sympathy."[7] If the classification of types was the initial business of an objective anthropology, and if this can be related to the "threshold of modernity" identified by Foucault as a

6. We have discussed this movement elsewhere (Pels and Salemink 1999: 34–39; Pels 2011: 793) but I hope to give it the extended treatment it deserves in another essay.

7. The politically incorrect term "native" indicates that even left-liberal anthropologists or enlightened colonial administrators gave racial classification a place in their research designs ("the native is not the natural companion of the white man" [Malinowski 1922: 7]). It reminds us of the colonial embedding of anthropology (cf. Pels and Salemink 1999).

new focus on bodies and populations (1980: 139), the era of high imperialism saw anthropologists first making room for the trained judgment needed to understand a "native point of view," while subordinating the earlier mechanical focus on objects (modeled on museum collections of specimens of bodies and artifacts) to a critique that emphasized the *relations* between humans, and between humans and artifacts—relations that were only several decades later to be expressed in terms of function, culture, and structure.

The natural history paradigm of collecting was at this point subjected to a more critical training of judgment. In the 1912 *Notes and queries*, fieldworker and museum curator Barbara Freire-Marreco noted that specimens could no longer be simply collected, but needed the "constructive description" that went together with observing "native craftsmen" at work, preferably following the process of manufacture at least twice, recording it by "unposed photographs" and continuous questioning of the craftsman, and even "taking a lesson" to get his own explanations of it (Stocking 2001: 181). George Stocking regards the "centrepiece" of this edition of the *Notes and queries*, William Rivers' "General account of method," as the "foundation document of the modern British ethnographic tradition," expressing Rivers' preference for a full knowledge of the language and a performance of "sympathy and tact" toward natives that should genuinely avoid expressions of amusement or disgust to achieve its aim (ibid.: 181–82). Rivers stressed that information "voluntarily offered" by natives was much more valuable than the "too orderly" mind of the observer could elicit, and he suggested that the value of his genealogical method of collecting native pedigrees and kinship categories rested in the fact that "you are yourself using the very instrument which the people themselves use in dealing with their social problems" (ibid.: 182). R. R. Marett sought, in the lemma on "religion" in the same volume, to keep both "our own theological" as well as "our anthropological" concepts at arm's length, since they were neither framed to understand "savagery" nor framed "by savagery to understand itself." Instead, "it is their point of view" that the observer wants to reproduce, without intruding any meanings the observer has brought along, much less a comparison with our own practices (ibid.: 184). Clearly, the practice of collecting specimens destined for a museum had ceased to be the primary goal of research.

If intersubjective engagement indeed enters anthropology by the exhortation to exercise trained judgment and sympathy toward natives, it also changed the anthropologist's classifications. Rivers and Marett now went an important step beyond Tylor's 1874 warning against researchers' subjective classifications: Rivers' genealogical method emphasized, like Marett, that a methodical treatment requires the observer to realize that "the savage arranges his universe in categories different from those of ourselves" and that these categories are the proper object of anthropological inquiry (quoted in Stocking 2001: 183). This did not overturn the hierarchical relationship: Rivers' most fundamental rule of method remained that "the abstract should always be approached through the concrete" because, while "people of the lower culture" were "highly developed so far as the concrete is concerned" they hardly possess an abstract terminology like the observer (quoted in Stocking 2001: 183). This explicit reference to the need to study "primitive classifications," of course, recalls the contemporary discussion by Emile Durkheim and Marcel Mauss: whereas the latter started out their essay by stressing, in a similarly

asymmetrical manner, the colonial sense of "primitive classification" as a "fundamental confusion of all images and ideas," they ended it with the far more radical conclusion that *all* classifications are products of social relationships and therefore carry strong affective ties with them ([1903] 1963: 5, 85–86). This conclusion led translator Rodney Needham to proclaim in 1963 that classification was "the prime and fundamental concern of social anthropology," an assertion underpinned by paraphrasing Evans-Pritchard's argument that the anthropologist gradually learns "to see the world as it is constituted for the people themselves, to assimilate their distinctive categories" (Needham 1963: viii).

Daston and Galison's view that the critique of mechanical objectivity resulted, on the one hand, in a focus on the training of judgment to see family resemblances between observations, and on the other, to structural objectivity's replacement of direct observation by a (predominantly mathematical) attempt to access "relational invariants," corresponds, I think, to anthropology's novel focus on native categories and their interconnections (which sympathy would show) on the one hand, and the increasing importance of (not directly observable) cultural and social functions and patterns on the other. However, the emergence of the asymmetrical treatment of intersubjectivity in terms of primitive classification gives rise to a paradox vis-à-vis Daston and Galison's work. If, in natural science, trained judgment was often compared to the physiognomic sight trained to observe family resemblances in order to come to accurate racial classifications, how do we understand the movement in anthropology *away* from racial classification, and toward the intersubjective recognition of native classifications at more or less the same time? Here, my reinterpretation of Daston and Galison's structural objectivity against the background of the field sciences and colonialism (rather than from metropolitan science) becomes, I feel, critical. Trained judgment in anthropology was, as we have seen, predicated on the needs and hierarchies that conditioned the work of field practitioners in a colonial environment. The emphasis on the families of objects that, from Rivers to Needham were identified as primitive classifications, emerged from the triangle of researcher, researched, *and colonial authorities* (Mauss played a role similar to Risley and Rivers by giving direction to the ethnographic and linguistic survey of Indochina from 1903 [Pels and Salemink 1999: 37]). Conditioned from within this colonial triangle, structural objectivity in anthropology took a specifically hierarchical and asymmetrical turn. Assuming that primitive classifications would in themselves never provide authoritative and objective classifications, anthropologists set themselves to discovering the relational invariants of the societies they studied in terms of "function," "culture," and "structure"—object categories that made them not only superior to those whose classifications they studied but also to those colonizers who naively used the racial and tribal classifications of an earlier era. This development only emerged *after* Rivers and his contemporaries established the professional independence of the field scientist on the basis of studying the native point of view.

Recombining native classifications into a new, more "relationally invariant" and coherent view of the native point of view started, of course, in the ethnographic field itself, in the shift away from naïve collecting to the qualitative understanding of native items (including artifacts) in terms of the indigenous categories they

expressed. But the influence of quantification and mathematics that Daston and Galison observed in the rise of structural objectivity also played a role in translating and appropriating the families of objects collected in the ethnographic field into the authoritative theories of twentieth-century "classical" anthropology. Tylor's love of statistics hardly rose above a naïve tabulation of traits, and his approach remained marginal to twentieth-century British sociology and American anthropology (Tylor 1889; Hobhouse, Wheeler, and Ginsberg [1915] 1965; Murdock 1950). Durkheim, however, turned statistical methods of finding relational invariants into sociology's basic way of moving beyond naïve observation, distinguishing the latter from anthropology's more qualitative—but methodologically equivalent—focus on "elementary forms" whose functions would tell us about the origins of social classification in religion (Durkheim [1897] 1951, [1915] 1965). Even if Durkheim thereby epitomized the parting of the ways of statistics and ethnography in social science (Asad 1994), this does not rule out that quantification and mathesis decisively influenced qualitative anthropological reasoning. Malinowski's close acquaintance with Ernst Mach's philosophy (about which he wrote his dissertation in Poland) made him model his notion of "function" on mathematics rather than biology (Malinowski [1907] 1993). This (together with his reading of Nietzsche) turned whatever he has later said about the relationships between indigenous customs and ideas and biological needs into something far less naïvely "biologically determinist" than commonly presumed (Thornton and Skalnik 1993: 4, 7). Likewise, Boas' critique of "premature classifications" of racial types among his colleagues arose from the fact that he came to physical anthropology from physics and mathematics, and concluded that given changes in the form of human heads and the inadequacy of frequency distributions, one should privilege "cultural historical" explanations of the relations between humans instead (Stocking 1988: 10). Both "function" and "culture," therefore, were "relational invariants" cast in mathematical mold.

These seminal shifts in theorizing the anthropological object were never divorced from the determination of field research by colonial hierarchies. Malinowski, at least, struggled to place the experts he educated above the "practical men" still dominating the colonial field (Pels 2011). Structural objectivity, however, entered yet another phase in anthropology with Claude Lévi-Strauss' structuralism. Modeling itself on linguistics rather than mathematics, its overt relationship to field science was one of derision ("I hate travelers and explorers") or nostalgic regret of the destruction that anthropology had to witness, or, perhaps, caused itself (Lévi-Strauss [1955] 1977: 3, 33, 376). While reiterating the irreducible concreteness of "savage thought" over and against scientific engineering's break with the empirical, he rehabilitated the primitive "science of the concrete" at the same time (Lévi-Strauss [1962] 1966: 1). In Lévi-Strauss, the asymmetrical processing of indigenous classifications into a hierarchy of understanding perhaps reached its twentieth-century peak—at the risk of losing sight of the essential conditions posed for anthropological knowledge by the anthropological tradition of trained judgment in fieldwork. It was only when postcolonial anthropology turned back to the materialities of fieldwork, in a reflexive and material turn, that these epistemological conditions could again became a matter of reflection.

Reflexivity, materiality, and globalization, or: How "inter" is the anthropological subject?

The preceding sections have, I hope, demonstrated that anthropology, in its nineteenth-century manifestations, assumed a starkly asymmetrical conception of the collection of objective classes of human types. In the late imperial period, the intersubjectivity of fieldwork, while introducing the need for studying the native point of view, still cultivated this hierarchical conception: even when replacing race by the relational invariants of function, culture, and structure, anthropology clearly reaffirmed the scientific subject's classificatory prerogative. We recapitulated elsewhere how the 1960s critique of anthropologists' involvement in colonialism started to refigure the discipline's location in the "world system" and set the stage for a novel critique of the anthropological subject itself (see the summary in Pels and Salemink 1999). However, I need to highlight three important steps in its development: Pierre Bourdieu's "theory of practice," which put forward the thesis that, after "objectivism" broke with empirical observation—a movement that he (rightly or wrongly) associated with Lévi-Strauss' critique of Maussian phenomenology—a "second break" was needed with the subject position of the objectivist anthropologist (Bourdieu 1977: 2–3).[8] The scientific subject's location as an "outside observer" inclines him "in his preoccupation with *interpreting* practices, . . . to introduce into the object the principles of his relation to the object." Thus, the anthropological subject "constitutes practical activity as an *object of observation and analysis*, a *representation*" (1977: 2, 5; emphases in original). Hence the (erroneous) description of culture as a "map" or a "set of rules"—that is what the observer has made of it. This critique of the anthropological subject's representations was soon to become more political (a postcolonial engagement that remained curiously absent in Bourdieu's work). As such, it was applied outside academic relationships, first by identifying scholarly representations of "orientals" as political strategies in a history of colonialism (Said 1978), and subsequently by showing how the anthropological project constructed its object by specific temporal strategies of representation from its very inception (Fabian 1983).

Recent Whiggish revisions of anthropology's postcolonial history tend to turn the resulting critical revision of ethnographic writing by Clifford and Marcus (1986) into the pivotal event of the 1980s (see, for example, Rees 2008; for a critique, see White 2012). This obscures that two perhaps more important movements were elaborating the critiques of Bourdieu, Said, and Fabian by means of a critical sociology of the classifications used by the anthropological subject: the material turn and the anthropology of globalization. The materiality of social interaction—now *including* the scientific subject as actor and performer—raised new questions about the autonomous status of the subject by rehabilitating, in a revision of historical materialism practiced by Bourdieu and Fabian in particular, the body and its sensory surrender to its surroundings. Said, while perhaps not fully consistent in his borrowings from Michel Foucault, prefigured the spread

8. The context outlined above (trained judgment and structural objectivity) makes it difficult to see Mauss as a simple empiricist, as does his complex comparative "phenomenology."

of Foucauldian studies of governmentality: *Orientalism* mutually implicated subjects and objects in colonial projects of social engineering and included, therefore, both Enlightenment amateurs as well as late-nineteenth-century professionals in the study of global cultural relationships and histories of discipline and control. Fabian brought this line of analysis home to anthropology with particular success. For my present purposes, it is particularly important to note that the apparatus that anthropologists developed over two centuries for studying cultural classifications had now itself become an object of study. Now, anthropological classifications themselves could be studied as primitive classifications of the modern transnational societies in which anthropologists themselves were materially embedded.[9]

Our current understanding of the intersubjective emerged as we criticized the classifications of time ("primitive") and space ("oriental") that anthropologists employed, the unequal practices (colonialism, development) for which they were employed, and looked for alternatives that not only respect indigenous worldviews but also help to redress their exploitation and the violation of their rights. I wanted to contextualize that emergence historically, however, to show that approaches that implicitly or explicitly define intersubjectivity in terms of the primary ethical responsibility of anthropologists toward the views and interests of people studied (Scheper-Hughes 1995; and the literature cited in the introduction to this article) may reinstate an illusory autonomy of the anthropological subject in dyadic terms. A plea for intersubjectivity cannot, as Daston and Galison help to show, make "objectivity" in anthropology go away. Even if it would be possible to intellectually divorce academic anthropologists from their embedding in broader sociocultural arrangements and inequalities by artificially isolating the dyad they form with the people studied, this charmed circle will always be breached by state and private sponsors and employers, people "studied up," unequal distributions of authority and power in the societies studied, and the colonial legacies that often determine what these outsiders think of both anthropologists and the people they study (Pels 2008). As I have tried to show, even the reduction of anthropological relationships to a dyadic core of researchers and researched is a fundamental or primitive classification produced by classical anthropology to exclude a third party. In practice, the subordination of "practical men" that Malinowski wanted his dyadic definition of anthropological expertise to accomplish only worked by adopting the agenda of colonial administrators (Pels 2011). This gesture was repeated in the 1970s and 1980s by "development anthropologists" (Escobar 1991). This historical repetition itself suggests that we should interrogate rather than assume "intersubjectivity" in anthropology and question how it may carry the mission of anthropology into the future.

9. Geschiere, Meyer, and Pels (2008); Pels (1997); and Rabinow (2007) give overviews and comments on this vast literature. Here, one can gloss "primitive" as classifications fundamental to the "modern constitution" (Latour 1993): "tradition" vs. "modernity"; "nature" vs. "culture"; and, importantly, the temporal classification of being more or less developed or modernized.

Conclusion

In the final chapter of their book, Daston and Galison note that "a peek at scientific atlases right now" suggests a rise of "virtual images" and "haptic images" in science, the production of which has made "seeing" often identical to "making," and in which the "presentations" of engineers seem to displace the "representations" of observers, leading to images that have become "part toolkit and part art" (2007: 415). As these images are no longer separate from the nature they depict, the regime of representation in which "objectivity" emerged seems to break down. While I still have difficulties getting my head around this situation in natural science, it is, in fact, quite familiar within anthropological circles. Ever since the crisis of representation told us that our own representations are themselves historical constructs, ethnographic *topoi* such as "the Balinese" or "the Nuer" have, indeed, been identified as virtual images, at best tools to grasp and adapt an experience of difference (by colonizers as much as their critics), at worst "inventions of tradition" that allowed for often violent social engineering. Among other things, the anthropology of global interactions has taught us that representations of human difference, rather than reflecting objective facts, are *primarily* inventions, interventions and legacies of interventions in the social relationships that they (also) try to depict—by all parties, if not by each party in equal measure. Our proper object of study today, therefore, seems to be the process by which knowledge and classifications of human difference were generated by global parties in interaction (before, that is, "us" and "them" crystallized as identities)—including the practice of the relationships within which such knowledge became action (Pels and Salemink 1999).[10]

This leads me to posit two theses: the first, that the objectivity of anthropology still lies (despite everything that happened since scientific racism emerged) in the historical accuracy with which it represents classifications (or classifies representations) of human differences; and the second, that the history of anthropological methods and methodology tells us (more than any emphasis on theory, ontology, or ethics alone) why certain of these classifications were more practical, and now seem more (or less) useful and reliable than others. The methodology shows what the "accuracy" of classifications or representations means at a specific historical conjuncture—including the present. Moreover, it demonstrates that the study of human categorization is, *of necessity*, embedded in hierarchical, asymmetrical social relationships—perhaps most insidiously present in those studies of modernization where the seemingly innocent classification "still" makes the difference between us developed people and the "not yet" (cf. Bhabha 1994: 92; Chakrabarty 2000: 8).

Cultural classification thereby still, as I propose following Needham, remains the main object of anthropology, even if we no longer assume that it represents some kind of racial or cultural essence and study how people use essentialism to invent and intervene in social relationships instead. The two historical steps of including the intersubjective in research—first, primitive classifications around 1900, and in the 1970s, the discursive representations used by ethnographers themselves—increasingly allowed us to see that the process of essentialization goes beyond race,

10. Cf. Malinowski: we study not merely indigenous ideals, but the ways they are actualized in practice (1926: 119).

culture, and class to include classifications of gender and the nonhuman (the animal, the inanimate, and the spiritual). Such a broader understanding of our classifications of human difference showed that the retreat of biological differences since their critique of the Victorian notion of "race" was *not* matched by a similar retreat of biopolitics from global power relationships. Today, it has become irresponsible to closes one's eyes for the fact that "culture" itself can inform discriminatory biopolitics (Stolcke 1995). The fact that the distinction between culture and nature has itself become a cultural classification that (often violently) intervenes in people's lives is only paradoxical when we continue to believe in the faulty premise that we can occupy an objective or outside position. However, just as translation is impossible from a position outside the historical inequality of languages (as Talal Asad famously argued [1986]), neither can the relationship between anthropologist and the people she studies be thought from an outside position of symmetry except as ethical utopia.[11] After objectivity—as Daston and Galison forcefully bring out—there is no going back on a symmetry of worldviews or subject positions, if only because objectivity itself has become a primitive classification of modernity (and, like "culture," is as highly valued as it is essentially contested in its multiple meanings: 2007: 371, 378). In order to remain persuasive as experts, therefore, present-day anthropologists have to build on their knowledge of such historical contests of classifications, and to avoid that the "intersubjective" degenerates into the decontextualized autonomy of an ahistorical collaboration between "us" and "them." This, I propose, is one way to realize an epistemology of "shared time"—intersubjective time, that is, history—that "must be involved in the founding of ethnographic objectivity" (Fabian 2001: 29).

I propose that we can avoid such historical decontextualization through rehabilitating methodology, by freeing the latter from the bad press it received in anthropology because of its association with positivism. What elevates the anthropological study of classifications and their use over other disciplines and over nonscientific (but not necessarily less valuable) social practices is the series of asymmetrical methodological moves that Pierre Bourdieu, I feel, inadequately summarized in his plea for a "double break" with everyday phenomena. As Edward Tylor first stated, and William Rivers (among others) elaborated, we require an initial suspension of belief in the classifications that the researcher brings from his own social surroundings, to be realized by a suspension of *disbelief* in and recognition of the indigenous classifications studied. This first double break is then followed by a further break with the indigenous classifications studied, first by setting the values and ideals expressed by them in the context of their "actualization" (Malinowski 1926: 119), and then subjecting the patterns we recognize on that basis to further operations of comparison with theories of cultural and social interaction (which are not "outside" metacultural classifications, but take part in a social history of intervention of their own). That subsequent set of representations and classifications has to be subjected to a third double break that subjects the work of classification by scientific observers to a next level of critical scrutiny, assessing the extent to which they

11. This suggests a rethinking of Bruno Latour's invocation of research symmetry (1993: 91). His anthropological inspirations rarely seem to break with an exoticizing tradition. However, another essay would have to bring that out.

"introduced into the object" classificatory practices of their academic position or the broader social relationships through which they relate to the people studied (Pels and Salemink 1999). Bourdieu's initial double break is inadequate because, today, the latter movement requires at least a *second* intersubjective moment, which David Mosse (following Bruno Latour) has described as a moment of "objection" by people studied or other relevant audiences (Mosse 2006). Significantly, Mosse realized he needed a moment of intersubjective objection while "studying up," that is, when the adequacy of his ethnographic account was questioned by his elite interlocutors (the policy-makers in the aid project for which he was an ethnographer and a consultant). Mosse then realized his anthropology had entered a *necessarily* "antisocial," asymmetrical, and agonistic moment that interrupted the expectations of advocacy or collaboration attached to his role as consultant. The remedy was to confront his accounts with the objections of his interlocutors. Even in this second phase, harmonious intersubjectivity was less likely than an agreement to differ. An epistemological analysis shows that the romance of harmonious collaboration is, indeed, an ethical injunction that intervenes in, rather than represents, the methodology of an ethnographic research process: whether it is the most desirable outcome of a negotiation of expertise or not, this conclusion shows how we can more adequately understand what we are doing (or teaching our students).

If anthropology is still to be regarded as producing expertise, it inevitably has to claim to produce a form of knowledge about a certain set of objects that is somehow "better" than what laypeople can produce—that is, it has to build on a series of asymmetrical breaks that form a necessary epistemological condition of expertise. Without this claim epistemological conditions are deflected into ethics (Fabian 2001: 11n1); as a result, employers and sponsors may come to perceive us as indistinguishable from political activists (and as equally deserving of funds as the latter). This is why the intersubjectivity is necessary, but not sufficient for producing anthropological expertise. Today, "objectivity" may be merely a name for an intangible quality that makes this expertise compelling to others, but the great achievement of Daston and Galison is to have shown that, even after we have come to realize that "it" does not exist, its historical introduction means that we cannot do without it as an ideal. "After objectivity," the intersubjective phases of the anthropological research process remain as necessary and ethically compelling as they were in the days of Alfred Wallace's sympathetic dealings with his collaborator Ali, but they do not sufficiently represent the conditions of the negotiation of anthropological knowledge.

References

Asad, Talal. 1986. "The concept of cultural translation in British social anthropology." In *Writing culture: The poetics and politics of ethnography*, edited by James Clifford and George E. Marcus, 141–64. Berkeley: University of California Press.

———. 1994. "Ethnographic representation, statistics, and modern power." *Social Research* 61: 55–88.

Barringer, Tim. 1996. "Fabricating Africa: Livingstone and the visual image." In *David Livingstone and the Victorian encounter with Africa*, edited by Joanna Skipwith and John M. MacKenzie, 169–99. London: National Portrait Gallery Publications.

Bhabha, Homi K. 1994. *The location of culture*. London: Routledge.

Blanckaert, Claude. 1988. "On the origins of French ethnology: William Edwards and the doctrine of race." In *Bones, bodies, behavior: Essays on biological anthropology*, edited by George W. Stocking Jr., 18–55. Madison: University of Wisconsin Press.

Bourdieu, Pierre. 1977. *Outline of a theory of practice*. Translated by Richard Nice. Cambridge: Cambridge University Press.

Bravo, Michael T. 1996. "Ethnological encounters." In *Cultures of natural history*, edited by Nicholas Jardine, James A. Secord, and Emma Spary, 338–57. Cambridge: Cambridge University Press.

Burton, Richard Francis. (1893) 1964. *Personal narrative of a pilgrimage to Al-Madinah and Meccah*. 2 vols. Memorial edition. New York: Dover Publications.

Camerini, Jane R. 1996. "Wallace in the Field." *Osiris* (2nd series) 11: 44–65.

Chakrabarty, Dipesh. 2000. *Provincializing Europe: Postcolonial thought and historical difference*. Princeton, NJ: Princeton University Press.

Chapman, William Ryan. 1985. "Arranging Ethnology: A. H. L. F. Pitt Rivers and the typological tradition." In *Objects and others: Essays on museums and material culture*, edited by George W. Stocking, Jr., 15–48. Madison: University of Wisconsin Press.

Clifford, James, and George Marcus. 1986. *Writing culture: The poetics and politics of ethnography*. Berkeley: University of California Press.

Daston, Lorraine. 1994. "Marvelous facts and miraculous evidence in early modern Europe." In *Questions of evidence: Proof, practice, and persuasion across the disciplines*, edited by James Chandler, Arnold Davidson, Harry Harootunian, 235–74. Chicago: University of Chicago Press.

Daston, Lorraine, and Peter Galison. 2007. *Objectivity*. New York: Zone Books.

———. 2012. "(Contribution to) Objectivity in historical perspective." *Metascience* 21: 30–38.

Dear, Peter, Ian Hacking, Matthew L. Jones, Lorraine Daston, and Peter Galison. 2012. "Objectivity in historical perspective." *Metascience* 21: 11–39.

Dégerando, Joseph-Marie. (1800) 2007. "The observation of savage peoples." In *Ethnographic fieldwork: An anthropological reader*, edited by Antonius C. G. M. Robben and Jeffrey A. Sluka, 33–39. Oxford: Blackwell.

Desmond, Adrian.1994. *Huxley: From devil's discipline to evolution's high priest*. Reading, MA: Addison-Wesley.

Durkheim, Emile. (1897) 1951. *Suicide*. New York: Free Press.

———. (1915) 1965. *The elementary forms of the religious life*. New York: Free Press.

Durkheim, Emile, and Marcel Mauss. (1903) 1963. *Primitive classification*. Translated by Rodney Needham. London: Cohen and West.

Dwyer, Kevin. 1977. "On the dialogue of fieldwork." *Dialectical Anthropology* 2: 143–51.

———. 1979. "The dialogic of fieldwork." *Dialectical Anthropology* 4: 205–24.

Eriksen, Thomas Hylland. 1995. *Small places, large issues: An introduction to social and cultural anthropology*. 2nd ed. London: Pluto Press.

Escobar, Arturo. 1991. "Anthropology and the development encounter: The making and marketing of development anthropology." *American Ethnologist* 18 (4): 658–82.

Fabian, Johannes. 1971. "Language, history and anthropology." *Philosophy of the Social Sciences* 1: 19–47.

———. 1983. *Time and the other: How anthropology makes its object*. New York: Columbia University Press.

———. 2001. "Ethnographic objectivity: From rigor to vigor." In *Anthropology with an attitude*, 11–32. Stanford, CA: Stanford University Press.

Fancher, Raymond. 1983. "Francis Galton's African ethnography and its role in the development of his psychology." *British Journal for the History of Science* 16: 67–79.

Ferguson, James. 1999. *Expectations of modernity: Myths and meanings of life on the Zambian Copperbelt*. Berkeley: University of California Press.

Foucault, Michel. 1980. *History of sexuality, vol. 1: An introduction*. New York: Vintage Books.

Geschiere, Peter, Birgit Meyer, and Peter Pels. 2008. "Introduction." In *Readings in modernity in Africa*, edited by Peter Geschiere, Birgit Meyer, and Peter Pels, 1–7. Bloomington: Indiana University Press.

Hacking, Ian. 1990. *The taming of chance*. Cambridge: Cambridge University Press.

Haddon, Alfred Cort. 1921. *The practical value of ethnology*. Conway Memorial Lecture. London: Watts & Co.

Hastrup, Kirsten, and Peter Elsass. 1990. "Anthropological advocacy: A contradiction in terms?" *Current Anthropology* 31 (3): 301–11.

Hobhouse, Leonard, Gerald Wheeler, and Morris Ginsberg. (1915) 1965. *The material culture and social institutions of the simpler peoples: An essay in correlation*. London: Routledge and Kegan Paul.

Jardine, Nicholas, James A. Secord, and Emma Spary, eds. 1996. *Cultures of natural history*. Cambridge: Cambridge University Press.

Jones, Matthew L. 2012. "(Contribution to) Objectivity in historical perspective." *Metascience* 21: 24–30.

Kim, Mi Gyung. 2009. "A historical atlas of objectivity." *Modern Intellectual History* 6 (3): 569–96.

Koerner, Lisbet. 1996. "Carl Linneaus in his time and place." In *Cultures of natural history*, edited by Nicholas Jardine, James A. Secord, and Emma Spary, 145–62. Cambridge: Cambridge University Press.

Kuklick, Henrika, and Robert E. Kohler. 1996. "Introduction to special issue on sciences in the field." *Osiris* (2nd series) 11: 1–14.

Lamphere, Louise. 2003. "The perils and prospects for an engaged anthropology: A view from the United States." *Social Anthropology/Anthropologie sociale* 11 (2): 153–68.

Lassiter, Luke Eric. 2004. "Collaborative ethnography." *Anthronotes* 25 (1): 1–9.

Latour, Bruno. 1993. *We have never been modern*. Cambridge, MA: Harvard University Press.

Lévi-Strauss, Claude. (1962) 1966. *The savage mind*. Translated by John Weightman. Chicago: University of Chicago Press.

———. (1955) 1977. *Tristes tropiques*. Translated by John and Doreen Weightman. New York: Pocket Books.

Malinowski, Bronislaw. 1922. "Introduction: The subject, method and scope of this inquiry." In *Argonauts of the Western Pacific*, 1–25. London: Routledge and Kegan Paul.

———. 1926. *Crime and custom in savage society*. Totowa, NJ: Littlefield, Adams & Co.

———. (1907) 1993. "On the principle of the economy of thought." In *The early writings of Bronislaw Malinowski*, edited by Robert J. Thornton and Peter Skalnik, 89–115. Translated from Polish by Ludwik Krzyzanowski. Cambridge: Cambridge University Press.

McCook, Stuart. 1996. "'It may be truth, but it is not evidence': Paul du Chaillu and the legitimation of evidence in the field." *Osiris* (2nd series) 11: 177–97.

Mosse, David. 2006. "Anti-social anthropology? Objectivity, objection, and the ethnography of public policy and professional communities." *Journal of the Royal Anthropological Institute* 12 (4): 935–56.

Murdock, George P. 1950. "Feasibility and implementation of comparative community research with special reference to the human relations area files." *American Sociological Review* 15 (6): 713–20.

Needham, Rodney. 1963. "Introduction." In *Primitive classification*, by Emile Durkheim and Marcel Mauss, vii–xlviii. Edited by Rodney Needham. London: Cohen and West.

Outram, Dorinda. 1996. "New spaces in natural history." In *Cultures of natural history*, edited by Nicholas Jardine, James A. Secord and Emma Spary, 249–65. Cambridge: Cambridge University Press.

Pels, Peter. 1997. "The anthropology of colonialism: Culture, history and the emergence of Western governmentality." *Annual Review of Anthropology* 26: 163–83.

———. 1999. "The rise and fall of the Indian Aborigines: Orientalism, Anglicism and the emergence of an ethnology of India." In *Colonial subjects: Essays in the practical history of anthropology*, edited by Peter Pels and Oscar Salemink, 82–116. Ann Arbor: University of Michigan Press.

———. 2008. "What has anthropology learned from the anthropology of colonialism?" *Social Anthropology/Anthropologie sociale* 16 (3): 280–99.

———. 2011. "'Global' experts and 'African minds': Tanganyika anthropology as public and secret service." *Journal of the Royal Anthropological Institute* 17 (4): 788–810.

Pels, Peter, and Oscar Salemink. 1999. "Introduction: Locating the colonial subjects of anthropology." In *Colonial subjects: Essays in the practical history of anthropology*, edited by Peter Pels and Oscar Salemink, 1–52. Ann Arbor: University of Michigan Press.

Pinney, Christopher. 1990. "Classification and fantasy in the photographic construction of caste and tribe." *Visual Anthropology* 3: 259–88.

Poovey, Mary. 1998. *A history of the modern fact. Problems of knowledge in the sciences of wealth and society*. Chicago: University of Chicago Press.

Prichard, James Cowles. (1813) 1973. *Researches into the physical history of man*. Edited by George W. Stocking, Jr.. Chicago: University of Chicago Press.

———. (1848) 1855. *The natural history of man*. London: H. Ballière.

Rabinow, Paul. 2007. *Marking time: On the anthropology of the contemporary.* Princeton, NJ: Princeton University Press.

Raheja, Gloria. 1999. "The illusion of consent: Language, caste and colonial rule in India." In *Colonial subjects: Essays in the practical history of anthropology*, edited by Peter Pels and Oscar Salemink, 117–52. Ann Arbor: University of Michigan Press.

Rappaport, Joanne. 2008. "Beyond participant observation: Collaborative ethnography as theoretical innovation." *Collaborative Anthropologies* 1: 1–31.

Rees, Tobias. 2008. "Introduction: Today, what is anthropology?" In *Designs for an anthropology of the contemporary*, edited by Paul Rabinow, George E. Marcus, James D. Faubion, and Tobias Reese, 1–12. Durham, NC: Duke University Press.

Risley, Herbert Hope. 1890. "The study of ethnology in India." *Journal of the Anthropological Institute* 20: 235–63.

———. 1911. "Presidential address: The methods of research." *Journal of the Anthropological Institute* 41: 8–19.

Rivers, William. 1913. "Report on anthropological research outside America." In *Report upon the present condition and future needs of the science of anthropology*, edited by W. H. R. Rivers, A. E. Jenks, and S. G. Morley. Washington: Carnegie Institution.

———. 1917. "The government of subject peoples." In *Science and the nation*, edited by A. C. Seward, 302–28. Cambridge: Cambridge University Press.

Rosenfeld, Louis. 1993. *Thomas Hodgkin: Morbid anatomist and social activist*. Lanham: Madison Books.

Said, Edward. 1978. *Orientalism*. London: Routledge and Kegan Paul.

Scheper-Hughes, Nancy. 1995. "The primacy of the ethical: Propositions for a militant anthropology." *Current Anthropology* 36 (3): 409–40.

Schumaker, Lyn. 2001. *Africanizing anthropology: Fieldwork, networks, and the making of cultural knowledge in Central Africa*. Durham, NC: Duke University Press.

Shapin, Steven. 1994. *A social history of truth: Civility and science in seventeenth-century England*. Chicago: University of Chicago Press.

Stagl, Justin. 1995. *A history of curiosity: The theory of travel, 1550–1800*. Newark, NJ: Harwood Academic Publishers.

Stocking, George W. 1968. *Race, culture, and evolution: Essays in the history of anthropology*. Chicago: University of Chicago Press.

———. 1971. "What's in a name? The origins of the Royal Anthropological Institute (1837–71)." *Man* (n.s.) 6: 369–90.

———. 1973. "From chronology to ethnology: James Cowles Prichard and British anthropology, 1800–1850." In James Cowles Prichard, *Researches into the physical history of man* [originally published 1813], edited by George W. Stocking, ix-cx. Chicago: University of Chicago Press.

———. 1983. "The ethnographer's magic: Fieldwork in British Anthropology from Tylor to Malinowski." In *Observers observed: Essays on ethnographic fieldwork*, edited by George W. Stocking Jr., 70–120. Madison: University of Wisconsin Press.

———. 1988. "Bones, bodies, behavior." In *Bones, bodies, behavior: Essays on biological anthropology*, edited by George W. Stocking, 3–17. Madison: University of Wisconsin Press.

———, ed. 1991. *Colonial situations: Essays on the contextualization of ethnography knowledge*. Madison: University of Wisconsin Press.

———. 2001. "Reading the palimpsest of inquiry: *Notes and queries* and the history of British Social Anthropology." In *Delimiting anthropology: Occasional essays and reflections*, edited by George W. Stocking, Jr., 164–206. Madison: The University of Wisconsin Press.

Stolcke, Verena. 1995. "Talking culture: New rhetorics of exclusion in Europe." *Current Anthropology* 36 (1): 1–24.

Stoler, Ann Laura. 2009. *Along the archival grain: Epistemic anxieties and colonial common sense*. Princeton, NJ: Princeton University Press.

Tedlock, Dennis. 1979. "The analogical tradition and the emergence of a dialogical anthropology." *Journal of Anthropological Research* 35: 387–400.

Temple, Richard Carnac. 1914. *Anthropology as a practical science*. London: G. Bell and Sons, Ltd.

Thom, Martin. 1995. *Republics, nations and tribes*. London: Verso.

Thornton, Robert, and Peter Skalnik. 1993. "Introduction: Malinowski's reading, writing, 1904–1914." In *The early writings of Bronislaw Malinowski,* edited by Robert Thornton and Peter Skalnik, 1–64. Translated from Polish by Ludwik Krzyzanowski. Cambridge: Cambridge University Press.

Tylor, Edward Burnett. 1889. "On a method of investigating the development of institutions applied to laws of marriage and descent." *Journal of the Royal Anthropological Institute* 18: 245–72.

Vicziany, Marika. 1986. "Imperialism, botany and statistics in early nineteenth-century India: The surveys of Francis Buchanan (1762–1829)." *Modern Asian Studies* 20 (4): 625–60.

Wallace, Alfred Russel. (1869) 1962. *The Malay Archipelago: The land of the orang-utan and the bird of paradise: A narrative of travel with studies of man and nature*. New York: Dover Publications.

———. (1874) 1896. *Miracles and modern spiritualism*. 3rd revised ed. London: George Redway.

Whitaker, Katie. 1996. "The culture of curiosity." In *Cultures of natural history*, edited by Nicholas Jardine, James A. Secord, and Emma Spary, 75–90. Cambridge: Cambridge University Press.

White, Bob. 2012. "From experimental moment to legacy moment: Collaboration and the crisis of representation." *Collaborative Anthropologies* 5: 65–97.

Après l'objectivité: Une approche historique de l'intersubjectif dans l'ethnographie

Résumé : L'intersubjectivité en anthropologie a rarement été étudiée dans le contexte de la place des échanges intersubjectifs dans l'histoire de l'anthropologie plus largement. Cet article tente de le faire en mettant en regard l'histoire de l'anthropologie et l'histoire de l'objectivité depuis les Lumières, suivant le travail novateur de Lorraine Daston et Peter Galison, *Objectivity* (2007). Sur cette base, il conclut que l'idéal romantique ordinaire d'interactions dyadiques entre le chercheurs et ceux objets de la recherche ne donne pas une impression juste de ce qu'est l'anthropologie. Il fait valoir que la mission de l'anthropologie repose davantage sur une expertise relative à la classification culturelle qui ne peut pas être dissociée des ruptures asymétriques d'avec les perceptions quotidiennes dérivées de la méthode ethnographique.

Peter Pels is Professor in the Anthropology of Africa at the University of Leiden since 2003. He graduated from the University of Amsterdam in 1993 on a study of interactions between missionaries and Africans in late colonial Tanganyika, and has since continued to work on the construction of differences of culture and power in human relationships. He was the editor of *Social Anthropology* between 2002 and 2007. He is currently finishing a book on material culture, religion, and the power of objects, and coordinates research into modern conceptions of the future, museums, and heritage.

Peter Pels
Institute of Cultural Anthropology and Development Sociology
Universiteit Leiden
P.O. Box 9555, 2300 RB Leiden, The Netherlands
pels@fsw.leidenuniv.nl