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**Common Knowledge is
published three times a year by
Duke University Press
in association with Bar-Ilan University**

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COMPARATIVE RELATIVISM

Symposium on an Impossibility

*Casper Bruun Jensen, Barbara Herrnstein Smith, G. E. R. Lloyd,
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Introduction: Contexts for a Comparative Relativism

There is only one method . . . the comparative method. And that [method] is impossible.

— *E. E. Evans-Pritchard*

This aim of this symposium is to place in unlikely conjunction the two terms *comparison* and *relativism*. On the one hand, comparison, in the most general sense, involves the investigation of discrete contexts to elucidate their similarities and differences. Comparative methods have been widely used in many social science disciplines, including history, linguistics, sociology, and anthropology.

The conference out of which this *Common Knowledge* symposium emerged was held in September 2009 at the IT University of Copenhagen and was organized by Casper Bruun Jensen, Morten Axel Pedersen, and Brit Ross Winthereik.

Common Knowledge 17:1

DOI 10.1215/0961754X-2010-029

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On the other hand, relativism, as a tendency, stance, or working method in social anthropology, and more recently in science and technology studies (STS), usually involves the assumption that contexts exhibit, or may exhibit, radically different, incomparable, or incommensurable traits.¹ Comparative studies are required to treat their objects as alike, at least in some crucial respects; otherwise it is impossible to establish measures that enable the researcher to determine what is shared and not between, for example, cultures or practices. Relativism, however, indicates the limits of this stance by suggesting that the observation of difference and similarity depends on a preestablished “outside” perspective from which comparison can be made—and, of course, relativism is skeptical of the possibility that a view from nowhere-in-particular can be established. Given the deep divide between these analytical and methodological entry points, the term *comparative relativism* is likely to seem incongruent or paradoxical.

A symposium focusing on such a term must begin from the assumption that incongruence or paradox is no bad thing and, indeed, can be productive. The contributions here delineate two basic and productive uses of its key term. Comparative relativism is understood by some to imply that relativism comes in various kinds and that these have multiple uses, functions, and effects, varying widely in different personal, historical, and institutional contexts; moreover, that those contexts can be compared and contrasted to good purpose. It is in this sense that, for example, Barbara Herrnstein Smith takes up the idea. On the other hand, comparative relativism is taken by other contributors, for instance Eduardo Viveiros de Castro and Marilyn Strathern, to imply and encourage a “comparison of comparisons,” in order to relativize what different peoples—say, Western academics and Amerindian shamans—compare things “for.” None of the contributors wants to develop a framework in which the methods of comparison and relativization may be integrated. Of central concern is rather the strategies and methods of comparison and relativization themselves. It is on this level that Isabelle Stengers, with her attention to the difference between unilateral and multilateral comparison, makes her contribution. Each of these four major pieces (by Smith, Viveiros de Castro, Strathern, and Stengers) is followed by a set of commentaries and then a response. The job of this introduction can only be to highlight some thematic threads that cut across these numerous contributions.

1. David Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976).

Models and Comparisons

In the chapter “Social Structure” of his classic text *Structural Anthropology* (volume 1), Claude Lévi-Strauss considers the different status that comparison and the notion of the comparative held for various ancestral figures of anthropology. He notes that for A. R. Radcliffe-Brown and Robert Lowie the problem with earlier anthropology was that it was full of merely “alleged correlations” between the structures of diverse societies and that these were “lacking empirical support.”² In the place of these spurious analytical practices, Lowie argued that anthropology should be put on a “broad inductive basis.”³ Lévi-Strauss contrasts this approach with that of Durkheim, whose reference point was the “laws of science.” As Durkheim put it: when a scientific law “has been proved by a well-performed experiment, this law is valid universally” (288). Considering these two possibilities—one associated with Lowie, the other with Durkheim—Lévi-Strauss formulated the dilemma of the anthropologist as whether “to study many cases in a superficial and in the end ineffective way; or [else] to limit oneself to a thorough study of a small number of cases, thus proving that in the last analysis one well-done experiment is sufficient to make a demonstration” (288). As is well known, Lévi-Strauss had no fear of seeking inspiration in the natural sciences, a fearlessness that has more than occasionally been regarded as negative. Given our present distance from the controversy over structuralism, and given the emergence of STS perspectives in the meantime, what is most interesting about both the scientism of Lévi-Strauss and the critique of structuralism on that ground is that both take the idea of scientific method and scientific law for granted. Since the heyday of structuralism and poststructuralism in the human sciences, STS research has repeatedly shown the variability of scientific methods and the different status that supposed natural laws have within and across scientific communities, as well as within and across historical periods. Indeed, STS research has established the uncertainty of the factual.

In his magisterial work *Image and Logic* (1997), the STS historian Peter Galison posed for modern physics roughly the same question as Lévi-Strauss had raised for anthropology.⁴ According to Galison, physics (like anthropology) exhibits two distinct modes of making knowledge, one oriented toward image and the other toward logic:

One tradition has had as its goal the representation of natural processes in all their fullness and complexity—the production of images of such

2. Claude Lévi-Strauss, *Structural Anthropology*, trans. Claire Jacobson and Brooke Grundfest Schoepf (New York: Basic Books, 1963), 287–88, quoting Lowie.

3. Lévi-Strauss, *Structural Anthropology*, 288, quoting Lowie.

4. Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

clarity that a single picture can serve as evidence for a new entity or effect. These images are presented, and defended, as *mimetic*—they purport to preserve the form of things as they occur in the world. Against this mimetic tradition, I want to juxtapose what I have called the “logic tradition,” which has used electronic counters coupled in electronic logic circuits. These counting (rather than picturing) machines aggregate masses of data to make statistical arguments for the existence of a particle or effect. (19)

The competition between image and logic is more commonly thought of as between experimentalists and theorists. The theorist “sacrifices the detail of the one for the stability of the many,” while the experimentalist relies on the idea that “information about a single event rendered with full detail is in all relevant ways equivalent to information deduced from partial details about many events of the same class” (20). The image-based experimentalists hold that the “passivity of their systems of registration” ensures that theoretical assumptions do not enter their analysis of experimental results. But the logic-based, statistics-oriented theorists hold that “anything can happen once,” for which reason singular exemplary cases (so-called *golden events*) remain dubious claimants to epistemic authority.

There are, of course, differences between these parallel debates among physicists and among anthropologists, though perhaps they are not the differences that would be expected. The problem is not that anthropology, as an interpretive discipline whose subject matter is culture rather than nature, is unable to become grounded in objective “laws of science.” For what Galison’s analysis shows—as does much other STS research—is that even within the “hardest” of natural sciences uncertainties comparable to those with which social scientists must deal are invariably found. The central difference that does emerge is that, in his argument, Lévi-Strauss mixes elements that Galison’s physicists separate. Like the advocates of the image-based tradition in physics, Lévi-Strauss seeks a golden event with which to establish his case. But unlike Galison’s image-based experimentalists, Lévi-Strauss does not purport to establish his case through strictly inductive means, untainted by theory. With regard to the necessity to theorize and model, he is on the logicians’ side. He indeed pondered whether the fidelity of anthropologists to the comparative method should be “sought in some sort of confusion between the procedures used to establish . . . models.”⁵ What he meant was that Durkheim’s demand for scientific laws could be met only under a statistical regime (similar to the logic tradition in physics) that relies on the gathering of large amounts of data. Yet such data are acceptable “insofar as they are all of the same kind,” which is a demand that cannot be met by ethnography. Thus, Lévi-Strauss eventually proposed that the way forward “lies in the selection of

5. Lévi-Strauss, *Structural Anthropology*, 288.

the ‘case,’ which will be patterned so as to include elements which are either on the same scale as the model to be constructed or on a different scale”—a proposal that raises all manner of relativistic questions about elements, scales, models, and their relations.⁶

The questions that Lévi-Strauss raised have remained, to this day, intractable. His own advice has not been followed up, at least not in the social sciences, because of its paradoxical character: he enjoins us to study singular cases—golden events—that somehow can count as general demonstrations.⁷ Studies of this kind would have no truck with comparison (at least not as commonly defined), informed or haunted as they are by Nietzsche’s warning that “to dream of two equal forces, even if they are said to be of opposite senses, is a coarse and approximate dream, a statistical dream.”⁸ But what might count as a golden event in the social sciences? What might a unique demonstration exemplify, show, or prove? Is there a potential for comparison of a different order in such a project; and if so, where and how might it emerge? Is our only alternative to comparison the method and phantom theory that Barbara Herrnstein Smith calls its “evil twin”?

Relativism: Comparison’s “Evil Twin”

While comparativism (or even comparison *tout court*) is problematic as a methodology in the social sciences, the problems are encountered at an elevated level—the level of theoretical rationale. Relativism, however, faces objections at *all* levels, including that of morality. Indeed, as Smith shows, *relativism* is less a descriptive term than an accusation when applied to research in anthropology, historiography, and other social sciences. Relativism is a versatile threat: its method and/or theory can render heterogeneous states of affairs homogeneous, but also render homogeneous states of affairs heterogeneous. In either case, epistemological paradox and/or “politically undesirable neutrality” may be seen to follow.⁹ When, as Smith has observed, histories of the Holocaust contextualize it by comparison with other “massive state-sponsored slaughters,” the comparison can be regarded as relativistic in that it may “lessen dramatic differences” and create “immoral equivalences.”¹⁰ Comparison and relativism often come together, when they do at all, in the crudest ways. In its capacity to enable the comparison of anything with anything else, relativism is seen as overly tolerant—as undermining our ability, for example, to understand the Holocaust as a unique

6. Lévi-Strauss, *Structural Anthropology*, 288–89.

7. For a purportedly *post*structuralist application of Lévi-Strauss’s advice to the *humanities*, see Catherine Gallagher and Stephen Greenblatt, “Counterhistory and the Anecdote,” in *Practicing New Historicism* (Chicago: University of Chicago Press, 2000).

8. Gilles Deleuze, *Nietzsche and Philosophy*, trans. Hugh Tomlinson (London: Athlone, 1983), 43.

9. Barbara Herrnstein Smith, *Scandalous Knowledge* (Durham, NC: Duke University Press, 2006), 3.

10. Smith, *Scandalous Knowledge*, 21.

event. In other circumstances, however, the threat of relativism is said to be its intolerance to comparisons of any kind. Evans-Pritchard's famous study of the Azande opens with an apparently broad-minded presumption—that witchcraft makes sense within Zande cosmology. Yet the literary scholar Satya Mohanty, for instance, finds Evans-Pritchard's remarks both "intolerant" and patronizing. Mohanty takes his recognition of differences among cultures to be strictly rhetorical: he really sees the Azande as dupes, since after all "we" know that witchcraft is absurd. By insisting on irreducible difference, relativism ends up, at least in this account, by privileging Western modes of knowing and by patronizing the capacity of our cultural others to obtain and credit genuine knowledge.

To a degree, this criticism converges with Isabelle Stengers's arguments against tolerance, but the upshot is not the same. Whereas Mohanty argues that rational agency is a universal human capacity and that the beliefs and practices of people everywhere may be assessed on that basis, Stengers instead suggests that notions such as rational agency are properly applicable nowhere, not even in the West. In that context, if Zande magic is judged irrational, Western physics will be judged equivalently so. It is not that the Azande are more like "us" than we assume; it is, rather, that we are no more like the image we have of ourselves than they are. Of course, by now there are social anthropologists and STS researchers who were trained almost entirely within constructivist and relativist idioms; and to such younger academics, the documentation and analysis offered by Stengers and Smith may seem to be flogging a dead horse. Yet in large regions of academic social science, there remains, alive and kicking, an array of fully positivist concepts. Indeed, as Smith makes clear in her remarks here on Scott Atran, contemporary evolutionary psychologists and cognitive anthropologists see evidence of a "psychic unity of mankind," in the form of "innate, evolved, universal mental mechanisms underlying all human thought, behavior and culture."

Multinaturalism

Few ideas could be further from Atran's "psychic unity of mankind" than Eduardo Viveiros de Castro's idea of "multinaturalism." Viveiros de Castro understands his own work, on Amerindian shamanism, as a move in support of "peoples' ontological autodetermination" and a step toward "permanent decolonization of thought." Redefining social anthropology as "field geophilosophy," he approaches relativism "not as an epistemological puzzle but as an anthropological subject, amenable to translative comparison." For him, comparative relativism is a means of contrasting "anthropological and indigenous modes of perceiving analogies between domains." It is a way of comparing comparisons.

In a recent paper, "The Crystal Forest: Notes on the Ontology of Amazonian Spirits," Viveiros de Castro discusses, along these lines, an account pre-

sented by Davi Kopenawa, a Yanomami shaman, of the world's structure and history—a narrative that doubles “as an indignant and proud claim for the Yanomami people's right to exist.”¹¹ The description we get of Yanomami cosmology is unsettling and difficult to understand:

The spirits have danced for shamans since the primordial times and so they continue to dance today. They look like human beings but they are as tiny as specks of sparkling dust. To be able to see them you must inhale the powder of the Yakoanahi tree many, many times. . . . Those who don't “drink” it remain with the eyes of ghosts and see nothing.

Viveiros de Castro reads this passage as itself an exercise in comparative relativism: Kopenawa, he argues, is not simply describing some epistemological contents of the Yanomami worldview. In speaking “about spirits to Whites and equally about Whites on the basis of spirits,” Kopenawa is being—he is doing the work of—a Yanomami shaman. Doing so he is elucidating, from the perspective of Yanomami cosmology, the differential basis on which the evaluative capacity of Yanomami and Westerners can be compared. Might this scene confirm or illustrate the possibility of an ethnographical golden event?

It should be clear that, for Viveiros de Castro, the question is how *not* to erase the differences between Yanomami and Westerners in the name of the “psychic unity of mankind” or of any other purportedly universal principle. He seeks to find what mode of existence spiritual entities can have, both for the Yanomami and for us. Thus his project, one might say, takes relativism to the extreme. But Viveiros de Castro resists that designation: the term he prefers is *perspectivism*, and he argues that Amerindian cosmology is perspectivism of a specific form. As he understands Kopenawa's account, the shaman is not presenting Yanomami consensual beliefs as a worldview; rather he is expressing the Yanomami world *objectively from inside it*. Contrary to the Western idea that there are many cultures but only one nature, in Yanomami ontology there are many natures but only one culture. As readers of this journal have reason to know, in Yanomami ontology each living entity believes that it is human and sees the world as a human being sees it.¹² Thus, when a Yanomami man or woman sees a jaguar eating blood, the jaguar knows itself to be a human drinking manioc beer. The way in which Viveiros de Castro compares Amerindian and Western ways of comparison does indeed suggest that we live not in different cultures but in different natures (hence the term *multinaturalism*). Once we come to see that we

11. Eduardo Viveiros de Castro, “The Crystal Forest: Notes on the Ontology of Amazonian Spirits,” *Imer Asia* 9.2 (2007): 153–72, at 153.

12. See Eduardo Viveiros de Castro, “Exchanging Perspectives: The Transformation of Objects into Subjects in Amerindian Ontologies,” *Common Knowledge* 10.3 (Fall 2004): 463–84.

live in different natures rather than, or more than, in different cultures, we also come to see how important it is to analyze, understand, and account for their differences, as well as to learn from their variousness.

Viveiros de Castro's approach, his multinaturalism, is difficult to compare with Smith's. After all, she is comparing (or rather, contrasting) two basic dispositions of Western academic discourse—and thus, no matter how much at odds she and her readers find those two dispositions, Viveiros de Castro as an anthropologist explaining Amazonian cosmology to Western readers, is exposing a much more radical set of differences. As he puts this point, forcefully: “there are infinitely more things in common . . . between the Nazis and Western liberal intellectuals than between the former and Amazonian peoples.” On the other hand, it must be said that the particular debates that Viveiros de Castro (like Marilyn Strathern, another anthropologist contributing to this symposium) engage in largely take the constructivist legacy for granted, in a way which Smith does not. Strange to say, the anthropologists' project is, in this respect, perhaps narrower than Smith's, that deals with vast conceptual incongruences that, indeed, bear specifically on how we can or cannot interpret the practices and beliefs of alien cultures and past times. Given Viveiros de Castro's identification of anthropology as “the most Kantian of all the Humanities,” one might suggest that Smith's analysis be consulted as a resource for freeing anthropology from that heritage.

Partial Comparisons, Changing Scales

Clearly, questions about what we compare *for* are wrapped up in every choice of *what* to compare, and in every comparative analysis. Moreover, as Strathern observes, the scale of comparison influences what count as data, analysis, interpretation, and theory—to which we should add that those scales fluctuate. The conventional categories or “persuasive fictions” of any discipline will influence deeply what can count as fact or as interpretation, also what can count as an explanandum and what as an explicator. Those categories have an effect as well on why an explanandum, explanation, or comparison is regarded as interesting (or not).¹³ The contributions to this symposium illustrate this situation, and Strathern's article treats it explicitly. Under the title “Binary License,” she unfolds an argument that, on one level, is about changing patterns of social practice in Mt. Hagen, Papua New Guinea, and about contemporary anthropological analyses of conflict. On a second level, however, her argument concerns analytical means for dealing with the unstable relations between theory and data, between the conceptual and the empirical. Her strategy is to pay special attention to “the point of

13. Marilyn Strathern, “Out of Context: The Persuasive Fictions of Anthropology,” *Current Anthropology*, 28.3 (1987): 251–81.

bifurcation” at which a “distinction between terms could lead analysis down different routes.” The example she offers is based on revisiting her own experiences on arriving in Mt. Hagen: engaging this material anew, she says, could as well move in the direction of “theorizing reflexivity” as of “elucidating ethnography.” Although this observation of Strathern’s might suggest that the choice of which turn to take is a matter of subjective preference, she makes clear that the social scientist is part of a disciplinary network that constrains what may be regarded as the proper scaling of phenomena. Distinctions both between and within disciplines rely crucially on the work of binary divisions such as that between theory and data. (Hence Strathern’s remarking, on the title of this symposium, that is a “provocation,” given the “unlikeliness” that a theory or practice will emerge to bridge the gap between comparativist and relativist social science.)

Still, it is by no means Strathern’s argument that the problems of choice a social scientist may face—the issues of analytical or empirical scale, and of interrelations between what are presumed to be concepts and what are presumed to be data—are resolvable at the level of epistemology. In her analysis, theories exist at the same level as practices: “Ideas are as contingent on themselves as on their objects,” she writes, and she argues that the theoretical and the empirical are both equally and fully empirical (as well as both equally and fully theoretical). STS researchers have drawn a similar conclusion from actor-network theory and related approaches (including Smith’s).¹⁴ It is conventional in STS to view nonhumans as well as humans (and thus our concepts too) as historically changing actors. In light of which, it is especially interesting to consider Strathern’s comments on the efforts of John Law and Annemarie Mol to theorize “the ‘multiplicity’ inherent in ways of knowing and acting” and to deploy the resultant theory against the “language of fragmentation that perspectivalism generates.” All these approaches share the premise that there exists no single, stable, underlying nature on which all actors have their perspectives. But their differences, despite that basic agreement, are evident. For if, Strathern contends, “multiplicity . . . exists in the numerous but invariably overlapping practical contexts that elicit diverse enactments of knowledge,” then “multiplicity is perspectivalism’s critique of itself.” Here she draws a distinction between perspectivalism, as defined and criticized by Law and Mol, and the perspectivism of the Amerindians and Melanesians: “to be perspectivalist acts out Euro-American pluralism, ontologically grounded in one world and many viewpoints; perspectivism implies an ontology of many worlds and one capacity to take a viewpoint.” If so, she concludes, perspectivalism is the “antonym” of perspectivism.

14. Christopher Gad and Casper Bruun Jensen, “On the Consequences of Post-ANT,” *Science, Technology, and Human Values* 25.1 (2010): 55–80.

One might say that we are faced here with two distinct kinds of relativism, and in the present context we ought to compare them. But the difference between them extends to their scales of comparison. A signature move of STS research, such as that of Law and Mol, is to render symmetrical and, thus, comparable the relations of concepts (or “micro-ontologies,” such as that of a particular hospital or technological project) and the things that shape them. An analogous move is made by Strathern and Viveiros de Castro as they enable comparisons between different formations, referred to as Euro-American, Amerindian, or Melanesian ontologies. But the micro-ontologies of STS and the ontologies that anthropologists tend to deal with do not operate on the same scale. Whereas the stance of STS—its kind of relativism—might grant agency to Melanesian *pigs* or Amazonian *jaguars*, the stance taken by these anthropologists leads them to a more human-centered (in this precise sense) focus on how agency is attributed to pigs and jaguars by Melanesian and Amerindian *people*. Classical terms do not precisely capture the overlaps and differences here; still, one might say that the multi-naturalisms of STS and anthropology take etic and emic forms, respectively.

About her own account, which moves from Melanesia to the Balkans, Strathern notes how easy it would be to object that it is based on “wild generalizations” or to bring up “specific counter-points.” She insists also that this objection would miss the point, since “both moves are encompassed” by the overall matter of concern that her contribution addresses. In defense of the “sheer provocation” of conjoining relativism and comparison, one might make a similar argument. Still, what Strathern’s article chiefly shows is how scales of comparison do constant work, both analytically and empirically, in tying together the persuasive fictions produced by academics in the human and social sciences.

Persuasive Fictions and the Capacity to Object

The status of fictions, comparisons, and facts in the humanities and social sciences differ markedly from those in the natural-science disciplines that Isabelle Stengers discusses. In her study of Galileo, Stengers argues that his aim was precisely to produce an incontestable fact, *not* a persuasive fiction. To do so, Galileo and other scientists had to struggle not only against the recalcitrance of nature but also, Stengers argues, against the ingrained skepticism of society. “Conquering skepticism” is her term for the efforts that scientists put into making facts out of what are initially hunches, suppositions, and fictions. Central to this endeavor are the nonhuman objects that ensure that facts are “not imposed” but, rather, seen to arise from “something belonging to the phenomena studied and that therefore could be turned successfully into an instrument for making comparisons.” A reductive ambition is likewise central because, as Stengers explains, the scientific domain “depends upon, or at least implies, eliminating the charms of

conversation.”¹⁵ But if, in Stengers’s account, the “charms of conversation” have little place in the practices of natural scientists, the development of one capacity on the part of nonhuman actors—the capacity “to object”—is crucial. If the establishment of new facts is dependent on events through which phenomena are made expressive in new ways, those events in turn are generated in and by settings—the constructions of scientists—in which nonhuman entities obtain the capacity to say No.

Stengers argues, however, that contemporary science, infused by the values and practices of the “knowledge economy,” is destroying scientists’ own capacities to “say No,” and therefore to produce good science. “Scientists as they are directly mobilized by competing industrial interests,” Stengers writes, “will no longer be mobilized by the duty to have their facts resisting their colleagues’ objections and compelling their colleagues’ agreement.” By focusing on standardized methods and “high throughput,” industrialized science creates a situation in which “the general wisdom will prevail that one should not object too much.” Thus, for Stengers, the knowledge economy functions as a vast “equivalency-producing machine” that renders the divergence of scientific practices impossible. Arguing for the necessity of an ecology of practices, Stengers views this situation as akin to an “ecological catastrophe.” She rejects the notion (found both in STS analyses and among policy makers) that the situation is a normal one, in which adaptation and transformation of existing practices take place in due course. “It is always possible,” she replies, “to speak of practices flexibly transforming themselves. What will have been eradicated, though, are all the diverging, practical attachments standing in the way of systemic flexibility—attachments that determine what matters for each practice, what motivates its practitioners to think, feel, and (if need be) resist.”

In other words, it is through very particular comparative and competitive ventures that scientific practices are generated; and, in the climate of flexibility that relativism encourages and sustains, the specific conditions that enabled these practices to flourish may fade away.

Although deeply concerned about the future of scientific practices, Stengers also suggests that we might view the challenges as related to the emergence of a new ecology of practices. As she reminds us, ecology, after all, “is not about predators and prey only, but also about connecting events, such as symbiosis.” Insisting that a precondition for attaining symbiosis is that practices are never rendered equivalent, she argues that the point is to create relations with others while *continuing to diverge*: “each needs the other in order to pursue its own interests.”

15. Isabelle Stengers, “Beyond Conversation: The Risks of Peace,” in *Process and Difference: Between Cosmological and Poststructuralist Postmodernisms*, ed. Catherine Keller

and Anne Daniell (Albany: State University of New York Press, 2002), 235–55, at 235.

Risks of Comparative Relativism

“Each needs the other in order to pursue its own interests” is a statement, I will hazard to suggest, that is firmly within the scope of comparative relativism. The relativism involved here is not the obverse of objectivism; it is not that threatening face of relativism that disturbs so many philosophers and social scientists. The upshot of comparative relativism is, rather, that knowledge is made through relations and that no methodological procedures sanction it, except provisionally.

Stengers follows Charles Péguy in suggesting that one requirement of relativism be: “Pay attention to consequences.” The corollaries of this requirement are (a) that “no comparison would be legitimate if the parties to be compared are not able to present their own version of what the comparison is about,” and (b) that each party would appear and be “concerned to appear, in his particular full force.” In this regard, Stengers’s own work is exemplary. The ecology of practices about which she writes comprises physicists and psychologists, anthropologists and witches, hypnotists and junkies, who share only occasionally a capacity or willingness to experiment with new relationships, situations, contrasts, and appetites. The variety of relativism that Péguy defines and Stengers exemplifies is likewise illustrated, I believe, by the expression in “full force” that Amerindian shamans and Hageners are given in the writings of Viveiros de Castro and Strathern—and indeed by the force of expression that Smith permits her academic interlocutors.

Lévi-Strauss proposed that anthropology should stick to single case studies, letting go the ambition for statistical significance. He also suggested that cases be selected that are likely to be suitable as models—likely to facilitate a golden event. But as Stengers argues, there is no method for creating the event, even though we should strive for its creation: there is only the experiment and the occasional success. At the same time, the event, whether in the natural or the social sciences, will not emerge of its own accord; and it is never, of course, found simply in the form of empirical data. The event must be induced, prodded, supported, mediated—and it furthermore must be constructed in thought. The contributions that Strathern, Smith, Stengers, and Viveiros de Castro make to this symposium all testify to that group of observations. If one can speak of comparative relativism as a conceptual matrix of any sort, then it has (in Stengers’s words) “no authority of its own.” The matrix works, if it does, “through insinuation and transformative effects as an infectious lure for new creative contrasts.”¹⁶ The aim is not agreement but alliance.¹⁷ Whether or not alliance is effected by this experiment, the risk of the experiment seems well worth taking.

— *Casper Bruun Jensen*

16. Stengers, “Beyond Conversation,” 245.

17. Stengers, “Beyond Conversation,” 248.

THE CHIMERA OF RELATIVISM

A Tragicomedy

Barbara Herrnstein Smith

Whatever sort of thing relativism is taken to be—doctrine, thesis, crime or folly, insight or abyss—it is certainly, from the perspective of intellectual history, exceptionally elusive. Is there a single specifiable claim or denial, even a minimally describable “family” of them, shared by Protagoras, Montaigne, Nietzsche, Heidegger, Franz Boas, Paul Feyerabend, Jacques Derrida, Bruno Latour, and the majority of undergraduates on today’s college campuses? All these have been said to “embrace” or “espouse” relativism, or to have “slipped” or “fallen” into it, by reason of some utterance they have made or failed to make, some attitude they have displayed or failed to display.¹

Logicians suggest that we are in the presence of relativism when one or another self-evidently solid and important thing (for example, *truth, value, mean-*

1. See Rom Harré and Michael Krausz, *Varieties of Relativism* (Oxford: Blackwell, 1996), for an effort to distinguish various types of relativism and “to extricate and examine the arguments, abstracted from sources ancient and modern, that have been offered for and against the main varieties” (1). See Maria Baghramian, *Relativism* (London: Routledge, 2004), for the idea, in the absence of any clear definition of the topic of her book, of a “family” of claims, positions, or doctrines. For an unhappy evocation of the moral relativism of contemporary college stu-

dents, see Judith Jarvis Thomson, *Goodness and Advice*, ed. Amy Gutman (Princeton, NJ: Princeton University Press, 2001). For a “postmodern relativism” identified by a failure on the part of some scholars to offer certain emphases or raise certain questions in their work, see Satya Mohanty, *Literary Theory and the Claims of History: Postmodernism, Objectivity, Multicultural Politics* (Ithaca, NY: Cornell University Press, 1997), esp. 130–31.

ing, or *reality*) has been said by some perverse or logically injudicious person to be “relative to” something soft or slippery (for example, *context*, *culture*, *language*, *conceptual scheme*, *individual perspective*, or *political interest*). But such X-is-relative-to-Y statements are almost always distortions of what has actually been said. What has often actually been said—as in the case of the figures I have mentioned, from Protagoras to Latour—are statements to the effect that human perceptions, interpretations, and judgments are not absolute, universal, or objective in the sense of being independent of all perspectives and/or invariant under all conditions; that what we take to be real, true, and good depends upon and varies with, among other things, our assumptions, expectations, categories, and existing beliefs as these are affected to one degree or another by, among other things, our particular experiences and situations, both past and ongoing; and that these in turn are affected to one degree or another by, among other things, our historically and otherwise particular social, cultural, and institutional environments, including the conceptual and verbal idioms prevailing in our communities.

These statements of variability and contingency have challenging implications for familiar ways of talking and thinking about truth, value, and reality; but they are not claims about any of these terms or concepts taken as autonomous entities or properties. Statements about the dependence of our perceptions, interpretations, and judgments on various more or less unpredictable and uncontrollable sets of conditions do not amount to denials of the existence of anything that could be called *truth* or to claims about the purely subjective, purely verbal, or purely social status of things such as stones, mountains, quarks, germs, or genes that we may take to be, in some unproblematic sense, real. Rather, statements of these kinds, often said to be relativistic, alert us to the relational aspects of seemingly autonomous entities and seemingly inherent properties and to the fact that the quite heterogeneous situations that we name *truth*, *fact*, *knowledge*, *science*, or *reality* are often quite complexly constituted and sustained. Such statements also alert us to the historicity and the often far-from-unproblematic meaning of such ideas as *objective truth* or *transcendent value* and to the deeply problematic nature of *reality* if it is understood as an autonomous, absolutely privileged realm of being.

The sorts of statements just described are not shallowly reductive, deterministic, or gloomy. They do not claim that human perspectives vary *only* historically or culturally; they do not require us to believe that the human mind is a blank slate; they do not commit us to the view that people from different eras or cultures have nothing in common, or that each culture or period is a distinct and isolated universe, or that the differences among our perceptions, interpretations, or judgments are always very large or very significant. I note all these negatives because the sorts of observation that I am describing here—observations of contingency, variability, and/or relationality, often labeled relativist or taken as

reflections of an implicit doctrine of relativism—are commonly paraphrased in just these ways or said to have just such dubious or unhappy implications.

I

In the human sciences, what often elicits the charge or label “relativism” is the display of certain attitudes, notably those of epistemic tolerance, or the recommendation of certain methodological principles—especially, in recent years, principles involving explanatory or evaluative symmetry. The two disciplines that have proved most generative of the attitudes and principles in question are history and anthropology or, more precisely, historiography and ethnography—the charting of other times and other people. Of course, the observation of human variety in travel, including time travel, does not always increase tolerance or chasten egotism. It can just as readily deepen misanthropy or ratify a sense of the perfect reasonableness and absolute propriety of one’s own views and practices. Nevertheless, reports of what seem to be other ways of being human have operated virtually from their beginning as a reservoir of counterexamples to standard views of what is natural, necessary, or inevitable for members of the species to do, feel, or think.

Along with more particular forms of sophistication arising from their work in the archives and the field, historians and anthropologists often develop a generally heightened consciousness of the variability of human practices, institutions, and individual responses. While a sharpened awareness of this kind may develop from everyday observation, as reflected in the prudential axioms of folk relativism (*different strokes for different folks* and so forth), of particular interest here is the explicit cultivation of such awareness in the pursuit of disciplinary aims as recommended by such influential early twentieth-century historians, anthropologists, and social theorists as Durkheim, Weber, Carl Becker, and Ludwik Fleck and, later in the century, by such important figures as Benjamin Lee Whorf, Thomas Kuhn, and David Bloor.² While not all of these figures described their views or programs as relativistic (and none of them proclaimed any doctrine under the label of “relativism”), all of them stressed the dependence of our ideas and practices on historically and culturally variable conditions and individual perspectives, and all emphasized the consequent need to cultivate a self-conscious wariness in their respective fields. These scholars and theorists made such points against what they saw as dubiously self-privileging aims, claims,

2. For more on these figures, Becker and Fleck in particular, see my contribution to a prior symposium here on relativism and its reception: Barbara Herrnstein Smith, “Relativism, Today and Yesterday,” *Common Knowledge* 13.2–3 (Spring–Fall 2007): 227–49, esp. 234–44.

and methods in their particular fields; and their remarks were directed more or less exclusively and explicitly to fellow practitioners. The principles in question were not, in other words, produced out of the blue, nor were they directed to humanity at large or intended to govern the general conduct of ethical or intellectual life.³ This last point requires special emphasis because it is routinely missed by commentators who, after plucking those recommended principles from their intellectual, institutional, and historical contexts and improperly absolutizing and universalizing them, go on to register outrage or alarm at the absurd, unwholesome, or debilitating implications that they thereupon derive from them—the implication, for example, that all beliefs and cultural practices are equally worthy of respect or that reasons cannot or should not count in our assessment of truth claims.⁴ Indeed, the obliteration of relevant historical, intellectual, and institutional contexts is crucial in the generation of the doctrines, “claims,” or “theses” that make up the chimerical beast—part straw man, part red herring—commonly evoked under the name “relativism.”

This chimera—or, as I have called it elsewhere, fantasy heresy—is not the product of dishonest or intellectually incompetent people. On the contrary, it is largely the issue of critical efforts by intelligent, sometimes ethically motivated, often exceptionally well-trained people. What must be added, however, is that they are trained in quite *particular* conceptual idioms. Such idioms may be well established in their own fields and serve their professional purposes satisfactorily in the domains of their customary disciplinary practices. Nevertheless, the concepts that are central to those idioms, and the conceptual syntax that is central to their deployment in descriptions, analyses, and arguments, are themselves historically contingent and local, not necessary or universal. Relativism-refuting scholars typically take for granted certain definitions, distinctions, and

3. These intentions are clear in the following passages by anthropologist Ruth Benedict and sociologist David Bloor):

Any scientific study requires that there be no preferential weighting of one or another of the items in the series it selects for its consideration. In all the less controversial fields like the study of cacti or termites or the nature of nebulae, the necessary method of study is to group the relevant material and to take note of all possible variant forms and conditions. . . . It is only in the study of man himself that the major social sciences have substituted the study of one local variation, that of Western civilization. (Ruth Benedict, *Patterns of Culture* [1934; Boston: Houghton Mifflin, 1959], 3)

If [the sociologist’s] theories are to satisfy the requirement of maximum generality they will have to apply to both true and false beliefs. . . . The sociologist seeks theories which explain the beliefs which are in fact

found, regardless of how the investigator evaluates them. . . . The approaches that have just been sketched suggest that the sociology of scientific knowledge should adhere to the following four tenets. . . . (David Bloor, *Knowledge and Social Imagery* [1976; Chicago: University of Chicago Press, 1991], 5, 7).

4. For the extraction of the latter implication from an improperly universalized paraphrase of Bloor’s four tenets for the sociology of scientific knowledge, see Paul Boghossian, “Constructivist and Relativist Conceptions of Knowledge in Contemporary (Anti-) Epistemology: A Reply to Barbara Herrnstein Smith,” *South Atlantic Quarterly* 101.1 (Winter 2002): 213–27; repeated in Boghossian, *Fear of Knowledge: Against Relativism and Constructivism* (New York: Oxford University Press, 2006). See Smith, “Reply to an Analytic Philosopher,” *South Atlantic Quarterly* 101.1 (Winter 2002): 229–42, for discussion of these and related relativism-refuting moves.

conceptual relations that other scholars—precisely those whom they charge with embracing an absurd or appalling relativism—have come to view in crucially different ways. Because the former appeal in their arguments to those presupposed definitions, distinctions, and relations, their arguments appear, at least to those who share their conceptual idioms and syntax, to demonstrate that the alleged claims of the alleged relativists are indeed absurd or appalling.⁵ But, for the same reason—that is, because the relativism-refuters deploy and depend on the very concepts and relations that are at issue (concepts such as *truth* and *reason*, relations such as those between what are referred to as *facts* and *evidence*)—their arguments can have no intellectual force for the alleged relativists, who know themselves not to be saying the foolish things they are charged with saying and who do not and cannot take for granted the concepts, definitions, distinctions, and relations to which the relativism-refuters appeal. The result is pure nonengagement and perfect deadlock. The chimerical beast called Relativism lies always already slain, but—as one of “the philosophical undead,” it always walks again.⁶

There are signs that this tragicomic episode of intellectual history may have run its course. Most cultural anthropologists and historians of science abide by the now well-established methodological principles of their respective disciplines without giving them much thought or finding it necessary to explain or defend

5. A recent example of the significant operation of such presuppositions can be found in John Searle, “Why Should You Believe It?” *New York Review of Books* 56.14 (September 24, 2009): 88–92, where historically and otherwise particular discursive-conceptual deployments of the terms *statement*, *fact*, *evidence*, and *rationality*, along with the idea that certain logical entailments are “built into the fundamental structure of thought and language,” figure in a set of purportedly relativism-refuting arguments. Thus in discussing, for example, what a Native American might say about how his tribe came originally to occupy its lands, Searle—here summarizing and endorsing Paul Boghossian’s arguments against relativism in his book *Fear of Knowledge*—observes that it is a “requirement of rationality” that “anyone who makes such a statement is thereby committed to the existence of a fact,” and that this “commitment in turn carries a commitment to being able to answer such questions as . . . What is the evidence?” and that “only certain kinds of things can count as evidence.” For Searle, as for Boghossian, it is self-evident that the things that can count as evidence do not include traditional accounts transmitted in tribal legends. Accordingly, neither Searle nor Boghossian can understand—and both therefore regard as merely ideologically motivated—the empirical observation by an outsider (here, an anthropologist) to the effect that, in some communities, traditional accounts can and do count as evidence in the psychologically and socially significant sense of having conviction-affecting weight. Of course, Native American tribal leg-

ends do not count as evidence in United States courts, or in the arguments of Western philosophers, or when Western anthropologists address each other regarding factual claims. But the reasons they do not have evidentiary weight in these contexts have nothing to do with anything “built into the fundamental structure of [all?] thought and [any?] language.” Moreover, those reasons have little to do with “the requirement[s] of rationality” except insofar as *rationality* is itself defined in historically and otherwise quite particular ways that operate with intervalidating reference to systematically related definitions of terms like *statement*, *fact*, and *evidence*. For an analysis of the self-affirming circularity involved, see Smith, *Belief and Resistance*, 118–24. For discussion of the resultant question-begging structure of many arguments aimed at refuting relativism, see Smith, “Unloading the Self-Refutation Charge,” *Common Knowledge* 2.2 (Fall 1993): 81–95, reprinted in *Belief and Resistance*, 73–87.

6. See Joseph Rouse, “Vampires: Social Constructivism, Realism, and Other Philosophical Undead,” *History and Theory* 41.1 (2002): 60–78. For all the logical ink poured over their heads, none of the major thinkers thus putatively refuted—for example, Feyerabend, Foucault, Derrida, Rorty, or Latour—has acknowledged the alleged error of his or her thinking on the crucial issues.

them to anyone.⁷ At the same time, the scope, force, and interest of formal exposures and refutations of what is named “relativism” appear to be diminished in the current philosophical literature and elsewhere. Indeed, some younger logicians and philosophers now unapologetically detail and defend relativity-affirming theses as such, while, for better or worse, a genial folk-relativist ethos (*live and let live*) seems to prevail among students on many of our de facto multicultural campuses.⁸ Where full-throated formal denunciations of relativism continue to be voiced in contemporary discourse, they seem to issue primarily from protectors or would-be restorers of some threatened or faded orthodoxy—for example, positivist scientism, realist-rationalist epistemology, or Vatican infallibilism—and are clearly designed to discredit one or another currently significant challenge.⁹

In spite of these hopeful developments (hopeful, at least, from some perspectives), anxieties about the implications or consequences of what is identified as relativism linger, and there are also recent efforts to refute relativism on what are said to be scientific grounds. Two contemporary sites of antirelativist energy require special attention. The first centers on the claim that cultural relativism is refuted by the demonstrated existence of cognitive universals. The second involves the fear or charge that relativist convictions lead to politically debilitating neutrality in the face of oppression and other social ills.

II

According to an influential group of evolutionary psychologists and cognitive anthropologists, we now have evidence of what they refer to as “the psychic unity of mankind”—specifically, the existence of innate, evolved, universal mental mechanisms underlying all human thought, behavior, and culture.¹⁰

7. I refer here to such principles as historical and cultural contextualization, explanatory symmetry, and self-inclusive reflexivity, not to such hypertrophic views (where they are maintained) as the total uniqueness of every culture or the impossibility of any cross-cultural generalizations about human behavior or institutions.

8. For recent works attesting to the extent of linguistic relativity and to the historicity of such concepts as *science*, *knowledge*, and *objectivity*, see, e.g., John J. Gumperz and Stephen C. Levinson, eds., *Rethinking Linguistic Relativity* (Cambridge: Cambridge University Press, 1996); Ronald N. Giere, *Scientific Perspectivism* (Chicago: University of Chicago Press, 2006); Steven Shapin, *The Scientific Life: A Moral History of a Late Modern Vocation* (Chicago: University of Chicago Press, 2008). For a rigorous articulation of “epistemological relativism,” defended handily against a set of standard philosophical refutation-arguments, see Martin Kusch, *Knowledge by Agreement: The Programme of Communitarian Epistemology* (Oxford: Oxford University Press, 2002). For other recent philosophical defenses of

what is explicitly named “relativism” and for a useful overview of current philosophical treatments of the issue, see Manuel García-Carpintero and Max Kölbel, eds., *Relative Truth* (Oxford: Oxford University Press, 2008).

9. See, e.g., versus “postmodern relativism,” E. O. Wilson, *Consilience: The Unity of Knowledge* (New York: Knopf, 1998), 182; versus “relativism and constructivism,” Boghossian, *Fear of Knowledge*; versus a “dictatorship of relativism,” Joseph Cardinal Ratzinger (now Pope Benedict XVI), homily, “Pro Eligendo Romana Pontifice,” April 18, 2005 (www.vatican.va/gpII/documents/homily-pro-eligendo-pontifice_20050418_it.html), translated into English and discussed by various contributors in *Common Knowledge* 13.2–3 (Spring–Fall 2007).

10. For the general claim, see esp. John Tooby and Leda Cosmides, “The Psychological Foundations of Culture,” in *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*, ed. Jerome H. Barkow et al. (Oxford: Oxford University Press, 1992), 19–136.

Questions can be raised about the empirical basis and conceptual coherence of this view, which is by no means accepted by all psychologists or cognitive scientists. What concerns me here, however, is the attendant argument that the demonstrable existence of such universal mechanisms undercuts certain alleged “relativist” claims.

The argument just described appears in a recent book by cognitive anthropologist Scott Atran, who maintains that “there is a long-standing claim on the ‘relativist’ side of anthropology, psychology, and the philosophy and history of science to the effect that people who live in ‘traditional’ cultures . . . live in conceptual worlds that are profoundly and incommensurably different from our own world (and each other’s worlds).”¹¹ According to Atran, “this claim is mistaken in light of the following facts”:

1. There is considerable recurrence of symbolic content [of supernatural beliefs] across historically isolated cultures. . . .
2. This recurrence owes chiefly to universal cognitive mechanisms that process cultural input (information) in ways that are variously triggered but subsequently unaffected by the nature of the input.

After describing various other general features of supernatural beliefs, Atran continues:

The most striking support for cultural relativism is thought to come from those “primitive,” “exotic,” or “traditional” societies where, from a Western standpoint, natural and supernatural phenomena are so seemingly intermeshed that the people in those societies just live in “another world.”¹²

Atran then lists some beliefs recorded among the Itza’ Mayans of Mexico that “we” (contemporary educated Westerners, presumably) would find hard to believe or to restate in any way that made sense—for example, that a certain sorcerer transformed himself into a dog, that a person “ensouls” a house, and that a house has a soul.¹³ On such beliefs, he comments as follows:

11. Scott Atran, *In Gods We Trust: The Evolutionary Landscape of Religion* (Oxford: Oxford University Press, 2002), 84.

12. Atran, *In Gods We Trust*, 84. Atran gives no sources for the views thus stated or for the phrases, here and below, that he puts in quotation marks.

13. Atran’s examples appear singularly ill chosen to make his point. Many readers and certainly many cultural anthropologists, self-described cultural relativists included, would find such beliefs readily recognizable and

have no trouble restating them in familiar, credible-enough terms. But his point here is already strained. The cultural relativism of most cultural anthropologists is not motivated by, and certainly does not depend on, their having found the beliefs of the people they study nonsensical—as distinct from significantly (and sometimes, as Eduardo Viveiros de Castro suggests, instructively) different from beliefs generally accepted by educated people in Western cultures.

From the foregoing we might conclude that we and the Itza' just live in conceptually different everyday worlds. That people abide such apparently different worlds may, in turn, be taken as support for the flexibility of the human mind, that is, a mind unconstrained by cognitive structures that are evolved . . . task-specific or innately determined and content-constraining. But this conclusion is wrong.¹⁴

The somewhat awkward phrasing here obscures the tenuousness and circularity of the argument. Atran speaks of a conclusion wrongly drawn, and an idea wrongly drawing support, from ethnographic data about the strange beliefs of other people. The conclusion is that people can live in conceptually different worlds; the idea is that the human mind is flexible. But *is* either of these wrong, and *are* they wrongly concluded? Is it not the case that people, even some who eat daily in the same faculty-club dining rooms, can live in conceptual worlds that are profoundly different—for example, as I suggest above, relativizing anthropologists and relativism-refuting philosophers? And is it wrong to conclude from, among other things, the wide variety of cosmologies encountered by ethnographers that “the human mind”—which of course names a range of capacities and activities—is flexible?

Two other questions may be asked here that are significant for the logical and rhetorical force of Atran's argument. One is whether the quite common, quite general observation that “the human mind is flexible” is properly glossed as the technically very particular (and incoherent as stated) claim that the mind is “unconstrained by cognitive structures that are evolved . . . task-specific or innately determined and content-constraining.”¹⁵ The second is what makes a “fact” out of Atran's cognitive-universals explanation for the cross-cultural recurrence of the thematic content of various supernatural beliefs. Atran claims that such recurrences are evidence for the existence of universal cognitive mechanisms, but the only basis he offers for *that* claim is his simultaneous insistence that the only thing that can explain such recurrences is the operation of such mechanisms. The universalist claim and the ethnographic evidence for it are bootstrapped onto each other, and the purported explanation-proof refutation is totally circular.

Contrary to Atran's contention, the widespread recurrence of various mythic and religious themes can be explained without recourse to the positing of highly specific, content-constraining universal cognitive mechanisms. One notes, for example, such widespread—indeed pancultural—conditions of human existence as sunrise and nightfall or the recurrence of such humanly salient objects,

14. Atran, *In Gods We Trust*, 86.

15. Atran's writing is careless. Presumably he would not bother to debate the manifestly incoherent claim that, as he puts it, the mind is “unconstrained by cognitive structures that are . . . content-constraining.”

events, and experiences as birds and snakes, journeys and warfare, illness and dreams. There are also important contemporary alternatives to the strongly innatist, adaptationist view of mind and cognition that Atran invokes here as factually established. Such alternative views do not, as he suggests, come down to the blank slate of classic empiricism or cultural-environmental determinism; they do not claim that the human mind is altogether unconstrained. What they indicate instead is that, although various specieswide (“universal”) cognitive capacities, traits, or tendencies may exist, they must, in their actual operations, interact continuously with our other more or less highly individuated traits and tendencies and also with the traces of our individual experiences in particular physical, social, and cultural worlds.¹⁶ The point is significant for the operation of any putative human or cognitive universal, from language ability to the currently popular “moral sense.”

Theorists and scientists proposing these alternative views of human cognitive or psychological development would reject Atran’s reductive and otherwise dubious account of the generation of supernatural concepts by highly specialized mental mechanisms. They would also reject the mind-as-blank-slate view that Atran refers to, somewhat oddly, as “relativism” and implies is the only major alternative to his own strongly mentalist account of cognition. The individual mind, however we understand the term, is shaped by multiple forces, and its operations and products are of course limited or “constrained” in many respects; *and* “the human mind,” taken either individually or as a species characteristic, is flexible. These are not mutually exclusive observations. To maintain that the mind is flexible, in the sense of being responsive and capable of ongoing modification, is not to deny the existence of constraining forces on cognitive processes and products, including forces arising from general features of human neurophysiology as shaped over evolutionary time. The conflict of views that Atran evokes here is spurious. If there are any pure cultural determinists remaining in anthropology or strict environmental determinists among behaviorists or Jesuits, none of them has much authority in the current intellectual world.¹⁷ There are real conflicts here, but they are not over whether Nature or Nurture, evolved neurophysiology or culturally contingent experience, is decisive in shaping the content of our beliefs. The conflicts are over the contested institutional dominance of different factions in the contemporary social sciences, with so-called cognitive approaches seeking not only to displace older and no doubt limited approaches but also seek-

16. For a more extensive examination of Atran’s argument and examples, as well as for discussion of relevant alternative views of human cognition, see Barbara Herrnstein Smith, *Natural Reflections: Human Cognition at the Nexus of Science and Religion* (New Haven, CT: Yale University Press, 2009), 10–19, 62–64.

17. Behaviorists and Jesuits have famously spoken of the power of early conditioning or training to shape the minds of the young.

ing to hold their ground against newer and arguably more fertile developments.¹⁸ “Relativism” here, as often elsewhere, is a straw herring.

III

I turn now to the other current objection to relativism that I mentioned earlier. This is the fear or charge that relativistic convictions lead to ethically reprehensible neutrality or political passivity.¹⁹ One can understand how the charge or anxiety originates. As the theoretically described variability of human perceptions, interpretations, and judgments become consequential conflicts, and as those conflicts come closer to home, efforts by historians, anthropologists, and other scholars to maintain methodological symmetry and to treat all sides evenhandedly will appear improper and, from certain perspectives, objectionable. Moreover, efforts at neutrality under such conditions are likely to become strained for the scholars themselves. For example (quite close to home for Americans), an otherwise conscientiously impartial sociologist of science may find it hard to treat current, local promotions of biblical creationism symmetrically with efforts by biology teachers to present evolution without disclaimers in public high schools. Similarly, a Western-educated anthropologist may find it hard to report impartially on such exotic practices as female genital cutting. Under such conditions, the determinedly impartial sociologist of science or symmetry-maintaining anthropologist may be too involved in the outcome of such struggles, or too conscious of the effects of such practices on the lives of people he or she knows well (or can imagine vividly enough), to maintain an otherwise proper neutrality.

Where a conflict of views—perceptions, interpretations, or judgments—is neither hypothetical nor in the remote past, but actual, sharp, current, and caught up in one’s personal history or sense of personal identity, it is understandably difficult to be impartial. Where, moreover, the conflict involves people or communities to whom one has generally recognized obligations of kinship, friendship, membership, or alliance, it may be ethically improper—and, from some perspectives, politically culpable—for one to remain neutral. Thus, many sociologists of science were dismayed by their colleague Steve Fuller’s recondite account of

18. Prominent among the latter are Developmental Systems Theory (see Susan Oyama et al., eds., *Cycles of Contingency: Developmental Systems and Evolution* [Cambridge, MA: MIT Press, 2001]) and other views of cognitive processes that stress their dynamic, interactive features (see, e.g., Raphael Núñez and Walter J. Freeman, eds., *Reclaiming Cognition: The Primacy of Action, Intention, and Emotion* [Thorverton, U.K.: Imprint Academic, 1999]; Alva Noë, *Action in Perception* [Cambridge, MA: MIT Press, 2004]; Derek Melder, *The Act of Thinking* [Cambridge, MA: MIT Press, 2004]; Michael Wheeler, *Reconstructing the Cogni-*

tive World: The Next Step [Cambridge, MA: MIT Press, 2005]).

19. Related fears and charges of “quietism”—along with defenses of certain forms of it—are discussed in the extended symposium on these themes recently published in this journal. See “Apology for Quietism: A *Sotto Voce* Symposium,” *Common Knowledge* 15.1 (Winter 2009), 15.2 (Spring 2009), 15.3 (Fall 2009), 16.1 (Winter 2010), 16.2 (Spring 2010), 16.3 (Fall 2010).

the comparable scientific status of evolutionary theory and Intelligent Design during his testimony in a recent school-board trial in the United States.²⁰ Similarly, many feminist scholars were disturbed by anthropologist Saba Mahmood's representation of Muslim women's self-subjection to patriarchal teachings as a mode of personal agency comparable to forms of agency valued by Western feminists.²¹ There is, it might be said, a time for programmatic symmetry to be laid aside and for strong partisanship and explicit advocacy or critique to be taken up. I believe there *are* such times. But I also believe that they are not determinable in the abstract or in advance. Rather, I would say, those times are determined for each of us by the relevant particulars of our personal histories, identities, and obligations, and also by the relevant particulars of the conditions that present themselves. Here, as often elsewhere, the best—most ethically responsive and intellectually responsible—way to handle the apparent difficulties created by relativist commitments is to relativize even further—that is, to acknowledge the significance, for oneself as for others, of even broader ranges and more subtle forms of contingent circumstances.

It may be presumed that, in their public actions as scholars, both Fuller and Mahmood were responsive to the relevance of various contingent circumstances. There is room for argument, however, about their respective decisions and also reason to speculate about how they arrived at them. It is not clear, for example, that Fuller considered as carefully as he might have done the long-range intellectual, educational, and political consequences of his testimony, as a credentialed social scientist, on behalf of the crypto-creationist side in the school-board trial. Similarly, it is not clear that Mahmood presented all the crucially relevant features of the lives and situations of the Muslim women she studied—for example, the history or threat of physical violence under which they may have acted. One may also wonder to what extent Fuller's generally populist sentiments, directed against the science establishment, put him in ideological alignment with the promoters of Intelligent Design.²² And similarly, one may wonder to what extent Mahmood's strict neutrality in her representation of the Egyptian women's mosque movement reflected a degree of protectiveness toward Islam, which, as a Pakistani-born woman (and in view of ongoing displays of Western arrogance and condescension), she could share with her subjects. In short, it is not clear that

20. For an exchange between Fuller and his critics regarding his testimony in *Kitzmiller v. Dover*, see *Social Studies of Science* 36.6 (December 2006): 819–53. Fuller himself pointedly rejects the idea that the pursuit of sociology of science entails a refusal to offer judgments on public issues involving science.

21. Saba Mahmood, *Politics of Piety: The Islamic Revival and the Feminist Subject* (Princeton, NJ: Princeton University Press, 2005).

22. In a subsequent commentary on his participation in the Dover trial, Fuller explicitly disavows relativism and, noting that science-studies fields “have a weak public presence and a history of being treated as pawns by more powerful players,” observes: “Had I been more of a relativist, presumably I would have taken heed of these features of the situation and refrained from offering my services.” Steve Fuller, “Letter to the Editor,” *Isis* 100.1 (2009): 115–16.

these two seeming demonstrations of objectionable relativistic impartiality were actually so impartial. Indeed, what may disturb critics of relativism in such demonstrations is not politically objectionable neutrality but evidence of a relevant bias, muffled by a claim or show of fairness, for what is viewed as the wrong side of some current political struggle.

The preceding discussion acknowledges the genuine difficulties that may be presented in ethically and/or politically charged situations by a cultivated consciousness of the dependence of our perceptions, interpretations, and judgments on culturally and historically variable perspectives—or what are often called relativistic views. But I want to say something against the facile and, I think, fundamentally improper association of such a consciousness with ethically irresponsible or politically culpable quietism. Clearly, the ways we act politically, the forms taken by our partisanship and either advocacy or critiques, can be determined more rather than less thoughtfully and on the basis of information that is more rather than less extensive, accurate, and relevant. Political actions can also be determined with greater rather than lesser concern for possible consequences, for broader and longer-term rather than only immediate and local consequences, and for consequences for wider rather than narrower ranges of people. In all these respects, political actions can be judged better rather than worse in the sense of being more rather than less ethically responsive and more rather than less effective in achieving either the particular political ends sought or some more broadly shared social goals. For these reasons, relativistic views, in the senses I have evoked here, do not make ethical or political judgments impossible. Moreover, when political activities are described and assessed in terms of the broad ethical and pragmatic dimensions just described, it is clear that their energies are not diminished by what are called relativist convictions. On the contrary, it seems obvious that such convictions—that is, an acute consciousness of the historical and cultural contingency of human perceptions, interpretations, and judgments (including one's own) and of the sometimes significant variability of human interests and perspectives—would tend to make someone's activities both more ethically responsive than and also, in the long run, at least as politically effective as actions undertaken by someone with resolutely universalist, absolutist, and/or objectivist convictions regarding the Truth and the Way.

Relativistic considerations do not commonly paralyze personal agency. They may, however, affect the form of the actions one takes and the processes by which one arrives at them. For example, a strong consciousness of the possible relevance of unknown conditions and alternative perspectives may qualify the terms and tones in which one issues a denunciation or calls for an intervention. Such considerations may also make one hesitate—take more time, review a wider range of options—before one grabs a gun or gives an order to fire one. It is not clear, however, that these are politically undesirable effects. As the sorry history

of many political movements and interventions suggests, the contingencies we deny and the variability we overlook for the sake of solidarity or for a show of unshakable conviction commonly come back to hit us or haunt us.

When thoughtfully worked through and put into practice responsibly, relativist convictions—in the sense of a cultivated consciousness of the variability and contingency of what operates as the real, the true, or the good—are neither ethically nor politically compromising. But, of course, not all relativistic convictions *are* thoughtfully worked through or responsibly evoked. On the contrary, expressed in sloganized forms (*everybody has his own opinion, who's to say what's good or bad*, and so forth), such ideas can be voiced mindlessly, lazily, and often with very bad manners (for example, condescendingly) or with very base motives (for example, to justify otherwise objectionable self-serving policies or practices). This recognition brings me to some concluding observations.

IV

There are many reasons why the invocation of relativism, or even just reference to it as a topic, can be distasteful. For one thing, its exceptional elusiveness, as detailed earlier, makes relativism a genuine headache to think about, difficult to describe coherently, and almost impossible to argue about productively. Also the recurrent philosophical equation of relativism with foolish or crude positions, such as an everything-is-equal-to-everything-else egalitarianism or an anything-goes nihilism, operates as a distinct disincentive to introduction of the term, even where it would be descriptively apt.²³ Most significantly, perhaps, the mindless, lazy, or cynical voicing of relativist slogans, as just described, gives relativism an understandably bad name among intellectually and ethically scrupulous people and, for that reason and others, leads to its strenuous disavowal by thinkers whose views, given a range of familiar characterizations, might well be regarded as relativistic.²⁴ If we seek to dispel fire-breathing chimeras, these difficulties must be recognized.

23. For analysis of these dubious equations, see Barbara Herrnstein Smith, *Contingencies of Value: Alternative Perspectives for Critical Theory* (Cambridge, MA: Harvard University Press, 1988), 150–54, and *Belief and Resistance*, 77–78. The strenuous rejection of relativism among philosophers may also involve its ancient association with the intellectually shaming charge—and, of course, sometimes quite valid exposure—of self-refutation. For discussion of this point, see Smith, “Unloading the Self-Refutation Charge.”

24. Such a disavowal occurs, for example, at the end of an analysis by Jacques Derrida of various contemporary proposals for responding to the use and/or abuse of drugs.

The analysis itself is thoroughly mindful of contingency and explicitly committed to methodological symmetry and evenhandedness:

Depending on the circumstance . . . the discourse of “interdiction” can be justified *just as well or just as badly* as the liberal discourse. . . . Since it is impossible to justify absolutely either the one or the other of these practices, one can never absolutely condemn them. In an emergency, this can only lead to equivocations, negotiations, and unstable compromises. And in any given, progressively evolving situation, these will need to be guided by a concern for the singularity of each individual experience and by a socio-political analysis

General observations to the effect that meanings and values are radically contingent, or that perceptions, interpretations, and judgments are essentially variable, have been articulated with a variety of affects and motives: earnestly, ironically, in good faith, in bad faith, despairingly, gleefully, to challenge dubious claims of objectivity or universality, to explain incomprehensible difference, to plead for tolerance, to justify neglect. But occasional or even frequent delivery with bad manners or base motives does not make such observations invalid. Nor does it oblige us to shore up or reaffirm otherwise dubious conceptions of objective truth, universal value, or transcendent criteria. Rather, when general ideas of variability or contingency are invoked and applied objectionably—for instance, where an ill-considered view is lazily excused as “just one man’s opinion” or an immediately consequential objectionable practice is shrugged off as “traditional in our/their culture”—then exposure and criticism are properly directed at the laziness, cynicism, or obscurantism involved, not at general observations of variability or contingency, or at that cloudy creature “relativism.”

Finally, the horror or embarrassment of relativism is, I suspect, the horror or humiliation of mortality. Observations of radical contingency and irreducible variability are disagreeable because they remind us that our achievements are fragile, that our meanings are not altogether under our control, and that there may not be truth at the end of our efforts or justice at the end of our struggles. They are disagreeable because they oblige us to recognize the limited significance of all that we hold important, the perishability of all that we cherish, and our own fickleness and faithlessness. Some people regard these reminders as pessimistic or nihilistic; others see them as useful in pursuit of a sensible and ethical life. It’s no doubt a matter of personal temperament. As folk-relativist wisdom has it, it takes all kinds to make a world and there’s no point arguing about tastes. Of course that would include intellectual worlds and philosophical tastes.

that is at once as broadly and as finely tuned as possible. I say this not to avoid the question any more than I do to argue for relativism.

Jacques Derrida, “The Rhetoric of Drugs,” in *Points . . . Interviews, 1974–1994*, ed. Elisabeth Weber (Stanford, CA: Stanford University Press, 1995), 239. The passage is quoted by Martin Hägglund in his recent study, *Radical Atheism: Derrida and the Time of Life* (Stanford, CA: Stanford University Press, 2008), 234. The quotation follows Hägglund’s sympathetic explication of a form of political thinking that he terms “hyperpoliticization”:

For a hyperpolitical thinking, nothing (no set of values, no principle, no demand or political struggle) can

be posited as good in itself. . . . Rather than having an ultimate legitimacy, it [any political order] can be challenged on the basis of what it does not include and must remain open to contestation because of its temporal constitution. To assert such a condition is not to give in to relativism” (184–85). These disavowals of an undefined relativism appended to exceptionally rigorous articulations of absolutism-rejecting views raise the question of what, in each case, *relativism* and either “argu[ing] for” or “giv[ing] in to” it are understood to mean. More generally, one might ask: on the basis of what specific considerations of intellectual history, or as the result of what individual experiences of usages of the term, are such disavowals thought necessary?

MULTIDIMENSIONAL REALITY

G. E. R. Lloyd

Barbara Herrnstein Smith does an excellent job of exposing the polyvalence of “relativism” and of identifying the principal common assumptions concerning such concepts as truth, values, meaning, and reality, to which it is thought to pose a threat. My aim, in this short commentary, is not so much to critique her analysis as to supplement it. In a whole series of contexts, relativism is not only no threat but must be accepted as a part of any complete answer, though not by itself the whole of such an answer. My tactics will be first to rehearse some historical data from ancient Greek and Chinese thinkers to illustrate their recognition of some of the problems and indeed some attempted resolutions of them. I shall then turn to our contemporary issues, where I shall concentrate on criticizing some of the oversimplifications found on both sides of the polemic. It is not a matter of choosing between psychic unity and psychic diversity, for both evidently capture elements of what we need for a correct understanding, leaving the main problem as that of charting the extent of the manifestations of each. Again the issue is not whether value judgments are possible—in my view, they are inevitable—but rather to recognize their provisional status and to unmask the covert assumptions by which they may be influenced.

By Plato’s day, it was already a commonplace of Greek thought that the different peoples by whom the Greeks were surrounded had divergent views on right and wrong, good and evil, and that they adopted various customs and practices with regard to sexual behavior, how to treat the dead, and so on. Scores of

examples of that recognition appear in Herodotus, the Hippocratic writers, the dramatists, and Plato's bogeymen the Sophists, including most notably Protagoras, at least as Plato represents him. Plato's response, as is well known, was to postulate absolute standards provided by transcendent Forms. What exists *per se* (*kath' hautō*) must be sharply contrasted with what exists only in some relation or other (*pros ti*). Beauty itself comes in the former category. But beautiful objects or events are beautiful in some respects but not in others, to some people but not to others, at one time but not at another, and so on.¹ While he did not exactly invent the contrast between the absolute and the relative (as Parmenides' distinction between Truth and Seeming anticipates some of his points), he certainly made it central to his metaphysics. But Aristotle's reaction both to Sophistic relativism and to Platonic absolutism is instructive. He rejected Plato's transcendent Forms, but in his ethics Aristotle combines an element of relativism with a continued adherence to objectivity.² The correct response to a moral dilemma varies from one individual to another. Generosity is a mean between the extremes of extravagance and stinginess. But what would be, for a poor person, a generous donation to a charity would rate as niggardly if the donor were a millionaire. Besides, how to behave demands the exercise of practical intelligence (*phronesis*); but just because there are no algorithms to determine the correct response does not mean that on any given occasion, for any given individual, there is *no* correct response. Aristotle evidently held that there is no incompatibility between the mode of relativism he accepts and his insistence on there being right answers to moral issues—*not* precise ones, to be sure, but correct ones nevertheless.

Ancient Chinese authors are as aware of cultural diversity as Greek ones, and again offer some instructive observations. For example, the *Lüshi chunqiu*, a third-century BCE cosmological treatise, explicitly remarks on the differences between the customs and practices of foreign tribes and those of the Chinese themselves.³ But having drawn attention to their different languages, clothes, houses, encampments, boats, carts, vessels, and tools, the writer goes on to insist that these foreigners are the same as the Chinese in respect of their feelings and desires. Indeed, feelings and desires unite all human beings, Chinese and barbarians, sage kings and tyrants. All are *ren*—a term that, like the Greek *anthropos*, is used of human beings in general, males and females, masters and slaves.

But if ancient writers sometimes maintained both cultural diversity and the unity of humanity, have modern advances in cognitive science transformed the

1. Plato, *Symposium* 210e–211a.

2. Aristotle defines virtue or excellence (*arete*) as “a disposition involving choice and lying in the mean relative to us (*pros hēmas*) as defined by reason and as the person with practical intelligence would define it” (*Nicomachean Ethics* 1106b: 36ff.).

3. *Lüshi chunqiu* 19/6/2, cf. John Knoblock and Jeffrey K. Riegel, *The Annals of Lü Buwei* (Stanford, CA: Stanford University Press, 2000), 497–98.

status quaestionis? Certainly those advances have been substantial. John Tooby and Leda Cosmides, for instance, have insisted on our common ancestry as hunter-gatherers in the long centuries of the Pleistocene, although of course they do not deny our current cultural diversity.⁴ But other claims for cognitive universals sometimes finesse the complexity of the issues, and that is before we factor in the distortions that Smith identifies in how the original studies have been manipulated for polemical purposes.

Smith criticizes Scott Atran's theses on religious experience, but his claims for a natural classification of animals bring the issues into sharper focus.⁵ Following the model of the work done by Brent Berlin and Paul Kay on color perception, Atran has insisted that a commonsense understanding of the different groups of animals underlies folk taxonomies and even scientific ones worldwide.⁶ But first, as regards color perception, I argued in *Cognitive Variations* that, for all the extensive empirical investigations that Berlin and Kay marshaled, their thesis was fundamentally flawed.⁷ This was for two main reasons. First, they paid insufficient attention to the fact that some color taxonomies focus not on hues, but on saturation or intensity. Second, they ignored the fact that when indigenous peoples were asked to identify certain hues on the Munsell chips with which they were presented, the terms they used sometimes did not have color as their primary significance at all, but rather the contrast between the living and the dead, for example, or the fresh and the desiccated. This was notably the case with the Hanunoo, where that point was made by Conklin, the ethnographer responsible, but was then ignored by Berlin and Kay despite their awareness of Conklin's study.⁸ Those objections do not lead to a denial that there are cross-cultural universals in this domain, those that relate to the anatomy of the eye and the neurophysiology of vision, as well as to aspects of the analysis of the input that is the physics of light. But studies by J. D. Mollon and his associates reveal that the discriminations that different individuals are capable of are very variable.⁹ Without using particular color terms to describe what they saw, subjects, when asked whether two presentations were or were not identical, had different responses. That finding led Mollon to remark that it is in fact quite rare for two individuals to have

4. John Tooby and Leda Cosmides, "The Psychological Foundations of Culture," in *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*, ed. Jerome H. Barkow, Cosmides, and Tooby (New York: Oxford University Press, 1995), 19–116.

5. Scott Atran, *Cognitive Foundations of Natural History: Towards an Anthropology of Science* (Cambridge: Cambridge University Press, 1990).

6. Brent Berlin and Paul Kay, *Basic Color Terms: Their Universality and Evolution* (Berkeley: University of California Press, 1969).

7. G. E. R. Lloyd, *Cognitive Variations: Reflections on the Unity and Diversity of the Human Mind* (Oxford: Oxford University Press, 2007).

8. Harold C. Conklin, "Hanunoo Color Categories," *Southwestern Journal of Anthropology* 11 (1955): 339–44.

9. John Mollon, "Seeing Colour," in *Colour: Art and Science*, ed. Trevor Lamb and Janine Bourriau (Cambridge: Cambridge University Press, 1995), 127–50.

exactly the same perception of color, a point masked by the apparent success with which we are able to communicate with one another on the subject. That work was, to be sure, not available to Berlin and Kay, but it continues to be ignored by those who continue their quest for cross-cultural universals in the domain of color. This provides one example of what I call the multidimensionality of reality. Another relates to Atran's own thesis on the classification of animals—the view I mentioned earlier that, despite the obvious variety in the actual systems of classification reported in the ethnographic record, there is an underlying commonsense appreciation of which animals are “companions” to which that is common to all humans and that converges more or less with the findings of science. Yet once again that underestimates the complexity of the problems. Mayr already referred to the “species problem.”¹⁰ Which groups of animals merit species status? Different criteria, morphology, interfertility, and DNA analysis have been tried and yield different results—which is not to say that the application of any such criterion is merely arbitrary. On the contrary, there can be more, or less, accurate analyses using each. But there is no neutral, totally theory-free, means of deciding which criterion to privilege. That does not mean that anything goes and that all claims to objectivity are illusory. The proper conclusion, I submit, is to acknowledge that in this domain, too, reality is multidimensional. Once again, a claim to access aspects of reality is compatible with relativizing moves that allow that there is no definitive answer to the question. While there is, to be sure, reasonable agreement over the main lines of the evolutionary history of most of the higher animals, that is certainly not the case with many lower groups—the Excavata, the Rhizaria, and the Chromalveolata, for instance—and not just because of the considerable gaps in the fossil record that provides our main evidence.¹¹

On the second main issue on which Smith focuses, whether cultural relativism leads to political quietism, I must be even briefer. From the denial of moral absolutes it does not follow that no value judgments are legitimate. Of course, passing judgment on the behavior of people in other societies or groups is always difficult, but it is surely not impossible. Otherwise we would be reduced to condoning the atrocities committed by the Nazis in the concentration camps on the basis of the argument that they saw nothing to be ashamed of—just as, coming closer to home, American servicemen appear to have not hesitated to record with obscene delight the treatment they meted out to Iraqi prisoners in Abu Ghraib. The exploration of the diversity of moral systems certainly underlines the need for caution and the recognition of complexities. But it provides the opportunity to hone our moral sensibilities, not to suppose they are incapacitated. As Smith implies, we are faced here, once again, with an example of fallacious dichotomies.

10. Ernst Mayr, ed., *The Species Problem* (Washington, DC: American Association for the Advancement of Science Publications, 1957).

11. Alastair G. B. Simpson and Andrew J. Roger, “The Real ‘Kingdoms’ of the Eukaryotes,” *Current Biology* 14.17 (September 7, 2004): R693–96.

RAISING THE ANTI-, OR RELATIVISM SQUARED

Martin Holbraad

One way to read Barbara Herrnstein Smith's subtle paper is as a robustly fair-minded valedictory on debates and controversies that raged in academia in the past few decades under the banners of "culture wars," "science wars," and other such military-sounding denominations. These battles concerned such notions-by-proxy as "relativism," "positivism," and so on— notions, that is, to which no one would own up these days without wishing to append the kind of subtlety of argument Smith herself displays here but that plenty still are happy to bandy around as terms of academic abuse. To point this out in all of its tragicomic effect, as Smith does, is of course also to take a position within these often sense-numbing position-takings. Smith's lesson, in other words, can be read as that of showing how "taking a position" can be done more intelligently than it often has been in these disputes, by making explicit some of the chimeras from which dismally bellicose position-takings have drawn their strength. Taken together with Smith's past oeuvre on the topic, the charge of "relativism" being chosen for this kind of intelligent treatment would bear out the idea that Smith's intervention stakes out a position within the coordinates of the disputes it so elegantly seeks to defuse. While I have little doubt that Smith would happily recognize that a similar treatment could be proffered to the putatively "opposite" charge of, say, "positivism," there is also no doubt where her heart would lie on such an

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DOI 10.1215/0961754X-2010-032

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all-too-simple axis. Indeed this, to my mind, profoundly fecund tension between intelligence and commitment is reflected also in Smith's language: the paper is as laudable in its conciliatory tone as in taking a deliberate stand against the "distortions" and "improper paraphrase" of the relativism-refuters' language and the unfair thoughts that they have so often expressed. Hers, if you like, is the fair-minded stance of the "anti-anti," recalling Clifford Geertz's famous essay, written with a not-dissimilar purpose.¹

Part of the fun of Geertz's joke, of course, was its own dose of tragedy—namely, that of the included middle: properly speaking, either you are anti- or you are not. What I want to suggest here, however, is that, read in the vicinity of this symposium's concern with that other apparently uncomfortable coupling "comparative relativism," Smith's paper allows us to imagine a way out of this logical tragedy (or, as Smith would say, scandal) and, with it, a different "way of behaving" in academia, to borrow an expression from Isabelle Stengers's paper in this same symposium. Radicalized in a particular direction, I would argue, Smith's position yields to an ethos of argument that makes a merit of, and takes pleasure in, oppositional differentiation at all levels, including the level of first methodological principle at which the tribal deadlocks on which she comments have played themselves out. We may imagine, in other words, a "pluriverse" of thinking (to borrow Stengers's borrowing from William James) in which academic papers would seek not to shield themselves from the anti- but to embrace it. Taking the anti-, that is, as a compliment rather than a rebuke or, better, as a compliment through a particular quality of rebuke. Such a pluriverse might be part of the promise of a properly comparative relativism.

In imagining further installments to Smith's account, I take my cue from two points she makes herself. First, the acrimony of the "wars" of the 1980s and 1990s has abated somewhat in more recent years—a point from which one may infer that her own spot-on responses to what she calls "contemporary sites of antirelativist energy" are offered as a kind of rearguard action. To compound the military hyperbole, Smith's intervention could, as she also indicates, be described as a blast at arguments that are to some degree past. Second, she points out that one of the forms of violence perpetrated by the antirelativism industry is that of distortion by way of oversimplification. There is no single and identifiable thesis called relativism, only a heterogeneous range of arguments and approaches whose main common denominator is the fact that to militant antirelativists they all appear merely as negative instances of what *they* hold dearest. So-called relativism is in this sense an anti-thesis par excellence. We may conclude from these two

1. Clifford Geertz, "Anti Anti-relativism," *American Anthropologist* 86.2 (June 1984): 26–77.

points that there are indeed further installments to be spun out of Smith's story and that these may pertain to the internal differentiation of what from the outside looks like a "relativist bloc."

The matter goes back to the relativism-busting charge of self-refutation: if all claims to truth are relative, goes the familiar thought, then so is that claim itself, so why should we accept it? Smith does not seek to "unload" this charge explicitly in her present paper, though she has done so in an eponymous article published in this journal years ago, and her position can be gleaned from much of what she says here too.² Her response can perhaps be glossed as follows. If so-called relativism is self-contradictory, then the charge is itself a tautology. Only if you have assumed, to start with, that claims to truth must be absolute can you present the relativist claim as a self-contradiction. But the very point of the positions that opponents brand as relativist is to dispute that truth is only worth the name if it is absolute in the way that antirelativists assume. The whole point of projects as diverse, for example, as pragmatism and constructivism has been to formulate accounts of how variable claims to truth may variously be accepted or rejected on grounds that may be nonabsolute (read: contingent, context-dependent, goal-dependent, or, in short, relative). In her "Unloading" article, Smith mentions criteria such as "applicability, coherence [and] connectability" as alternatives to "objective" (read: absolute) standards of truth, adding that these will "depend on matters of perspective, interpretation and judgment, and will vary under different conditions."³

The move certainly takes the sting out of the self-refutation charge—indeed it bites its bullet. There is clearly no contradiction in claiming that all truth-claims are relative if the truth of that claim is itself intended as relative—that is just being consistent. But still, *how* consistent do we want to be here? The question turns on an issue of what logicians (quite helpfully in this case) call "scope ambiguity." When Smith asserts that the truth of the claim that all truth claims are relative is itself relative, is the logical scope of relativity meant to extend just to the truth of the claim or also to its relativity? Put more slowly: we can imagine two variant ways of arguing, and both of them would be successful in defusing the self-refutation charge as it stands. The first would be to stick to the letter of the argument I have ascribed to Smith and say that what is relative about the view that all truth-claims are relative is its truth and assume that, in saying so, we have a good story to tell about what we mean by "relative"—for example, because we

2. Barbara Herrnstein Smith, "Unloading the Self-Refutation Charge," *Common Knowledge* 2.2 (1993): 81–95; reprinted with changes in *Belief and Resistance: Dynamics of Contemporary Intellectual Controversy* (Cambridge, MA: Harvard University Press, 1997).

3. Smith, *Belief and Resistance*, 77–78.

have some independent argument about the nature of relativity, whether philosophical, sociological, cognitive, or what have you. Alternatively, we could put the notion of relativity itself into the mix. On this view, one of the reasons for which the view that all truth-claims are relative is itself relative is because it varies depending on what we take *relative* to mean, and its meaning is just as contingent and provisional as that of *truth*.

Now, when put in this way, it seems obvious that the latter option is the more enticing one, at least if we are playing the “Be consistent!” game. On what grounds, after all, would one exclude the notion of relativity from its own scope? Would doing so not be another instance of self-refutation, albeit one order removed? (That is what the antirealist would want to say, of course, though this is not my main point here.) Nevertheless, I would suggest that anti-antirealist moves have often tended toward the first option, and I do think this tendency is occasionally in evidence in Smith’s argument. For example, she points out that claims to relativity are not typically made as metaphysical assertions about truth, value, and reality “taken as autonomous entities or properties,” but rather they emerge in the course of the “observation of human variety” (the observation, for example, by ethnographers and historiographers). So the suggestion is that appeals to relativity are founded on good and solidly empirical grounds—for instance, the observation that claims to truth are demonstrably correlated to the social conditions of their production and that different accounts of reality and value can be shown up as a function of their genealogy. The notion of relativity is first established, *founded* empirically, and then applied variably (that is, relatively) to sundry claims about truth, value, and reality.

If this is a fair interpretation, then Smith’s account in this respect exemplifies a discernible tendency in anti-antirealist arguments, which, as the very term would suggest, is to oppose the content of antirealist arguments while duplicating their form or (returning to the military idiom) their strategy. Note the symmetry: antirealists muster arguments about things like nature, reality, or common sense to establish a basis for the utter reasonableness of absolute criteria of truth; anti-antirealists muster arguments about things like culture, representation, or imagination to establish a basis for the always wiser insight that such criteria are ultimately relative. And this foundational move, much as in the construction of a building, is meant to render the logically subsequent arguments robust—to make sure they are strong enough to withstand the pressures of challenge and attack. The purpose in both cases, in other words, is to elicit the reader’s approving nods. The difference lies mainly in the degree of modesty with which the two sides pursue this goal of assent: where the antirealist aspires to incontrovertibility, the anti-antirealist admits, albeit incontrovertibly, that this is a chimera. So, in short, while the agendas of the “anti-” and the “anti-anti” are diametrically opposed, their game is pretty much the same.

Now, as mentioned earlier, there is no *prima facie* reason to assume that this symmetry is unsustainable—although to make sure it is not would involve finding a way to reconcile the relativizing content of anti-antirealist arguments with their foundationalist form. Still, the position does suffer from a basic image problem: it is, if you like, half-measured. If the whole point is to show in one way or another that truth-claims are inherently relative, contingent, and provisional, then why, having admitted it, do so-called relativists join in the macho game of nevertheless showing that they are, in one way or another, hard enough to stand up to challenge? Why insist on measuring what you have admitted is fragile and perishable on a scale of relative endurance? Would it not be better to go the whole hog and, as they say, embrace relativity in all of its (these) implications?

This is what I mean by subjecting the concept of relativity to its own scope. Consider the prospect: a concept of relativity that is itself relative. This is no contradiction—quite the contrary: it is what logicians call a “virtuous circle.” For what it implies is that every time you set out to show how the concept of relativity is relative, you in the very act have to change the concept of relativity itself. What you thought counted as relativity when you started off cannot be what you end up with. What you took to be the premise of your inquiry turns up, transformed, as a novel conclusion. Squared in this way, then, relativity becomes a motor for the creative generation of conceptual differences. This kind of radicalized relativism—one that operates upon itself to yield thinking that is in perpetual motion—arguably lies at the heart of notions that have become familiar to us thanks in large measure to the work of contributors to this symposium—Roy Wagner’s “obviation,”⁴ Marilyn Strathern’s “recursivity,”⁵ Stengers’s “humor,”⁶ Eduardo Viveiros de Castro’s “controlled equivocation,”⁷ as well as Smith’s own concern with the dynamics of “incommensurability.”⁸

All I would add, in closing, is that part of what makes this mode of inquiry so radical is that its motion is peculiarly backward or, better, downward—to the root. Which is to say that its orientation is in profound conflict with the habitual intellectual impulse of eliciting assent—the ethos of the pro- and its anti-. If the productivity of rendering relativism comparative to itself is in the incessant potential of undermining one’s own initial assumptions—the risk of having one’s rug pulled from under one’s feet—then the desired outcome must be an iterative

4. Roy Wagner, *The Invention of Culture*, rev. ed. (Chicago: University of Chicago Press, 1981).

5. Marilyn Strathern, *The Gender of the Gift: Problems with Women and Problems with Society in Melanesia* (Cambridge: Cambridge University Press, 1988).

6. Isabelle Stengers, *The Invention of Modern Science*, trans. Daniel Smith (Minneapolis: University of Minnesota Press, 2000).

7. Eduardo Viveiros de Castro, “Perspectival Anthropology and the Method of Controlled Equivocation,” *Tipiti* 2.1 (January 2004): 3–22.

8. Barbara Herrnstein Smith, “Microdynamics of Incommensurability: Philosophy of Science Meets Science Studies,” in *Mathematics, Science, and Postclassical Theory*, ed. Smith and Arkady Plotnitsky (Durham, NC: Duke University Press, 1997).

anti-. Such an anti-, however, in its iteration, would indicate not so much a “No, rubbish!” as a “Yes, but what next?” Deleuze, famously, had his own metaphor for just this kind of activity—one that is too rude to repeat here.⁹ But I guess the message would be that if you like doing a Deleuze—and who doesn’t?—then you must also like having one done to you.

9. See Gilles Deleuze, *Negotiations*, trans. Martin Joughin (New York: Columbia University Press, 1995), 6.

CULTURE

A Site of *Relativist* Energy in the Cognitive Sciences

Andreas Roepstorff

It is easy for me to agree with Barbara Herrnstein Smith's description of relativism as a "fantasy heresy" and to sympathize with her remarks on the consequent difficulty in getting a proper grasp on the issues at hand. I also concur in her identification of two current sources of "antirelativist energy": namely, the claim that there are cognitive universals and the fear that relativism leads to a politically debilitating neutrality. As an anthropologist in the cognitive sciences, I face a situation somewhat different from Smith's in the humanities, which, as she makes clear, are continually beset by accusations of relativism coming from scientists and philosophers. One of the emerging "sites of antirelativist energy" that Smith identifies is located in my own field. I would like, in this context, to explore a case—one with potentially important effects on cultural politics—in which discussion of cognitive universals is at the heart of the debate.

Cultural neuroscience is a latecomer among the budding neurodisciplines. It has been defined as "an emerging research discipline, which investigates cultural variation in psychological, neural, and genomic processes as a means of articulating the bidirectional relationship of these processes and their emergent properties."¹ Since cultural neuroscience attempts to show that both cognitive

1. Joan Y. Chiao, "Cultural Neuroscience: A Once and Future Discipline," *Progress in Brain Research* 178 (2009): 287–304, at 289.

capabilities and brain functioning may vary systematically across cultural differences, the discipline is at the center of discussion of cognitive universals.² But where is the “culture” in cultural neuroscience? One of the key topics of research in the field has branched off from a now-classic paper in cultural psychology by H. R. Markus and Shinobu Kitayama.³ The article, published in 1991, argues for a difference between, on the one hand, Eastern or Asian cultures, where the “emphasis is on attending to others, fitting in, and harmonious interdependence with them,” and, on the other hand, American culture, where “individuals seek to maintain their independence from others by attending to the self and by discovering and expressing their unique inner attributes.”⁴ Markus and Shinobu suggest that these differences in how the self is construed could make a powerful theoretical tool for explaining why key psychological findings can fail to replicate in different cultural contexts. The distinction made between independent and interdependent selves has proven highly successful (as indicated by the 5,965 citations of the paper, according to Google Scholar at the time of writing).

Some key publications in cultural neuroscience have recently tried to translate this distinction from cognitive psychology to neuroscience. A central problem in any cognitive brain imaging experiment is to identify an experimental paradigm that allows for translation between the abstract concepts studied and the very concrete and colorful brain scans. Identification of putative neuronal correlates of the self has been a key challenge for functional brain imaging, and one of the most widely assigned tasks for identifying this has been to have subjects perform an adjective-ascription task while in a brain scanner. Briefly, people read a number of adjectives and are asked to evaluate whether the adjectives are appropriate descriptions of themselves and of some famous person, such as the American president or the Danish queen. The contrast in brain activity between relating (adjectives) to the self and relating (adjectives) to the other has in this line of research been understood as indicative of a neural correlate of the self.⁵ In the search for culturally variant aspects of the neuronal self, Shihui Han and colleagues took this paradigmatic experiment and cleverly added an extra condition: they asked the experimental subject also to relate the adjective to his or her

2. Current attention to this research field is concretely indicated by the recent publication of papers on the topic in the two leading review journals, *Nature Reviews Neuroscience* and *Trends in Cognitive Science*, as well as in an edited volume of a key neuroscience book series. See Shihui Han and Georg Northoff, “Culture-Sensitive Neural Substrates of Human Cognition: A Transcultural Neuroimaging Approach,” *Nature Reviews Neuroscience* 9.8 (August 2008): 646–54; Kai Vogeley and Andreas Roepstorff, “Contextualising Culture and Social Cognition,” *Trends in Cognitive Science* 13.12 (November 2009): 511–16; and Joan Y. Chiao, ed., *Cultural Neuroscience: Cultural Influence on Brain Function* (Amsterdam: Elsevier, 2009).

3. Hazel R. Markus and Shinobu Kitayama, “Culture and the Self: Implications for Cognition, Emotion, and Motivation,” *Psychological Review* 98.2 (April 1991): 224–53.

4. Chiao, *Cultural Neuroscience*, 224.

5. William M. Kelley, C. N. Macrae, C. L. Wyland, Selin Caglar, Souheil Inati, and Todd F. Heatherton, “Finding the Self? An Event-Related fMRI Study,” *Journal of Cognitive Neuroscience* 14.5 (July 2002): 785–94.

mother.⁶ Running this experiment on Chinese and Western students allowed them, by logical extension, to check whether the putative interdependence of the “Asian self,” here translated into how one would relate to one’s mother, has a neural marker. Indeed, the researchers found differences between the two groups in the activation of MPFC, the middle prefrontal cortex—the region found in a number of self-other discrimination studies.⁷

To the authors, these findings suggested that “Chinese individuals use MPFC to represent both the self and the mother whereas Westerners use MPFC to represent exclusively the self, providing neuroimaging evidence that culture shapes the functional anatomy of self-representation.” The authors were thus able to anchor, in apparently observable brain states, the distinction between independent and interdependent selves: “our fMRI results demonstrate,” they concluded, “that, in Chinese subjects, representation of both ‘self’ and ‘mother’ engages MPFC, whereas in Western subjects MPFC is reliably engaged only by self-judgments. The results suggest that Western independent self is mediated by unique neural substrates, whereas East Asian (e.g., Chinese here) independent self depends on overlapping of neural substrates for close self and others.”⁸ A number of follow-up experiments with subjects taken from other groups have since explored these claims across different and more or less dynamic cultural domains.⁹

This symposium is not the proper context in which to review, in any detail, this body of literature, or to discuss potential pitfalls or weaknesses in the approach, but it is obviously not without problems to extrapolate from the studies of a limited number of graduate students to millions or even billions of “Asians” or “Westerners.” For the argument here, it is more important to note that by modifying slightly the commonly used adjective-ascription task and running it in cross-cultural settings, a body of literature is emerging, published in the most influential cognitive-science and brain-imaging journals, which appears to shift cultural differences in self-other relations—a key issue in the discussion of cognitive universals—to the level of brain science. Many authors in this field carefully add disclaimers warning against taking the framework (of independent and interdependent selves) to indicate “that culturally divergent individuals inhabit

6. Ying Zhu, Li Zhang, Jin Fan, and Shihui Han, “Neural Basis of Cultural Influence on Self-Representation,” *Neuroimage* 34.3 (February 2007): 1310–16. See also Li Zhang, Tiangang Zhou, Jian Zhang, Zuxiang Liu, Jin Fan, and Ying Zhu, “In Search of the Chinese Self: An fMRI Study,” *Science in China, Series C: Life Sciences* 49.1 (January 2006): 89–96.

7. David M. Amodio and Chris D. Frith, “Meeting of Minds: The Medial Frontal Cortex and Social Cognition,” *Nature Reviews Neuroscience* 7.4 (April 2006): 268–77.

8. Zhu et al., “Neural Basis of Cultural Influence,” 1315.

9. Joan Y. Chiao, Tokiko Harada, Hidetsugu Komeda, Zhang Li, Yoko Mano, Daisuke Saito, et al., “Dynamic Cultural Influences on Neural Representations of the Self,” *Journal of Cognitive Neuroscience* 22.1 (January 2010): 1–11; Joan Y. Chiao, Tokiko Harada, Hidetsugu Komeda, Zhang Li, Yoko Mano, Daisuke Saito, et al., “Neural Basis of Individualistic and Collectivistic Views of Self,” *Human Brain Mapping* 30.9 (September 2009), 2813–20; Shihui Han, Lihua Mao, Xiaosi Gu, Ying Xhu, Jianqiao Ge, and Yina Ma, “Neural Consequences of Religious Belief on Self-Referential Processing,” *Social Neuroscience* 3.1 (March 2008): 1–15.

incomparably different worlds.”¹⁰ However, from an anthropological perspective, it is very difficult not to see as well the potential for what Ian Hacking calls a looping effect of classifications.¹¹ Brains may become both models of and models for classifications of people, as differences come to seem anchored in brain states. These experimental results may therefore also become part of identity politics in a world where, so far, 90 percent of brain-imaging experiments are being done in Western countries.¹² Accordingly, this version of cognitive relativism, which instrumentalizes a highly abstract idea of “culture” (Western versus Asian, independent versus interdependent), appears to be a matter also of political (as much as of scientific, philosophical, and anthropological) concern.

Yet these novel findings are readily available and are in need of proper contextualization and explanation. One key aspect of that task may be to examine the potentials of and limits to the plasticity of mind and brain. To what extent are we as humans responsive and capable of ongoing modification? Do we react and adapt in characteristic ways to certain traits in the environment and in the behavior of others, and how much does context matter? How well do brain-scanning experiments generalize?

Another equally important task is to examine how the discourse on mind, brain, and cognitive universals feeds back into an understanding of who “we” are. Although the emerging body of literature on cultural selves may seem on first reading an attempt to locate culture in the brain, it was a classification into “cultures with independent and interdependent selves” that prompted the analysis to begin with. Underlying this discussion of the cultures produced by human nature are some very strong assumptions about the nature of human culture.¹³ Ultimately, current discussions of cultural effects on the brain may not “reduce” identity to brain states; rather the notion of identity is being transformed and reconfigured by its relation to the “brain domain.”

Barbara Herrnstein Smith suggests that at the base of the horror of relativism is a horror of mortality, of the idea that all what one, as a scientist, has been working to establish may, in a later time, be proven wrong. This is a great intuition; however, in this particular version of cognitive relativism, the stakes seem not only to be about death but also about understanding key dynamics in the unfolding of human life

In this very brief analysis, I have been much inspired by a variety of literary behaviorism that, I take it, some of Smith’s own work embodies. That approach

10. Markus and Kitayama, “Culture and the Self,” 248.

11. Ian Hacking, “The Looping Effect of Human Kinds,” in *Causal Cognition: A Multidisciplinary Debate*, ed. Dan Sperber, David Premack, and Ann James Premack (Oxford: Oxford University Press, 1995), 354–94; see also Vogeley and Roepstorff, “Contextualizing Culture and Social Cognition,” 511–16.

12. Chiao, “Dynamic Cultural Influences,” 288.

13. Andreas Roepstorff and Nils Buband, “Introduction: The Critique of Culture and the Plurality of Nature,” in *Imagining Nature: Practices of Cosmology and Identity*, ed. Roepstorff, Buband, and Kalevi Kull (Aarhus: Aarhus University Press 2003), 9–30.

permits discussion of a position (or a text) on the basis of what it potentially does, without caring much about the motives and inner lives of the persons who have taken the position (or written the text). I see this approach both in Smith's work on relativism, as presented here, and in her recent discussion of the nexus of religion and the cognitive sciences.¹⁴ The kinds of interchange between science and religion—from identification of neural correlates of religious experiences to the work of the Mind and Life Institute—are very much in need of careful contextualization and explanation.¹⁵ Smith's approach appears to be highly useful for tracking how ideas, practices, and facts unfold—for setting the record straight—and it may be one very unpredictable outcome of a summer spent working with pigeons in a famous Harvard lab many years ago.¹⁶

14. Barbara Herrnstein Smith, *Natural Reflections: Human Cognition at the Nexus of Science and Religion* (New Haven, CT: Yale University Press, 2010).

15. Mario Beauregard and Vincent Paquette, "Neural Correlates of a Mystical Experience in Carmelite Nuns," *Neuroscience Letters* 405.3 (September 2006): 186–90; Uffe Schjoedt, Hans Stødtkilde-Jørgensen, Armin W. Geertz, and Andreas Roepstorff, "Highly Religious Participants Recruit Areas of Social Cognition in Personal Prayer," *Social Cognitive and Affective Neuroscience* 4.2 (June 2009): 199–207. For the Mind and Life Institute, see www.mindandlife.org.

16. Barbara Herrnstein Smith, "Science and Religion: Lives and Rocks," *New York Times*, January 25, 2010, opinionator.blogs.nytimes.com/2010/01/25/science-and-religion-lives-and-rocks/ (accessed June 28, 2010).

CHINESE COMPARISONS AND QUESTIONABLE ACTS

Barbara Herrnstein Smith

Andreas Roepstorff's description of cultural neuroscience, taken together with Geoffrey Lloyd's coincidentally related comment here, reminded me of a visit I made to China in 1983 with a group of American scholars of comparative literature. Since the discipline of comparative literature as such did not exist at the time in Chinese universities, I was repeatedly asked by the Chinese scholars we met to explain the sort of research we did. I usually began by saying something like, "Well, someone who specializes in English and Chinese literature might compare the themes or plots of Elizabethan drama and Peking opera." The response I almost invariably received was, "And which is better?" I was, of course, amused but also puzzled by this. Were these scholars, I wondered, attempting—teasingly or otherwise—to elicit a betrayal of my presumed Western snobbery? Or did their experience of the recently ended Cultural Revolution make them sensitive about the status of traditional Chinese culture? Or perhaps the only Chinese term available to translate *comparative* involved ideas of ranking or preference, so that, for my interlocutors, comparing things just meant seeing or saying which was better.

I never arrived at a satisfactory resolution to my puzzlement, either from my respectfully posed but language- and custom-tangled questions at the time or from subsequent conversations, some of them rather awkward, with the

Chinese-American scholars in our group. In their very inevitability, however, these old perplexities are relevant here. For they illustrate vividly the conceptual and methodological—and, I would add, sometimes ethical—hazards of intercultural comparison to which Lloyd alerts us; and, accordingly, they suggest why the technologically and otherwise inventive studies described by Roepstorff must be approached with some wariness.

Here as elsewhere in his work, Lloyd is especially attentive to the complex play of difference and sameness in cultures and cognition. Indeed, complexity—as distinct from simple contrast or binarism of any kind—is his signature theme as a classicist and comparatist.¹ Thus he notes similarities as well as differences in ancient Chinese and ancient Greek responses to cultural difference and also the significantly different views of these matters among the Greek philosophers themselves. In the same vein, discussing studies of cultural/linguistic variability or counterclaimed universality among humans in color perception, he stresses the complexity of such cognitive activities, including the ongoing interactions among the multiple variables presumably involved. Noting the challenge that such intrinsic complexity and inevitable interactivity present to standard dichotomies of universality and cultural relativity, Lloyd observes that these and other familiar dualisms have been made obsolete by a century of research in genetics, ethnography, psychology, and related empirical disciplines.

Traditional dichotomies and related dualisms can, however, be extremely resilient, even among practitioners of the empirical disciplines themselves. Thus Lloyd's cautions appear especially apt with regard to the contrasts drawn or assumed in some of the studies described by Roepstorff. Among other troubling features of those studies is the casually shifting nomenclature used by researchers to frame questions and conclusions. Can it be proper, one wonders, to move without comment from “Chinese” to “Asians” and from “White [sic] American students” to “Westerners”? And, if the rather obviously different *cultures* of, say, Norwegian fishermen, Spanish flamenco dancers, and the college-age children of middle-class Americans are judged insignificant in regard to the cognitive traits at issue, then one must wonder what explains the exclusion, as it appears, of black Americans from the pool of experimental subjects. Conflations and exclusions of these kinds lead one to suspect that the terms “Asian/s” and “Western/ers” are operating in these studies not (or not only) to describe persons distinguished by the particular *cultures* they inhabit but (or to some extent also) as biological-racial

1. See, e.g., G. E. R. Lloyd, *Demystifying Mentalities* (New York: Cambridge University Press, 1990); *Ancient Worlds, Modern Reflections: Philosophical Perspectives on Greek and Chinese Science and Culture* (New York: Oxford University Press, 2004); *Cognitive Variations: Reflections on the Unity and Diversity of the Human Mind* (New York: Oxford University Press, 2007).

categories. To the extent such suspicions are warranted, claims made by cultural psychologists regarding the putatively contrasting cognitive traits of “Asians” and “Westerners” (as, for example, having “interdependent” versus “independent” views of the self or being “collectivist” versus “individualistic” in regard to other people) appear problematic, both conceptually and methodologically dubious and, in some contexts, at least potentially invidious (“And which is better?”).²

A number of the problems noted here are evident in a foundational cross-cultural study that Roepstorff cites, “Culture and the Self,” by psychologists Hazel Markus and Shinobu Kitayama.³ Its authors’ stated objectives suggest a corrective intention directed at the provincialism of much psychological research, similar to the disciplinary self-disciplining efforts of the historians and anthropologists that I describe in “The Chimera of Relativism.” Thus, commenting on psychologists’ tendency to overgeneralize from findings on particular local populations, Markus and Kitayama write:

[M]ost of what psychologists currently know about human nature is based on one particular view—the so-called Western view of the individual as an independent, self-contained, autonomous entity. . . . As a result of this monocultural approach to the self[,] . . . psychologists’ understanding of those phenomena that are linked in one way or another to the self may be unnecessarily restricted.⁴

Their central aim, however, is to establish the psychological significance of what they assume from the beginning are two specific, sharply contrasted ways in which people “view the self.”⁵

Some basic difficulties of conceptualization in the article—and, thereby, in the tradition of cross-cultural research that it continues to generate, including recent neuroscience studies cited by Roepstorff—can be seen in the authors’ introductory statement:

In this article, we suggest that construals of the self, of others, and of the relationship between the self and others may be even more powerful than previously suggested and that their influence is clearly reflected in differences among cultures.⁶ In particular, we compare an *independent*

2. See Richard E. Nisbett, *The Geography of Thought: How Asians and Westerners Think Differently . . . and Why* (New York: Free Press, 2003), for extended contrasts of these kinds based on such studies.

3. Hazel Markus and Shinobu Kitayama, “Culture and the Self: Implications for Cognition, Emotion, and Motivation,” *Psychological Review* 98.2 (1991): 224–53

4. Markus and Kitayama, “Culture and the Self,” 224.

5. The assumption is based largely on earlier studies by H. C. Triandis and others that are comparably problema-

tic. For discussion of the lineage and some of the problems, see Douglas L. Medin, Sara J. Unsworth, and Lawrence Hirschfeld, “Culture, Categorization, and Reasoning,” in Shinobu Kitayama and Dov Cohen, *Handbook of Cultural Psychology* (New York: Guilford, 2007), 615–44.

6. The direction of the causality or influence claimed by cultural psychologists—whether from culture to cognition or vice versa—varies from one study to another or is essentially ambiguous. In this passage, a cognitive trait (“construals of the self”) is said to be “reflected in” cultural differences. But that is the reverse of the claim made in

view of the self with one other, very different view, an *interdependent* view. The independent view is most clearly exemplified in some sizable segment of American culture, as well as in many Western European cultures. The interdependent view is exemplified in Japanese culture as well as in other Asian cultures. But it is also characteristic of African cultures, Latin-American cultures, and many southern European cultures.⁷

As the latter part of this statement makes clear, Markus and Kitayama seek to be scrupulous in indicating the specificity of the groups whose presumptively sharply different “views of the self” concern them. Indeed, additional caveats and further qualifications are added immediately and pile up over the course of the lengthy article: “The distinctions that we make . . . must be regarded [only] as general tendencies . . . The prototypical American view of the self . . . may prove to be most characteristic of White, middle-class men with a Western European ethnic background.”⁸ A footnote here adds: “The prototypical American view may also be further restricted to a particular point in history. It may be primarily a product of late, industrial capitalism.” Indeed it may be, but the authors do not consider the sizeable implications of that possibility for their research. A cascade of further qualifications appears at the conclusion of the article:

[T]here may well be important distinctions among those views [of the self] we discuss as similar and . . . there may be views of the self and others that cannot easily be classified as either independent or interdependent. Another thorny issue centers on the assessment of cultural differences. . . . Another persistent issue is that of translation and equating stimuli and questionnaires.⁹

And so forth: the final paragraph continues in this way for several more sentences. The acknowledgment of such problems is, of course, admirable as such. But, as the authors’ caveats cut more deeply into their claims and as the issues they identify become thornier and more fundamental, it becomes increasingly difficult to say exactly what their studies reveal about “Asian” versus “Western” “views of the self” and what, if anything, they demonstrate about the influence of culture on cognition (or vice versa).

other studies cited by Roepstorff that cognitive traits (and correlated brain activities) differ as influenced by—or as a *reflection of*—cultural difference (see, e.g., J. Y. Chiao, ed., *Cultural Neuroscience: Cultural Influence on Brain Function* [Amsterdam: Elsevier, 2009]). Only claims of the latter kind, of course, would make the new field of cultural neuroscience a site of “cultural relativism” as the term is commonly understood.

7. Markus and Kitayama, “Culture and the Self,” 224.

8. Markus and Kitayama, “Culture and the Self,” 225.

9. Markus and Kitayama, “Culture and the Self,” 247.

Evidently recognizing the possibility of such a skeptical reaction, Markus and Kitayama make a final point of interest here. “A failure to replicate certain findings in different cultural contexts,” they write, “should not lead to immediate despair over the lack of generality of various psychological principles *or to the conclusion of some anthropologists that culturally divergent individuals inhabit incomparably different worlds.*”¹⁰ The otherwise gratuitous-seeming disavowal of this latter rather extravagant idea, a disavowal repeated, as Roepstorff notes, in other studies he cites, appears to have become something of a ritual in cross-cultural research. Allusion to and rejection of just that idea also appears, we recall, in a passage I discuss by Scott Atran, who explicitly identifies the rejected idea with “cultural relativism.” In each case, rejection of the vaguely attributed idea that culturally divergent humans “inhabit incomparably different worlds” is attached to a strong affirmation of the existence of general psychological principles and/or universals of human nature. And, in each case, that affirmation is offered in the face of, and in order to *discount*, evidence of significant cultural variability in a cognitive trait said to be crucial in human behavior.

Roepstorff is eager to represent cultural cognitive neuroscience as a site of contemporary cultural relativist energy. Perhaps it will, in time, become such. At the moment, however, a good bit of research in the field seems to be otherwise motivated and directed.

After a professional lifetime of being hooted for “extreme,” “radical,” “all-the-way-down” relativism, it is bracing to be charged with not being relativistic enough, especially by so artful a challenger as Martin Holbraad. But the hooters had it right all along, at least under definitions of relativism that I have taken care to spell out. Most of the supposedly radicalizing moves that Holbraad urges have been evident in my work from the beginning,¹¹ and the other moves he urges either are not especially radical from my perspective or would be at odds otherwise with my tastes or purposes. No one engaged by literary and linguistic theory over the past half century needs to be told that the meaning of the term *relative* is itself relative or that the scope of an assertion of relativity can include itself. And, while reveling in semantic and conceptual proliferation is certainly an available activity, I would not myself forego other intellectual pursuits to indulge in it overmuch. Similarly, while I would, like Holbraad, stress the intellectual *productivity* of intellectual controversy (the point is central to *Belief and Resistance*), I have been no less interested in exploring the rhetorical, psychological, and social-

10. Markus and Kitayama, “Culture and the Self,” 247, emphasis added.

11. See, e.g., Barbara Herrnstein Smith, “Contingencies of Value,” *Critical Inquiry* 10.1 (1983): 1–35: “All value is radically contingent.”

institutional operations and effects of such controversies.¹² Moreover, while I have certainly partaken of what he celebrates as the pleasures of “oppositional differentiation” (though he interprets it, in my case, as “macho” combativeness), I have also pursued, and found happiness in, more irenic ventures.

Holbraad is not obliged to be familiar with the complete works of B. H. Smith, but, had he looked more carefully at the texts he did consult, he would not have needed to guess at so many of my presumptive “positions” and “arguments.” As it is, his guesses are generally quite wide of the mark. For example, I do not claim, as he supposes, “that appeals to relativity are founded on good and solidly empirical grounds.” I do observe that the relativistic views of anthropologists and historians commonly arise from their experiences in the field or in the archives. But to remark a likely source is not to claim an ultimate grounding, and it takes quite a bit of inventive glossing to extract a beefy empiricist foundationalism of that kind from my stated views.

In the passage from *Negotiations* that Holbraad evokes at the end of his comment, Gilles Deleuze writes of his youthful impatience with the history of philosophy (he mentions Kant and Hegel) and of seeing his own early philosophical efforts as “taking an author” rudely (to use Holbraad’s term) “and giving him a child that would be his own offspring, yet monstrous.” “It was really important,” Deleuze adds, “for it to be his own child, because the author had to actually say all I had been saying.” Holbraad, evidently modeling his efforts here on Deleuze’s creative overcoming of venerable philosophers, seeks accordingly to give my arguments a more fruitful turn. The attempt picks up steam with his apparent demonstration of my alleged mirror duplication of the form of the familiar charge of self-refutation (“When Smith asserts that the truth of the claim that all truth claims are relative is itself relative . . . ,” and so forth) and moves from there to his would-be *überrelativist* endgame. Less diligent than Deleuze, however, in ascertaining that the authors thus “taken” had “actually sa[id]” what was attributed them, Holbraad generates this assertion by Smith out of a crucially improper paraphrase of the text he cites plus a good bit of thin air. Thus himself duplicating the definitive ploy of the antirelativists of yore, Holbraad delivers here a litter of baby chimeras.

12. See, e.g., Smith, *Belief and Resistance: Dynamics of Intellectual Controversy* (Cambridge, MA: Harvard University Press, 1997), 122–23: “It is out of the endless dance and clash of skepticism and belief that all knowledge emerges.”

COMPARISON AS A MATTER OF CONCERN

Isabelle Stengers

We are all comparativists, and so probably are all animals. Comparison is an important part of any animal's equipment for dealing with its respective world. This symposium, however, is not about comparison as a matter of fact (I am using Bruno Latour's terminology here) but as a matter of concern. But comparison may be a matter for many kinds of concern. The concern that gathers around those of us who are haunted by the polemical opposition of relativism and universalism demands to be situated in order to avoid the trap of generality. More precisely, it may demand that those it gathers around speak about the situation that made it a matter of effective concern.

Do we impose comparison or are we authorized to compare by the subjects we address? I will not attempt to deal with this question as if from the outside—as if it were an epistemological or critical problem. To take it as such would be to enter, from the very beginning, into a polemical confrontation with those practices for which this question is already a matter of crucial concern: practices that in one way or another present themselves as inheritors of the Greek claim that to understand is to identify a *logos*. Both *logos* and the Latin *ratio* are an etymological source for terms such as *reason* and *account* but also *proportion*, which signifies an operation of comparison. The French word *rapport* has inherited this constellation of meanings, while its usual translation, “relation,” has lost it. Every-

thing may be described as related, but not everything entertains “rapports.” My text will connect comparison with the creation/discovery of what I will describe as “rapport.” The disturbing effects due to the idiomatic senses of the term in English are quite welcome since they will slow down readers at a very relevant point, avoiding the too easy connection between relation and relativism.

We know the mathematical origin of the classic *logos*, linking understanding and a rapport that authorizes comparison. To compare magnitudes such as weights or lengths is unproblematic, because comparison is the very point of defining measurements and inventing such measuring devices as scales for weight or the yardstick for length. But where to go from this unproblematic point? The quarrel may begin here and, if so, it will start with the claims associated with experimental sciences. The critical temptation could be to identify these sciences with an extension of the art of measurement. We would then directly arrive at the idea that they embody the methodological decision to identify reality and measurability—that is, at the “relativist” thesis that sciences discover only what they have first presupposed and then unilaterally imposed.

I intend to resist this move, not in order to defend these sciences but to dramatize cases that actually concern me, when comparison entails ethical and political challenges. Often the practitioners confronting such challenges will appeal to the experimental, “objective” sciences as a justification. They will claim that if science is to be possible it must obey and extend their example. It is this alignment, making experimental sciences a model to be approached by other sciences, that I wish to call into question. In order to do so, I first want to insist on the singularity of the achievements proper to experimental sciences, characterizing them as the production of situations that authorize them to claim that the subject matters that they address *lend themselves* to quantitative comparison.

This approach, by the way, marks me as a Whiteheadian. In *The Concept of Nature*, Whitehead remarked, in opposition to skeptical theories of knowledge, that such theories attack not just the claims of science but also “our immediate instinctive attitude towards perceptual knowledge.” “We are instinctively willing to believe,” he wrote, “that by due attention, more can be found in nature than that which is observed at first sight. But we will not be content with less.”

I refuse to be content with any “relativist” claim about experimental scientists only “believing” that they discover in nature more than is observable at first sight. It may well be that those scientists’ attention functions like a sieve or filter, but it does not follow that what they retain is only what they have already, unilaterally, defined as significant. The question is rather: “To what,” in Whitehead’s words, “do they pay due attention?” And the answer would be: to the distinction between measurements as usual, acting like a unilateral sieve, retaining only what can be measured, and measurements as related to the *creation* of a rapport or *logos*. A creation of this kind has the character of an event rather than of a

methodological enterprise. It may be characterized in terms of “relevance” as a matter of crucial concern. But to speak about the discovery that phenomena may “lend themselves to” measurement is to insist on the rather particular meaning of relevance when an experimental achievement is concerned.

Such an achievement certainly needed more general conditions. Pierre Duhem can help us here. Duhem emphasized that Aristotle’s understanding of nature in terms of qualitative opposites (warm and cold, for instance) precluded understanding the relevance of quantitative assessment. For Duhem, the conceptual event that made the modern sciences possible happened in the fourteenth century, when thinkers first defined qualities not in opposition to each other, but in terms of increasing or decreasing degrees of intensity. This new conceptual definition was necessary before Galileo could characterize the motion of heavy bodies in terms of increasing and decreasing degrees of velocity.

However, what Duhem pointed out was a necessary, not a sufficient, condition for modern science. For medieval thinkers, any quality could be redefined in terms of varying degrees; for instance, varying degrees of charity and sin during the course of a human life. If Galileo was able to compare the varying degrees of velocity of a body at determinate moments of its fall, it is because the inclined plane enabled him to do so. The inclined plane is the first experimental device, the first device the achievement of which is to create a very unusual kind of rapport: a rapport that authorizes claiming that what is measured lends itself to the measurement.¹ But such a rapport is never a general one. The inclined plane is obviously not relevant to all qualities, nor is it relevant to space-time motion in general. It identifies a quite specific kind of motion, that of what we may call Galilean bodies, which have no internal source of motion (as opposed to a car or a horse) and move in an ideally frictionless manner (as opposed to an avalanche).

Experimental sciences are not objective because they would rely on measurement alone. In their case, objectivity is not the name for a method but for an achievement, for the creation of a rapport authorizing the definition of an object. Each such creation is an event, the production of a new way to measure that the rapport itself specifies. The measurements created act as a sieve or filter, but what matters is the singularity of what is retained by the filter. When experimenters do find more in nature than is initially obvious, the due attention they pay encompasses the specificity of the rapport. “More” also means that what may appear to be secondary differences can come to matter a good deal. In the case of Galilean motion, for instance, the question of friction matters as the rapport refers to an ideally frictionless motion. It matters to such an extent that it occasioned the distinction between sets of professional concerns—those of the physicists, from

1. See Isabelle Stengers, *The Invention of Modern Science*, trans. Daniel W. Smith (Minneapolis: University of Minnesota Press, 2000).

Galileo to Hawking, who are still working with tools derived from “rational” mechanics, and those of the engineers, who deal with friction as constitutive of their subject matter—no machine works without friction.

Measurement in chemistry is another example. Here we deal with bodies that are not in any way Galilean. Since they cannot be submitted to the common reference of some uniform quality, like inertial motion, they do not respond to questions about the increasing or decreasing degrees of given qualities. Paying due attention in experimental chemistry requires addressing bodies as agents; that is, as capable of entering into correlated transformations with which the chemist must learn to play along. There is no equivalent of an inclined plane in chemistry.

I will limit myself here to eighteenth-century chemistry and, more specifically, to the labor of those eighteenth-century chemists who composed increasingly exhaustive tables of affinities or rapports. Such tables were organized by columns, headed by an element, followed by all the elements liable to combine with it, in the order determined by their mutual displacements. The starting point of this enterprise had been Newton’s reasoning in the *Opticks*: “a solution of iron in aqua fortis dissolves the cadmium which is put into it, and abandons the iron,” which means that “the acid particles of the aqua fortis are more strongly attracted by cadmium than by iron.” Two chemical elements were thus compared in their “rapport” to a third one with which both could be associated; the one with the stronger affinity for the third would displace the other from such an association. The tables published by Torbern Bergman between 1775 and 1783 would bring together the results of thousands of chemical reactions organized in twin tables of forty-nine columns each, one for reactions in solution, one for dry reactions.

The affinity tables are now outmoded, but chemistry is still full of tables characterizing chemical agents. Like Galileo’s measurements of velocity, such tables enact a non-Aristotelian definition. Chemistry is not a science of transformations; it deals with combinations of elements that conserve some identity while associating and dissociating. But while the tables follow from the idea of combination, they also conceive the combination event as effecting a comparison between two elements in relation to an unchanged third—that is, they endow elements with an agency that is both specific and relational.

The point I wish to emphasize with the two rather different cases I have touched on is the matter of concern that characterizes experimental scientists ever since: they should be able to claim that they benefit from states of affairs that they did not impose, that pertained to the phenomena studied, and that therefore could be turned successfully into tools for making comparisons. It is important to underscore that such claims, upon which the realism particular to the experimental sciences depends, are relative to the creation of a “rapport,” such as the ones made possible by the inclined plane or by the use of chemical reactions to

order affinities. The very specificity of this rapport is to authorize the “objective” definition of a state of affairs. As such, the event of its creation may be forgotten: the experimental practice then appears to follow from the objective definition. Underlining the event—the creation of a rapport—is however important when the abstract universal/relative dilemma is concerned. Experimental objectivity is relative to a very unusual concern. Neither the concern nor the exacting demands the experimental rapport must satisfy because of this concern are “universal.” As a consequence, resisting the equation identifying experimental sciences with universality does not require our “deconstructing” realist claims. It is sufficient to know that any general extension of an experimental objective definition also means the loss of the legitimacy of such claims. “Experimental reality” only extends through the demanding and always situated exploration of the consequences of a new “rapport,” which Imre Lakatos named a “research program” and Thomas Kuhn a “paradigm.”

Kuhn’s famous claim of incommensurability between rival paradigms has been widely thought to reevaluate the history of science, long understood to be a tale of reason and progress, as a succession of socially constructed frames of interpretation. As a result, for many critical thinkers, Kuhn is the first social constructionist, having shown that scientists impose a socially transmitted interpretive frame on what they study and have thus no special access to reality. This use of Kuhn, however, and the identification of his position as antirealist and relativist are questionable. Why, if Kuhn was describing the history of science as contingent, dependent upon socially accepted frames of interpretation, did so many physicists in his time agree with his description of paradigmatic revolutions? Why moreover was Kuhn himself so unhappy with the antirealist interpretation of his book? I would argue that the physicists who agreed with Kuhn were right. They understood an important feature of his account that social constructionists neglected, which is (yet again) the specific matter of concern shared by scientists engaging in the evaluation and comparison of rival paradigms.

Kuhn’s claim about incommensurability means that there is no way that rival paradigms can agree on a single test that would reliably decide in favor of one against the other. But his claim does not mean that the reasons for deciding cannot be created. Kuhn indeed characterizes the period following the proposition of a new paradigm as dominated by critical discrimination, by the exploration of the diverging consequences of both paradigms. So the scientists involved work to develop whatever may enable them to compare and evaluate research paradigms. In other words, they work to produce and activate reasons for a decision the consequences of which matter for them.

Typically, they will imagine new experimental situations, for which the new paradigm B promises the possibility of new kinds of reliable experimental results, while paradigm E tells nothing about them in particular. In case of success, para-

digm E will probably be able to produce, in one way or another, an interpretation of the new result. This is both the meaning of the incommensurability argument and the reason for Max Planck's famous remark that some scientists, insisting they have not been objectively defeated, will stand by their interpretation until death. Indeed; but it does not mean at all that the final decision will be irrational, devoid of "good" reasons. What defeats a paradigm is never an objective, disinterested comparison, but rather an active and interested one made by a collective that shares the same matter of concern and that privileges whatever can be associated with emergent questions and experimentally challenging consequences. As Lakatos argued (without recognizing that it was also Kuhn's point), defeat comes when a paradigmatic research program is forced to produce increasingly complicated and defensive interpretations, and this is precisely what protagonists pay due attention to.

I would conclude that if Kuhnian revolutions are not about arbitrary decision making, and still less about crowd psychology, it is because Kuhn wove them into a tale of competent and passionate hesitation in a matter of intense concern—which is to say, a matter on which researchers bet their careers and reputations, as well as the future of their fields. In other words, Kuhn described a situation in which comparison is made possible only by a common concern and by the concerned protagonists lending themselves and their work to comparison. Or, more precisely, the antagonists in such competitions are regarded by colleagues as required to lend themselves and their work to comparison and will be deemed defeated if they do not do so. Everyone involved knows that any attempt to play dirty or evade facing an objection will be considered an infraction of the rules of the game. Interpreting the position of a colleague as contingent upon philosophical or social factors—which is what critical constructionists freely do—is a possible but very dangerous move in this game. It means betting that the case is virtually closed and that any one still objecting is virtually defeated in the eyes of his or her colleagues. If well placed, such a move will herald the end of a controversy; but if not, it will endanger the position of whoever risks making it.

The famous tale of the three blind men and the elephant, one man recognizing a trunk, the second a snake, and the third a fly swatter, has sometimes been used to illustrate the workings of Kuhnian incommensurability. But this example misses the point about collective concern that, so I believe, must be associated with Kuhn's description. The blind men all investigate the elephant, but the diverging ways in which they characterize it appear as an end point. The divergence is not a matter of crucial concern to them. If it had been such, the story would not end when the blind men make their first contradictory assessments; they would next move around the elephant to explore the possibility of a coherent account that could turn outright contradictions into very interesting contrasted standpoints. In other words, the blind men would have lent themselves

and their respective interpretations to active comparison, giving that which they all address the power to impose “due attention.”

Let me be clear about the standpoint from which I am making these observations. I am dealing with paradigmatic sciences as seen from Kuhn’s perspective, a perspective that ratifies the closure of scientific communities and ignores what those who operate within a research paradigm agree to think of as “outside.” I would also underline that my point here is not Popperian. I would not characterize such scientists as having a special capacity or training enabling them to make comparative judgments about their colleagues’ research, while, in the context of other practices, conflict would stubbornly prevail. What distinguishes paradigmatic sciences is the possibility of a collective game to bind colleagues. A paradigm is not a doctrine held in common. A paradigm follows from a special and exclusive kind of event or achievement: the production of facts the interpretation of which can resist the charge that they have been imposed on some mute reality. What binds practitioners is the continuation and reproduction of such very particular events when it can be claimed the interpretation is authorized by the way “reality” lends itself to experimental measurement.

I should add that what binds practitioners binds them just as long as it binds them (and no longer). Indeed the knowledge economy, so called, is in the process of destroying such bonds. Scientists, as they are directly mobilized by competing industrial interests, will no longer be mobilized by the duty to have their facts resisting their colleagues’ objections and compelling their colleagues’ agreement. Industrial interests do not need experimental reliability; they need claims that seem good enough for patenting, demonstrating promise, and stimulating the appetite of investors. Moreover, scientists under such conditions are bound to keep aspects of their work secret or to ignore questions the answer to which (given already existent patents) would be of no commercial interest. The collective game I have been describing, in which colleagues are welcome to object because reliability has no other meaning than resisting such objections, will probably soon be a thing of the past; and the general wisdom will prevail that one should not object much if the weakness of a scientific argument might lead to weakening the promise of a field. (You do not saw off the branch on which you and everyone else are sitting.) The parable of the three blind men would at this juncture become only too relevant.

Kuhn’s “incommensurability argument” has been invoked by most of its advocates to resist the use and (mainly) abuse of the “scientific rationality” argument—and it may be said that, however important the bathwater, a crucial baby was thrown away; that is, the kind of event that has had scientists dancing in their labs and the achievement of which was the reason why they lent themselves to objections and comparisons. These advocates had no power other than to produce scientists’ outrage and blind rhetorical retaliation. With the advent of the

knowledge economy, the antirealist critiques may well be fully verified. Dancing in the lab is becoming a thing of the past. But critics have no cause for rejoicing since this verification will not make claims for objectivity, scientific rationality, and authority any weaker, only more arrogant and dangerous, having lost any connection with the kind of achievement that relates objections and reliability.

If experimental sciences are of interest in the much more general context of this symposium, it is in order to approach some aspects of the ethical and political challenges associated with comparison. I will effect the transition starting with the analogy that Kuhn offered between incommensurability and the practice of translation. Diverging paradigms means a breakdown in communication between protagonists, and there is no neutral language in which to restore it. But such a breakdown does not make it impossible for the protagonists in this crisis to learn how to translate each other's words. It is even the prerequisite to their designing experimental situations to challenge one another and put one another on the defensive. Kuhn's analogy is not about comparability in general; it concerns an art of comparison, the aim of which is as clear as the aim of the hunter who understands the behavior of his prey, in spite of the incommensurability of their experiences. Commensurability is created and it is never neutral, always relative to an aim. The ethical and political challenge begins with the aim. Does it, or does it not, require an agreement between the terms that are being compared?

Ethnologists tell us about hunting practices where the understanding of the hunter involves the agreement of the prey. The point that matters here is that these practices are effectively different, and the very fact of naming them with the same word we use for some of our own hunting practices is a very dubious and probably unilateral comparative operation. It is as dubious a comparison as the one assimilating scientists hesitating between rival paradigms to a mob hesitating between two rival leaders (choice as a matter of "mob psychology").

Agreeing is accepting a "rapport." And when the need for agreement is underscored, the creation of the rapport is no longer primarily a matter of legitimating knowledge or of deciding what may be compared with what. Comparison is now more like a contest—one with importance for both sides, one that requires that both sides accept as relevant the terms of the contest.

Charles Péguy's beautiful text *Note conjointe sur M. Descartes et la philosophie cartésienne* is a good place to begin outlining my approach.² In this text, Péguy defines *Polyeucte*, a tragedy of Corneille's, as a case of "perfect comparison." The comparison is between the Christian martyr Polyeucte and Severus, a Roman knight whose character embodies all the virtues of the pagan world. Péguy underscores that the contest between the two men is a fair one. Each appears, and is

2. Charles Péguy, *Œuvres en prose complètes*, vol. 3 (Paris: Gallimard, 1992), 1278–1477, at 1367–75.

concerned to appear, in his particular full force. They share the idea that who will win does not depend on them. What depends on them is that the one who wins will not have won by having managed to weaken the other. Any foul play would abase their respective causes. In other words, they actively lend themselves to the comparison—which is why, according to Péguy, *Polyeucte* had to display the full greatness of the pagan civilization that Christianity would eventually destroy.

Péguy's proposition is very demanding and, as such, very interesting. His implication is that no comparison is legitimate if the parties compared cannot each present his own version of what the comparison is about; and each must be able to resist the imposition of irrelevant criteria. In other words, comparison must not be unilateral and, especially, must not be conducted in the language of just one of the parties. To take a prime example, the contrast between mythological and rational or scientific discourse is of Greek origin and belongs to what is now called the Euro-American traditions of thought. In a comparative inquiry involving East and West, or North and South, words such as *myth* and *science*, if we follow Péguy, should be ruled radically out of order. As the word *nature* should be. What do we mean by *nature*? A term that covers neutrinos, pigs, and tornadoes has nothing obvious about it. As Geoffrey Lloyd has argued, the existence of the category *nature*, which did not exist in ancient China, cannot be dissociated from the polemical maneuver that, in Greece, counterposed the new proponents of "rationality" (as they themselves called it) both against the tales and gossip related by travelers about wonderful distant lands and against the many deities that inhabited Greek rivers, mountains, and caves. Nature as consistent and intelligible has nothing neutral about it. It appears as the ally of the polemicist, as what lends itself to rational inquiry and rewards it. Following Péguy, I thus would characterize any comparison involving or implying the nature/culture opposition as foul play—definable here as a play in relation to which the Euro-American protagonist is both a participant and the arbiter. I should perhaps mention in this context that the deliberately oxymoronic term *multinature* (as employed by Eduardo Viveiros de Castro, Bruno Latour, and others) may be ill protected from foul play of this kind. The term's use could well lead to undue extensions of their own concern about the ruling disconnection between "nature" and "culture," taking the others as witnesses to a question that perhaps does not interest them.

Finally, the abusive consequences of our routine opposition between so-called natural and supernatural causations should be reason enough not to retain the Greek polemical apparatus or its Christian continuation. The crucial question of the existence of "supernatural beings" is a heritage of the Christian missionary past, though it was raised later against the Christian God itself, along with a demand that whatever exists demonstrate its existence against "the critique." This demand, when pressed, leads straight to what Péguy denounces as unfair comparisons. The mode of existence of experimental beings, such as elec-

trons and neutrinos—which indeed exist only because they have satisfied such a demand—is taken as the yardstick (when it is not itself the target of critique). A being of faith, the Blessed Virgin for example, is precluded from revealing her own particular force. Typically she is relegated to a category such as the famous “efficacy of the symbolic”: a very wide and reassuring category that includes all that is efficacious but does not exist. Symbolic entities do not, that is, have the power to trouble our distinction between what exists and what does not.

Worse yet arrives when, at Lourdes and other miracle sites, the church hierarchy awaits, before confirming a miracle, the verdict of physicians empowered to decide if a healing can be explained away in terms of hypothetical “natural causes.” Thus the only (and very poor) definition of a “supernatural intervention” is the impossibility of explaining away an event in terms of natural causes, where *natural* means, foremost, exclusion of the *supernatural*. The brilliance of this feat indicates that it is polemics, rather than relevance, that shapes the whole scene.

My point here is not about universalism versus relativism so much as about what really concerns me: the poverty of our ruling definitions—and the point is widely applicable. Another example is found in sociobiology, with its definition of society in terms of altruistic self-sacrifice. Once again, this rather strange definition has emerged from a polemical scene, requiring that two causes be compared in terms favorable to one of them. Sociobiology casts individual selection and group selection as rival causes whose relative power must be compared. To do so demands a clear-cut separation between the rivals: social behavior must be defined as anything that cannot be explained in terms of individual interest (this definition is the real scandal of sociobiology). Likewise in the case of clinical testing, when the efficacy of a drug is compared with that of a placebo. This comparison is a legitimate one only if it concerns a drug that claims to work no matter what the patient feels, thinks, or understands. To impose this criterion on all drugs using healing practices frames a significant issue in (yet again) polemical terms, since it is not analytically the case that the healing arts must disqualify the imagination as a therapeutically effective force. That modern medicine does so, that it ignores the healing rapport, is one result of a polemically informed choice to draw and enforce an artificial distinction between the charlatan (with his or her ointments, mixtures, or herbs) and the certified practitioner of medical science.³

The examples I have offered are of unilateral, one-sided definitions that impose on others categories that do not concern them—categories the apparent objectivity of which only recalls that the general idea of objectivity itself can never be dissociated from an overpowering determination to silence or eradicate storytellers, teachers of popular creeds and customs, and other inadequately cre-

3. See Isabelle Stengers, “Le Médecin et le charlatan,” in Tobie Nathan and Stengers, *Médecins et Sorciers* (Paris: Le Plessis-Robinson-Synthélabo, 1995).

denialed claimants to knowledge. Ethnologists may well discover the destructive character of “Euro-American” categories when applied elsewhere. I would underscore that in Europe, where they were born, these categories have always been used to dismember, eradicate, or appropriate. I would thus avoid saying, as a European, that they are “ours,” rather that they “happened” to us first.

The problem is not with comparison and the operation making commensurability possible. The problem is not that some such operations get forgotten, the matter of concern being turned into a matter of fact. The problem begins with the imperative “comparison *must* be possible.” This imperative may be justified in terms of objective knowledge to be mastered, or in the name of rational governance, or because progress means to crush illusion. The imperative always means the imposition of a standard that presupposes and enacts silence, the impossibility of objecting or of demanding due attention. This silencing power affects both terms it enrolls. For instance, science as it has been enrolled in order to determine what must be recognized as “really existing” has produced sad inquisitors, bad metaphysicians, or fearless explorers of the “beliefs of others.”

The ethical and political challenge associated with comparativism makes itself felt here. I selected my examples so as to recall that this challenge concerns us in the modern West as much as it does our distant others. And it now urgently concerns “us” academics—that is, the contributors and readers of such texts as mine. Technoscience is in the process of redefining our own worlds in terms that make them available for its comparative operations. The relative passivity of the academic world in facing the ranking systems and “objective” productivity comparisons that are reshaping academic life radically is sufficient to demonstrate how simple it is, even for people who are not naive or easily impressed or overpowered, to submit to questions that are not only irrelevant but that indeed sound the death knell for all that matters most to them.

As academics, we belong now to worlds on the brink of defeat, as so many worlds have been before ours. Péguy’s characterization of the contest between paganism and Christianity is (as all of his work is) the cry of a defeated man. The experience of defeat, that of Christianity, however, gave him the vigor to fabulate and create. I would like our own imminent defeat to give me the strength to do so as well. I want our fate to sound like destruction, not like the logical outcome of a process that would finally demonstrate what we really served beyond our illusions. Daring to speculate will not likely save us, but it may provide words that disentangle us from this process and that affirm our closeness with those who have already been destroyed in the name of rationality, objectivity, and the great divide between nature and culture. Affirming closeness, in this sense, is not the same as affirming similarity. It is not a question of comparison. What relates modern practices and the many practices and forms of life that were already destroyed is that all are equally subject to eradication.

Eradication may, as is the case with the knowledge economy, preserve the appearances of continuity, appear as a mere “adaptation” to new conditions. We will still have specialists busying themselves in their laboratories. What will have been destroyed, however, is what I call *divergence*. Hence I would take the term *practice* in a rather unusual sense, as denoting any form of life that is bound to be destroyed by the imperative of comparison and the imposition of a standard ensuring equivalency, because what makes each one exist is also what makes it diverge.

It is crucial here not to read “diverge from others,” as doing so would turn divergence into fuel for comparison. Divergence is not between practices; it is not relational. It is constitutive. A practice does not define itself in terms of its divergence from others. Each does have its own positive and distinct way of paying due attention; that is, of having things and situations matter. Each produces its own line of divergence, as it likewise produces itself. Experimental sciences are practices, because what matters for experimenters, the creation of a rapport that authorizes an “objective” definition, is an event. And it is an event that cannot be separated from the community for which it crucially matters and which is to test it and to imagine and verify its consequence. To describe this divergence as a divergence *from* other practices is a trap. Such a trap has been laid by scientific propaganda since Galileo: “we diverge because objectivity is what matters for us.” As if what is called objectivity in this case were not a name for the event the possibility of which is what makes the experimental practice exist! What if pilgrims going to a miracle site were to affirm: “we diverge from science because the transformative force of the Blessed Virgin’s gaze upon us matters”?

Some years ago, I introduced the idea of an “ecology of practices” to emphasize both the divergence and the possibility of destruction that characterize what I have called *practice*.⁴ I use *ecology*, as a transversal category, to help define relational heterogeneity—by which I mean situations that relate heterogeneous protagonists. Situations in natural ecology induce naturalists to define their subject matter not in general terms, but rather in the quite specific terms of how the *ethos*—that is, the needs, behaviors, habits, and crucial concerns—of each protagonist diverges positively (and not from the others). Using the term *ecology* is meant to indicate as well that practices should be characterized in terms that do not dissociate the *ethos* of a practice from its *oikos*—the way it defines its environment (including other enviring practices).

The simplest situation for ecologists is defined in terms of predator/prey relations. It may be said that a predator/prey ecology obtains wherever the criteria

4. See Isabelle Stengers, “Introductory Notes on an Ecology of Practices,” *Cultural Studies Review* 11.1 (2005): 183–96.

associated with terms like *objectivity* and *rationality* are universally applied, since practices that maintain stronger definitions of objectivity will freely define others as potential prey; and all sciences will define as prey whatever is not scientific. The gain in clarity of the usage *predator/prey* over the more usual term for the same set of phenomena—*naturalism*—is palpable.

Another interest of the recourse to ecology is that it has no point of contact with the ideal of harmony, peace, and goodwill (in which all parties are asked to bow down to some general interest). The idea of ecology is incompatible, moreover, with neutrality: in an ecological situation, there is no neutral position from which an arbiter could assess rights and duties, nor is there any central and highest position from which a ruler could assign to each protagonist its part in a harmonious whole. Whatever the pretensions of rationality or (good) governance, the comparative operations they authorize are describable as an ecological catastrophe. It is not people who will die, obviously; and it is always possible to speak of practices as flexibly transforming themselves. What will have been eradicated, though, are all the diverging, practical attachments standing in the way of systemic flexibility—attachments that determine what matters for each practice, what motivates its practitioners to think, feel, and (if need be) resist.

Still, ecology is not about predators and prey only, but also about connecting-events, such as symbiosis, that positively relate heterogeneous terms even as the terms diverge. Symbiotically related beings go on diverging, go on defining in their own manner what matters for them. Symbiosis means that these beings are related by common interests, but *common* does not mean having the same interest in common, only that diverging interests now need each other. Symbiotic events are a matter of opportunity, of partial connection, not of harmony. It is as such that they are these days taken to be the very source of innovation in the history of life. And they may also indicate a way out of the “either/or” that haunts us: either universality (meaning that all practices have something in common) or else relativism (meaning that each practice has its own incommensurable standpoint and that practices are thus blindly indifferent to each other, except insofar as they destroy or are destroyed by each other).

As it is the case with many an either/or, this one has nothing neutral about it. It demands that we choose universality, and it can even be said that universality is nothing other than what must be postulated in order to escape the relativist menace. It is the very rhetoric whereby commensurability and equivalence are imposed as conditions for science, governance, or the need to identify charlatans. Alternative to all this commonplace thinking is the interest in the many kinds of rapport that symbiosis may bring about. The importance of symbiotic events is suggested by William James’s idea of a *pluriverse*. Unsatisfied with the choice on offer in metaphysics between, on the one hand, a *universe*, with its ready-made oneness, justifying efforts at overcoming discordance, and, on the other

hand, a *multiverse*, made of disconnected parts indifferent to each other, James proposed that the world is a pluriverse *in the making*. Connections are in the making, breaking indifference but bringing no encompassing unity. Plurality means divergences that communicate, but partially, always partially. What I called the creation of a “rapport,” whatever its meaning (experimental, religious, therapeutic, or otherwise), then participates in James’s pluriverse in the making. Each such creation is an event to be celebrated as adding a new dimension to the whole and having it “rise in value.” This vision is the inverse of the unthinking dream of eradication—the dream of a world improved by a universal agreement among its denizens about what matters. James’s pluriverse may be related to Donna Haraway’s idea of “situated standpoints,” in which each standpoint, in situating itself, becomes able to assert the legitimacy of other diverging standpoints.

If the making of the pluriverse that James celebrates is to be thought in terms of symbiotic events, the connection between heterogeneous ways of life or being as such, it also demands that we not accept settled ways of life or being as given, with survival or eradication as their only prospect. Such was for me the very hypothesis that developed into an “ecology of practices.”⁵ If experimental practices, the invention of which marked the birth of what we call “modern science,” are to survive eradication, they will not be by trying to defend some sad remains of their past autonomy. Rather than lament over the loss of this autonomy, it may be well to consider its price. Science as the famous goose that lay golden eggs claimed that it should be unconditionally fed because of its contribution to general progress. But it also meant that scientists would be free to try and interest allies that could turn the “eggs” into gold while proclaiming utter nonresponsibility for the eventual outcome. Allies that fed public scientific research have now decided to turn a deaf ear to the goose’s warning that she should not be killed. However, others still need scientific research to be reliable and would furthermore welcome scientists learning to present what they know in as demanding a way when they deal with nonscientists as when they deal with colleagues. It may well be that the only possibility for scientists to keep their divergence, their very specific way of having what they learn from matter, alive, is to betray their role as consensual proponents of reason and progress. Connection with groups needing their cooperation and expertise to formulate relevant arguments against the technoscientific transformation of our world(s) would be a “symbiotic event,” the creation of a new kind of demanding “rapport,” a contribution to the Jamesian pluriverse. And it may well be that here some ethnologists and sociologists show the way, as they struggle to create connections that allow them to learn and also allow those they learn from to learn as well, and for their

5. See Isabelle Stengers, *Cosmopolitics I*, trans. Robert Bonomo (Minneapolis: University of Minnesota Press, 2010).

own sakes. The point in this case is not to learn from the others “as they are,” but to learn from them as they become able to produce relevant ways of resisting what defines them as prey.

It may be objected that what would be learned in this case cannot be called “science,” since there is no way to disentangle what is learned from the situation. The answer, “such is also the case in the creation of experimental rapport,” is insufficient, because what is lacking in our case is the “research program,” the succession of “but then . . .,” “what if . . .,” “why did it not work?” that both extends and tests the scope of the rapport. In other words, the rapport does not authorize a dynamics of inventive consequences that clearly disentangles the scientists from what answers their questions.

It may well be that accepting this difference is the crucial point, the one that may free us from any nostalgia for academic research institutions, invented as they were around the generalization of research programs. It may well be that these institutions were never good places for learning what science means when addressed to sentient beings, beings who enter a rapport for their own reasons. Whatever the case, I want, in closing, to be clear that my reference to James’s pluriverse does not imply an impending utopia of universal peace. While everything is always related to everything else, the creation of a rapport is always a local, precarious event and, more crucially (I agree utterly with Donna Haraway), never an innocent one.

This is the ultimate interest of our reference to ecology. Ecology understands conflicting interests as being a general rule. Ecological, symbiotic events, the creation of rapport between divergent interests as they diverge, mean novelty, not harmony. From an ecological viewpoint, the questions raised by a creation of rapport are not epistemological, but rather political, pragmatic, and (again) never innocent ones. Who is, or will be, affected, and how? The answer to such questions ought to be a matter of collective concern and accountability. Rather than critical reflexivity, our answering requires that, collectively, we learn the art of situating knowledge, which involves learning how to pay due attention to situations and consequences. Relativism, then, is not the debunking of universalistic claims, but an affirmation that there is no “innocent” knowledge because there is no knowledge without a creation of rapport. If a debunking there must be, it should be directed at the many ways in which the production of rapports may evade the challenge of considering the consequences fully.

Returning to comparativism as a method of learning, I would conclude that there is only one general rule, which may be derived from both Péguy and the experimenters, as in both cases what matters is that rapports be created between terms in their “full force,” with no “foul play” weakening one and ensuring the position of the other. This is why, when those one wishes to learn from are what we call “humans,” common humanity is a trap, since it defines divergence as

secondary. Those you address must be empowered to evaluate the relevance of your interest, to agree or refuse to answer, and even to spit in your human, too human, face. This demands that you present yourself in terms of your own divergence—of what matters to you, and how. “Learning from” requires encountering, and encountering may indeed imply comparison, but there is no comparison if the encountered others are defined as unable to understand the point of the comparison. We are returned here to the Latin etymology of “comparison”: *com-par* designates those who regard each other as equals—that is, as able to agree, which means also able to disagree, object, negotiate, and contest.

COMPARISON AS PARTICIPANT

Helen Verran

Resist the easy interpretation of the image of a fetching young man in flax loin-cloth stretching a rope to (re)constitute ancient Egypt's muddy expanse of the Nile delta as measured fields. This origin story of comparison as achieved in practices, gestures with ropes and sticks alone, leaves out far too much. This is comparison by fiat, but then so are the Platonic versions of comparison, which regard qualities as abstract forms and, on the basis of them, impose measurement, value, number—in short, these are comparison as imperium. The imperial spokesmen say: “comparison *must* be possible, because objective knowledge *must* be attained.”

In my terms, such imperial comparisons, whether universalist or relativist, are kinds of “foundationism.”¹ And I agree with Stengers that rendering the contemporary experimental sciences as either of these kinds imposes on them “a standard ensuring equivalency”—a machine that destroys “in the name of rationality, objectivity, and the great divide between nature and culture.” In contrast to both these foundationisms, Stengers persuades us of the liveliness of comparison as itself a participant in collective action. The specific “participant comparison” that scientists deal with and through (often by use of numbering) is their familiar or agent in the action of going on together.

1. For “foundationism,” see Helen Verran, *Science and an African Logic* (Chicago: University of Chicago Press, 2001), 14–18.

As Stengers sees it, we need to understand that entities that emerge in the standardized practices of scientists actually *participate* in the specific comparisons underway. Without this understanding we will not attain the better grasp we need of ethical and political challenges involving the sciences. She is concerned that, as foundationist comparison joins the market in an ever-strengthening alliance, “dancing in the lab is becoming a thing of the past.” While bad-faith comparison, conducted by scientists beholden to industry, is by no means solid enough for epistemic contest, it is nevertheless “good enough for patenting, demonstrating promise, and stimulating the appetite of investors,” which is an end in itself when the sciences lay golden eggs for commercial interests. I will come back to this claim, which I take seriously, perhaps more seriously even than Stengers does, for I see it as symptomatic of nothing less than the demise of nature.

The sciences can indeed be characterized as addressing comparison, Stengers argues, but we need to understand comparison not as a practice or method, “a sieve or filter,” but as an event: the creation of “a rapport or *logos*.” I call these events “participant-comparisons.” As participants, scientific measures and comparisons are in no way innocent. In Stengers’s paper, we meet a series of such comparisons. The first introduced is Galileo’s, with which, or so we are often told, he almost single-handedly brought about the Scientific Revolution. Two significant changes, however, needed to occur first. As Duhem pointed out, people had to stop thinking of qualities in Aristotelian terms, as opposites like heat and cold. Instead qualities like length—or in the case of Galileo’s enumerated entity, velocity—were repurposed as a single extension: length and velocity as a matter of degree. But these accomplishments were not all that was required. Stengers shows us that the crucial final element was the inclined plane:

If Galileo was able to compare the varying degrees of velocity of a body at determinate moments of its fall, it is because the inclined plane enabled him to do so. The inclined plane is the first experimental device, the first device the achievement of which is to create a very unusual kind of rapport: a rapport that authorizes claiming that what is measured lends itself to the measurement. But such a rapport is never a general one. The inclined plane is obviously not relevant to all qualities, nor is it relevant to space-time motion in general. It identifies a quite specific kind of motion, that of what we may call Galilean bodies, which have no internal source of motion (as opposed to a car or a horse) and move in an ideally frictionless manner (as opposed to an avalanche).

Qualities, attributes, and properties constituted as singular extensions of varying degree, which somehow inhabit bodies, along with very specialized material devices, are crucial for the participant-comparisons and enumerated entities of science. I would add to this list various other series of material routines and rules of engagement, each of them specifying that it is only with *these* gestures and

words at *this* time (and not at other times) during the course of the event that will bring about the desired result.

I tell a similar story of a new sort of enumerated entity, a new member of the family of participant-comparisons of the contemporary sciences, which came to life before my eyes in Yoruba classrooms in Nigeria in the 1980s. Mr. Ojo is the hero—the Galileo of the Yoruba postcolonial classroom. In this story, Mr. Ojo begins with a Yoruba type of participant-comparison that is present as degrees of dividedness (not with qualities as degrees of extension), which, along with some specialized devices and routines, constitute Yoruba numbers. In other words, in coming up with a new device—a little card, 10 cm long and marked out in cm divisions, plus a length of string, Mr. Ojo brings to life a new participant comparison. This device, along with a new set of material routines and rules for using this gesture or word here and others there, a new sort of event, a new sort of rapport, emerges in his lessons. The collective action that is a classroom math or science lesson in Nigeria gives birth to a new sort of enumerated entity, just as the collective action that Galileo was part of gave birth to a new sort of enumerated entity: a new participant-comparison emerged onto the scene.

Here is how I have described the participant-comparison I met up with in Nigeria:

In Mr. Ojo's classroom a little boy 'Diran was standing before the class becoming enumerated. The string, which a moment before was stretched with one end under his heel and with Mr. Ojo holding it at a point near the top of his head, is now wound around a little card. Mr. Ojo touches each 10 cm interval of the string *Ökan, èjì, èta, èrin, àrùún, èfà, èje, èjo, èsán* (one, two, three, four, five, six, seven, eight, nine). Then holding the bit of string remaining against the graduations on the card: "*Mòkònléléàádòràán, méjì léléàádòràán, mètàléléàádòràán, mèrìnléléàádòràán*" (ninety-one, ninety-two, ninety-three, ninety-four centimeters). "Yes, we have ninety-four centimeters. 'Diran's height is ninety-four centimeters." The sequential lifting and announcing of the strings so arranged interpolates the numeral settled upon, *mèrìnléléàádòràán* is 'Diran, as much as the boy is *mèrìnléléàádòràán*. Numerals become as enumerated children and children present as numbers. The becoming thus is a particular sort of presentation, a particular figuring.²

What I have been doing so far is retelling Stengers's story, rendering her terms into my own more homely (and odder) terminology. In rendering Stengers's terms, I understand her narrative's Galileo and his newly hatched participant-comparison, or enumerated entity, by analogy with my story of Mr. Ojo and his enumerated entity. This rendering is of course just another form of relativizing comparison—showing samenesses and differences.

2. Verran, *Science and an African Logic*, 105–6.

With this re-rendering, I have been showing a means (quite analogous to Mr. Ojo's little card, string, and routines of words and gestures) that came to life as I struggled to tell responsible stories of Yoruba classrooms. A help in this struggle was the writing of others: Kathryn Pyne Addelson (who, like Stengers, is an intellectual granddaughter of Whitehead); Bruno Latour, John Law, and Michel Callon, as they invented and worried about actor-network theory; and Annemarie Mol, as she assiduously tracked down the many atheroscleroses living their lives in a Dutch hospital. To some extent retooling their terms to tell a new story about enumerated entities of science, I struggled with the ancient ghosts of Athens and Jerusalem, as well as the ghosts of fifteenth-century mercantile Italy and nineteenth-century Enlightenment Britain, in order to disinter assumptions that are embedded in our ways of doing numbers in classrooms and in our stories about numbers.

For Stengers, comparing comparisons involves an ethical and political challenge:

Péguy's proposition [of comparing comparisons] is very demanding and, as such, very interesting. His implication is that no comparison is legitimate if the parties compared cannot each present his own version of what the comparison is about; and each must be able to resist the imposition of irrelevant criteria. In other words, comparison must not be unilateral and, especially, must not be conducted in the language of just one of the parties.

Once again, and even more immodestly, I will translate Stengers's terms into my own. I have often worked with and told stories about scientists struggling to learn from Aborigines and vice versa. Scientists find unacceptable the Aborigines' participant-comparison, which is achieved through the device of a dual recursion modeled on kinship; and Aboriginal elders remain unconvinced by a participant-comparison effected through qualities. Each struggles to achieve rapport with, and to credit the participant-comparison of, the other as agent.³ In my stories, we see that Aborigines and scientists need to act in good faith and with an explicitness concerning their own ontic and epistemic commitments. They need to be able to recognize how these are expressed in the constitution of their familiars, the participant-comparisons they hang out with. Only then might they usefully address the issue of the crossing over of knowledge traditions. Or in

3. See Helen Verran, "Re-Imagining Land Title in Australia," *Postcolonial Studies* 1 (1998): 237–54; "A Postcolonial Moment in Science Studies: Alternative Firing Regimes of Environmental Scientists and Aboriginal Landowners," *Social Studies of Science* 32.5–6 (2002): 1–34; "Transferring Strategies of Land Management: Indigenous Land Own-

ers and Environmental Scientists," in *Knowledge and Society*, ed. Marianne de Laet (Oxford: Elsevier, 2002), 155–81; "Science and the Dreaming," *Issues* 82 (March 2008): 23.

Stengers's terms, the events that are the comparison events of both scientists and Aborigines might be connected enough to enable them to learn from each other and go on together in burning (managing) the land:⁴ "‘Learning from’ requires encountering, and encountering may indeed imply comparison, but there is no comparison if the encountered others are defined as unable to understand the point of the comparison. We are returned here to the Latin etymology of ‘comparison’: *compar* designates those who regard each other as equals—that is, as able to agree, which means also able to disagree, object, negotiate, and contest." Yet, as Stengers notes, "the ethical and political challenge associated with comparativism [comparing our comparisons] . . . concerns us in the modern West as much as it does our distant others."

Despairing at the ways in which the academy currently connives in its own looming defeat, submitting to and complying with demands that are sounding its death knell, Stengers sets herself the task of articulating the conditions for an honorable defeat. She wants to find a way for us to acknowledge, when defeat occurs, that we were—in our own terms—really defeated. As scholars and researchers, what we need to do, she says, is make it clear, to ourselves and others, that this imminent defeat (by foundationist comparison) is truly a defeat.

As I see it, in urging this course upon us, she would have us do exactly what Mr. Ojo and my Aboriginal friends *refuse* to do when faced with foundationist comparisons. But there is a significant difference between the resources available to those who live by science and those who live by other knowledge traditions. The way science deals with its comparisons makes it more fragile, brittle, and vulnerable than either Yoruba or Aboriginal traditions of thought. Unlike the sciences, these knowledge communities have not abandoned awareness of the metaphysical resources that inspire and enable resistance and challenge.

So how might the sciences learn from knowledge traditions like those in which Mr. Ojo and Aboriginal landowners participate? Scientists need to find in their own goings-on the disconcerting creativity of Mr. Ojo's divergent practices; they need to recognize that they have the same capacity to innovate that Aborigines exhibit when trying to negotiate with pastoralists. Stengers wants us to recognize the positive divergence in practices, yet it seems that the sciences' insistent refusal to recognize their ontic and epistemic commitments gets in the way of this recognition. Or as Whitehead and Stengers might have it: science, by allowing itself to be misled by philosophy about the nature of its ontic and epistemic commitments, leaves itself with nowhere to turn.

4. See "Aboriginal Hunting and Burning Increase Australia's Desert Biodiversity, [Stanford University] Researchers Find," *ScienceDaily*, May 5, 2010, accessed July 23, 2010, www.sciencedaily.com/releases/2010/05/100503174030.htm.

Still, Stengers is going in the right direction in identifying divergent practices. I suggest that we not only need to recognize what is involved in generative divergent practices but also to locate them. Recognizing and locating the disconcerting creativity of practices might enable us to face the death of science with some equanimity, in the same way that Deleuze, following his friend Foucault, suggests we face the death of man (and nature): “We must take quite seriously the idea that man is a face drawn in the sand between two tides: he is a composition appearing between two others, a classical past that never knew him, and a future that will no longer know him. There is no occasion either for rejoicing or weeping.”⁵ Understanding the specific place and circumstances of our looming defeat, we are in a better position to understand the practices by which power is exercised in what is to come. But where to search and how to nurture such positive divergences? My suggestion is that, to begin, we need to look into the temporal disjunctions embedded in science.

Immersing Pasteur and Whitehead in a bath of lactic acid, Latour makes much of the temporal disjunction between the focused ontic work of translation and the backward-looking epistemic work of purification in science.⁶ The disjunction no doubt relates to the older contrast between contexts of discovery and justification made by the positivist Hans Reichenbach in 1938.⁷ In Latour’s story, the entity is stripped of all signs of its “birth” in the here and now, as universality and transcendence are ascribed to it in the process of epistemic contest. Here we have a temporal disjunction between a presently emergent entity and an entity always already there in nature. The canny scientist has always, paradoxically, both recognized and not recognized the disjunction, so science has both never and always been modern.

As science becomes a service industry and the good of truth becomes an optional extra in a privatized science, a completely different sort of temporal disjunction is played out, one that sets the present against an uncertain and unknowable future, not in (a now vanished) nature, but in the market. Canny contract scientists must work in this disjunction. On the one side, there is the painstaking labor of solidifying comparison as a thoroughly reliable participant in the here and now, including enough engagement in epistemic battles to validate their workmanship. And then, on the other side, they must surrender their familiar—their participant-comparison—to the venture capitalist, handing over what the contract has specified as “deliverables.” The entity might well fail, even though scientists have done their best to equip their participant-comparison for

5. Gilles Deleuze, *Foucault*, trans. Sean Hand (London: Continuum, 1999), 74.

6. Bruno Latour, “Do Scientific Objects Have a History?: Pasteur and Whitehead in a Bath of Lactic Acid,” *Common Knowledge* 5.1 (1999): 76–91, at 76.

7. Hans Reichenbach, “Meaning,” in *Experience and Prediction* (Chicago: University of Chicago Press, 1938), chap. 1, www.ditext.com/reichenbach/reich-c.html.

an uncertain future in a sociotechnical order where epistemology has no clout, where money and profit rule instead. This development is not a happy one—no cause for dancing—yet the temporal disjunction is there, just the same. The present is set against an uncertain future.

It was in just such a temporal disjunction that Mr. Ojo worked. Refusing to comply with the then/there of a colonizing school curriculum, he remade the here/now of his Yoruba classroom by diverging from official practices. But the enumerated entity that emerged in his divergences faced an uncertain future in the classrooms of a colonial state. Indeed, this novel entity had an ephemeral life. Temporal disjunction also characterized the invention (albeit minimalist) of divergent practices by scientists and Aborigines as they struggled wittingly and in good faith to connect their participant-comparisons enough to expand trust in the here and now.

RATS, ELEPHANTS, AND BEES AS MATTERS OF CONCERN

Steven D. Brown

Not all rats are suitable for conditioning experiments. “White” or albino rats are the best candidates for this kind of task, being relatively easy to handle. Most popular is the “Wistar” strain, originally introduced to the University of Chicago by the neuropathologist Adolf Meyer and subsequently bred by the physiologist Henry Donaldson. Over half of all laboratory rat strains are descendants of Donaldson’s original colony.¹ The Wistar rat is a potent and controversial symbol for psychology. As the title of R. V. Guthrie’s historical analysis of racial bias in the discipline has it, *even the rat was white*.² The rat of psychological research is most commonly represented in the form of a single animal traversing a maze. It is this scene that has stood for a natural science of social behavior for generations of psychologists. Yet it is difficult to think of any image further removed from communal human life than this bizarre spectacle. “These animals,” Kurt Danziger observes, “represent individuals even more drastically thrown on their own resources than the lone gunmen of the mythical Wild West.”³

1. F. J. Wertz, “Of Rats and Psychologists: A Study in the History and Meaning of Science,” *Theory and Psychology* 4.2 (1994): 165–97.

2. R. V. Guthrie, *Even the Rat Was White: A Historical View of Psychology*, 2nd ed. (Boston: Allyn and Bacon, 1998).

3. Kurt Danziger, *Naming the Mind: How Psychology Found Its Language* (London: Sage, 1997), 99.

Isabelle Stengers's meditation on comparison as a matter of concern takes these sorts of foundational epistemic practice seriously. We might be tempted to dismiss the comparison of the Wistar rat with the human as involving such a high degree of reductionism as to be entirely laughable. And yet, as Stengers makes clear, reductionism is not simply a matter of false analogy or of an unsustainable comparison. Reductionism is what occurs when one practice applies a form of "objectivity" or "rationality" to another in such a way that the latter becomes the "prey" of the former. In this case, the real reductionism comes from our ready laughter in dismissing behavioral psychology as misguided or as blinded by its commitment to the Wistar as objects of study. The laughter began as early as 1950, when the then-president of the American Psychological Association, Frank Beach, inverted Browning's Pied Piper of Hamelin tale to dismiss behavioral psychology: "Now the tables are turned. The rats play the tune and a large group of humans follow."⁴ Behavioral psychology is the prey that must be sacrificed for the survival of psychology in general, since it has unwittingly allowed the Wistar to take the lead.

Over the course of her work of the past twenty years, Stengers has patiently described an image of science that differs markedly from the predator/prey relationship of critic and object of criticism. In this article, she reminds us again that her work follows in a normative tradition within philosophy of science. Social scientists who have sought solace in Kuhn's work, as though it authorized a retreat to "incommensurability" whenever their own practice is challenged, are again reminded that for Kuhn "communication breakdown" between paradigms is a spur to comparison and translation. Invoking the parable of the blind men and the elephants, Stengers suggests an alternative interpretation. When a community is constituted around a "collective concern"—which indeed is what Kuhn sought to describe with his notion of paradigm—then it seeks to organize contradiction into "contrasted standpoints" by "exploring the possibility of a coherent account." To spare once again the blushes of social scientists—who famously lack a paradigm in Kuhn's sense and, hence, collective matters of concern—Stengers sees in this not a superior form of rationality at work in the "paradigmatic sciences" but rather a greater readiness to be bound by a "collective game."

The invention of such games and their materialization in "events" and "inventions" that offer new articulations of the real is one of the principal issues in Stengers's *The Invention of Modern Science*. There she cuts through the pointless attempt to separate reality from construction (in debates about the status of experiments) with the decisive formulation that what is at stake is "the invention of the power to confer on things the power of conferring on the experimenter the power to speak in their name."⁵ Galileo and classical chemistry are once again her

4. Wertz, "Of Rats and Psychologists," 179.

5. Isabelle Stengers, *The Invention of Modern Science*, trans. Daniel W. Smith (Minneapolis: University of Minnesota Press, 2000), 88.

touchstones; here she observes scientists endow their objects, through the process of making continuous comparisons, with an agency of their own. The outcome is that scientists are unable to justify or explain the kind of understanding they had achieved, since the conceptual definition that it confirmed did not warrant their anticipating it. In other words, continuous comparison sets up a kind of game in which objects can be seen to exert their own preferences and effects that exceed the original conceptual terms from which comparison departed.

Behavioral psychology lacked a Galileo. It made do with J. B. Watson. In the title of his doctoral thesis, “Animal Education: An Experimental Study of the Psychological Development of the White Rat,” Watson implicitly suggested who was conferring the power of comparison in his lab. He had begun experimenting with the “puzzle box” devised by Edward Thorndike. The box could be altered to create different ways of imprisoning Wistar rats, whose escape attempts could then be systematically observed. It was a variant of the puzzle box—a maze with moveable blockages—that proved decisive in the supposed repudiation of behavioral psychology by the nascent cognitive psychology of Edward Tolman, whose foundational experiments were conducted in 1948. If blockages are moved in sequence within a rat maze and the response of the rat appears to anticipate the consequences of these moves, then it must possess a “cognitive map.” This kind of experiment better resembles the “collective games” of scientists (for whom comparison matters) than it does the operations of their critics, who view it as an irrelevance that does not speak in any way to their own practice. Behavioral psychology has its own “elephant.” Its critics do not have one and so have made their rejection of experiments a point of honor. Stengers’s notion of an “ecology of practices”—organized around “elephants” such as the puzzle box, the inclined plane, or tables of chemical affinities—appears to rob critical social science of a privilege that it thought it had earned through its exclusion from the high table of normative science.

That exclusion may soon be repeated, but this time at the heart of normative science itself. In an apocalyptic section of her main symposium piece, Stengers invokes the specter of a science “mobilized by competing industrial interests,” which will, she claims, make collective games “a thing of the past.” Once capital fully subsumes science, there will be no games, no more efforts to solicit the interests of others, no further constituted differences within a collective coherent account. The economics of patenting will see to that. Things look little better for the academy in general—“we belong now to worlds on the brink of defeat.” Surely among the most remarkable moves that Stengers makes in her piece is to reassess the image of science running through her previous works as a “state of exception” that is now about to disappear. One cannot but wonder what has brought about this pronouncement on Stengers’s part, given that industrialized science has been the default mode of operation for natural scientists (and

indeed many social scientists) for some time now. Surely industrialized science too has characteristics, procedures, and modes of invention that are worthy of the close scrutiny that Stengers has paid in the past to Galileo or Lavoisier? Even J. B. Watson found that a career in industry was not entirely unbearable following his expulsion from Johns Hopkins (by Adolf Meyer, the “father” of the Wistar).⁶

All is not quite lost, though. In typically pugnacious style, Stengers refuses to go quietly. Drawing on Péguy, she imagines what would amount to “perfect comparison.” It would consist not of a play of similarities and differences, but rather of a new kind of collective game in which opposing parties display their worthiness to enter into comparison by refusing to make the outcome depend on anything other than their efforts to appear in “full force.” This would be akin to an “argument” between actors that consisted entirely of uncritical, positive self-assertions (“I believe . . .,” “From my perspective . . .,” “Looked at in this way . . .”). Neither party would attempt to undermine the position of the other: a “very demanding” and “very interesting” operation, Stengers observes. Yet in the arguments that follow, Stengers very rapidly switches to a series of what she calls “unfair comparisons” and dismisses at length, deploying terms like “multi-nature,” attempts made to overcome the limitations of the nature-culture opposition. Is Stengers’s accusation of “foul play” itself to be disallowed by the demands of the “perfect comparison”?

In the final sections of her piece, Stengers comes full circle with her opening comments: “We are all comparativists, and so probably are all animals. Comparison is an important part of any animal’s equipment for dealing with its respective world.” Here Stengers draws upon an ethological account of categories and comparisons that has affinities with Bergson (see his account of the “judgment” shown by acids in *Matter and Memory*) and Deleuze (especially in his work on Spinoza). If comparison is part of the “equipment” that living beings draw upon in their worldly engagements (think here of Deleuze’s discussion of Von Uexkull’s tick, whose universe is defined by just three possible modes of comparison), then it becomes a matter of artfulness rather than reason. The “art of comparison” is an ecological strategy for understanding the behavior of others who may become one’s prey or who, alternatively, may render you such.

Comparison is ultimately not the preserve of reason, nor indeed in essence is it a cognitive matter. Comparison is the basis of relations between the diverse plurality of actors who make up what is inelegantly called “nature”—“neutrinos, pigs, and tornadoes.” To set the “perfect comparison” as the bar to aspire to in rendering these tangled relations as matters of concern means allowing each party to appear in “full force.” We must learn from pigs as they apply their

6. K. W. Buckley, *Mechanical Man: John Broadus Watson and the Beginnings of Behaviorism* (New York: Guilford, 1989).

comparative apparatus to sorting and organizing humans. An *animal education* is required. Equally we must experience the comparative powers of neutrinos and perhaps even, as in Neko Case's lyrics, learn to feel the love and longing expressed in the tornado's smashing "every transformer into every trailer." Bidirectional modes of learning are fundamental to the "perfect comparison." The old joke that it is the rats who are running the experiment has some purchase. As someone whose undergraduate training included operant conditioning of Wistar rats, I can attest that this certainly feels to be the case. It is the highly experienced rats who condition students to be attentive experimenters. Or perhaps it would be more precise to say that there is a loop formed of rats-as-educators assisting in the production of new generations of rat-observers, where something of the one is already implied in the other. The comparison is not between two independent actors—human and rat—but between two hybrids who embody a whole history of intersecting modes of comparison.

To open oneself to animal education is to seek symbiosis. Relations of predator/prey give way to relations between "heterogeneous interests." The image that Deleuze and Guattari—who have themselves been objects of Stengers's exemplary form of epistemic experimentation—provide us for thinking about symbiosis is of the orchid and the wasp.⁷ The orchid transforms itself into the figure of the wasp, attracting its attention. The wasp then becomes part of the orchid's reproductive apparatus. But in so doing the orchid becomes part of the affective universe and territory of the wasp. The lines of orchid and wasp are folded back into one another. Stengers's thinking about such relations emphasizes their contingency, their precious and precarious nature. Symbiosis is an "ecological . . . event," one that poses etho-ecological questions: "Who is, or will be, affected, and how?" These are questions, Stengers concludes, that are answerable only through experiencing resistance or rejection from that which we seek to compare: "Those you address must be empowered to evaluate the relevance of your interest, to agree or refuse to answer, and even to spit in your face."

Karl Bühler, president of the German Psychological Society between 1929 and 1931, once described the passion for experimenting on the Wistar as "really American."⁸ He suggested that, if one wanted to find a suitable analogue for human behavior in the animal world, one might instead look at bees. The comparison is more promising—bees have something like a language, a division of labor, and complex social relations. Nothing could be further from the isolated Wistar rat, marooned in a maze or trapped in a puzzle box, than the sight of bees

7. Isabelle Stengers, "Experimenting with *What is Philosophy?*" in *Deleuzian Intersections in Science, Technology, and Anthropology*, ed. Casper Bruun Jensen and Kjetil Rødje (Oxford: Berghahn, 2009).

8. See Danziger, *Naming the Mind*.

working and communicating together to achieve collective goals. We can only wonder what kind of behavioral psychology might have emerged had it chosen to make the bee a matter of concern. As we have recently discovered, that possibility is now closed. The bees are dying or disappearing.⁹ The fate of psychology is to live literally and metaphorically in *a world without bees*.

9. Alison Benjamin and Brian McCallum, *A World Without Bees* (London: Guardian Books, 2008).

HOPEFUL COMPARISONS ON THE BRINK OF THE GRAVE

Brit Ross Winthereik

In “Comparison as a Matter of Concern,” Isabelle Stengers conjures for us one of the concerns that matter most to her. She argues that tight relations between academia and industry do deadly harm to comparative science, as they cut off the “collective game” in which colleagues are welcome to object. Objecting to one another’s research and its results will soon be a thing of the past. At the heart of this gloomy picture we find a battle between comparisons: imposed comparisons, where extraneous, irrelevant criteria are laid down, and active, interested comparisons, where rapport is established between the scientist and the phenomenon she studies. Though Stengers notes that it is “always possible to speak of practices as flexibly transforming themselves,” her concern is that the connection of science and industry will not be transformative of the sciences but, rather, fatal to them. What rendered scientific practices distinct from—divergent from—other practices is at stake.¹

1. During the conference out of which this symposium emerged, Stengers was invited by a student in the audience (Birgitte Gorm Hansen of Copenhagen Business School) to reflect upon whether the connection between science and industry might bring about a transformation of science in some positive sense. To which Stengers replied, her mouth close to the microphone, voice echoing throughout an auditorium packed with hopeful research-

ers: “No transformation—death!” Then she added quietly but no less fiercely: “I’m a Darwinist; species don’t transform, they die.” What a splendid outburst at a conference hosted by an institution, the IT University of Copenhagen, that was established to explore and exploit the potentials of science in the service of industry!

Common Knowledge 17:1

DOI 10.1215/0961754X-2010-038

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I would argue that Stengers, in proclaiming the death of science, is trying to work a kind of transformation herself. Her claim that the knowledge economy is responsible for the death of good science breathes new life into this important question: what can be done about the science-industry relation other than listen acquiescently to the death knell of good science? What Stengers opposes most vigorously about the influence of the knowledge economy on the knowledge practices at universities is that “mechanisms” producing “equivalency” among practices introduce a flexibility in each individual practice that hinders active, interested comparison. On this point, Stengers is in agreement with Marilyn Strathern. During a recent lecture, Strathern diagnosed a particular case of science-industry collaboration as an instance of what she termed “speculative synergy” and argued that it was biased against “intellectual elaboration.”² Science-industry collaboration, which is a trademark of the knowledge economy, is characterized by a perpetual promise, Strathern argues, as it is supposed to yield value for every possible stakeholder—scientists, industry, and “wider audiences.” But without any space for substantial intellectual discussion, the question of how the actual impact of the collaboration should be assessed remains open.

In the same vein, Stengers points out that scientists entering into partnership with industry are compelled to engage with questions and criteria that are scientifically irrelevant. Scientists find more in nature than what is obvious, because they pay due attention to relevance. Yet, Stengers argues, scientific relevance holds no or little promise to industrial stakeholders. Scientific relevance is what makes scientists “dance in their labs.” The parable of the elephant and the three blind men is, according to Stengers, wrong: it is not about eliciting incommensurable aspects of the elephant but about how exploring and arguing over the constitution of the elephant as a shared object of concern ought to lead to the death of the weakest argument.

To both Stengers and Strathern, science-industry “partnerships” threaten the epistemic core of scientific work, because the collaboration brings scientists to betray it. The comparisons required by science and the comparisons demanded by industry are incommensurable; there is no way in which a Whiteheadian “real togetherness” between science and industry can arise. No “cosmopolitical” proposal is possible; in a knowledge economy, active, interested comparisons and good science are, therefore, stone dead. In contrast, real togetherness can happen between scientists and their objects of study. It is this process that Stengers talks about as “rapport.” Being able to establish rapport is a significant feature of comparison-as-event.

2. Marilyn Strathern, “Hope, Uncertainty, and the Research Proposal: A Tale from the UK” (public lecture, “Organizing Uncertainty,” Copenhagen Business School, September 1, 2009).

Through their attention to what constitutes a good experiment, Stengers and her colleague Vinciane Despret have inspired scholars in studies of science and technology. Stengers and Despret see transformative processes—of the scientists, of the experimental subjects and objects, and of the experimental design—as key to any good experiment. In addition, Stengers insists on a radical mutuality between scientists and the nature on whose behalf they speak. Mutuality has an affective dimension but primarily comes into being through “due attention,” as expressed in the use of experimental tools that enable comparisons.³ Employing devices like Galileo’s inclined plane (Stengers’s example) or (in Despret’s work) a food bowl, mutuality and authorization processes go on among scientists, measurement devices, nature, and other members of the scientific community.⁴ And the deaths of arguments and positions form a part of these processes.

In the context of a symposium titled “Comparative Relativism,” perhaps the crucial point to make about what characterizes Stengers’s matter of concern is that, in being utterly uninterested in defining absolute scales for comparison and in focusing only on the generative potential of comparing, she relativizes the very act of comparing. What matters to her is how experimental devices simultaneously sensitize their users to the phenomena at hand and to the workings of particular comparisons. Since (a relativized) comparison is a matter of concern to the scientists involved, finding out what other ways of comparing there are (besides objectivist comparison) should be a matter of concern to those of us who are engaged in studying knowledge production.

Now, what if science-industry partnerships kill science only because scientists lack devices to strategically disconnect and separate scientific practices from the practices and concerns that matter for industrial actors? What if we see the work of Stengers—her active, interested comparisons—as a device for creating trust in and integrity for science in the knowledge economy? What if the examples of good science that she offers become companions for scientists who enter into the knowledge economy’s zones of discomfort? As an academic whose career has been thoroughly shaped by industrial demands for academic knowledge, I, like Stengers, have difficulty in identifying spaces where scientific practices may be found that are unpolluted by the imposed relevance formulated in the knowledge economy. Not entering into partnerships with industrial actors, not defining what is scientifically relevant in collaboration with partners, and not embracing the knowledge economy are no longer options. But does the lack of another option mean that science is threatened? Still, in my view, we should

3. See also Bruno Latour, “What is Given in Experience?: A Review of Isabelle Stengers’s *Penser avec Whitehead: Une libre et sauvage création de concepts*” (Paris: Gallimard, 2002), www.bruno-latour.fr/articles/article/93-STENGERS.html (accessed January 13, 2010).

4. Vinciane Despret, “Sheep Do Have Opinions,” in *Making Things Public: Atmospheres of Democracy*, ed. Bruno Latour and Peter Weibel (Cambridge, MA: MIT Press, 2006), 360–70.

take seriously Stengers's position that there are fundamental differences between scientific and industrial practices—not, however, in order to find a way out of the knowledge economy, but rather to find a way of maneuvering within it. Thus, the work of Stengers and colleagues may be read in a way that eradicates hype and creates the hope that new spaces for interested comparison can be created from within the knowledge economy.

Death and hope can be closely intertwined, though not in the sense that death brings new beginnings. Death inspires social practices that connect kin across time and space. To make clearer my sense of what is hopeful in Stengers's despair, I want to draw on the hopefulness in a Fijian mortuary exchange described by the anthropologist Hirokazu Miyazaki.⁵ The anthropological challenge for which Miyazaki seeks a solution is how to access the here and now of the ritual present. He shows how lists that document the gifts exchanged among the participants in a Fijian mortuary ritual function as tools for capturing the hope that is most important to his informants at the time. The temporal disjunction between creating and completing the lists installs a moment of waiting that makes hopeful anticipation possible. The mortuary exchange (as an event and an end point) makes it possible, in Miyazaki's account, for the Fijians to go on with life in the face of death.

When Stengers declares the death of good science, she stands up for science. Even if what Stengers says is that good science is dead, such does not appear to be the case. What she says is not convincing, because the way she stands by science bespeaks—even performs—a living, vigorous practice. Her situated performance is hopeful in that it makes her audience curious: what other hopeful performances might the knowledge economy generate?

In Miyazaki's work, the performativity of the lists becomes his entry into understanding how the end point of death is experienced by the Fijians. A similar ambition (to understand an end point) is at the heart of a joint work by Miyazaki and the anthropologist Annelise Riles. Together they write about how Japanese traders used to be eager to learn the newest mathematical models for predicting stock market behavior. Having abandoned what they took to be superior knowledge, the traders refrain from using even simple trading strategies. The traders' apprehension of the limits of knowledge about the market marks the end point of a practice—and the Miyazaki-Riles article, "Failure as an Endpoint," reflects on how to deal with the end point of a practice anthropologically.⁶ In the same way as Stengers dislikes how the notion of "transformation" is used to define what she regards as an end point (namely, the demise of active, interested comparisons),

5. Hirokazu Miyazaki, "Documenting the Present," in *Documents: Artifacts of Modern Knowledge*, ed. Annelise Riles (Ann Arbor: University of Michigan Press, 2006), 206–25.

6. Hirokazu Miyazaki and Annelise Riles, "Failure as an Endpoint," in *Global Assemblages: Technology, Politics, and Ethics as Anthropological Problems*, ed. Aihwa Ong and Stephen J. Collier (Malden, MA: Blackwell, 2005), 320–31.

Miyazaki and Riles are critical of the way that end points of knowledge practices are described in terms of “emergence,” “complexity,” and “assemblage.” They argue that, instead of turning end points into new starting points, anthropologists, like stock traders, should focus on end points “in a sustained way.”

Returning to science and the knowledge economy: would “knowing the endpoint in a sustained way” imply an ethnographic study of the decay of good science? I would opt for an approach that looks for active, interested comparisons in the knowledge economy. Such an approach might lead to studies of partnership as a material-semiotic practice, focusing on the particular work carried out by the “cherished objects” of the knowledge economy, such as research proposals, terms of reference, flow charts, and collaborative reports. Such objects are designed to achieve something that may be comparable to Stengers’s “rapport.” Knowing about the practices that craft these objects, what the objects do in specific encounters, how they are shared or not, may be one way of facing in a sustained way the end point of the science that Stengers depicts as dead or dying.

Studying the “partnership” objects of science-industry relations is both a way of staying with the present moment—of not turning the claim that good science is dead into an immediately new beginning—and a way of understanding industry as something other than a powerful monolith under which a vulnerable science is crushed. Such studies may compare practices and objects that are designed and used for solitary intellectual work with objects designed and used for collaborative projects. The way science-industry partnerships are organized, often as projects with short time frames, might be studied as well. The approach I am suggesting would also entail close attention to how particular objects bridge practices while other objects ensure that gaps between scientific and industrial practices remain. What objects make for (in)commensurability, and how are they handled, managed, and valued?

On a final note, I should add that being guided by an ethnographic desire to face end points “in a sustained way” would not require us to accept the claim that we are sitting on the brink of the grave dug by the knowledge economy. It would require, however, that we take seriously Stengers’s concern for unimposed and relativized comparisons.

SCIENCES WERE NEVER “GOOD”

Isabelle Stengers

The contributions made to this discussion by Wistar rats, Mr. Ojo, and Australian Aborigines, along with their scientists, are considerable. Beyond the case that each makes against unilaterally imposed relations, their presence is important, because they help in disentangling my proposition from what can be perceived as privileging the kind of achievement that is central to the experimental sciences. The importance I gave to the case of the experimental sciences was meant to renew their role as the starting point for thinking about science “in general.” I wished to uphold the singularity of their achievement and collective game (which Helen Verran characterizes as the “painstaking labor of solidifying comparison as a thoroughly reliable participant”) in order to call for a pluralization of sciences. I proposed thinking about the sciences in terms of the contrasting demands bearing on such a “participant” (nonunilaterally imposed) comparison.

For instance, I am not sure that all scientific achievements lend themselves to solidification, while I would think that the collective dimension belongs to sciences as such. But this collective dimension should then be reduced to very generic terms: a science exists when its practitioners are interested in each other’s work, learn from each other, refer to each other. Any science must then include “reporting home” (rather than “going native”), but the definition of what is to be reported has nothing general about it and depends on how the collective achievement is defined.

Moreover, in speaking of an achievement as an event, I wished to emphasize the dissymmetry between “the rule of objective knowledge” and the adventure of relevant relations. I am not very surprised that J. B. Watson was not unhappy with his career in industry, if the industry was busy with ergonomic unilateral measurements. But if rats possess a “cognitive map,” the question of relevance is open. What matters from their point of view? How do they map “us”? How do they relate to the situation? Learning relevance then entails learning what is relevant for them. And the “them” itself may be problematic—what of past experience, of trust, of their map’s reconfigurations, of the “personality” of each rat? Wherever relevance is a matter of crucial concern, there remains a kinship with experimental sciences; in these cases, I would propose, “progress” means that what is defined as the “object” becomes more interesting and more challenging, while questions inspired by the “rule of objective knowledge” become more disgusting.

Correlatively, if, as Steven Brown remarks, when addressing scientists dealing with other “humans,” I turned to a series of “unfair comparisons,” it is because “rat scientists” enjoy a privilege in comparison with those scientists. Given the opportunity, rats have happily served as witnesses to their cognitive map. In contrast, humans, as soon as they are in a scientific lab, agree (I am sad to say) to answer questions or produce performances that reproduce lab dissymmetry: scientists are wondering, learning, hesitating about the relevant interpretation while the object performs without questions. Humans, when they serve science, lack recalcitrance in contrast to some of those we have characterized in terms of beliefs (I refer to one side of what Bruno Latour calls the “Great Divide” between “we” who have science and all others who desperately mix up nature and culture). The recalcitrance they developed about the regime that colonizing powers imposed was able to inspire nagging doubt in many ethnographers. As such, they were the cause of some of those ethnographers “going native,” and the source of the learning trajectory that leads to Viveiros de Castro.

But so many powers depend and feed upon the knowledge/belief divide that the question of “human sciences” cannot be generically disentangled from the political question of empowerment. Whatever the achievements in the human sciences, they depend upon an increasing recalcitrance about accepting irrelevant or insulting questions. This is also the lesson that Latour learned during the unhappy “science wars” episode, which he takes as the *felix culpa* of the social sciences. Social scientists had addressed the same kinds of question to scientists as they address to anybody else. But some scientists, mainly physicists, were not impressed by the “science” of those who claimed to characterize *them*. They not only felt insulted but publicized their outrage. The question is now open: how to avoid people mutely “feeling insulted,” or actually “being insulted” without even feeling it because they have already been deprived of the possibility of attributing value to what they know and feel. The Pandora’s box is open, and safety has flown

away. If what remains in the box is hope, the hope for a nonpredatory human science, I am convinced that in this case it demands that the event known as “achievement” be recognized in its political dimension. Recalcitrance is required for learning relevant, knowledge-producing connections, but the production of recalcitrance is not in the power of scientists. Unable to “prepare” recalcitrant protagonists, scientists must depend on an event, usually political, to produce them (see Dewey on the emergence of publics).

I turn now to a point common to the three commentaries on my article: their vivid reaction to my description of the knowledge economy and the predictable destruction of our academic world. Each contributor accepted the concern but endeavored to propose alternatives to the “death knell of science” that they heard.

I was amused by the contrast between my “pugnacious” style and the announcement that “we” were defeated. I was even taken as proof that science is still alive. About that last point, let me briefly answer that I am not a scientist but a philosopher, and a philosopher who considers that her practice is “just surviving” (and has already been killed off in many countries by the attempt to mimic the authority of the sciences) and that the implementation of the rule of objective evaluation will finish destroying it—at least, as an “academic” practice. Under this regime, either I would never have got a university job or else, if I had, I am ashamed to imagine what kind of philosopher I would have been. Yes, I am alive; but I am defeated, because I know that my students will soon realize they must turn their backs on what turned me into a philosopher but belongs now to a romantic past. And the same is true for many scientists of my generation. In such a situation, recognizing defeat, as Péguy’s example shows, occasions a standpoint whose consequences need to be explored but that at least serves as a protection against cynicism, howling with the wolves, or complaining about unfairness or misunderstanding.

But who is the “we” that has been defeated? On one point, my article clearly failed. I was heard as foretelling the death of “good” science, or “normative” science, and as generally denouncing alliances with industry or even applied science. If I spoke about a “we,” it is in reference to an academic world that generally has lacked recalcitrance, the capacity to say no—to resist a technology of objective evaluation that insults us all, despite the plurality of our practices. Proclaiming defeat creates a time for togetherness, for “perfect” comparison as Charles Péguy defines it, when each of us may try to tell others, without rivalry, what is in the process of being destroyed, what each special strength of our practice was and its own way of divergence. And we may also feel a closeness with other (nonscientific) practices that have been destroyed.

This is what the ecology of practices is about: to give words to our defeat, our lack of recalcitrance—words that free us from nostalgia about our (academic)

world that is being destroyed. It contrasts the predatory nature of this world with the speculative possibility of practices disentangled from authority, from competition for power and legitimacy, and presenting themselves through their respective particular divergence. Ecology is about ways of life that are recalcitrant to unification, recalcitrant strengths, making each connection between them an event adding new dimensions to the world.

I must admit I was very surprised to have been heard foretelling the destruction of “good” science, as if it existed as such, as a matter of fact. What is being destroyed, at least when experimental sciences are concerned, is what Latour in his *Pandora’s Hope* figure 3.3 names links and knots, links between colleagues in the game, and knots between them all and that which they attempt to address. It is not “good” science. It is only an aspect of science, the one that makes its practical specificity. And it is also the one that is under threat, in contrast with the four other aspects described by Latour, mobilization, public representation, the process of autonomization of the discipline, and alliances with industry or the state. In other words, it is not “good science” that will be destroyed but what differentiates the ongoing construction of experimental sciences from the general category of “social construction,” or at least makes it a very specific kind of construction. What we call science was always dependent on the allies it recruited. As actor-network theory made clear, academic scientists were always on the lookout for so-called applications, actively trying to interest industries and the state. The “goose that laid the golden eggs” metaphor corresponds only to the unaccountability that academic scientists claimed for the use of the eggs. And the price they paid for their autonomy was to turn a blind eye on the complete nonautonomy of scientists working in and for industries. What is new is that this “alliance” has been broken. Allies have invaded the territory defined by concern for the crafting of links and knots.

Correlatively, the “temporal disjunction” described by Verran also belongs to the past, as it was indeed the very point of the construct that is being destroyed. The scientific “eggs” were laid in a very special environment, with demanding collective constraints, but their value as “gold” depended on another kind of environment. What would hatch out depended not on “links and knots” but on the market, while still benefiting from the eggs having been “scientifically laid”—that is, carrying the trademark of progress and rationality. When there is no longer a place for the “painstaking labor of solidifying comparison as a thoroughly reliable participant,” there is no longer an end point, each point being equally defined in terms of speculative opportunities. I am not sure that researchers will be able to trace out, as Brit Ross Winthereik proposes, the path followed by research proposals and all that, at least where biotechnology or nanotechnology are concerned. Secrecy may well prevail now on many occasions. Just try to enter into Monsanto files. I am also very doubtful about the possibility that the relations

between these researchers and scientists “partnering” in research on genetically modified organisms will resemble the relations between Verran’s Aborigines and their scientists. Also I would insist that the knowledge economy not be confused with what, in the commentaries on my article, is called industry.

What we are dealing with is not a “powerful monolith,” as Winthereik remarks. We never were. However, I would propose that the knowledge economy characterizes a process of destructive redefinition that concerns both academic sciences *and* industries. The same despair, cynicism, and resentment are invading both scientists and industrial workers. For these workers also, the possibility of giving some definition to an “end point” is disappearing as production has become an element in strategies, sometimes a rather indifferent element when compared with shareholders’ satisfaction, for instance, or strategic games. If ethnographers want to observe a process of decay, let them go to France Telecom, where suicides multiply; let them observe seminaries or self-help initiatives organized around harassment and burnout. Working “for” an industry is quickly becoming a thing of the past, just as working for reliable or interesting or relevant knowledge is.

If I am fiercely resisting the “transformationist” temptation, it is because too many souls are despairing or dying today in the name of flexibility to add insult to what they already suffer: the loss of meaning, pride, the feeling of being part of an adventure, whether scientific or industrial. My question is not what each of us shall do in order to go on. It is rather how to resist—not in the name of the past, but in calling for a different future.

My hope is slim, so slim that it seems to have escaped the attention of my commentators. It depends on what relation is created between scientists and those who struggle for a sustainable future and want relevant, reliable knowledge to be produced. Which is to say they do not need academic geese claiming that their eggs are golden, but rather accountable interlocutors whom they can trust when trying to “make public,” to turn into a public affair, the “temporal disjunction” that occurs any time that knowledge may have a bearing on the future composition of our worlds.

BINARY LICENSE

Marilyn Strathern

The textual person is composed by combining distinct relations: although *data/theory*, *spoken/unspoken*, *originality/analytic precedence*, and *literal/figurative* are kept scrupulously separate, they are also combined according to various kinship-like structures.

— *Tony Crook*, *Exchanging Skin*¹

There was one respect in which my first spell of fieldwork in colonial Mt. Hagen, Papua New Guinea, was like being at home, though I did not phrase it in that way at the time. In the 1950s, I walked about anywhere at home, day or night, with minimal attention to safety or security. Hagen too was peaceful. There were plenty of local disputes and troubles and frustrations, but people had embraced “the law” (and “business”); and a tiny force of expatriate administrative officers could report that the countryside was pacified. There would be no more “tribal fighting.” Hageners were prepared to credit the idea of “government” that came along with law—but I will leave this episode there and return to it in the end.

1. For the “textual person,” see Tony Crook, *Exchanging Skin: Anthropological Knowledge, Secrecy, and Bolivip, Papua New Guinea* (Oxford: Oxford University Press for the British Academy, 2007), 218.

Recently, I returned to an account I had written in the 1970s of migrants from rural Hagen to Port Moresby, the capital city, and found both what I was looking for and something else.² What I had recalled was a supposition about Hageners' encounters with the many residents of the city who hailed from all over the country. But I also found I had remarked on how designations of origin presented themselves in two forms. Names of populations were derived from distinct areas or places, so a mixture of "tribal" names and "place" names emerged that look "ethnic." One way of designating origin was a nesting, though a shallow series of the kind familiar to Euro-American taxonomy. Papua New Guinea was divided into Papua (lowlands and coastal) and New Guinea (lowlands and highlands); "The Highlands" were divided into Hagen, Enga, Chimbu, and so on; and Hagen was divided into council districts, though only if people were especially familiar with the area. The other way of designating origin we could call antitaxonomic, given how terms crossed different levels.³ A unit at one level, I wrote at the time, could be in counterpoint against a unit at another, such as in the real-case example of conflict between "New Guinea" and "Goilala" (the country name versus the [Papuan] people's name).⁴ Perhaps the clumsiness of my comments points to their origin not in what I was explicitly told but in my Euro-American, social-science gloss. What began by looking like the workings of a segmentary, and in places binary, nomenclature was not consistently keeping the terms in place.

As I looked back on my work of the 1970s, binary division was on my mind, since I had intended to open the present article with the enactment of such a division. Before starting to write this article, I had been trying to finish an account of changing disciplines in the social sciences—I had reached an impasse and was resorting to a distinction to get me out of it. Only the distinction was too complex to "add on" to the argument, and I set it aside for use in another paper: *this* one. What I had been writing about was the role of repetition in the ideas that purportedly uphold differences between disciplines, and I was concerned with the language of social-science analysis. I drew heavily on Andrew Abbott's concept of "fractal distinction."⁵ In essence, Abbott says that the preoccupation with certain distinctions, such as that between positivist and interpretivist styles of comparison, or between quantitative and qualitative methods, is repeated for all kinds of argumentative splits. Such distinctions flow across levels, as happens when the positions that divide biological from social anthropology get labeled by the same distinction that separates different positions within social anthro-

2. Marilyn Strathern, *No Money on Our Skins: Hagen Migrants in Port Moresby*, Bulletin no. 61 (Port Moresby: New Guinea Research Unit, 1975).

3. "Antitaxonomic," since of course the notion of crossing levels contains a taxonomic presupposition.

4. Strathern, *No Money on Our Skins*, 288.

5. Andrew Abbott, *Chaos of Disciplines* (Chicago: University of Chicago Press, 2001).

pology.⁶ The distinctions may or may not precipitate enduring structures, and Abbott lays some emphasis on crossovers and asymmetries that mean that “lower level” applications of a distinction may have quite different effects from “higher level” ones. It is the distinctions that are the constant.⁷

There came a point in the account of fractal distinctions when I had to divide that paper off from what I was planning for this one, and I was going to make some play with the fact. The point of bifurcation came at the moment when I could no longer hold certain terms steady but needed to make evident what I was assuming in the relationship of the language of description or analysis to the object of study.⁸ It was this distinction that overcame the impasse.⁹ I simply could not proceed without being explicit about the relativity of (to put it crudely) analytical and indigenous concepts. More specifically, I had been borrowing some images related to notions of personhood in Melanesia, and it became impossible to justify the comparison involved in using them to deal with Euro-American social science without opening up this divide. However, the distinction I had wanted to elaborate in the present article has also come to organize the relationship between the two articles. If the earlier one focused on the knowledge practices of Euro-American social science, this present one has inadvertently privileged understandings deriving from elsewhere. Moreover, I had intended the story about Hagen migrants to take just an illustrative paragraph or two—but my chancing upon a second pertinent issue makes that material more central than I had surmised at the outset.

What for Abbott the sociologist is of intermittent concern (“Every now and then social scientists recall that their ideas are as contingent on themselves as on their objects of study”) is for present-day anthropologists a constant issue.¹⁰ In anthropology, the distinction between language and the object of analysis proliferates at all levels of theory and description. Indeed the distinction between theory and description (or between “data” or “ethnography”) is closely related

6. For a real-life example, see Rena Lederman, “Unchosen Grounds: Cultivating Cross-subfield Accents for a Public Voice,” in *Unwrapping the Sacred Bundle: Reflections on the Disciplining of Anthropology*, ed. Daniel Alan Segal and Sylvia Junko Yanagisako (Durham, NC: Duke University Press, 2005).

7. While such distinctions may appear to lead down forks in a road, they arise from a process that works without maps or plans—they simply arise from the replication or repetition of a formula. All that happens is that a position already occupied is occupied again (unless the circumstances are identical, a historical impossibility, the effect is not-quite-similar repetitions).

8. “I find myself having to divide this paper from another, to which it thus becomes a companion. I should add that I did not start out intending to do so—the pressure of competing perspectives forced this on me at the point at which my comparisons spilled over into Melanesia. The companion paper picks up on an iterated distinction common to anthropology, between the language of description and the object of study.” Marilyn Strathern, “Innovation or Replication? Crossing and Criss-crossing in Social Science” (lecture, “Changing the Humanities/the Humanities Changing,” Centre for Research in the Arts, Social Sciences, and Humanities, Cambridge University, July 16–18, 2009).

9. Abbott, *Chaos of Disciplines*, 230–31.

10. Abbott, *Chaos of Disciplines*, 15.

to it. I am not concerned with whether other disciplines have long dispensed with these distinctions or whether they result in pseudoproblems. My point is to draw attention to the power of bifurcation in how we (anthropologists, writers) compose our texts.

In accounts of proliferating concepts, it is often the endlessness of repetition, the scattering of points that result, the networks that take one everywhere, that are of interest. I dwell instead on the point of bifurcation, the moment of division, which need not take a binary form but very often does. It is the moment at which a distinction between terms could lead analysis down different routes. The distinction I have just discussed could take the anthropologist either into theorizing about reflexivity or into elucidating ethnography—or, as the reference to “relationship” indicates, it could invite both explorations at the same time. For a distinction between terms also maintains them in relation: they can still be found in one another’s company, to be repeated, at any juncture, later.¹¹ We can repeatedly bring ourselves back to the point from which we started. In short, distinctions can keep terms from dissipating.¹² When we see them doing so, what is fractal in propagation appears binary in form.

Using One’s License

This symposium brings two terms, *comparison* and *relativism*, into conjunction; and the unlikelihood of their pairing is of course a sheer provocation. But I dare say that, instead of finding that the terms get more radical as they are discussed—more pushed apart—they will be held together by the very distinction that the organizers have laid out. This distinction is between, on the one hand, taking a viewpoint to compare what is thus externalized from the point of comparison and, on the other hand, occupying a context that makes everything contingent on its own particularities. And perhaps the urge to elaborate the previous sentence by calling the latter an “internalist” viewpoint comes from the handiness of the habitual distinction between inside and outside. Here is an example of what I mean: adding the dimensions of inside and outside would work

11. I have in mind the regular realization that the difference between what one might ascribe to Melanesian and to Euro-American viewpoints is already the viewpoint of Euro-American discourse, and it is a realization to be encountered at any order of analysis. Otherwise put, the point at which, say, Melanesian material is generalizable is in its encompassment by Euro-American epistemology—generalizable, then, solely as a product of Euro-American constructs and in its reflection back on them. Becoming aware of one’s own epistemic practices in dealing with interlocutors who may or may not share them is a familiar experience for a social anthropologist.

12. A distinction sets up a dynamic of oscillation, so however exploratory or adamant adherence is to one side of the distinction or the other, the thinker (the concept-user) is brought back to its relation with the other term. I have been influenced here by Alberto Corsín Jiménez’s writings on proportionality (e.g., “Managing the Social/Knowledge Equation,” *Cambridge Anthropology* 28.3 [2008/9]: 66–90).

to keep the two components of the initial distinction (*comparison/relativism* or *viewpoint/context*) together even more closely. Whereas one might otherwise say that it is only with some difficulty that such terms can be compared.

A feature of the kinds of bifurcations I am describing for Euro-American knowledge practices, specifically in social science, is that distinctions easily contain or lead to other distinctions (either by replication of the same at some other site or by being brought in from a different dimension, as I just brought in the *inside/outside* distinction). The terms of a bifurcation do not have to be opposites or to constitute pairs, nor do they need to cut a whole into dichotomous halves. A binary move simply allows an argument to take off in one direction by rendering another (direction of argument) also present. I think there is a kind of arithmetic here that unitizes or individualizes the concepts that drive arguments—and any concept can be individualized—so that there is great latitude in what can be brought into conjunction. In much the same way as Euro-Americans imagine that any persons, however anonymous or foreign to one another, can set up a relationship between themselves, so concepts previously unrelated can be brought together. In this epistemology, a binary move can either call on a stock of well-tried and thus traditional distinctions, such as interested Abbott and Rena Lederman, or throw up newly invented ones.¹³

I wonder if one of the facilities of a binary distinction is to render the multitudinous aspects and characteristics of things, the infinity of particularisms, as though there were invariably a *choice* of emphasis to be made among them. And I wonder also if this feature might be one element of a perspective that Euro-American knowledge practices encourage. What Lederman identifies as positivist-versus-interpretive, or essentialist-versus-contextualizing, ways of knowing offer a choice of grounds—positions from which to take a stance—whether the issue at hand is on the place of the humanities in the university or the merits of particular methodologies in a social anthropology curriculum.¹⁴ “Making a choice” is not necessary to “taking a stance,” but it seems that practice often has it thus. So when a population of academics divides on an issue, it seems they have chosen to take a particular viewpoint (a “constructed” position).

This observation sits side by side with the apprehension that the world is full of persons, known or unknown to one another, who all have their own viewpoints: they will make up their own minds (consciously or not) about how they “choose” to see things.¹⁵ In other words, “choosing one’s stance” belongs to the

13. Lederman, “Unchosen Grounds.”

14. Lederman, “Unchosen Grounds,” 50.

15. I am including here situations where people are reconciled to the circumstances even if they can do little about them. One might want to distinguish these radically from situations where opportunities allow for different pos-

sibilities, but the prevalent Euro-American discourse of improvement, the idea that people (by and large) do not have to put up with their conditions of existence, denies irrevocability.

epistemic universe that has been labeled “perspectivalism.” The name neatly captures the idea that perspective creates a singular viewpoint, and if there are many of them—as there are—then they exist as a plurality.¹⁶ The name comes from John Law, who expounds, by contrast with perspectivalism, the “multiplicity” inherent in ways of knowing and acting described so graphically by Annemarie Mol in relation to Euro-American medical knowledge.¹⁷ The multiplicity of objects in the world is, she argues, an effect of constantly shifting contexts of knowledge. She deliberately takes this conceptual stance against the widespread language of fragmentation that perspectivalism generates, for perspectivalism (I would add) rests on a mathematics that seeks to resolve the world into discrete entities: a unit that does not seem to hold its parts together seems thereby to fragment. By contrast multiplicity implies entities joined with, and disjunct from, other entities that cannot be reduced to a plurality gatherable into wholes. Multiplicity also exists, in this view, in the numerous but invariably overlapping practical contexts that elicit diverse enactments of knowledge. Multiplicity is perspectivalism’s critique of itself.

It goes without saying that perspectivalism, together with its self-critique, is the antonym of perspectivism, at least of the Amerindian sort (and also of the sort found in Inner Asia).¹⁸ To be perspectivalist acts out Euro-American pluralism, ontologically grounded in one world and many viewpoints; whereas perspectivism implies an ontology of many worlds and one capacity to take a viewpoint. Making this distinction will introduce a turn in my language of argument. I now want to take back the notion of *division*, though I have already used the word, and reserve it for an epistemology/ontology much closer to perspectivism than to the critiques that perspectivalism elicits. I want to reserve *division* for a different mathematics altogether.

Divisions

It should also go without saying that the perspectivist tenet that there is no universe yields a different kind of relativism from the kind in which any viewpoint can be mobilized as, so to speak, the viewpoint of choice. For the latter kind,

16. I take plurality, in turn, as a state implying a composition of definably singular or discrete units.

17. John Law, *After Method: Mess in Social Science Research* (Abingdon, U.K.: Routledge, 2004); Annemarie Mol, *The Body Multiple: Ontology in Medical Practice* (Durham, NC: Duke University Press, 2002).

18. Eduardo Viveiros de Castro, “Cosmological Deixis and Amerindian Perspectivism,” *Journal of the Royal Anthropological Institute* 4.3 (1998): 469–88; “Perspectival

Anthropology and the Method of Controlled Equivocation,” *Tipiti* 2.1 (2004): 3–22; “The Crystal Forest: Notes on the Ontology of Amazonian Spirits,” *Inner Asia* 9.2 (2007): 153–72; Morten Pedersen, “Totemism, Animism, and North Asian Indigenous Ontologies,” *Journal of the Royal Anthropological Institute* 7.3 (2001): 411–27; Morten Pedersen, Rebecca Empson, and Caroline Humphrey, “Inner Asian Perspectivism,” *Inner Asia* 9.2 (2007): 141–52.

relativity lies in acknowledging the contingency of “choice” (or life chance) and the possibility of numerous other viewpoints. The relativity of perspectivism, on the other hand, rests in the absoluteness of the body one inhabits; and it works by engaging the view of another upon oneself.¹⁹ Now, if non-Amerindian extensions are permissible, then that perspectivist reciprocity may be between human as well as between human/nonhuman entities.²⁰ Or, in the Melanesian situation where the term *human* is redundant, perspectivist reciprocity may be between “social persons” (parties to a relation). Persons offer perspectives on one another because of the relationship between them. Their being locked into each other in this way, whether symmetrically (as between enemies or brothers-in-law, regarded as men in partnership) or asymmetrically (as between mother’s brother/sister’s son or brothers-in-law, regarded as wife-giver/wife-receiver), creates the relation in question. It is always a particular relation and is created as the point of view from which other relations are perceived. I have also described this process in terms of *division* or *partition*—in terms, that is, of the manner in which persons divide (parts of) themselves off, either from themselves or from others, as sister’s son divides himself from his mother’s brother. It sounds awkward in English, but I am referring to the relationship that is divided between them. I say *division* because the word resonates with a mathematics that presupposes the constant division of an entity into parts of itself, so that multiples are fractions of one.²¹

As already indicated, this division takes two forms, symmetrical and asymmetrical.²² Symmetrical divisions are found, for example, in the dynamics of clan or tribal “groupings” in Hagen. Whereas in systems such as those of the Iquaye the highest order unit is the one cosmic being, of which the world is so many innumerable partitions, in Hagen the sense of a unit fades beyond that of the largest political (tribal) categories.²³ So while, hypothetically, people can group tribes into regional phratries or present-day council districts, distinct tribes can also be seen as so many entities in parallel, each one itself a multiple, being internally in a state of potential divisibility. Connections between such groups are

19. Martin Holbraad and Rane Willerslev, “Transcendental Perspectivism: Anonymous Viewpoints from Inner Asia,” *Inner Asia* 9.2 (2007): 329–45. Note that Holbraad and Willerslev also say that relativity makes comparison impossible.

20. Cf. Pedersen, Empson, and Humphrey, “Inner Asian Perspectivism,” 143–44. The formula of course queries the ground of what is meant by “human” (cf. Viveiros de Castro, “Perspectival Anthropology,” 16).

21. Such division is taken notionally as segmentation—, for example, in the splitting of higher categories into lower ones, in order to receive larger portions of a payment; or as in fission, where distinct identities become permanent (to use the vocabulary of unilineal descent group theory).

22. I am using the terms here in a way different from that found in Pedersen, Empson, and Humphrey, “Inner Asian Perspectivism.”

23. For a comment on the classical example from Iquaye (Jadran Mimica, *Intimations of Infinity: The Mythopoeia of the Iquaye Counting System and Number* [Oxford: Berg, 1988]), where the counting system literally follows suit, see Roy Wagner, “The Fractal Person,” in *Great Men and Big Men: Personifications of Power in Melanesia*, ed. Maurice Godelier and Marilyn Strathern (Paris: Editions de la Maison des Sciences de l’Homme, 1991), 167–68.

often managed as individual enmities and alliances (including intermarriage), on an asymmetric model. Asymmetrically, the language of division also resonates with the partibility presupposed by gendered relations, where male is divided off from female. Instead of a person, so to speak, being divided into similar same-sex versions of itself (like two subclans of a clan), the person may appear to eliminate or assimilate cross-sex attributes (as when “males” shed “female” attributes or when a “father” shows he is a kind of “mother”).²⁴ Gender is the prevalent rubric in Melanesia under which such analogies are visible, which is why I invoke it here; and indeed it offers an indigenous source of metaphor. It can be applied to intergroup encounters in such a way as to render, in the classic case, donors separate from recipients, who become locked into a relationship as though each were divided from the other. In neither case is there any *choice* of perspective. A person is held by the relation (and it is always a specific relation) of the moment: parties to a relation exist in that relationship for each other. This feature may be rendered visible through its embodiment in a third party, as in the case of brothers-in-law whose very tie is constituted in the difference between their separate relations to the person who is sister of one and wife of the other.²⁵ Indeed, in Hagen idiom, it is she who divides the two men.²⁶ What such a relationship does is create a universe of relations that turn on its enactment. In occupying different positions, then, a person switches not individual viewpoints but relationships.

Amid all this complexity of relations, comparison seems either omnipresent or superfluous. Yet there is an element in these relations that one might wish to highlight by the term *comparison*, and it is an element occasioned by considering what happens when people move away from the relationships with which they are familiar into an unfamiliar world.

Mixing in Moresby

What had I gone back to find in the 1970s material on Hagen migrants to Moresby? I had always been struck by something I could not quite put my finger on in the migrants’ orientation to non-Hageners, whether local inhabitants or immigrants like themselves. In the end, I speculated that the Hagen migrants were making comparisons based on moral analogy.

24. Marilyn Strathern, “Gender: Division or Comparison?” in *Practising Feminism: Identity, Difference, Power*, ed. Nick Charles and Felicia Hughes-Freeland (London: Routledge, 1996). Partition engages “whole” reorientations, as when a man switches from one relationship to another/is switched by the other party to that relation (for example, in thinking of the children of his brother-in-law [to whom he is male affine] as the children of his sister [to whom he is another kind of mother]).

25. Viveiros de Castro, “Perspectival Anthropology,” 19.

26. That division is translated very often, for the woman, into relations fraught with tension for which she is held responsible (the starting point of the original Hagen ethnography).

Hageners relished their individual contacts with people from elsewhere and, even when there was not much depth to the relationship, would take pleasure in pointing to whom they knew. There was some intermarriage, and some of those who intermarried spent so much time away that they “turned into” the people they were with. In talking to an outsider, people designated such contacts by their area (place) or origin (tribal) names, the same names that surfaced in rivalries and conflicts between whole congeries of people. Antagonism that involved collectivities would escalate by the degrees of inclusiveness—and of ignorance. Stereotypes attached to different categories of people abounded. Hagen men (from the Western Highlands) were only too aware that they could be classed as “Highlands” or as “Chimbu” (from the Eastern Highlands) by other city dwellers who (in seeking vengeance for an injury, say) would pay little attention to internal differences.²⁷ Indeed, interest groups formed on the basis of these area or origin names were labile: “The advantages a particular group claims at any one moment,” I wrote in 1975, “may be to the disadvantage of other groups of *any* span”—and although I used the epithet *ethnic*, I concluded that we could not really talk of ethnic strategizing as was current in anthropological thinking at the time.²⁸ I now think I should not have used the word in the first place, and the reason will I hope become clear.

In effect, the pitting of collectivities against one another was sporadic, and I would not even say that a latent possibility for conflict was what made the names so salient. The names were, for these Hagen migrants, doing something else.²⁹ For a start, the names meant that no one was anonymous. Everyone had an origin that could be specified: not knowing a personal name did not render someone unknown, for everyone also bore an origin name. And even if one did not immediately know somebody’s origin name, one knew that the person had such a name and, moreover, knew that the name would signify the order of connection with oneself. To rehearse a Melanesianist aphorism, people were already related. There is a further point, for which I now introduce a term, *analogy*, that I did not use in the original account.³⁰ *Analogy* resonates with what also seemed at the time to be the best way to describe overall relations with others, whether individually or categorically speaking. I speculated that the Hagen migrants approached others as moral beings on the basis of a kind of comparison or resemblance: you might not know the particular marriage practices or “customs” of a particular set

27. One strategy of aggravation was in any event to pick off victims with neighboring affiliations to cause maximum trouble for the alleged perpetrators, who would find themselves with another set of grievances on their hands.

28. Strathern, *No Money on Our Skins*, 289; new emphasis.

29. I offer thanks here for comments from Almut Schneider (in a personal communication).

30. I use the term with some similarity to Pedersen’s “analogous identifications” for North Asian “animism” (“Totemism, Animism,” 416), though I do not imply the reflexivity of parties each imagining him- or herself in the other’s situation. Rather I would suggest that each knows the other in the way that the self is known—for example, in that each comes from a particular social context.

of people, but you knew that, like yourself, they observed (their own) protocols.³¹ They were embedded in their own context, with its demands and mechanisms of support—just as you were. People lived, in the vocabulary I was using then, in comparable moral communities. *Analogy* in English has a useful spin-off, insofar as the word indicates how one may say or know about one thing through knowing about another thing. We could observe that a Hagen migrant in Moresby had a shrewd idea about how a Mekeo or Goilala might respond to an interaction because he had a shrewd idea about his own reactions. But I also have a strategic use for the term *analogy* and want to reserve *resemblance* for another phenomenon.

The basis of comparison here was not any common ground or viewpoint outside the entities being brought together, as one might imagine Euro-American appeals to humanity or citizenship lie outside.³² On the contrary, my impression was that people lived so to speak parallel lives. When they wanted to point to common circumstances, they turned the association into one of presumed unity (*wantok*, *wantrening*, *wanwok*, etc., in neo-Melanesian or pidgin English), but this unity was itself a further series of identifications (based on language, education, and workplace, in the examples here; but the possibilities are infinite) that were parallel to the names. In this universe of many parallel positions—of “the replication of like units”—any particular encounter beyond close acquaintances could summon the notion that one was dealing with someone in circumstances analogous to one’s own.³³ Their world might be largely unknown, yet to this extent the general behavior of others (whether an interest in forging relationships or a reaction to an injury or insult) was predictable.³⁴

As I have picked my way through these descriptions, none of them new, I have made evident those junctures where there has been a choice of vocabulary. Doing so is in part routine scholarly (Euro-American) practice but is also a

31. Hence the practice among different Highlands people, however slightly acquainted, to greet a person using his or her *own* term for “brother” or “sister” (Strathern, *No Money on Our Skins*, 276–77). (There were few women from Hagen present when I conducted that research, and I wrote only of men in the body of the text.) I am not referring to the commonality of brotherhood mentioned elsewhere in this article; each Highlander is irreducibly locked into his or her own context, but with the capacity for extension or relation to others.

32. I write of the ethnographic present of the early 1970s; things are no doubt different now. Viveiros de Castro makes repeatedly the point I make here, and with the same example (common humanity); he likewise brings in Euro-American knowledge practices, in which the idea of a relation is that terms have something in common—namely, the *same* relation to a third term—in contrast with a situ-

ation where relations are created by *different* ties to a third party. See, for instance, Viveiros de Castro, “Perspectival Anthropology,” 18–19.

33. Quotation from Strathern, *No Money on Our Skins*, 289. As for “circumstances,” I think I mean ideational or moral circumstances, possibly in echo of the Amazonian “soul,” since obviously people are separated by fortune and opportunity.

34. For the same reason, one can fear the unknown intentions of others, whether of those who are closely related or of distant persons known only generally. That is, there is no protocol of anonymity, no supposition that because they are “strangers” people will not have anything to do with one another. One may, deliberately or inadvertently, draw anyone into one’s affairs.

special proclivity of anthropologists, given their sensitivity to the relationship/distinction between the language of description and the object of study.³⁵ A main reason for my going over this old material in this way is to reconsider what on the surface looks like a familiar ethnic taxonomy (in the deployment of collective names and in the apparent segmentation of higher order and lower order “groups”). The further, and to my mind crucial, point about the comparisons being made among the city residents—namely, that no common ground need be summoned—may get overlooked.³⁶ For while I think there can be similar effects in the way people mobilize themselves, wherever they are, so that a dispute between “Hagen” taxi drivers and “Papuan” market traders looks like any other ethnic conflict, I do not think that the ideational route by which the resemblance appears (between the Hagen/Papuan standoff and the “ethnic” classification) is a single one or has a common origin.³⁷ The same goes for the appearance of bifurcations and binary divides. Laboring, as I have been doing, over the choices one may wish to make with the terms of analysis for people’s relations with one another will yield, I hope, some dividends for the conceptual work that anthropologists also do in setting up comparisons and talking about one set of materials through another set.³⁸ What I wish to suggest is that the kind of binarisms that enable so much intellectual creativity in scholarly exposition *work with a certain conception of relationship*.

Counterparts, Counterexamples?

With Melanesian materials in mind, I have reserved the concept of division for the elucidation of certain social distinctions in evidence there. I now want to reallocate the term resemblance and (for all that I have used it in writing about analogy) make clear that resemblance also belongs to a Euro-American language of analysis. I introduce the Balkans in doing so. Now in the Balkans, it looks as though there is no escape from ethnicity. I shall not dwell on the implications for political action entailed in getting the description right; they should be evident. Indeed Balkanization feeds hugely on self-description and the ascription of others. So I am not turning away from the politics when I refer to this region for a

35. Hence one might wish to labor a distinction between saying that sons have a common origin in their father and saying that in his sons a man produces multiple versions of himself—the one phrase being immediately intelligible in English, and the other too obscure for words (one will always sound more awkward than the other).

36. Nor need common ground be summoned when person is divided from person: succinctly put, fathers and mothers are not different kinds of “parents,” but fathers are a kind of mother and vice versa.

37. In an earlier exercise, I had drawn a distinction between *division* and *comparison* (in the way that gender ideas can be described for Melanesia and Euro-America). See Strathern, “Gender: Division or Comparison?” to get at something rather similar.

38. Crook is even more specific for Bolivip (*Exchanging Skin*, 218). Bolivip composes knowledge after the form of the person.

purpose that also serves this article. The purpose is to explore certain processes in social theorizing a bit further.

There was no need for me to demonstrate that the phenomena from Hagen or Port Moresby that I was describing reflect a species of perspectivism. My point was and is that anthropologists who write about Amerindian or Inner Asian perspectivism provide a vocabulary for talking about those phenomena that is closer to what I want to convey than is the conventional language of ethnic differentiation. When I draw on Eduardo Viveiros de Castro's model, I am in effect redescribing Amazonian sociality in using it to think through its Melanesian counterparts—which is a good example of what I mean by *analogy*. Otherwise put, I am repeating a position taken in respect of one set of materials for another set, and the effect is not-quite-replication. There is moreover no need for me to demonstrate—indeed doing so would on the face of it be absurd—that the Balkans are Euro-American, in order to suggest that they offer an insight into Euro-American knowledge practices, whether modernist or postmodern. It is, rather, that the stereotype of deep commitment in the Balkans to ethnic solidarity and differentiation as matters of primordial identity³⁹—as if segmentation and partition dislocate society, leaving disconnected fragments—is a good comparator for such knowledge practices.⁴⁰ Moreover, there is also an argument to be made about closeness. People of the Balkans evince an epistemology that seems closely enough related to Euro-American epistemology (so to speak) that the closeness can upset social theorizing about both.⁴¹

The long history of the Balkans, from a Euro-American (and especially modernist) perspective, is composed of countless accounts that push them to the edge of Europe. The chaotic way in which identities proliferate and fragment there makes them the antonym of regular Euro-American understandings of the formation of identities based on personal and group allegiances. One could stop at this point, of course, since the antinomy makes my point. However, it is worth expanding a little; and I take Sarah Green's reflections on her ethnography as my guide.⁴² Green is clear that the chaotic and fragmented picture that the

39. I am having to compress a subtle set of arguments here. Sarah Green relocates this essentialist viewpoint in respect of contemporary "multiculturalism" and the new fractal patterning that it has elicited. See Green, *Notes from the Balkans: Locating Marginality and Ambiguity on the Greek-Albanian Border* (Princeton, NJ: Princeton University Press, 2005), 155–56.

40. I began this argument in the companion paper but take matters here in a different direction. Anthropologists will appreciate that "the Balkans" is rather like "Melanesia" or "Amazonia" insofar as it is an epistemic field for countless accounts of it. (In other respects, of course, I am

arguing for a contrast.) There is no point in objecting that these are wild generalizations or in raising specific points in contradiction, since both moves are encompassed in the overall term.

41. Though, for an example where the issue seems by contrast *too little* relation—with a congeries of people (the Mountain Ok, Papua New Guinea) whose knowledge practices have repeatedly defeated anthropological attempts at analysis—see Crook, *Exchanging Skin*.

42. Green, *Notes from the Balkans*.

Balkans present is an image of them from a Euro-American perspective that is making comparisons with polities elsewhere. At the same time, she is clear that the perspective is hegemonic, for it has put in place, locally as well as internationally, a vocabulary for understanding the Balkans that, to put it crudely, influences people's experience of conflicts and solidarities.⁴³ What is hegemonic, we might say, is understanding or knowledge that propagates fractally.

So Green examines the fractal imagery that is implicit in many descriptions (and that is, in some cases, an explicit tool of analysis by outside observers) to bring out the constant application of formulae of differentiation (for example, Albanian and Greek, in the area where she worked) and of the way that parts continually proliferate into more parts. Yet the Balkans, lacking stable distinctions between one thing and another, seem always to throw up “messy remainders” from any orderly accounting.⁴⁴ Indeed one might observe that the very application of a distinction has its own effect, for there is always another dimension implied. Something is left over from making a distinction, not least people's knowledge of other applications (Albanian and Greek applications, say, in respect of European Union development projects, rather than in respect of a population or environmental census).⁴⁵ One effect is for people—locals, observers—to perceive connections everywhere. The Balkans are in a state of permanent disconnection and, at the same time, form a crossroads for influences from every direction. There is too much relationship there, as Green crisply says, rather than too little.⁴⁶

A presumption that solidarity follows similarity—a kind of enlarged familism—has to be the premise upon which division is seen as “fragmenting” society.⁴⁷ So perhaps it is *resemblance* that defeats people in the Balkans. For in making connections it would seem that they endorse the particular kind of relation that Viveiros de Castro describes as pertinent to Euro-American epistemology/ontology: this is a relation that presupposes common ground so it is the similarity of the parties to each other that brings them into relationship.⁴⁸ His archetypal image of an Amazonian relation is that between brothers-in-law (as differentiated by the sister/wife), while the archetypal relation in the Euro-American sense is that between brothers, which is surely at the heart of ethnicity. Brothers ought to have interests in common.⁴⁹

43. Green, *Notes from the Balkans*, 142.

44. Green, *Notes from the Balkans*, 144.

45. Green reminds the reader of the difference between the application of unvarying formulae in fractal patterning, mathematically described, and the heterogeneous influences on fractal discourse about places and people—thus reminding us of the limits of the fractal analogy. See *Notes from the Balkans*, 153; also 142.

46. Green, *Notes from the Balkans*, 129.

47. To press home the point: nothing comparable to a “family” as a domestic group could be at the heart of Hagen intimacy when households are based on a categorical division of the spouses' social affiliations: the hearth (and kinship) is divided at the source.

48. Viveiros de Castro, “Perspectival Anthropology,” 18.

49. Hence the drama when brothers do not have much in common and the intense interest in inequalities generated (for instance, through inheritance laws).

The proliferation of viewpoints in and about the Balkans—about its relational fragmentation—has an epistemic entailment: any entity divisible into its attributes can, depending on the attributes selected, be redescribed as part of another entity.⁵⁰ All the bits that make up “Greek” identity are already (so to speak) connected elsewhere too. At each enactment, however, the (relevant) connection has to be chosen.⁵¹ Should one think of Greece as fundamentally “Eastern” or “Western”? That moment of selection, the bifurcating road, produces a binary divide between the choice of direction and other possibilities. This notion of choice is a prop to the notion that relations (actively) link terms, parties, and entities, so that links can be made (more or less) anywhere in this regime of plenitude. Despite many critiques of this latter assumption, it is over and over again the terms, not the relation, that are regarded as prior, which means that Euro-Americans and all those they recruit to this stance continue to live in the perspectivalist world of things with preexisting attributes—the world that Mol describes.⁵² Hence the effort: one strives to create connection. Implicitly, there has to be a *cause* for a relation. To follow Vivieros de Castro again, relations are archetypically detected through (some form of) resemblance between the terms.

Here we see some of the crucial work of binary distinctions: they maintain intervals between terms, simultaneously keeping them connected and establishing the terms as prior to the relation that shows up the resemblance (the common ground) between them. Binary distinctions are thus a particularly powerful enactment of the kinds of connections that must be made, over and over again, in order to compose an exposition and that fuel the intellectual endeavor Abbott describes with respect to social theory. Quite obviously, this generalization does not suggest that there are not other epistemologies that proceed with pairs, contrasts, or oppositions, any more than bifurcating decision-making is a Euro-American prerogative. And there are other ontologies that produce fractal patterns, of which the best described is the “fractal person,” an entity with relations implied, as we could well redescribe the moral analogies made by Hagen migrants in Moresby.⁵³ There each person is at once himself and sees himself in another; and the perception can be endlessly replicated. Relationship is implied: that other

50. Green, *Notes from the Balkans*, 130. Green herself draws a comparison (based on resemblance) to what in the context of English kinship/knowledge practices I formulated as a “merographic connection.” See Marilyn Strathern, *After Nature: English Kinship in the Late Twentieth Century* (Cambridge: Cambridge University Press, 1992). My argument about merographic connection is that any part of or term for a relationship could also be part of or a term for another relationship and thus be apprehended from another viewpoint.

51. In *perspectivalist* fashion, the choice will create a new field of relevance (a context) without eliminating other possibilities or modifying them to any great extent. Contrast the *perspectivist* transformation by which one enacted relationship alters the world one inhabits.

52. Mol’s critique is, of course, that the multiplicity and “thinginess” of this world is not only “constructed” but (and here Law agrees) has to be maintained.

53. Wagner, “Fractal Person, 159–73.”

person is already related, neither completely separate nor totally assimilated. The point is that, in the delicate process of describing social life, the anthropologist is faced with a constant choice of language. One would not want the heady binarisms of intellectual propagation to interfere with how other kinds of relations are seen to be enacted.⁵⁴ Yet seemingly one cannot do without the binarisms either.

Exemplification

Of course, I have been highly selective in my own weaving of this tale; the partial reading is evident. But what would a less than partial account look like? Partiality is the remaindering effect of being aware that so much more lies “out there,” so many accounts into which one could, with great intellectual profit, be locked. And there already exists an alternative and more comprehensive account than my brief rendition—Simon Harrison’s book *Fracturing Resemblances*, which manages to encompass Melanesia, among other regions, and takes a serious interest as well in ethnicity.⁵⁵ Harrison has a radical thesis about conflict, arguing that contrary to the received notion that it is differences that thrust people into conflict with one another, it is their similarities that do so. Similarities engender rivalries, he shows, and much of his book is concerned with the substance of rivalry—identity, property, culture, cultural property. Like Mol, Harrison turns on its head the prevalent perspectivalist model of differentiation, though to different (of course!) effect. Concerned not just with ethnic but also with national confrontations, he points to a countercurrent in Euro-American social science (Georg Simmel, Gregory Bateson, Anton Blok, and above all René Girard) that has always concentrated on what Freud called the “narcissism of minor differences.”⁵⁶ From Girard, Harrison derives the notion that conflict is itself an imitative process. Many other streams of thought feed into this one, including the idea that identity is inherently contrastive, and ethnicity always relational.⁵⁷

What Harrison adds is that people strive to assert their dissimilarities from others against an underlying homogeneity, exemplified above all in the exertions of nations and ethnic groups. From this observation comes his wonderful comment on how it is that national and ethnic conflicts show so little variety in the

54. In using the very vocabulary of “relations,” the anthropologist is deploying a powerful Euro-American tool—albeit one that unlocks all kinds of comprehensions of phenomena. Law in a “too much relationship” fashion, observes “that the world, its knowledges, and the various senses of what is right and just, overlap and shade off into one another” (*After Method*, 63).

55. Simon Harrison, *Fracturing Resemblances: Identity and Mimetic Conflict in Melanesia and the West* (Oxford: Berghahn, 2006).

56. Harrison, *Fracturing Resemblances*, 2–3. Freud’s comments on the “narcissism of minor differences” are in “The Taboo of Virginity” (1918), *Group Psychology and the Analysis of the Ego* (1921), and especially *Civilization and Its Discontents* (1929).

57. Thus, “cultural differences are social relationships” (Harrison, *Fracturing Resemblances*, 63). As I myself have suggested, in certain Euro-American knowledge practices binary distinctions work to keep terms together.

way they symbolize their identities and pursue their interests.⁵⁸ The mimetic nature of conflict may be openly acknowledged as such (which Harrison argues is the case in Melanesia) or it may be disguised, as in the altered cognition apparently fostered by ethnicity and nationalism (and prevalent in the “West”).⁵⁹ The latter engenders particular kinds of dissimilarities, he argues, that disguise commonalities now rendered covert. Such modes of identity are best viewed as forms of denied resemblance. Harrison sums up his argument by saying that much “difference,” so striven after, is to be understood as resemblance “denied, muted and fractured.”⁶⁰ By seeing this Western mode of identity formation as a concealed form of what Melanesians act out, overtly, Harrison has *no need to create a binary divide in his language* and indeed demonstrates his model across the global stage.

The result is a remarkable work of synthesis, which does not mean that the book does not offer particular insights, including with regard to colonial Melanesia, where Harrison’s interest is largely in what was involved in people’s strenuous efforts to differentiate themselves.⁶¹ His account seemingly echoes the way in which I have imagined persons being divided off from one another. But then he writes of “mutually hostile resemblances,” it being “a mutual likeness that divides.”⁶² Of course, with respect to Melanesia, I would be bound to rehearse the point that it is relations that divide (persons from one another) and moreover add that perceiving analogous relations is not the same as perceiving resemblances.⁶³ Harrison’s synthesis does not deny different conceptions of cultural identity. But it does draw back from making the language of description a central problematic. So ethnic conflict becomes another version of, if not positively paradigmatic of, what people do everywhere. It is partly this stance that encourages his general account of possessiveness, stimulated by present-day culturalisms (cultural identity, cultural property)—which turns out to be a message that may be more than acceptable in our (Euro-American, still modern) times. I imagine it being received as doing the important work of putting us all on an equivalent footing and, very nicely for a Melanesianist to savor, of showing how that part of the

58. Harrison, *Fracturing Resemblances*, 152.

59. Harrison, *Fracturing Resemblances*, 64.

60. Harrison, *Fracturing Resemblances*, 152.

61. Harrison introduces (without discussing further) a most interesting critique at the end of the book, in which he differentiates (!) dissimilar presumptions about the person. Arguing that it is the denial of resemblance that produces the “individuality” of the ethnic group (and other individuals, such as authors), he returns to the two contrary traditions in social sciences concerning social commonalities noted above, suggesting that both of them presuppose the social actor—singular or collective—as an individual. And he does so against the background of per-

sons differently construed in colonial Melanesia (*Fracturing Resemblances*, 154).

62. Harrison, *Fracturing Resemblances*, 5, 152.

63. It would be interesting to come, at this juncture, to the binary distinction I began with. If, as Holbraad and Willerslev (“Transcendental Perspectivism”) wonder, in a perspectivist vision “ethnography” would be to “theory” as body (the inhabited world) is to perspective (what the world makes one see), then in a fashion modified by analogic thinking, I might ask about parallel alignments and the effect that each (“theory”/“ethnography”) has on the other by being already in relationship with it.

world has something to say about conflicts elsewhere.⁶⁴ In short, Harrison points to common ground.

So perhaps it really does not matter that what may look like very similar disputes and troubles between congeries of people across the world have been created out of quite different social and conceptual antecedents. Who cares? We may all become like the Balkan peoples and accept a hegemonic description of conflict that makes ethnicity a universal model for loyalty and cultural solidarity. When one day we all use the same vocabulary, there will be no need to worry about where it has come from! Although, as Yael Navaro-Yashin would say, when it comes to “conflict resolution,” how antagonisms are effectively laid to rest may well depend very much on their antecedents in different social configurations.⁶⁵ In any case, I would defer the day as long as I possibly could.

And my vignette, from forty years ago, about pacification and peace in the PNG Highlands? Well, it was not long before misunderstanding, awkwardly pitched expectations, man-murdering vehicles, education, and too much/too little money did their work. After a couple of decades of quiet, mounting dissatisfaction with the introduced judicial system (among other things) led to a resurgence of tribal fighting along both established and novel alignments. Today such troubles, whether in the Highlands or exported to Moresby, look simply like continuity with what people imagine was the past—a matter of tradition. Present-day Papua New Guinean magistrates and civil servants, among others there, have been using the vocabulary of ethnic conflict for some time. The early interlude on which I reported is hardly recalled by anyone. Conflict described as ethnic is also being described now as inevitable.

64. See the critique of this widely applauded stance in Yael Navaro-Yashin, “Governing Social Relations Internationally: The Legal Management of Conflict” (lecture, “Legal Knowledge and Anthropological Engagement,” Newnham College, Cambridge, October 3–4, 2008). I am grateful for permission to cite this paper.

65. Navaro-Yashin, “Governing Social Relations Internationally.”

STRATHERN'S NEW COMPARATIVE ANTHROPOLOGY

Thoughts from Hagen and Zambia

Bruce Kapferer

Marilyn Strathern, recalling her Hagen ethnography, asks challenging questions about the character and potential of anthropological generalizations grounded in comparative ethnographic knowledge. In taking a new look at the issue of comparison, Strathern aims to overcome all sorts of dualisms (or simple binarisms), distorting oppositions, and simple negative inversions (whereby what is defined as distinct is frequently presented as the obverse of Euro-American realities). The bifurcation of difference that she stresses opens the potential of partial connections rather than absolute distinctions: differences that separate can also be integral with other aspects that establish connections through similarity. Strathern emphasizes the multiplicity of perspectives (akin to Alfred Schutz's "multiple realities"?) and refuses notions of coherent and discrete totalities or unities that characterized comparisons in the highly relativist anthropology of yore. This article of Strathern's, as also her recent work as a whole, pushes beyond the negative dialectics of modernism versus postmodernism versus poststructuralism that have governed anthropological and sociological debate and discussion, though less extensively than in philosophy and the humanities. I note, however, a leaning in her orientation toward the current technologically influenced discourse

of the posthuman. A major reference of her article is to the neorealist positions of John Law, Annemarie Mol, and especially Bruno Latour (who similarly stress connectivities rather than separations), as well as to Eduardo Viveiros de Castro's perspectivism. With regard to the last, she makes a distinction, on the one hand, between perspectivalism or perspectivism—the multiple positions of human and nonhuman within a single open, networked, horizontal field—and, on the other hand, the multiplicity of perspectives that are potential within a specific ontological context and that are united through the commanding viewpoint of the anthropologist.

Ontology (as Graham Harman has reminded us recently) is used in a great many different ways. One usage conceives of all human beings as united in the one ontology as a fact of their common embodiment. The differentiation within human being, or the differences that are ethnographically demonstrated between human beings, are such *within the one ontology*, every difference expressing a dimension of an ontological universal. So Jadran Mimica, whose Iqwaye research Strathern cites, is able to show how the apparently unique Iqwaye approach to numbers evidences an embodied human imaginary with general ontological possibilities, one that is likewise manifested in advanced mathematics. Another form of ontology—which, according to Strathern, is that of perspectivism in Eduardo Viveiros de Castro's sense—involves a more discrete embodied orientation that is cosmologically defined and enclosed. These different forms of ontology need not be mutually exclusive, which I think is Strathern's impression also (though she is ambiguous on this point), for she contests the idea that perspectivalism in the broad, primary sense makes comparison irrelevant and pointless. She claws her way back to the importance of comparativism (and to a more restricted concept of ontology) for the uses of anthropologists, in the face of apparently radical new perspectivist and neorealist directions in anthropology that, for the most part, are casting questions of ontology aside. Strathern can be understood as combining the two notions of ontology to which I have referred, the former underpinning connectivities that do not necessarily erase a more restricted sense that Strathern's approach to comparison would reveal. Unlike many anthropologists, Strathern does not necessarily equate ontology with culture, a conflation that is a factor in the dualistic relativism (bifurcation?) that dogs much anthropological comparison and is still apparent in some who are trying to develop alternative approaches. Thus she suggests that the risks of a dualism are evident in Viveiros de Castro's perspectivism (as applied to Amazonia), as well as in Simon Harrison's discussion of ethnic conflict, which occupies much of Strathern's essay. In it, she takes an important step toward restoring comparison to a central place in the anthropological project, while avoiding some of the pitfalls. At this point, I would like to place Strathern's discussion in the context of other arguments relating to anthropology's comparative project and to more specific ethnographic concerns about

ethnicity and urbanism. Her revitalization of anthropological comparison has some resonance with the work of Louis Dumont, although her general approach more explicitly acknowledges that of McKim Marriott, Dumont's major critic.¹ Dumont tried, as Strathern does, to steer a difficult course between the Scylla of relativism and the Charybdis of universalism—a universalism that, albeit, is imbued with an ideological and normative individualism (of Euro-American post-Enlightenment provenance) that has been essentialized in much political philosophy, as well as in anthropological ethnography.² In a similar spirit to the body of Strathern's work, which depends on Melanesian materials, Dumont attempted to develop conceptualizations, based on his ethnographic work in India, for a comparative anthropological methodology.

Dumont's concept of hierarchy (and encompassment) and Strathern's approach to the "partible person" (related to Marriott's concept of the dividual) are comparative principles. Although initially grounded in their Indian and Melanesian ethnography, these concepts are, in different ways, then reconceptualized as abstract universals. The concepts are both concrete and general, distinguishing and connecting—demonstrating irreducible ethnographic specificity and, simultaneously, a unity through the difference. Thus, in Dumont's analysis, diverse practices in India are, in their various concrete ways, expressive of hierarchy. On the surface, this situation differs from that in Europe. However, the surface contrast or opposition that his comparative method reveals also discloses a deeper unity. Hierarchy is suppressed in accordance with egalitarian and individualist value but has continued to show its effects in racism and in notions of homogeneous national communities and unity in identity (extreme instances being segregation in the United States, apartheid, and Nazi racist essentialism). Strathern conceives of Melanesian dividualism as a principle that permits interconnections precisely because the person or individual is not conceived as a coherent integrated entity prior to the relations that people come to form with others. As Dumont argues about India, Strathern effectively argues that the individual as value does not exist in Melanesia, but rather exists as a partible being relationally constituted, and that this understanding has general force for sociological understanding beyond Melanesia.

The comparativism of Dumont and Strathern are very different from previous attempts in anthropology, for they stress principles and processes that simultaneously connect as they separate. They promise an anthropology that is able to demonstrate the continuities despite differences among human beings (though

1. Louis Dumont, *Homo Hierarchicus: The Caste System and Its Implications* (Chicago: University of Chicago Press, 1980); *Essays on Individualism: Modern Ideology in Anthropological Perspective* (Chicago: University of Chicago Press, 1986); *The German Ideology: From France to Germany and Back* (Chicago: University of Chicago Press, 1994).

2. See also Marshall Sahlins, *Culture and Practical Reason* (Chicago: University of Chicago Press, 1976).

Strathern seems more reticent than Dumont about the continuities) and to deal with the significance of both. Anthropology is divested of the spoiling reputation whereby its comparative and ethnographic work is assigned a positive rather than negative role.

Apart from their similarities in purpose or spirit, the perspectives of Dumont and Strathern are distinct. Dumont is very much a formalist in the tradition of Durkheim and later Lévi-Strauss, although Dumont aims to escape what he considers to be their Eurocentrism. Dumont is a holist for whom the part can only be conceived through its relation with the whole. Strathern is wary, I think, of such holism placing parts and wholes into more dynamic relation, the parts being points for the emergence of new wholes or assemblages. In contrast with that of Strathern, Dumont's position might be seen as far more static, although this is not necessarily the case.³ While Dumont can be described as a traditionalist (if not an Orientalist, committed, as some critics claim, to homogeneous orders), Strathern's viewpoint is more open and contemporary. Nonetheless, it is useful to raise a few questions that Dumont might have considered in relation to Strathern's approach, if only to expose some potential risks.

Methodologically, she aligns herself with increasingly dominant social science approaches, such as actor-network theory, as well as with the individualist orientation of Marriott. Both in some ways accord with her Melanesian ethnography, which indicates that the person is not a coherent integrated singular individual, but rather partible, a point of differentiation that can be plugged into or unplugged from a variety of relations that cross over what may appear as differences and separations and that a previous comparative anthropology may have held to be unbridgeably distinct. Actor-network theory, at least in the work of Latour (which bears some resemblance to Manuel DeLanda's assemblage theory, which in turn has its kinship to Strathern's perspective), is set unabashedly within the contemporary Euro-American re-imagination or re-ontologization of the nature of existence as conceived in a world of digitalized technology, cyberspace, and virtuality.⁴ Dumont, I surmise, would see this development as blatantly individualist, rooted in the conceptualizations of recent history (and its futurisms). Marriott's individualism, for example, is presented as a shift away from the individual as value, a concept that, as Dumont explains, is at the root of Euro-American social scientific thinking and that is seen to be centrally involved in the failure of comparative and generalist objectives. But I think that Dumont would

3. See Bruce Kapferer, "Louis Dumont and a Holist Anthropology," in *Theory and Practice in Anthropology: The Holist Approach*, ed. Ton Otto and Nils Bubandt (New York: Blackwell, 2010), 213–38, where I argue that Dumont's hierarchical conception posits a logic of differentiation in which hierarchy is a dynamic frame for the continual actualization of new potential.

4. Bruno Latour, *Reassembling the Social: An Introduction to Actor-Network-Theory* (Oxford: Oxford University Press, 2005); Manuel DeLanda, *A New Philosophy of Society: Assemblage Theory and Social Complexity* (London: Continuum, 2006).

see Marriott (and, by extension, Strathern's notion of the partible person) as an expansion of the individual-as-value idea and as a form of hyperindividualism. Thus the individual is no longer a coherent entity (or a unitary self) but is broken down into a composite set of individuals, each of which effectively acts like an individual from which a variety of different social assemblages can be generated.

Whether or not this critique of Marriott is fair, it does seem that Strathern sustains Western authority while appearing to destabilize it with her comparative Melanesian ethnography. Furthermore, her destabilization may not be as radical as it at first appears, since she is discovering in Melanesia directions that were already present, if suppressed, in Western philosophical and social thought.⁵ There could be a return to the comparative anthropology of old, despite the radical appearance. The fault of early comparative anthropology was to discover the West in other systems, which, for all their apparent strangeness, ended up revealing and confirming ideologically embedded dimensions (at least, in Dumont's sense) of the world from which the anthropologists mainly came. Dumont largely avoided this trap by insisting on a thorough critique of his own historical situation, which he regarded as crucial for developing an authentic comparative understanding that had real generalizing potential. Strathern is also critical (in her present article, as in much other work), but in a more tentative and subdued way; and here a comparative binarism could be creeping in the back door. An example would be her suggestion that a cosmo-ontology of the Iqwaye sort, as described by Mimica, may be underpinning the relationalities of Hageners.

As a demonstration of her argument, Strathern describes the complex, differentiating, even fractalizing way in which Hageners carve up their realities to establish the multiplicity of their relations. She claims no originality for her observations, which to me resonate with those of Max Gluckman's Rhodes-Livingstone Institute (RLI) researchers in what is now Zambia. Rather like the situation in Hagen as Strathern defines it, in Zambia migrants to town from rural areas developed many novel, situationally relevant, and therefore constantly differentiating and shifting categories, in terms of which their social relations were defined and through which new social assemblages were created that crossed over thoroughly separate social and cultural orders, or what in other situations would have been regarded as such. The urban practices of Zambians, like those of Strathern's Hageners, involved a thorough consciousness of social and cultural difference (they behaved like anthropological comparativists of the past), which they used to generate new social and cultural categories in terms of which people who would have been considered separate could form new unities. Difference was engaged to produce communality. In urban Zambia, relations, often fleeting,

5. See Gilles Deleuze, *Difference and Repetition*, trans. Paul Patton (London: Continuum, 2004); *The Fold: Leibniz and the Baroque*, trans. Tom Conley (London: Athlone, 1993).

were constituted in and through the category, which expressed a virtual relation in terms of which actual relations were formed. Such dynamics can be widely attested, especially in the current conditions of migration and urbanization and in the context of what some commentators are referring to as contemporary cosmopolitanism. Strathern's critical take is that such globalizing intermixture may mask persisting ontological differences that (if I understand her argument) may underpin the new associations and relations formed. Further, Strathern would disagree with the urban/rural division and separation that marked the RLI perspective and for which these researchers were criticized by many anthropologists at the time. (J. Clyde Mitchell, the key urban anthropologist of the RLI group, eventually attempted to overcome the dichotomization with his "network" approach, which is distinct from that currently being popularized by Latour.)

However, Strathern's resolution of the conceptual divide seems to replace a horizontal distinction with a vertical one. That is, the ontological dimensions of relations in migrants' villages persist into urban space as the occluded base of relations in town. Playing on Gluckman's extremist position (which opposed town situations to rural tribal situations), Strathern conceives of the Hageners as simultaneously tribesmen, their tribal ontological roots, as it were, still deeply relevant, if suppressed, for the relations that they are forming in town.

The RLI point is, of course, more complicated than I have so far represented it. Mitchell, for example, did not conceive of Zambian urban life as an integrated totality.⁶ As he saw it, life flowed through a diversity of events and situations, in which relations were being constantly recreated. These relations were in accordance with the manifold perspectives of participants whose motivating conditions, rather than ontological fundamentals (ideological constructions being preferable in this case), were predominantly those of the political economy of the colonial and postcolonial order (in both urban and rural spaces).

Here I would suggest (still in the context of the RLI situationist perspective) that Strathern's argument intimates a dimension of social/societal integration more than she might otherwise prefer. The event-centered and situational approach that the RLI researchers experimented with (in explicit reaction against notions of the inherent unity of society) is more fluid and oriented toward potential (that is, toward the production of something entirely original). This is so despite their privileging of structure and their antagonism toward individualist psychologism, which is always a risk, though not a necessity, of an orientation that stresses ontology of the restricted and even cultural kind that Strathern sees as relevant to the Hageners.

6. J. Clyde Mitchell, *Social Networks and Urban Situations: Analyses of Personal Relationships in Central African Towns* (Manchester, U.K.: Manchester University Press, 1969); *The Kalela Dance: Aspects of Social Relationships among Urban Africans in Northern Rhodesia* (Manchester, U.K.: Manchester University Press, 1956).

The RLI point for Zambia, as distinct from Strathern's observations about Hagen, would be that rural ontologies would be suspended or radically transformed or created entirely anew in urban zones, while in rural areas, if not suspended, they would increasingly be suppressed and, in effect, changed. Furthermore, the similarities and differences, the fractionalizing dynamics of comparison and association engaged in by Hageners in urbanizing contexts, as well as the power of certain anthropological and sociological concepts, are thoroughly connected to wider political forces that might systematically push aside the relevance of local ontologies.

Strathern's kind of questioning goes to the heart of what makes anthropology important. But her approach continues to manifest the difficulties of the bifurcated orientation integral to anthropological comparison, which attempts to discover generalities or universals through the examination of what appears on the surface to be ethnographically based difference. Strathern exemplifies the potential contribution of an anthropology that discovers a common humanity through the study of difference, a program that can effectively explode the apparent separations of difference through the very study of difference. Strathern shows how anthropological ethnography can decenter dominant (which usually means metropolitan) theory, but doing so is rendered problematic when the conceptual apparatuses of description and their assumptions are thoroughly those of some dominant theory. This obstacle is likely to be an enduring one for any sort of anthropology, but some of the dangers or risks may be reduced through a critique (which is not necessarily a rejection) of the conceptualizations engaged (including those that anthropologists might derive from their particular ethnographic experience). I have referred to Dumont, who may appear outdated to many of us today. He attempted a rigorous investigation of his own realities simultaneously with his exploration of people who on the surface appeared to live outside them. His comparative methodology was developed as a kind of internal experimental method for the testing of both particular and general conceptual and theoretical assertions. Vital to this method is a concern to develop a thorough and continuing critique of the historical grounds of one's own understanding, one that is sensitive to the very descriptive concepts through which both differences and similarities are determined. Strathern's work is in line with such a project but, in my view, insufficiently recognizes some of the ideological or ontological underpinnings that may be shaping aspects of her exciting and always illuminating venture.

ONE, TWO, THREE

Cutting, Counting, and Eating

Annemarie Mol

Divided they stand, perspectivalism and perspectivism.¹ Here: one object (one nature to observe) and many subjective (cultural) perspectives on it. There: one subject position (where knowing resonates with the invariable concerns of the eater-who-might-be-eaten) and many natures (prey or predators). Here one world, many viewpoints; there one viewpoint, many worlds. Multiculturalism versus multinaturalism: the binary is stunningly clarifying and movingly beautiful.² But where to go from here?

The white skin has been painted. It is yellow with iodine — a tangy smell. Around the yellowed skin, green cloth. Green cloth also largely hides the bodies of those standing around the operating table. A gloved hand holds a knife and, with a corrective movement, takes a better grip on it.

It may be important to stress the differences between us. Not just because, in a world of “ethnicity,” similarities are as likely to engender fights as differences. But also because, as it happens, if differences are not attended to, the conceptual

1. The author wishes to thank Mieke Aerts and John Law for their intellectual support.

2. Strathern draws this image and analysis from Eduardo Viveiros de Castro, “Exchanging Perspectives: The Transformation of Objects into Subjects in Amerindian Ontologies,” *Common Knowledge* 10.3 (2004): 463–84.

repertoires of the others run more risk of going unheard, of being crushed, than those of Euro-Americans.

And yet: is being different all we can do? There is so much to learn. For instance, if the Hageners have no nature and no culture, then why, just because I have been born in the Netherlands, should I? Why should I stay caught in a “culture” that turns me into a woman and thus into “nature” and thus into someone “uncultured,” not quite able to talk? In the eighties, I read Marilyn Strathern in an activist mode:³

“Euro-America” (of course) is full of frictions — its many vocabularies are not the same. It is the English language that (though since not so long ago) divides sex from gender. With these terms what it is to be a woman (or a man) gets situated either in nature or in culture — so that that divide, too, is once more reiterated. If only I could write in Dutch (and yet be read): the term *geslacht* is not similarly cut up.⁴ Which makes one wonder: what promises are contained in other languages?⁵

The question I would like to raise here does not constitute a critique of Strathern’s “Binary License” but relates to and follows on her text. The question is what Euro-American social scientists and empirical philosophers who study sites and situations Elsewhere can learn not only *about Elsewhere*, but also *about us*. More particularly: might ethnographic analyses offer something other than more perspectives on who “we” are? If we do no more with our ethnographies than proliferate perspectives on “Euro-America,” then we risk turning our culture into no more than a box among contrasting boxes (on the outside) and subdivided into sub-boxes (on the inside). Such a box may be “full of frictions” and contain variety inside it; still, there is no escape from the box.

The surgeon makes a cut. The patient’s yellow skin is breached, the patient-body, lying on the table, opened up. Subcutaneous fat becomes visible. Blood also appears, a little blood, it seeps in various directions, but so far only tiny vessels have been cut.

I would like to call upon this situation as my ethnographic moment: “. . . a relation which joins the understood (what is analyzed at the moment of observation) to the need to understand (what is observed at the moment of analysis).”⁶ What this particular moment reveals: in the operating theater a surgeon is not in the business of knowing. He cuts.

As long as it is conceptual configurations that analysts compare and con-

3. For the text in question, see Marilyn Strathern, “No Nature, No Culture: The Hagen Case,” in *Nature, Culture, and Gender*, ed. Carol P. MacCormack and Strathern (Cambridge: Cambridge University Press, 1980).

4. See Stefan Hirschauer and Annemarie Mol, “Shifting Sexes, Moving Stories: Feminist/Constructivist Dialogues,” *Science, Technology, and Human Values* 20.3 (1995): 368–85.

5. One of my favorites is the analysis that Kwasi Wiredu, trained as an analytical philosopher at Oxford, gives of the (obviously different) theoretical repertoires embedded in his native Akan language. See Kwasi Wiredu, *Cultural Universals and Particulars: An African Perspective* (Bloomington: Indiana University Press, 1996).

6. Marilyn Strathern, *Property, Substance, and Effect: Anthropological Essays on Persons and Things* (London: Athlone, 1999).

trast, the common ground between us will seem to be *knowledge*. The comparing and contrasting will necessarily reveal that what constitutes knowledge differs from one site to another. Then the question of its relativity arises.⁷

But do we need to stop our thinking short at that question? Do we need to rest content with having irreconcilable points of view?

*As the knife cuts deeper, it becomes important to avoid the blurring of the surgical field of vision by blood. A second surgeon therefore coagulates the slightly larger vessels with a hot electric device. The smell of burning flesh is nasty, far worse than iodine. Still larger arteries (those large enough to be depicted in the anatomical atlas, those that have a name) are not to be closed off, not at all. Instead, they are to be opened up. That is what the surgical team is after. They momentarily clamp (in this case) the femoral artery, to make a small cut in its wall and through this they slide a device that allows them to clear away the debris inside it, the atheromata, so that afterwards the lumen of the artery is wide again and blood may flow unhampered.*⁸

There is a lot of knowledge in the operating theater: anatomical knowledge; knowledge about diseases, this disease, this patient's case, this patient; knowledge about the strength of the gloves (at some point the hospital economized, cheaper gloves were bought, but they tore); knowledge about the time left before the next operation is scheduled; knowledge about the name of the new nurse (what was her name again?). But the knowledge does not imply that surgical cutting equals (contains, is contained by) knowing. *It is cutting.*

What kind of lessons may be learned once we start comparing not conceptual schemes, but practices of cutting between hospital Z (in the Netherlands, in the nineties) and other sites and situations elsewhere? Killing, butchering, preparing food: from one place and moment to another, cutting (like knowing) may show interesting differences and similarities.

Warwick Anderson tells us that the (often young and not very experienced) doctors who went to the Papua New Guinea Highlands to research the disease of *kuru* were given advice and assistance during autopsies by the locals who, as they were in the habit of eating their dead loved ones, had a good sense of how bodies may be cut.⁹ (The painful twist is that the doctors—after many years—came back to say that it is the eating of dead bodies, particularly brains, that transmits *kuru* from one person to another.)

As she discusses counting, Helen Verran insists on foregrounding the dif-

7. For an excellent way of addressing that question, see Bruno Latour, "A Relativist Account of Einstein's Relativity," *Social Studies of Science* 18 (1988): 3–44.

8. For further details on the surgical treatment of atherosclerosis and its complexities, see Annemarie Mol, *The*

Body Multiple: Ontology in Medical Practice (Durham, NC: Duke University Press, 2002).

9. Warwick Anderson, *The Collectors of Lost Souls: Turning Kuru Scientists into Whitemen* (Baltimore: Johns Hopkins University Press, 2008).

ferences among counting practices.¹⁰ Trained as a natural scientist in Australia, at some point she found herself teaching teachers in a Yoruba part of Nigeria. The subject was mathematics. In English, counting is done by adding fingers: one, two, three. In Yoruba, by contrast, counting involves both hands and feet: smaller numbers are not digits, added up, but divided parts of twenty. At first, freshly arrived, Verran did not know about this, but once she had learned about Yoruba counting, she looked into it and took it more seriously. Then, in good anti-imperialist, relativist mode, she wrote articles in defense of it, arguing that Yoruba numbers, while seriously different, are equally effective.

Verran relates her ethnographic moment as one where everybody laughed. The teachers had all been sent out to teach their pupils to measure length. The schools were underfunded, so pupils were to measure each child with a cheap piece of rope, and then compare it with the only available measuring stick: a wooden meter stick lying on the floor. One of the teachers, however, Mr. Ojo, reported that he had worked in a different way: he had taught his pupils to make cards ten centimeters across, and then roll their ropes around these. If you can roll your rope seven times round your card, your length is 1.40 m. This method was wrong: rolling is not like adding centimeters, one after the next. But no, it was not wrong, it was Yoruba and therefore involved folding up units of twenty. And it revealed a tension: everyone present belly laughed when they heard the story.

In theory, there is a clash. Verran meticulously laid out the implied conceptual incompatibilities. But as she analyzed the collective laughter, her own included, Verran began to realize that in practice these modes of counting can coexist. They do not need a shared conceptual apparatus in order to be combined. Rather than continuing to defend their relative equality, in her later work Verran shifted to arguing for the possibility of *interactions* between them—interactions in practice. This is no longer a multicultural argument (you have your culture, I have mine). Instead, it is a (kind of) multinatural one: in a noncoherent practice, there is room for different “natures” (numbers).

These “natures” are not tied to a single viewpoint (like that of the Amazonian prey/predator), nor are they expressions of various viewpoints (as Euro-Americans tend to imagine them); these natures are not *viewed* at all. And neither can they be compared with reference to a shared, fixed zero, because they are not situated in a single set of X/Y coordinates. They noncoherently coexist. The mathematics is non-Euclidian.¹¹

The patient lying on the table was operated on because his right leg hurt when walking. The hope was that once his femoral artery was opened up, his muscles might have enough blood again so that, consequently, his pain would subside. However, patients with

10. For the full version of her text, see Helen Verran, *Science and an African Logic* (Chicago: University of Chicago Press, 2001).

11. For a comparable, if different, analysis of noncoherently coexisting objects, see John Law, *Aircraft Stories: Decentering the Object in Technoscience* (Durham, NC: Duke University Press, 2002).

*the same symptoms may also engage in walking therapy. If you walk for half an hour, twice a day, for months on end, your arteries don't open up, but your pain may subside even so. The implication: in both operations and walking therapy the reason for treatment is "pain when walking." But the object of treatment is different. It is a different object. During an operation, it is "a clogged artery"; during walking therapy, it is "pain when walking."*¹²

Hospital practices are noncoherent. The symptom related in the consulting room does not cohere with what becomes visible on the operating table. One diagnostic technique may be conceptually incompatible with another, while in practice they coexist. What is diagnosed does not necessarily equal what is treated. Doctors may target one object, in the hope of interfering with the other, or try something out for a while and, when they fail, try something else. Shift, adapt, adapt again: care practices churn, fold, clash, incorporate, and relate. Doctoring is never straight.¹³ There are fluidities and overflows, fractal complexities and partial connections; there are always *others within*.¹⁴

Is this a proper way of working, to take the images that Strathern brought back from Melanesia (or made in England out of what she had brought back) and "find" them diffracted in the hospital around the corner? Is doing so a wise way to analyze, relate, combine; or is it a way to refuse or escape? I pose this as a question, since I do not know who hands out the licenses here.

And now, as promised in my title, I turn to the practice of *eating*. Amazonians conceptualize all kinds of relations in metabolic terms; and the rest of us, even if we talk about knowledge and eyes, are, in practice, eaters as well. In some places, it still happens that human beings are *being* eaten; but overall, globally, we have killed off the majority of our predators. Thus, we relate to most of the world as our prey. Literally so: by far the largest part of the global biomass is currently being grown or raised for human beings to feed on.¹⁵ Where are we to go from here?

Like other practices, those to do with eating both differ and remain the same from one situation to another. They travel and transform, we relate through

12. See Annemarie Mol, "Cutting Surgeons, Walking Patients: Some Complexities Involved in Comparing," in *Complexities*, ed. John Law and Mol (Durham, NC: Duke University Press, 2002), 218–57.

13. In this context, *choice* is by no means a fitting term for the way binaries are dealt with, while difference does not necessarily present itself as a binary. See Annemarie Mol, *The Logic of Care: Health and the Limits of Patient Choice* (London: Routledge, 2008).

14. For these terms, see, e.g., Marilyn Strathern, *Partial Connections*, rev. ed. (Lanham, MD: Rowan and Littlefield, 2004). I read the earlier version of that book and drew heavily on it as I analyzed the body and its diseases in "hospital Z." The framing of Euro-American "perspectivalism," kindly ascribed by Strathern to John Law and myself, was something that we thought we had imported from her.

15. See, among many other recent books and articles on this issue, N. B. J. Koning et al., "Long-Term Global Availability of Food: Continued Abundance or New Scarcity?" *Netherlands Journal of Agricultural Science* 55:3 (2008): 229–92.

them, interact around them.¹⁶ But while consensus used to be a humanist dream, this commonality—we all eat—is no consolation. Commensality signals more than cozy meals. Conceptually, these days, as Strathern warns us, all too often fighting becomes “ethnic”: it is done in the name of differences between group identities, while it draws, at the same time, on brotherly similarities. But practically, something else is going on as well. That we eat so much implies that we are quite likely to start fighting ever more overtly over our food, rather than (unequally) sharing it. In the process, we may yet deplete our prey. But in what kind of vocabulary to write about something as complex and painful as *that*?

16. In this context, it would be interesting to compare Viveiros de Castro's stories/analyses with those of Serres, in, e.g., Michel Serres, *Le Parasite* (Paris: Grasset, 1980).

NON-IDENTITY POLITICS

Morten Axel Pedersen

What are “ethnic” relationships about if they do not result from any politics of identity—or from any politics of difference? These are among the questions addressed by Marilyn Strathern in her contribution to this symposium, though they are by no means the only ones she raises. Here I wish to concentrate on the theme of “ethnicity” or, more precisely, on those relations that are usually taken to concern ethnic identity but that, on Strathern’s alternative description, revolve around what might be called the politics of non-identity.

For Strathern, ethnographic analysis is all about getting one’s descriptions right, which helps us to pinpoint the problem with most if not all anthropological studies of ethnicity: they are based on insufficiently scrutinized depictions (and therefore assumptions) of what a relationship is. It is on these grounds that one may judge Simon Harrison’s argument about “denied resemblances” to be flawed, despite its seemingly radical departure from conventional studies of the politics of identity.¹ Harrison’s description of the genesis of conflictual social relationships rests on the pluralist (perspectivalist) assumption that the world consists of things, including individual persons, that may or may not enter into relationships with one another and that therefore can be compared according to their degree

1. Simon Harrison, “Cultural Difference as Denied Resemblance: Reconsidering Nationalism and Identity,” *Comparative Studies in Society and History* 45.2 (2003): 343–61.

of mutual sameness and difference, as if “sameness” and “difference” (and indeed “persons” and “relationships”) were self-evident, readily observable phenomena in the world.

Indeed, someone who, like Strathern, is wedded to “post-plural” analytics must hold that a phenomenon such as ethnicity cannot be explained at all with reference to the politics of identity. After all, in an intensive universe—one in which all entities are taken to be enactments of particular relations, which are in turn “related” to all other actual or potential relations—nothing can ever remain the same, and no two things are ever identical.² Thus the concept of identity simply cannot pertain to Strathern’s Melanesia, for her postplural subjects are never similar to anyone, not even to themselves. But so long as the concept of difference denotes a lack of liaison between things, it cannot be taken for granted either. Differences are as relational as similarities; or, put differently, divisions relate as much as connections do. The concept of perspectivism is meant to convey that, in “a universe of relations . . . a person switches not individual viewpoints but relationships.”

So, if the encounter among urban migrants with divided origins in Papua New Guinea in the 1970s did not involve a politics of identity, what then was its politics or—put in terms favored by Strathern—its social mathematics? It is in addressing this question that her article here deals most explicitly with the problem of comparative relativism, for in setting up a binary division (not to be confused with a dualist contrast) between two forms of comparison, and therefore also between two forms of relativism—namely, between what she defines, respectively, as perspectivalist comparison by resemblance and perspectivist comparison by analogy—Strathern opts for the very binary license, performs the same form of conceptual bifurcation, that she is out to study.

My question now is: does Strathern’s characteristically lateral or recursive analytical strategy—her deliberately rendering her ethnographic descriptions in forms similar to the forms of her object of study—imply that her analysis will still remain confined within the pluralist and perspectivalist bounds of social scientific reasoning?³ Strathern’s own answer to this question would probably be in the affirmative.⁴ It seems to me, though, that a somewhat different, significantly more upbeat but perhaps no less appropriate answer to this question might be possible. For what if the form of Strathern’s account is *analogous to*, more than

2. Marilyn Strathern, *Partial Connections*, rev. ed. (Walnut Creek, CA: Altamira, 2004). See also Martin Holbraad and Morten Axel Pedersen, “Planet M: The Intense Abstraction of Marilyn Strathern,” *Anthropological Theory* 9.4 (2009): 371–94.

3. On “recursive” analysis, see Martin Holbraad, “Definite Evidence, from Cuban Gods,” *Journal of the Royal Anthropological Institute* 14.1 (2008): 93–109. On “lateral”

analysis, see Bill Maurer, *Mutual Life, Limited: Islamic Banking, Alternative Currencies, Lateral Reason* (Princeton, NJ: Princeton University Press, 2005).

4. Strathern has on several occasions (e.g., in *Partial Connections*) stressed that she does not consider herself able to go beyond the limits of Euro-American thinking.

it *resembles*, the (perspectivalist, pluralist, Euro-American) analytical form that she is describing? What if, in other words, we were to take literally her invitation to be (reflexively) binary and seek to apply this same license, metarecursively (so to speak), to the analytical bifurcations posited by herself between, for example, “Euro-America” and “Melanesia”? Another road should be available at the fork between binary and dualist divisions, and perhaps it is a comparative relativist path. This less traveled road, instead of (in dualist fashion) presuming an insurmountable contrast between (Euro-American) comparison by resemblance and (Melanesian) comparison by analogy, would take to its logical conclusion the capacity of bifurcations to bring *any two* terms together, including those originally separated to set the analysis in train. We might then be able to explore the conceptual limit of the us/them distinction (bifurcation), for which many anthropologists, including Strathern, have so often been criticized.

An attempt, however tentative, to draw out and compare the different politics inherent in these two forms of “ethnic” comparison may prove useful at this juncture. For what Strathern is implying, it seems to me, is that intertribal relations in Port Moresby in the early 1970s were based on a politics of non-identity, whereas conventional (Euro-American) ethnic relations are assumed to be based on a politics of identity and/or a politics of difference (people classify one another as similar or different, the same or other). In Port Moresby, persons and groups were neither similar to (resembling) nor different from (contrasting) one another; rather, each person/clan/tribe was a version, extension, or analogue of every other person/clan/tribe.⁵

How does this logic—so familiar from Strathern’s writing—of “partial connections” (or partial disconnections) work for social relations that anthropologists would normally consider ethnic? According to Strathern, the stereotypical names bestowed on one another by migrant groups in Port Moresby in the seventies meant that “no one [was] anonymous. . . . even if one did not immediately know somebody’s origin name, one knew that the person had such a name and, moreover, knew that the name would signify the order of connection with oneself. . . . people were already related.” It is this productive stigmatization (productive in the sense of being conducive to further social interaction) that is based, in Strathern’s account, on a logic of analogy rather than a logic of resemblance. Highland migrants did not consider their lacking resemblances among themselves to be a problem, for lack of identity (division) was a precondition for relations and persons to exist in the first place—which is not to say that all was in a state of harmony. On the contrary, Strathern implies that tension (already) existed between migrant groups in Port Moresby. But such tension was not (understood

5. Roy Wagner, “Analogic Kinship: A Daribi Example,” *American Ethnologist* 4.4 (1977): 623–42.

as) caused by a (perceived) lack of resemblance between parties. Put in more formal terms, there was no functional or logical correlation between the degree of tension and the degree of sameness, which one might (had one wished to do so) have tried to measure in the case at hand. Instead the relationship (including, as an inevitable aspect, the tension) between the parties was based on moral analogy (as Strathern calls it without much elaboration): a sort of “universal sympathy,” or could one say “state of culture” (or “state of multinature”), that enabled beings inhabiting entirely parallel worlds to interact in a social manner, while remaining in a perspectivist correlation of mutual non-identity.⁶

The Papua New Guinean case presented by Strathern is by no means unique; analogous (as opposed to similar) examples from different parts of the world can be identified. Matei Candea, for instance, reports on an ethnographic case of non-identity politics, based on his fieldwork experiences as a newcomer to a rural village community in Corsica.⁷ “How might an outsider become an insider?” he asks rhetorically; and, as in the case of stereotypical naming that Strathern discusses, doing so is a matter of creating a situation in which strangers can appear to one another as if they had always been related, even before their first encounter. More precisely, an outsider becomes an insider by being introduced, “in the etymological sense of insertion, of leading inside, as in the introduction of one thing into another—here, of one person into a collective entity or relational assemblage.”⁸ According to Candea, the villagers deliberately do not ask for people’s names when first meeting them. Instead a sophisticated sort of small talk, which he calls “anonymous introduction,” is widely practiced, whereby people slowly discover that they do have mutual acquaintances through a characteristic indirect inquiry into each other’s backgrounds. In that sense, as Candea puts it, “by bracketing identity, anonymous introductions allow for connection to emerge not through difference but *before* it.”⁹

At first glance, the practices described by Strathern and Candea could not be more different; indeed, the contrast between them is so clear-cut that it almost looks like the outcome of a process of conceptual bifurcation of the kind with which Strathern is concerned. Yet because they seem to constitute a binary—anonymous introduction in Corsica versus productive stereotyping in Papua New Guinea—what divides them is also something that they have in common. In opposite ways, the two ensure that people are always already related. One could thus say that, if in Port Moresby strangers were made a priori known through peoples’ overeager attempts to communicate others’ origins (bestowing

6. On “universal sympathy,” see Claude Lévi-Strauss, *The Savage Mind* (Chicago: University of Chicago Press, 1962).

7. Matei Candea, “Anonymous Introductions: Identity and Belonging in Corsica,” *Journal of the Royal Anthropological Institute* 16.1 (March 2010): 119–37.

8. Candea, “Anonymous Introductions,” 123.

9. Candea, “Anonymous Introductions,” 133.

names on them), Corsicans perform the same relational con trick by postponing the exposure of strangers' names much longer than any non-Corsican would expect to be the norm.

An analogous case emerges from my own fieldwork in northern Mongolia in the late 1990s, where encounters between insiders and outsiders, or locals and visitors, revolved around a social mathematics that on first sight looked like a straightforward case of the politics of difference. A small and marginal group (Darhad Mongols) was classified as “other” by the bigger and dominant group (Halh Mongols) through a sustained process of ethnic generalization, if not ethnic stigmatization—a cultural stereotype that Darhads in turn seemed more than happy to turn into the basis of their own subaltern identity. Still, as Strathern discovered on reexamining her data from Port Moresby, I found on closer inspection that what seemed to take place in northern Mongolia was better understood as a process of self-differentiation. The figure of the outsider acted as the necessary tool to extract parts of persons or parts of groups that were then distributed between Darhads and non-Darhads in a sort of ethnic economy, in which cultural stereotypes played a role akin to gifts in more conventional exchange contexts. As I argue in more detail elsewhere, one implication is that Darhads have no identity, let alone ethnicity (in the conventional sense of these terms), for they are always “other” to themselves, distributed as they are across an intensive scale of multiple subjectivities.¹⁰ Theirs is the ultimate case of non-identity politics, one might say.

Perhaps one could describe these three examples, from Papua New Guinea, Corsica, and Mongolia, as instances of “intensive ethnicity,” as opposed to the “extensive ethnicity” with which anthropologists have mostly seemed to be concerned. Both forms of ethnicity are equally relational in the “anti-essentialist” sense of the word; in both cases, a given group’s self-understanding and self-designation are results of its relations with other groups. Yet ethnically extensive relations of the standard *Ethnic Groups and Boundaries* variety involve symbolic processes of boundary making and boundary maintenance whereby contrasting cultural traits or contents are arbitrarily assigned to and distributed among preexisting social forms and social scales (whether individuals, communities, or nations).¹¹ Whereas the more ethnically intensive relations discussed by Strathern, Candea, and myself are relational all the way down, given that everything about the terms of these relations (including their form, scale, and dimensioning) is defined by the particular quality—and, in that sense, the nonarbitrary relational configuration—in question. One final way of expressing the difference between the politics of identity and the politics of non-identity is to say that

10. Morten Axel Pedersen, *Not Quite Shamans: Spirit Words and Political Lives in Northern Mongolia after Socialism* (Ithaca, NY: Cornell University Press, forthcoming).

11. Fredrik Barth, ed., *Ethnic Groups and Boundaries* (Boston: Little Brown, 1969).

the former amounts to an ethnic economy in which subjectivities are exchanged in a commodity-like, alienable (random) way, while in the latter, practices of stereotypical naming and anonymous introduction amount to giftlike, non-random insertions of selves into others. The latter are practices that render all relations—including those with aliens—inalienable.

WHAT POLITICS?

Marilyn Strathern

Of the responses to “Binary License”—those of Bruce Kapferer, Annemarie Mol, and Morten Pedersen—the first situates the essay in a theoretical frame that reminds one of the responsibility one has for one’s words; the second, with characteristic wit, acts out some of the effects of the argument; while the third extends the ethnography to make a new analytical point. It is bracing, I may add, to have comments (in two of these cases) from colleagues who address the paper independently of the symposium’s rubric: comparative relativism.

Bruce Kapferer more than generously draws the argument into a comparison, namely with Dumont’s comparative anthropology. Although this is not the place to rehearse earlier discussions, he opens with an allusion to my general approach. He comments that my work “depends on Melanesian materials” and hence that positing an “abstract universal” (the partible person, in my case; hierarchy in Dumont’s) is to see general ontological possibilities in otherwise apparently restricted data. The point is clinched with his reference to Mimica’s stunning account of Iqwaye mathematics. At the same time, Kapferer argues that the concept of the partible or dividual person can work as a “comparative principle” by analogy with what is more explicit in Dumont’s use of hierarchy, insofar as the concept presumes a comparison with other constructions of personhood. So it did not just spring from Melanesia after all (and my work, for what it is worth, does not depend on Melanesia alone)! Of course, the concept came from my hav-

ing to find a language of description in vernacular Euro-American sources, which in turn implied some understanding of Euro-American. So I would extend the wariness that Kapferer rightly detects in my writing to formulae such as “universals” or “principles.” Other people seem better than I at these things. For my part, if one is working away at the language of description, the job is never done (and “Melanesia” never done with).

However, I am fortified by Kapferer’s commentary for its insight into the way connection and disconnection underpin the comparative maneuver that would contrast what seem(s) (the best way to describe) hegemonic Melanesian and Euro-American conceptions of relations. It is a Euro-American move, of course, to find unity in diversity (common brotherhood overcomes differences), but that is all right. There are many contexts in which that might be a good thing to do, and one I have made my own indeed involves constantly returning to Melanesian materials—not just for inspiration, though that is reward enough, but to keep the anthropological accounts of that region contemporary. What happens there (and what happened there) goes on mattering. They are “us” too. Now it is from a perspectivalist vantage point that people in Melanesia are “us,” even where their presumptions about the world do not take this form. For us to be “us” too, we might want to listen to what they say. But while a perspectivalist viewpoint can imagine a perspectivist or analogical one (that is the power of its relativism), it cannot enact one.

I liked the reference to the Rhodes-Livingstone Institute tradition and the reminder of the copper mines where Zambians had to work at a comparative understanding of their mixed backgrounds; it is an epoch that has no doubt often been revisited in accounts of African diaspora. The perspectivalist can always choose which analytical route to go down—were these experiences of dislocation and relocation harbingers of “globalization,” or did they elicit altogether other modalities of relationship? Ethnicity may not be the only ubiquitous descriptive to which to pay attention in a world that sees itself needy of “conflict resolution.” As to townsmen or tribesmen, for a long while it seemed that rural migrants from Hagen held their urban and rural associations in parallel, which is true also for the local elite and their village connections. People who were energetic, or lethargic, in one sphere were as energetic, or lethargic, in the other too. Of course, things have changed: many have learned that lifestyle can be a matter of (class) inclusion and exclusion.

While a perspectivalist viewpoint cannot enact a perspectivist one, it can coexist with the enactment of its critical opposite, “multiplicity.” Mol’s enactment of the argument about bifurcation and the question she poses—where to go from here?—make a scintillating example. Across noncoherent practices, there is room for different natures: another mathematics (after Helen Verran) prompts Mol to observe that modes of relating (here, *counting*) do not need a shared conceptual

apparatus in order to be combined. What is held in the juxtaposition of acts and practices seems to be the sense in which acts are not affected by how they are described—although, obviously, she holds the question of description at bay only for so long.

In any event, Mol insists on the action of her surgeon cutting. An incision is made to split open a part of the patient's body, the flesh separated rather than severed. This is hardly a matter of a point of view (perspectivalist or perspectivist). Fine. But to know exactly what is going on in the operating theater, we might wish to ask how this act speaks to the act of intellectual bifurcation, splitting open what up to then had been a seamless argument. What an insight to compare practices of cutting! For in referring to persons dividing themselves from one another, in terms of Melanesian gender constructs, I could as well have said cutting. To know exactly what is going on in the operating theater, we might also wish to ask how this act speaks to persons cutting themselves off from one another.

Suppose that cutting people off from one another were routinized, and that to be a man you had to shed feminine parts of yourself and discard a woman's world, you might be made to wash your eyes with abrasive leaves in a cold mountain stream to get rid of the women's things you had seen. Or suppose detachment had to be manifested in shedding blood, which is a maternal contribution to conception, you might induce a copious nosebleed by forcing a long cane down each nasal passage. Or the blood might come from piercing the nasal septum. But, then again, bleeding might be secondary to cutting that left its own mark, as when grooves are dug deeply across the skin of the back to show who you are through the imprint of (the totemic) crocodile's teeth. These examples come from diverse Papua New Guinean practices to do with male growth. An effect of the English language is that one appears to be speaking much more metaphorically in the case of persons cutting themselves off from one another, itself a perfectly acceptable figure of speech in English, than in the case of the incisions that bloody a novice's back. But is there not a sense, in this Melanesian nexus of concepts, in which washing eyes or cleansing oneself through a nosebleed is also a "cutting"? Is not the same term appropriate to cover all the techniques by which persons grow and establish themselves through separating themselves from others? It is not at all clear to me what independent force the idea of action or practice now holds.

We can ask where we go from here, but the anthropologist also struggles with the scientist's (rather than the surgeon's) predicament of describing where we are. There are many contexts in which one does not need to share points of view in order to interact; the writer is only stuck in "points of view" because they are implicated in the act of writing. For while there are many ways of learning, when the learning is caught up in apparatuses of description, then language and

the position from which one speaks or writes cannot be innocent. Mol puts it even better: “But in what kind of vocabulary to write about something as complex and painful as *that*?” and there is an urgency to her “that.” I am glad it was her final question.

The hope for change, where we go from here, is precisely the issue that Morten Pedersen develops so compellingly. Yes, I had hoped it was an “activist” move to show just what is lost by the ubiquitous contemporary readiness, on the part of people from within Papua New Guinea as well as without, to talk about ethnicity, as when they ascribe ethnic identity to conflicts with a group dimension. Of course, showing what is lost does not appeal as much as an image of movement forward does; a regret seems to have no momentum. The activist’s regret is not for conservation reasons but for the lost insight, namely insight into a mode of treating strangers that does not lock them into a hierarchy of identities, which is the curse of much identity politics. It was a mode, as Pedersen says, that made no automatic correlation between degree of tension and degree of sameness—another curse of primitive Euro-American visions of circles of intimacy and distance.

Crystallizing the migrants’ innovations as engaging “the politics of non-identity” clinches this in an analytical insight. Yet I might draw back from the implications of Pedersen’s comment that there are two forms of ethnicity, intensive and extensive, if a shared disposition is implied. Of course, the analytical vocabulary could go either way (two entities, or two types of one entity). However, as in expunging the concept of “property” from some of my writing where Harrison deploys the term up to its hilt, I would prefer now to avoid “ethnicity” altogether for characterizing the situation in 1970s Moresby. Pedersen makes this evasive move at one point for the Darhads (no identity, no ethnicity), as incidentally Sarah Green does for the Balkans outside modern hegemonic depictions of it.¹

If not a politics of identity, then what politics? One answer to which Pedersen points lies in the inalienability of relations between, and thus the entanglement of persons with respect to, one another. In relation to the Canadian Blackfoot, Brian Noble describes how you only need to assume that other people are capable of generating relations in order to activate relations with them.² (Capability was tellingly in doubt in Hageners’ very “first contact” with Australians.) For better or for worse: entanglements can of course be lethal. Incidentally, by contrast with the requirement for coherence in description, Noble observes that there are practices that make no such requirement: if transaction (interchange

1. Sarah Green, personal communication, 2005.

2. Brian Noble, “Justice, Transaction, Translation: Blackfoot Tipi Transfers and WIPO’s Search for the Facts of Traditional Knowledge Exchange,” *American Anthropologist* 109.2 (2007): 338–49, at 343.

between parties) rather than translation (dwelling on what each means) governs dealings with outside agencies, then there is no problem about incommensurate views of the world.

Pedersen ingeniously proposes first recognizing what it means to write within limits and then redescribing the perspectivalist isomorphism between the form of analysis and its object in a way that does not observe these limits. Suppose, to call my bluff, one seeks instead a relation of analogy? Here Pedersen produces both Corsican and Mongolian materials, not as examples that present commensurate social practices, far from it, but as parallel exemplars of nonidentity politics. (It must be a political choice on my part to be happier with the idea of different kinds of politics than with different kinds of ethnicity.) The anthropologist aspiring to a coherent account is relieved of any final reckoning in terms of difference or similarity. Perhaps the invitation is not just to imagine knowing about one thing through another, but to work through what it would be like in practice to write about Hagen migrants while writing about Corsica or the Darhads' Mongolia.

There is still something to say about being responsible for one's words. All three accounts bring out just how contextual the stability we give terms is. Before Mol, for example, I used *multiple* and *multiplicity* as in arithmetic (there is a trace in "Binary License"); to hold this in tension with subsequent usage would be to pinpoint a bifurcation. Bifurcation is nothing to be ashamed of.

ZENO AND THE ART OF ANTHROPOLOGY

Of Lies, Beliefs, Paradoxes, and Other Truths

Eduardo Viveiros de Castro

Translated by Antonia Walford

It's always night, or we wouldn't need light.

— *Thelonious Monk*, from Thomas Pynchon, *Against the Day*

The deliberately paradoxical nature of this symposium's title encapsulates a distinctive concern of some of today's most vitally important intellectual endeavors. There is only one of these that I can or should consider as my own untransferable matter of concern—the endeavor seeking performatively to redefine anthropology as consisting essentially of (a) a theory of peoples' ontological autodetermination and (b) a practice of the permanent decolonization of thought. I am aware that the very word *anthropology* may be jeopardized by this redefinition, given that it belongs firmly among the conditions of our current civilizational deadlock (or should I say, impending downfall), which bears a more than fortuitous relation to our unrelenting determination that the world continue to revolve around

the human in its various historico-conceptual guises. We could perhaps, in this case, rename the discipline “field geophilosophy” or (in reference to our armchair moments) “speculative ontography.” In any case, the relevant onomastics would continue to be Greek—a detail that, there is little need to add, is neither accidental nor inconsequential from an anthropological point of view.

The question for me is how to give the expression *comparative relativism* a meaning specific to social anthropology. Much of my work—at least since I swapped field geophilosophy for ontographical speculation—has consisted in analyzing relativism not as an epistemological puzzle but as an anthropological topic, amenable to translative comparison (or controlled equivocation) rather than to critical adjudication.¹ The Amerindian-derived conceit of “perspectival multi-naturalism” emerged as the result of an attempt to contrast anthropological and indigenous modes of perceiving analogies between domains; in other words, to *compare comparisons*.² The purpose was to trace a line of flight past those infernal dichotomies—unity/multiplicity, universalism/relativism, representation/reality, and nature/culture (to name but a few)—that are like the bars of our metaphysical cage, so as to be able to have a look at that cage (as it were) from the outside.

In the present context, I want to consider the idea of anthropology *as* comparative relativism and approach the theme by means of four “formulas”—four quotations—that illustrate what I intend in various ways. My inspiration for this approach is an article by Gilles Deleuze, “On Four Poetic Formulas that Might Summarize the Kantian Philosophy.”³ I will keep to four formulas for reasons of paraphrastic symmetry. That anthropology is perhaps the most Kantian of all the humanities is merely a coincidence as well. However, the decision to approach the theme by means of quotations is not contingent.⁴ Recourse to examples as a definitional tactic makes evident the “whatever being” (*qualunque, quodlibet*) nature of the passages chosen.⁵ They are neither individual nor generic, but exemplary or singular. They are also somewhat indirect, in the sense that they “exemplify”

1. For “controlled equivocation,” see Eduardo Viveiros de Castro, “Perspectival Anthropology and the Method of Controlled Equivocation,” *Tipiti* 2.1 (January 2004): 3–22.

2. Eduardo Viveiros de Castro, “Cosmological Deixis and Amerindian Perspectivism,” *Journal of the Royal Anthropological Institute N.S.* 4.3 (1998): 469–88.

3. Gilles Deleuze, “On Four Poetic Formulas that Might Summarize the Kantian Philosophy,” in *Essays Critical and Clinical*, trans. Daniel W. Smith and Michael Greco (1986; London: Verso, 1998).

4. For another recent instance of such recourse to quotations, see Émilie Hache and Bruno Latour, “Morality or Moralism? An Exercise in Sensitization,” trans. Patrick Camiller, *Common Knowledge* 16.2 (Spring 2010): 311–30.

5. Giorgio Agamben, *The Coming Community*, trans. Michael Hardt (Minneapolis: University of Minnesota Press, 1993), 8–10. For an encapsulated discussion of the terms, see Max Statkiewicz and Valerie Reed, “Antigone’s (Re)turn: The *Étōs* of the Coming Community,” in *The Enigma of Good and Evil: The Moral Sentiment in Literature*, ed. Anna-Teresa Tymieniecka (Dordrecht, Netherlands: Springer, 2005), 801: it is “the lack of any characteristic,” they explain, that “Agamben ascribes to what he calls *qualunque essere* or ‘whatever being.’ It is in fact its original Latin designation that reveals best the ‘nature’ of ‘whatever being’: *quodlibet ens*. . . . *quodlibet* is what is *loved* irrespective of any generic property.”

anthropology in terms that are, at least in part, restrictive: some quotations amount to extrinsic negations of anthropology that would paralyze it; others suggest intrinsic negativities (virtual or actual) that would propel it. All of the passages chosen evoke the idea of belief, which of course is profoundly implicated, in all possible senses (and especially the worse ones), in the majority of arguments that connect the themes of anthropology, comparison, and relativism.

The use of quotations here does not reflect merely a penchant for the fragment, which I do admit to. Like a postmodern intellectual or an Amazonian Indian, I think that everything has already been spoken—which does not mean, however, that everything has already been *said*. But I do not regard this effort as just one more *collage*; it is rather a *bricolage* (no etymological connection), rearranging things that have been spoken so that they say something relatively—which is to say, comparatively—new.

I

We Western liberal intellectuals should accept the fact that we have to start from where we are, and that this means that there are lots of visions which we simply cannot take seriously.

— *Richard Rorty, Solidarity or Objectivity?* (1985)

If at any point it was possible to feel solidarity with the antifoundationalist pragmatism of Richard Rorty, the sentence quoted above seems to indicate that he and we anthropologists are not on the same “side.”⁶ Clifford Geertz’s arguments against what Rorty was proud to call his own “ethnocentrism” are well known; there is no need to rehearse them here.⁷ My intention in highlighting this passage is principally heuristic. Can we learn something about anthropology from it?

I do not know of anything obviously equivalent to this passage in the anthropological literature, with which I am more familiar; perhaps Ernest Gellner or Adam Kuper has said similar things. Rorty’s sentence does bring to mind,

6. Richard Rorty, “Solidarity or Objectivity?” (1985) in *Objectivity, Relativism, and Truth: Philosophical Papers*, vol. 1 (Cambridge: Cambridge University Press, 1991), 21–34. Rorty’s “solidarity” means “culture,” his “objectivity” means “nature”; and he is all for solidarity, just as we anthropologists have been known to be very partial to culture.

7. Clifford Geertz, “The Uses of Diversity,” in *Tanner Lectures on Human Values*, ed. Sterling M. McMurrin, vol. 7 (Salt Lake City: University of Utah Press, 1986). Geertz likens Rorty’s ethnocentrism to certain positions assumed by Lévi-Strauss in “Race and Culture” (in *The View from Afar*, trans. Joachim Neugroschel and Phoebe Hoss [Chi-

cago: University of Chicago Press, 1992]). It seems to me that Geertz misses a crucial difference. Rorty is extolling the virtues of ethnocentrism from the vantage point of a civilization that imagines itself as increasingly dominant: “. . . the gradual expansion of the imagination of those in power, and their gradual willingness to use the term ‘we’ to include more and more different sorts of people” (Rorty, “On Ethnocentrism: A Reply to Clifford Geertz,” in *Objectivity, Relativism, and Truth*, 207). Lévi-Strauss, on the other hand, sees in a certain amount of ethnocentrism a society’s protective reflex against its absorption by hegemonic projects like those for which Rorty elected himself spokesperson.

however, that marvelous observation at the beginning of chapter four of *Witchcraft, Oracles, and Magic among the Azande*: “Witches, as the Azande conceive them, cannot exist.”⁸ E. E. Evans-Pritchard’s painstakingly detailed monograph was written exactly to resolve this problem: given that witches (as the Azande conceive them) “cannot” exist (as we conceive of possibility and existence), how then can the anthropologist take seriously the conceptions of the Azande concerning the existence of witches? How can the anthropologist reconceive—in other words, reconceptualize—witches so that they can assume a possible mode of existence—in other words, an interest—for us? (We will leave the question of who “we” are for the next paragraph.) If Evans-Pritchard’s solution no longer satisfies us today, he retains the merit of having at least tried to steer us away from “where we are” and toward the Azande. Rorty could be seen as perhaps confronting the same general type of problem; only his reply is purely negative (and dismissive). Each word of his admonition converges to a perfect antidefinition of anthropology.

It is not necessary for the anthropologist to imagine him- or herself as a postcolonialist critic to feel excluded from Rorty’s “we.” In any case, it sounds more like an imposition than an acknowledgment. Geertz, it is true, would recognize himself willingly as a “Western liberal intellectual” (which is why, apart from their long-standing friendship, his critical dialogues with Rorty have a somewhat chummy tone). But I do not see any relation of consequence between the anthropological point of view and a self-description of this sort by a Western intellectual. The awkwardness, however, resides not in the subject of the phrase but in its self-regarding metapragmatic structure. Rorty speaks here for his internal public, his “tribespeople”—there exist only liberal intellectuals in the United States, apparently—who already are where he is and who are, by implication, *very* different from “them.” This “them” are those others who do not regard themselves as liberals, perhaps not as “intellectuals” either, nor even (as Rorty is an author who is read far and wide) as “Western.” The problem is that “we anthropologists” are in general known for our inability to say “we” with any self-satisfaction. That incapacity derives from our subject matter and addressee: anthropologists speak principally *about* “them”—those who are more than ready to say “we are not you”—and increasingly we speak *to* “them.” And in both cases our business is to ask: *Who* are “we”? *Who says* “we” (and when, or how)? Our problem, in sum, is to determine the multiple conditions (not necessarily convergent) under which a “we” is possible. Rorty’s relativism of the rich and pragmatism of the powerful could not even begin to help us here, unless as a privative contrast: we are not t/his kind of “we.”

8. E. E. Evans-Pritchard, *Witchcraft, Oracles, and Magic among the Azande* (Oxford: Clarendon, 1937).

Now, what is the meaning of this idea we are enjoined to accept—that “we have to start from where we are”? Without question that is where we have to *start* from, but saying so does not in any way inform us of where we could, should, or want to *arrive*. Neither does it tell us where *exactly* we are. Regarding this point, I see many more similarities between the “ethnographic effect” so beautifully described by Marilyn Strathern and the problem—as pragmatic as one could ask for—formulated by J. M. Coetzee just before he transforms himself into Elizabeth Costello:⁹

There is first of all the problem of the opening, namely, how to get us from where we are, which is, as yet, nowhere, to the far bank. . . . People solve such problems every day. They solve them, and, having solved them push on. . . . Let us assume that, however it may have been done, it is done. . . . We have left behind the territory in which we were. We are in the far territory, where we want to be.¹⁰

In other words, we have to start from where we are, because here (on the Western Bank, as it were) is *not* where we want to be. On the contrary, we want anthropology to reach and remain in the far territory, out in the open, away from the ironical recesses of the liberal intellect and thus faithful to the project of exteriorizing reason—the project that, nolens volens, insistently takes our discipline out of the suffocation of the self. The viability of an authentic endo-anthropology, a desid-eratum that today finds itself at the top of the disciplinary agenda, for multiple reasons—some of them even reasonable—seems to me, therefore, to depend crucially on the theoretical airing that exo-anthropology has always enabled, it being an outdoor or “field” science in the sense that really matters.

But back to Rorty’s antidefinition: calling that which “we” cannot take seriously “lots of visions” is a less than subtle manner of begging the question. “Lots of visions” can only be a Pandora’s box, full to the brim with fantasies, delusions, and hallucinations—worlds worthy of “the Nazis or the Amazonians.”¹¹ As we all know, lies are multiple (and the devil is their father), but the truth is One (as God). It is true that pragmatism does uphold an intersubjective, consensual, and ethno-centric conception of truth; but the pragmatist’s truth is still One—which leads us to conclude that what lies outside the “conversational” sphere of the pragmatic community of similars is the essence of nontruth in all its proteic monstrosity. Rorty’s quantifier, “lots of,” is in this respect more crucial than its complement, “visions.” If there are lots of visions, it follows that we *simply* cannot take them seriously. There is nothing less simple or more dismissive than this adverb, which can (or must) be taken here in its two main senses: that of facility (it is easy not to

9. Marilyn Strathern, “The Ethnographic Effect I,” in *Property, Substance and Effect* (London: Athlone, 1999),

1–26.

10. J. M. Coetzee, *Elizabeth Costello* (New York: Penguin, 2004), 1.

11. Rorty, “Solidarity or Objectivity?” 31.

take seriously this motley bunch of visions) and of peremptoriness (it is imperative not to take them seriously).

It is here that we arrive at the nucleus of Rorty's antidefinition. It is the very subject matter of anthropology that Rorty declares impossible to take seriously—and the discipline indeed defines itself by not accepting any liberal prohibition such as Rorty's. Anthropology is that Western intellectual endeavor dedicated to taking seriously what Western intellectuals cannot, so Rorty tells us, take seriously. Anthropology takes very seriously as well the question of *how* to take seriously what Rorty refers to as "visions." The constitutive problem of the discipline is how to acquire the tools that allow us to do so. Anthropology faces a double task. First, it must construct a concept of seriousness (a way of taking things seriously) that is not tied to the notion of belief or of any other "propositional attitudes" that have representations as their object. The anthropologist's idea of seriousness must not be tied to the hermeneutics of allegorical meanings or to the immediative illusion of discursive echolalia. Anthropologists must allow that "visions" are not beliefs, not consensual views, but rather worlds seen objectively: not *worldviews*, but *worlds of vision* (and not vision only—these are worlds perceivable by senses other than vision and are objects of extrasensory conception as well). Second, and reciprocally, anthropology must find a way *not* to take seriously certain *other* "visions." The reciprocity here is fundamental, for while we strive to take seriously things that are far from or outside of us, almost all of the things that we must *not* take seriously are near to or inside of us. "Ethnocentrism . . . is essential to serious, non-fantastical thought," Rorty declares; there is always a moment in which the ironist begins to talk of seriousness—the moment when he starts to refer to himself.¹² The famous Deleuzian distinction between humor and irony, so important to Isabelle Stengers's ecology of practices, is germane here. To take seriously what we "cannot" take seriously demands as much sense of humor as its converse, namely not to take seriously what we "simply" cannot *not* take seriously. Relativism is seriously (and serenely) humorous, not self-indulgently ironical.

A final point on this citation: "the Nazis or the Amazonians" appear in Rorty's text as twin *topoi* of alienness, as people who do not share any relevant "premise" with us. The author gives the impression that he sees the Nazis and the Amazonians (also called "primitive tribespeople") as poles indifferently and, therefore, coincidentally antipodal to a pole of lucidity and civility represented by a liberal Western consensus. Speaking as an Amazonianist, I beg to differ: from the point of view of an Amazonian "tribespeople," there are infinitely more things in common—pragmatically speaking—between the Nazis and Western liberal intellectuals than between the former and the Amazonian peoples.

12. Rorty, "Solidarity or Objectivity?" 30.

II

One of the fundamental fantasies of anthropology is that somewhere there must be a life worth living.

—*David Schneider*, foreword to *Roy Wagner*, *The Curse of the Souw* (1967)

After the somewhat haughty tone of the previous citation, this one sounds almost tacky. The flip side of clearheaded American pragmatism, one is tempted to say, is this quality of dreamy sentimentality, a simpleminded readiness to believe in impossible worlds *somewhere*, as in “over the rainbow.” As we know, *that* somewhere was, in the end, exactly where we started from—where we were. “There’s no place like home”—indeed. And what a dire conclusion that is.

However, I think that David Schneider’s observation could be read very differently. It seems to me to contain a very serious, utterly “nonfantastical” thought relative to the project of anthropology. His use of the idea of “fantasy” is the key to the seriousness of the matter, of course.

The respective formulas of Rorty and Schneider could be opposed point for point. First, instead of a “fact” that we “should accept,” we have a “fundamental fantasy.” A fantasy is not something we are *forced* to accept or reject but something that we *assess* from a pragmatic point of view, in terms of its greater or lesser power to make us think differently, to take us elsewhere so that we might have a more precise idea, by comparison, of our current location. Second, instead of an exhortation to “start from where we are,” Schneider’s formula points to where we are heading. The unspecified character of his “somewhere” is necessary, not accidental, as far as anthropology is concerned—a determined indetermination, as it were. Third, the object of the fundamental fantasy, its “aboutness,” is not “lots of visions” but “a life”: a vital difference, it seems to me. And the question raised is that of the real value of this life; instead of lots of visions that we *simply* cannot take seriously, we have a life *really* worth living. Perhaps there are lives not really worth living; but how could one simply *not* take seriously a life, *any* life?

Among those matters that could rightfully be called fundamental to the “fundamental fantasy” of anthropology is that it must remain a fantasy. Anthropology is over once the anthropologist believes that the fantasy has been realized and that he or she has “really” found a life worth living.¹³ Such a belief would paralyze all conceptual creation—which is not to say that *nowhere* is there a life really worth living. Aside from being depressively nihilistic, that claim would be unaccountably definitive (in both senses) and therefore equally immobilizing.

13. There is nothing more hollow-sounding than those ethnographic reconstructions that confront us with Western ethical ideals impersonated by non-Western actors. I am thinking, for example, of those descriptions of Amazonian sociality in terms of a sharing-and-caring convivial

“community of similars.” These descriptions *entirely* miss the “boldness and invention,” the “continual adventure in ‘unpredicting’ the world” that Roy Wagner (*The Invention of Culture* [Chicago: University of Chicago Press, 1981], 88–89) sees in places like Melanesia or Amazonia.

In other words, Schneider is describing one purely regulative use, in the classic Kantian sense, of a motive fundamental to anthropology. For the question as to the existence of a life really worth living is not something we can ever objectively or satisfactorily determine, while at the same time being something we cannot refrain from contemplating. Hence the construction “there must exist” becomes the form of the epistemo-political imperative peculiar to anthropology.

In short, Schneider’s formula elucidates the extent to which anthropology is moved by a quest for authenticity. Rorty opposes his own pragmatic quest for consensus to a “quest for authenticity” that he implies is always ready to veer off toward “fantasy” (as opposed to “conversation”).¹⁴ But the notion of authenticity has full rights of citizenship within anthropology—we do not need to go to Heidegger for it—and there is no reason to revoke them. Edward Sapir’s article “Culture, Genuine and Spurious” is among the more profound reflections produced on the notion of culture, and it is perfectly clear on the subject of the difference between what the author calls the “maxima” and “minima” of culture—authentic and inauthentic collective forms of life.¹⁵ The maxima and minima have nothing to do with levels of civilization but everything to do with “life,” in the sense to which Roy Wagner refers in the phrase “life as an inventive sequence.” Wagner writes of “a certain quality of brilliance” exhibited by cultures that he classifies as inventive (or differentiating) and that exist everywhere. Note the purposeful vagueness with which he describes the bearers of these cultures: “tribal, religious, peasant peoples, lower classes. . . .”¹⁶ It thus appears that these cultures are to be found *everywhere* except precisely where *we* are—for methodological reasons, precisely, if no other. “Somewhere” is the name of this methodological negativity. Anthropology must therefore find—or rather, (re)invent conceptually—a life really worth living, which can be done only by deciding to theorize with seriousness the “lots of visions” imparted by these other lives.

But what does it mean to take seriously the lives of others? Would it mean believing in what Amazonian peoples, for example, think and say—taking what they think literally, as expressive of a truth about the world? The idea that “to take seriously” is synonymous with “to take literally” and, further, that to take literally means “to believe in” strikes me as singularly naive (or else the opposite—a case of bad faith). Only by being too literal-minded could one fail to understand that to take anything literally is heavy work, requiring good provision of symbolic competence rather than infinite credulity. In order to believe or disbelieve in a thought, it is first necessary to imagine it as part of a belief system;

14. Interestingly, it is in connection with this point that we find the only mention (critical) of Deleuze in Rorty’s book.

15. Edward Sapir, “Culture, Genuine and Spurious,” in *Selected Writing in Language, Culture, and Personality*, ed. David G. Mandelbaum (1924; Berkeley: University of California Press, 1985), 308–31.

16. Roy Wagner, *Invention of Culture*, 89.

but problems that are authentically anthropological are never posed in terms of psychological accounts of belief or in the logistic language of truth-values. Alien thoughts cannot be taken as opinions (the only possible object of belief and disbelief) or as collections of propositions (the only possible object of truth judgments). Anthropology has already caused a great deal of damage (in the bad old days) by casting the relation between natives and their discourse in terms of belief—thus making culture look like dogmatic theology—or by treating this discourse as an opinion or a collection of propositions—thus making the study of culture into an epistemic teratology: error, illusion, madness, ideology. Bruno Latour has observed that “belief is not a mental state, but an effect of the relation between peoples.”¹⁷ In which case, if Rorty is right—that “to be ethnocentric is to divide the human race into the people to whom one must justify one’s belief and the others”—then to be an anthropologist is to divide the human race into people whose beliefs one can legitimately challenge and the others.¹⁸ The problem is that each person is a people unto him- or herself (just as, in the Amazonian context, each species is human unto itself).¹⁹ Not much room is left for a *legitimate* challenge to any beliefs but one’s own.²⁰

As Wagner writes: “An anthropology . . . that reduces meaning to belief, dogma and certainty, is forced into the trap of having to believe either the native meanings or our own.”²¹ And as I have said, our refusing to pose the questions of anthropology in terms of belief is a decision that seems consubstantial with the concept of “seriousness” that we want to define. Anthropology wishes neither *to describe* Amazonian (or any other people’s) thought in terms of belief, nor *to relate* to their thought in terms of belief, whether by suggesting that it has an anagogical or allegorical “truth” (either a social truth, as for the Durkheimians, or a natural one, as for the cultural materialists or evolutionary psychologists) or by imagining that it does provide access to the intimate and ultimate essence of things, Amazonian thought being a vessel of infused esoteric wisdom. There is a Deleuzian argument that may help us here, taken from his well-known conception of *Autrui*. For Deleuze, *Autrui*—the other, another—is an expression of a possible world, but this world has always to be actualized by the self, in the normal course of social interaction. The implication of the possible in the other is explicated by me, which means that the possible undergoes a process of verification that entropically dissipates its structure. When I develop the world expressed by the

17. Bruno Latour, *Petite réflexion sur le culte moderne des dieux fâitiches* (Paris: Éditions Synthélabo, 1996), 15.

18. Rorty, “Solidarity or Objectivity?” 30.

19. See Eduardo Viveiros de Castro, “Exchanging Perspectives: The Transformation of Objects into Subjects in Amerindian Ontologies,” *Common Knowledge* 10.3 (Fall 2004): 463–84, at 464–68.

20. Though of course “legitimacy” is never the only consideration in deciding what to do (or believe!); neither is “belief” ever the true object of any serious confrontation with the other.

21. Wagner, *Invention of Culture*, 30.

other, it is either to validate it as real and enter into it or to disavow it as unreal. Explication in this way introduces the element of belief.

Describing this process, Deleuze recalls the boundary conditions that allowed his definition of the concept. “However,” he writes,

these relations of development, which form our commonalities as well as our disagreements with each other, also dissolve its structure and reduce it either to the status of an object or to the status of a subject. That is why, in order to grasp the other as such, we were right to insist upon special conditions of experience, however artificial—namely the moment at which the expressed has (for us) no existence apart from that which expresses it; the Other as *the expression of a possible world*.²²

Deleuze concludes by reiterating a maxim fundamental to his reflections:

The rule invoked earlier—not to be explicated too much—meant, above all, not to explicate oneself too much with the other, not to explicate the other too much, but to maintain one’s implicit values and multiply one’s own world by populating it with all those expresseds that do not exist apart from their expressions.²³

Anthropology would do well to take this lesson to heart. To maintain the values of the other as implicit does not mean celebrating some numinous mystery that they enclose. It means refraining from actualizing the possible expressions of alien thought and deciding to sustain them as possibilities—neither relinquishing them as the fantasies of others, nor fantasizing about them as leading to the true reality.

The anthropological experience depends on the formal interiorization of the “artificial and special conditions” to which Deleuze refers. The moment at which the world of the other does not exist outside its expression is transformed into an “eternal” condition—that is, a condition *internal* to the anthropological relation, which realizes this possibility *as virtual*. If there is one thing that it falls to anthropology to accomplish, it is not to *explicate the worlds of others* but rather to *multiply our world*, peopling it with “all those expresseds, which do not exist apart from their expressions.”

22. Gilles Deleuze, *Difference and Repetition*, trans. Paul Patton (1968; New York: Columbia University Press, 1994), 261.

23. Deleuze, *Difference and Repetition*, 324.

III

The arrow that some do not see leaving, others see arriving.
 — *Marcel Mauss and Henri Hubert, Outline of a General Theory of Magic* (1904)

“La flèche que les uns ne voient pas partir, les autres la voient arriver” is how Mauss and Hubert summarize their reflections concerning the “grave question” of deception and simulation in magic.²⁴ It is “impossible to imagine,” the authors insist in the section of the *Outline* entitled “Belief,” that magicians or sorcerers believe that they do what they say they do. They cannot believe that they artfully remove the liver of their victims without killing them in the act (rather than killing them slowly) or that they can cause lancinating pain in someone’s body by manipulating an effigy. Still, even if magicians cannot believe in their own magic, they may believe in magic per se: “The minimum of sincerity that can be attributed to the magician is that he believes, at least, in the magic of others.”²⁵ When a sorcerer falls sick and seeks the services of another “medicine man,” he will see the arrows being drawn from his body that he cannot see when he pretends to draw them from the bodies of his patients. And it is thus that the arrow that some do not see leaving, others see arriving.

Mauss and Hubert’s problem here is an enigmatic entanglement of credulity and skepticism, desire and perception, first-person and third-person perspectives, that is characteristic of magic. The solution they light upon makes reference to the definition of magical beliefs as being the original (social) form of synthetic a priori judgment, where collective forces provide the pure and invariable form of truth before experience can stock it historically with empirical contents. In archaic worlds, which are under the complete jurisdiction of such collective forces, form predominates overwhelmingly over content.

But the Maussian formula seems to me strategic, insofar as—by tracing the outline of the “pure form” of anthropology, which we might call the magic of difference and vice versa—it allows us to see that anthropology’s method is a particular case of its object, or rather, that the object and method of anthropology are versions of each other. In this sense, the formula could be taken as a definition of anthropology and, further, could be defined as a “definition that defines itself.”²⁶ For the French school of sociology, magic is the epitome of *doxa*

24. Marcel Mauss, *Sociologie et Anthropologie* (1893; Paris: Presses Universitaires de France, 1950), 88.

25. Mauss, *Sociologie et Anthropologie*, 88.

26. On “inventive definitions,” see Martin Holbraad, “Expending Multiplicity: Money in Cuban Ifá Cults,” *Journal of the Royal Anthropological Institute* 11.2 (2005):

231–54, and “The Power of Powder: Multiplicity and Motion in the Divinatory Cosmology of Cuban Ifá (or *Mana*, Again),” in *Thinking Through Things: Theorising Artefacts Ethnographically*, ed. Amiria Henare, Holbraad, and Sari Wastel (London: Routledge, 2007).

(common sense as belief), but Mauss and Hubert's phrase confronts us with a different object—*paradox*—with which anthropology (and magic) have a far more intimate relation.

As with the previous two formulas, our argument will continue to turn on the question of location. Where are we here, now? *Somewhere* along the trajectory of that mysterious arrow. As for the arrow that some do not see leaving but others see arriving, note that it is the same person doubling up in the positions of "some" (*les uns*) and "others" (*les autres*). In his capacity as an agent, the sorcerer does not see the arrow leave; in his predicament as a patient, he sees it arrive. But the magical decoupling can affect different persons, of course, who usually express their (political) differences by way of this perspectival disjunction—as a rule, there are far more arrows seen in the moment of arrival than in the moment of departure. It is not necessary to see an arrow leave from somewhere to see it arrive where we are, and that is how sorcery usually works.

This disjunction also mutually implies in a special way the points of view of the anthropologist and of the native. The witches that Evans-Pritchard could not see *causing*, the Azande saw *effecting*, but does that mean the anthropologist's relation with the phenomena he studies (native "beliefs") is analogous to the sorcerer's relation with his sorcery? And if so, to *which side* of this double relation of magician and magic—the side of the agent, or of the patient? More than one anthropologist has gone the way of Quesalid, to be sure; but his trajectory is not what I have in mind.²⁷ The sorcerer and the anthropologist share (in different ways) the same necessity, to make belief depend on seriousness rather than the other way around. The "minimum of sincerity" is a maximum of seriousness—because magic is always somebody else's.

Taken unprejudiciously (that is, slightly out of context), the Maussian formula does not allow one to say a priori who is right, not even if it must be the case that someone—either those who did not see the arrow leave or those who saw it arrive—is *not* right. The only sure thing, however, is that the two sides cannot in principle be correct *at the same time*, which does not deny that each has good reason to see or not to see the magic arrow from where they are. Mauss's problem is a problem of observation, or of measurement: who sees what, from where, and what happens when, being unable to see it, one does not know how to establish what exactly it is that one is or is not seeing. As Wagner memorably observes of his initial relations with the Daribi, "their misunderstanding of me was not the same as my misunderstanding of them."²⁸ It is as if we are dealing here with one more version of Niels Bohr's principle of complementarity; that is, the existence of simultaneously necessary but mutually exclusive descriptions of the same phenomenon. The magic arrow could be seen as a quantum particle, for which only

27. Claude Lévi-Strauss, "Le sorcier et sa magie," in *Anthropologie structurale* (Paris: Plon, 1958), 183–203.

28. Wagner, *Invention of Culture*, 20.

either position or momentum can be established. Analogously, that “some” do not see the arrow leaving reciprocally presupposes that “others” do see it arrive. It appears that the arrow can only arrive for some if others do not see it leave, and vice versa.²⁹

It is here that object and method meet, as this is the anthropological situation par excellence: how to connect the two arrows, that of the anthropologist and that of the native, so that they become one? Just as it was the same individual who did not see the arrow leave and yet saw it arrive, so also is it *in principle* the same arrow that leaves and arrives. The arrow of the anthropologist must be the arrow of the native and not any other (not a metaphorical arrow instead of a magical one, for example). Or, at the very least, it is necessary to make the two arrows coincide—to build a ladder of arrows starting with these two arrows, as exemplified by the heroes of Amerindian myths who, fastening a succession of arrows to each other, make a continuous stairway from the earth to the sky (starting at the end!), in so doing traversing the discrete interval—the abyss—that separates the two extremes of the cosmos. How to make ends meet? That is always the question.

A conjecture follows. It is possible to speculate that the perplexing mixture of spontaneity and obligation, gratuity and interest, generosity and aggressivity, that according to Mauss characterizes the “archaic” complex of the gift has a more than accidental relation to the ambiguity of magic with regard to skepticism and belief, charlatanism and sincerity, “voluntary illusion” and “perfect hallucination,” that Mauss had observed in the *Outline*, some thirty years earlier in his career. I am not thinking of the notorious incapacity of primitives to distinguish between persons and things, which shapes the gift as well as magic in a causally negative manner.³⁰ Rather I am referring to an epistemological effect on the observer, derived from a complex, overdetermined ontology common to the gift and to magic. The effect manifests itself as these two heterogeneous mixes of sentiments, both presenting an ambivalent dispositional nature (skepticism and belief, generosity and greediness) and also jointly involving a type of meta-

29. Lévi-Strauss was fond of quoting a remark of Bohr’s in which he compares the differences between human cultures to the mutually exclusive ways in which a physical experiment can be described. I also remember that “perspectival multinaturalism” (the “spin” I was able to give to the theme of relativism with the help of Amazonians) presupposes the same relation of complementarity or duality. Nonhumans see themselves as we see ourselves, as humans, but we cannot both see ourselves as humans at the same time: the apperception of one pole as human makes the other appear (makes the other be perceived) automatically as nonhuman. Much the same thing occurs as well, it seems to me, between the literal and figurative

modes in the semiotics of Wagner (“Scientific and Indigenous Papuan Conceptualizations of the Innate: A Semiotic Critique of the Ecological Perspective,” in *Subsistence and Survival: Rural Ecology in the Pacific*, ed. Tim P. Bayliss-Smith and Richard G. Feachem [London: Academic Press, 1977], 385–410) in the Saussurian theory of the sign, and in the anthropology of Lévi-Strauss (Patrice Maniglier, *La vie énigmatique des signes. Saussure et la naissance du structuralisme* [Paris: Léo Scheer, 2006]).

30. With the gift, people are treated like things (J. G. Frazer’s barter of women); with magic, things are treated like people (E. B. Tylor’s animism).

calculation that includes the other's point of view in defining the meaning of one's own actions for oneself. Gift and magic are intentional multiplicities, disjunctive syntheses *in vivo*.³¹ The theory of value condensed in this arrow, which links the gift to magic, seems to me closer to the mark than the famous "false coin of our own dreams."

It was only after contemplating for some time the Maussian formula concerning the two faces of magical intentionality that I noticed the nature of the object in question: an arrow. The archetypal mediator of action at a distance and one of the most ubiquitous images of effective intentionality in folklore the world over, the arrow is a universal symbol of the index (look where the arrow is pointing and you will get somewhere) as well as the elemental vector of the "distributed person" (look to where the arrow came from and you will find someone). Every arrow is magical: while it paradoxically transforms the far into the near and vice versa—as skepticism transforms itself into belief, aggressivity into generosity, and reciprocally so on—no arrow that we see arriving is exactly the one we saw leaving. But there is *one* magical arrow whose effect makes itself felt over very long distances. It was fired two and a half millennia ago; it has not stopped flying, to this day; and it crosses, in its trajectory, the Maussian arrow. I mean, of course, the arrow in one of Zeno's four paradoxes of movement, the arrow in flight that is always at rest, in eternal freeze-frame, never reaching its target. At each instant (indivisible, by definition), Zeno's arrow occupies a portion of space equal to itself; if it were to move during the instant, it would have to occupy a space larger than itself, for otherwise it would have no room to move. As Bertrand Russell says, "it is never moving, but in some miraculous [magical!] way the change in position has to occur *between* the instants, that is to say, not at any time whatever." And Russell concludes: "The more the difficulty is meditated, the more real it becomes."³² The scandal of the paradox is that the real difficulty is resolved in reality, for the arrow—against all odds, as it were—rapidly arrives at its destination.

The Maussian arrow is just like Zeno's: it "never moves," given that a straight line between its point of departure and its point of arrival cannot be traced, as if these two points belonged to heterogeneous dimensions or distinct series. Such an impossible quality assimilates both of these projectiles to another object of the same illustrious family. I mean *mana*, Lévi-Strauss's "floating signifier": the concept of a perpetual disequilibrium between two series that make up the two unequal halves of the symbol—the series that contains an empty

31. Eduardo Viveiros de Castro, "The Gift and the Given: Three Nano Essays on Kinship and Magic," in *Kinship and Beyond: The Genealogical Model Reconsidered*, ed. Sandra Bamford and James Leach (Oxford: Berghahn, 2009), 237–68.

32. Bertrand Russell, "The Problem of Infinity Considered Historically" (1929), in *Zeno's Paradoxes*, ed. Wesley C. Salmon (Indianapolis, IN: Hackett, 2001), 51.

case (the arrow that some did not see leaving) and the series that contains the supranumerary element (the arrow that others see arriving). As this mismatch lies at the radical origin of semiosis, it is probable that here we have arrived at the proper place for anthropology to erect its watchtower: the crossroads of sense and nonsense. Perhaps it is unnecessary to recall here another celebrated phrase of Evans-Pritchard's (as recalled by Joseph Needham): "There is only one method in social anthropology, the comparative method—and that is impossible."³³

I cannot conclude my remarks on the Mauss-Hubert formula without mentioning Gregory Schremp's splendid work *Magical Arrows: The Maori, the Greeks, and the Folklore of the Universe* (1992). The author explores the analogical (in the strong sense) relation between Maori mythology and the antinomies of the "Transcendental Dialectic" in Kant's first *Critique*, as well as the Lévi-Straussian doctrine concerning the "passage" of the continuous to the discrete in the origin myths of clans or natural species. (Schremp interprets the doctrine, quite correctly, as a mythical version, in the Lévi-Straussian sense of the term, of the Eleatic paradoxes.) Finally, Schremp connects the most famous of these paradoxes, the "Achilles" one, with Amerindian narratives about the race between two animal characters, which to him suggests that the theme has an archaic, conceivably paleolithic, origin. As he comments at the beginning of the book, "such familiar little images" (for instance, the race between ill-matched competitors that culminates in the victory of the weakest) "are, in philosophy and mythology, and within and without Western knowledge, precisely the stuff out of which some of the most grand mental creations have been brought to life."³⁴ This assessment we know to be true; and we do so, in large part, thanks to anthropology and especially to Lévi-Strauss. We know also that Zeno's paradoxes are a constitutive philosopheme of Western metaphysics; if there is one place, therefore, at which "we Western intellectuals" have to start—because we never manage to leave it—it is at this "vision" of Zeno's immobile arrow, floating in a supranumerary dimension equidistant between the two poles of meaning and nonsense, subject and object, language and being, self and other, the near and the far side of experience. And we do get to the far side, with a little help from anthropology.

A quick aside, *in fine*. Schremp calls our attention to the universality of the magical arrow theme; yet, curiously, he does not mention the frequency and centrality of the motive in *Mythologiques*, despite his taking *The Raw and the Cooked* as one of its principal axes of comparison among Zeno, Kant, and Lévi-Strauss.³⁵ It should be noted, if only in passing, that Amerindian myths mobi-

33. Apud James Peacock, "Action Comparison: Efforts Towards a Global and Comparative and yet Local and Active Anthropology," in *Anthropology, by Comparison*, ed. André Gingrich and Richard G. Fox (London: Routledge, 2007), 44.

34. Gregory Schremp, *Magical Arrows: The Maori, the Greeks, and the Folklore of the Universe* (Madison: University of Wisconsin Press, 1992), 10.

35. Schremp, *Magical Arrows*, 188–91.

lize an astonishing diversity of quite unusual arrows, archers, and firing techniques, bestowed with logically complex and evocative properties. There are the arrows that become deadly accurate only after being broken into segments and reconstituted by a supernatural animal; the arrows so powerful that they need to be weakened with a magic ointment, lest they return to kill those who fired them; and the arrows that reach their target only if the archer looks in the other direction—that is, that only arrive where one desires if they are not seen leaving (as in the Maussian formula). Respectively, these three sets of arrows, one might say, teach integral and differential calculus, the dangers of hyperreflexivity, and the art of indirection.³⁶ The anthropologist must have arrows possessing of all these qualities in her quiver; but most importantly, she must have those that connect disjunct worlds like the earth and the sky, or the two banks of a wide river of meaning. She must have arrows that serve to make ladders or bridges between where we are now and wherever we must be.

IV

Even if it is true, it is false.

— *Henri Michaux*, *Face aux verrous* (1954)

This fourth and final quotation—“*Même si c’est vrai, c’est faux*”—is my favorite one, of course. Science, as classically conceived, is based in the principle—to call it a “belief” would be a cheap shot—that it is possible and necessary to distinguish between true and false propositions, separating everything that is affirmed about being into truths and falsities. Or rather, science can only exist where it is possible (*de jure*) to separate the true from the false and where the law of the excluded middle (“*If it is true, then it is not false,*” and vice versa) is maintained. The most that one can admit—and it is a fundamental maxim of scientific good sense or “best practice”—is that *ceteris paribus* conditions always apply and that a frame of reference should always be specified as well. I would call this attitude “sensible relativism.” Anthropology’s mission, as a social *science*, is to describe the forms by which, and the conditions under which, truth and falsity are articulated according to the different ontologies that are presupposed by each culture (a culture here being taken as analogous to a scientific theory, which requires its own ontology—that is, its own field of objects and processes—in order for the theory to generate relevant truths).

36. They also call to mind another famous philosophical arrow: “Nature propels the philosopher into mankind like an arrow; it takes no aim but hopes the arrow will stick somewhere. But countless times it misses and is depressed at the fact . . .” (Friedrich Nietzsche, “Scho-

penhauer as Educator,” in *Untimely Meditations*, trans. R. J. Hollingdale [Cambridge: Cambridge University Press, 1983], 177–78).

Religious belief, on the other hand—dogma as the propositional form of belief—is based in the principle that the distinction between truth and falsity is subordinated to what we could call “suprasensible absolutism.” *Credo quia absurdum est*, I believe because it is absurd: in the terms of Michaux’s formula, this dictum of Tertullian’s is equivalent to affirming, “*Even if it is false, it is true.*” The dictum, which, as is well known, is a misquotation, does not accurately reflect the historical or theological truth of Christian dogma; but it does express rather well the French sociological theory of truth, which I briefly invoked when commenting on Hubert and Mauss’s phrase about magic. Magical and religious beliefs are synthetic a priori judgments (coming before individual experience), and such is the original form of all truth. It is society that separates the true from the false, in a way homologous to the self-separation of the social from the individual, the supersensible from the sensual. Truth is social because society is the source and the reference of truth; what is false could only originate in the individual. Therefore, whatever it is that society *authorizes* is true, even if it be false from the subordinate, a posteriori perspective of the individual. Per Durkheim’s notorious equation, God = Society, theological suprasensible absolutism becomes the cultural relativism of the social sciences. Anthropology’s mission, as a *social* science, is to determine which nontruths are taken as “God’s truth” in any given society.

Between science and religion there is, naturally, opinion or *doxa*—that vast ocean of statements that one cannot pronounce true or false, neither, or both. The caricatural, (auto)deconstructive form of *doxa* is, precisely, paradox, which exposes the impossibility of univocal meanings and the precariousness of every identification, a predicament (or a power) that is immanent to language. Epimenides’ paradox—the liar’s paradox—is a particularly apt example: “*If it is true, then it is false, and vice versa.*” Here, we are, in a sense, beyond cultural relativism, down among the paradoxical roots of human semiosis. Anthropology, conceived as a branch of semiology, shows in this case a predilection for studying the processes by which language and being, the signifier and the signified, the literal and the figurative, the sensible and the intelligible, are reciprocally determined. The anagrammatic foundation of all signification, the arbitrary differentiation between a “nature” and a “culture” that, as it predates them, does not belong to either of the two, becomes the prototypical anthropological object. *Doxa*—the culturally “different notions of common sense” that are “the object of study” of our discipline—should be taken in this case to be the result of a decay (as we speak of “radioactive decay”) of paradox, which is the true genetic element of meaning.³⁷

37. Michael Herzfeld, “Orientations: Anthropology as a Practice of Theory,” in *Anthropology: Theoretical Practice in Culture and Society*, ed. Herzfeld (Malden, MA: Blackwell, 2001), 2.

There is, however, a fourth possibility, the most disturbing of all, summed up in Michaux's dictum, which introduces us directly into the world of simulacra and the powers of the false, a world that is not only beyond relativism but also beyond paradox. Insofar as it is the inversion of Tertullian's pseudoformula (just as the formula of the paradox would be the inverse of the scientific principle of the univocality of truth), Michaux's aphorism shows that the true opposite of "religious belief" is not "scientific truth." Nor is it the indiscernibility of true and false as presupposed by formal anthropological semiotics. Michaux's formula is, literally, a magical formula: pace Mauss, it permits one to evaluate the width of the gap that distances magic from religion and, reciprocally, to appreciate the proximity of religion and science, which fight ferociously just as they unite in a common cause, both seeking possession of eminent causality. Magic, on the other hand, is a doctrine of *effects*; and all effect, from a point of view haunted by the cause (the concern) of the cause, is always an artifact, a "special effect," a lie. He who says, "even if it is true, it is false," is someone who is preoccupied with the effects produced by what is said—by its *effectiveness*, which has nothing to do with its truth. Even the truth—especially the truth, it is tempting to say—is capable of prodigious effects of falsity and falsehood. (As we all know, the best way to lie is to tell the truth.) The only possible pragmatics of truth depends on the axiom "even if it is true, it is false." The pragmatics of truth has nothing in common with the hermeneutics of suspicion, so typical of critical sociology, which seeks the (always nasty) truth behind the lies that are told within and by society. The truth is not a "particular case" of the lie but a "whatever" (again, in Agamben's usage) case of the lie. This "even-handed intolerance," to borrow Barbara Herrnstein Smith's vigorous expression, projects a possible image of anthropology as a type of enlightened, humorous demonology rather than as a dismal, laicized theology (in the spirit of the French sociological school and its innumerable descendants) and moreover suggests a path toward freeing our discipline definitively of the problematics of both belief and unbelief.

Ezra Pound defined literature as "news that stays news," as a discourse able to change, to *not* stay put, to exist as a perpetual, extrahistorical becoming—always new, always news. In the same spirit, we might say that anthropology is alterity that stays alterity or, better, that *becomes* alterity, since anthropology is a conceptual practice whose aim is to make alterity reveal its powers of alteration—of making a life worth living. Cosmology is gossip; politics is sorcery; and *anthropology is alterity that becomes alterity* (and I mean "becomes" also in the sense of "that hat becomes you"). This fifth formula is mine and suggests the proper way of taking life—our own as much as any other—seriously.

ENDO/EXO

Matei Candea

In recent years, anthropologists have increasingly admonished each other to “take seriously” the people they work with. And yet the moral valence of “taking seriously” has often been clearer than its precise meaning. One crucial achievement of Eduardo Viveiros de Castro’s article here is to infuse this phrase with a convincing, theoretically sophisticated, and precise content—and in the process to produce a characteristically exhilarating account of the anthropological endeavor, one that takes forward and refines the author’s previous definition of anthropology as “the science of the self-determination of the world’s peoples.”¹ I must declare an interest here and, in so doing, delineate a specific public for which “Zeno and the Art of Anthropology” will be of particular importance. I am an anthropologist working in Europe (among other places), and as a result my admiration for Viveiros de Castro’s work has always been accompanied by a slight uneasiness under one particular heading: the idea that the anthropological endeavor is properly that of engaging with non-Euro-American ontologies has left me with the apprehension that anthropologists who, in some respect or another, are probing European or American forms of life, having grimly fought their way into the relative acceptance of the discipline in the 1980s, would once again end up out in the cold. I think that this article—and its definition of seriousness—

1. Eduardo Viveiros de Castro, *And* (Manchester, U.K.: Manchester University Press, 2003).

allows one to envisage a more hopeful outcome. Although Viveiros de Castro is principally concerned with exo-anthropology in this essay, he also gives us the tools with which to articulate the specificity and particular value of an endo-anthropology (of sorts).

The article begins with a vigorous critique of Richard Rorty's avowed ethnocentrism. Rorty's refusal to take seriously "visions" that are too far from our own provides the precise antidefinition of Viveiros de Castro's anthropological project: the commitment to "taking seriously that which Western intellectuals cannot take seriously." Crucially, however, "taking seriously" here does not equate to believing in the truth of what people say, any more than it means "respecting people's beliefs." Indeed, the suspension of the traditional relativist problematic of belief is the very condition of "taking seriously" in this new sense. Drawing on Deleuze, the author casts "taking seriously" as a self-imposed suspension of the desire to explicate the other, to verify the other's possible world. This kind of verification of the other by the self, which—crucially—is what occurs "in the normal course of social interaction," dissolves the possibility of the other's world by resolving it into either the reality of our own world or mere fantasy. Taking seriously, by contrast, involves "refraining from actualizing the possible expressions of alien thought and deciding to sustain them as possibilities." "Taking seriously" recalls what the author has elsewhere described as a practice of enabling "ontological self-determination": refraining from either assent or critique, in order to allow the people themselves to specify the conditions under which what they say is to be taken.²

To this elegant and rousing argument, I wish to add a comment and a suggestion. The comment is this: a naive reader might object that Viveiros de Castro's commitment to "taking seriously" does not extend to a more intimate other, namely Richard Rorty (and the Western, liberal, multiculturalist "vision" for which he is made to stand). Our naive (or falsely naive) reader would be trying of course to transpose to this project the classic critique of relativism (articulated from the *Theatetus* onward); namely, that it can accept any vision except a universalist one and thus falls into self-contradiction. However, our naive critic would be missing the crucial distinction between Viveiros de Castro's new relationism and the old relativism: relationism is not some loose form of generalized "tolerance" but a precise and controlled instance of asymmetry. As Viveiros de Castro points out, taking some visions seriously requires that we not take other visions seriously. About the latter, the author is quite specific: "almost all of the things that we must *not* take seriously are near to or inside of us."

However, Viveiros de Castro's suspicion of liberal Western intellectuals, his call for us to get away from "where we are," from "the suffocation of the self,"

2. Viveiros de Castro, *And*, 18.

should not make us forget that not taking (ourselves) seriously has acquired a technical meaning here that is the exact the opposite of what our naive reader might think. On Viveiros de Castro's redefinition of seriousness, the things we do not take seriously are precisely those we do subject to explication, those we resolve into truths and falsehoods, those we agree or disagree with, adopt as our own, or reject as fantasies—in other words, those with which we have normal social interactions (and intellectual intercourse). Consistent with his position, the author subjects Rorty (along with the Western liberal, multiculturalist vision he stands for) and Deleuze (along with the Western antihumanist intellectual tradition he stands for) to such treatment. Unlike the Amazonians, Rorty and Deleuze are not taken seriously—not left in a state of sustained possibility. On the contrary, Rorty's possible world is verified and rejected, just as Deleuze's possible world is verified and provides a definition of anthropology.

To reiterate this somewhat convoluted point: what separates Viveiros de Castro's project from relativism (and renders it immune from the usual "self-refutation charge") is a sustained and pivotal asymmetry in the treatment of visions that are procedurally identified as "ours" (or close to us) and "theirs" (or far from us).³ Within the former sphere, there is intellectual debate (which involves agreement and disagreement, belief and disbelief) and normal social interaction; with the latter, there is "taking seriously" (which means leaving in a state of possibility) and a specifically anthropological relationality. Or as the author puts it: "to be an anthropologist is to divide the human race into, on the one hand, people whose beliefs one can legitimately challenge and, on the other hand, everyone else." The crucial question thus raised, of course, is where one might locate the pivotal distinction between us and them, close and far, that enables the entire project. My own ethnographic interests—most of my work to date has been on identity and relationality in Corsica, and I am now conducting research among British scientists who study animal behavior—make me rather resistant to the thought that this line could straightforwardly be drawn between "Euro-Americans" (or "moderns," or "the West") and everyone else.

While the rhetoric of Viveiros de Castro's article does occasionally suggest such sweeping divisions between "the Western Bank" and elsewhere, a careful reading offers us a more precise and hopeful possibility. In what is a very important refinement upon his earlier statement about "the ontological autodetermination of the world's peoples," the author here justly notes that "the problem is that each person is a people unto him- or herself." This caveat makes clear the immeasurable distance between the project of "ontological self-determination" and that of the "ontological determination of the self."⁴ The latter (as when people

3. Barbara Herrnstein Smith, "Unloading the Self-Refutation Charge," *Common Knowledge* 2.2 (Fall 1993): 81–95.

4. Eduardo Viveiros de Castro, personal communication.

essentialize around themselves the boundaries of “a” people or “a” culture) may well be a consequence of the former but is not, or should not be, a precondition. After all, within “a” people, there are always *other* people, and anthropology should take them seriously too. For some of the people I worked with in Corsica, being Corsican involved significant stable differences that required political (and anthropological) recognition. For others, being Corsican was just a version of being a French citizen like myself. Others did not live in either of those worlds and spoke to me as Europeans, Mediterraneans, teachers, or mothers. In cases such as these (which I suspect means most if not all cases with which anthropologists actually deal), allowing people to specify the conditions under which what they say is the case must crucially involve refraining from deciding who the “they” is, to begin with. Hence the importance of this new argument as a continuation of the author’s discussion in his essay “And” (2003). Viveiros de Castro’s earlier proposal for a science of the ontological self-determination of *the world’s peoples* was easy to misread as aligned with strategic essentialism and identity politics (I myself had done so).⁵ But “taking seriously,” as defined here, does not rely on the prior stabilization of difference at the level of peoples or cultures; even less so, therefore, at the level of metaconstructs such as “the West.” As the author notes, the conditions of possibility of a “we” (and I would add, therefore, of a “they”) are always under interrogation.

I would argue that this shift from “ontological self-determination” to “taking seriously” leaves room for the so-called project of “anthropology at home” to play a very specific and important role (here I am moving from my comment to my proposal). Viveiros de Castro writes: “The viability of an authentic endo-anthropology, a desideratum that today finds itself at the top of the disciplinary agenda, for multiple reasons . . . seems to me, therefore, to depend crucially on the theoretical airing that exo-anthropology has always enabled, it being an outdoor or ‘field’ science in the sense that really matters.” However, this claim in turn raises the symmetrical question: how might an endo-anthropology fertilize an exo-anthropology? What, in other words, is the complement/obverse of “theoretical airing”? I think the greatest contribution of endo-anthropology might be to keep firmly in our sights the problematic nature of the endo/exo contrast itself. Starting off from the Western Bank “inward,” as it were, one finds just as much difference as one might find in setting sail to farther shores. Taking these “internal” differences seriously (that is, asking “internal to what?”) ultimately highlights the constitutive impossibility of endo-anthropology as a straightforward account of “one’s own people”—an impossibility that any ethnographer supposedly “at home” constantly comes up against: there is always more difference within! But

5. Matei Candea, *Corsican Fragments: Difference, Knowledge, and Fieldwork* (Bloomington: Indiana University Press, 2010).

this impossibility cannot be taken as axiomatic, a reason to abandon the project: it must be an ever-repeated attempt, an ever-repeated failure, which forcefully underscores that the endo/exo contrast is not a starting point but an outcome (of anthropology, among many other things).

This corrective is useful because, while defining the discipline by opposition to “where we start from” is crucial, such a definition too easily leads us to assume we know where that is. For instance, pointing out similarities (from an Amazonian perspective) between Western liberal intellectuals and Nazis might be a powerful rhetorical device, but figuring out the very important differences between the two (and indeed, where in that landscape, or out of it, one might place the tradition of Western antihumanism) remains an anthropological task, a task that requires us to take seriously all sorts of things that are close to us, including some that an exo-anthropology cannot take seriously (such as Western liberalism, in its various incarnations). Working with schoolteachers in Corsica or with scientists in the U.K. raises just these kinds of question. “Zeno and the Art of Anthropology” gives us an excellent account of what makes such endeavors anthropological: the commitment to taking seriously the multiplicities internal to what we thought was simply “us,” instead of either taking these worlds for granted or subjecting them to the usual critical unveiling. This is Viveiros de Castro’s gift to endo-anthropologists: a new language in which to claim their place at the anthropological table. In return, the ever-repeated experiment (and failure) of endo-anthropology offers a salutary reminder, to the rest of the discipline, that the line between those visions we *ought* to take seriously and those we ought *not* to is never fixed or self-evident.

OF ARCHIPELAGOS AND ARROWS

Debbora Battaglia

Eduardo Viveiros de Castro's Amazonian perspectivism is beautiful to think with: a bricolage of lapidary refractions, ethnographically and theoretically precise, magic friendly. Indeed, the essay is so artfully wrought that its radical politics could easily be missed. Partly, the matter traces to Deleuze, the author's favorite philosopher-interlocutor, whose philosophy of values can make trouble for political theorists. Davide Panagia discusses how Jacques Ranciere, for example, cannot conceive of mobilizing political opposition in a world imagined as "in process, an archipelago . . . of loose uncemented stones where every element has a value in itself but also in relation to others."¹ How can one imagine pushing back against such a world's floating elements of "indistinct" value, particularly in view of certain value structures' disempowering effects on other, incompatible structures? It is not a bad question; only it is a delimiting one. Panagia makes the point from political philosophy: the political act is *aesthetic at the start*, an act of "partitioning of sensation that divides the body and its organs of sense perception and assigns to them *corresponding postures of attention*" in culturally specific ways.²

1. Gilles Deleuze, "Bartleby; or, the Formula," in *Essays: Critical and Clinical*, trans. Daniel W. Smith (Minneapolis: University of Minnesota Press, 1997), 71. See also Jacques Ranciere, *The Flesh of Words: The Politics of Writing*, trans. Charlotte Mandell (Stanford, CA: Stanford University Press, 2004).

2. Davide Panagia, "The Political Life of Sensation: On Disinterest, Disfiguration, and Dissensus" (paper, annual meeting of the American Political Science Association, Chicago, August 30, 2007), 2, www.allacademic.com/meta/p211482_index.html; emphasis mine.

Viveiros de Castro makes this point by way of Amerindian logic and practice, and on another level by his decision to fashion his essay as a Deleuzian object. Taking up many ideas to which Deleuze attends, and mixing in other ideas (including his own), the essay is what it describes, an analogic act of “worlding.” So, on one level, its thought vignettes are spaced to subvert conventional habits of reading, the spaces in between the vignettes putting up resistance to knowledge taken other than as a stuttering progression of moments encountering other moments. On another level, my experience as a reader was of a “doubling” or exceeding of the Deleuzian project—in one direction, an effect of folding back notions *to which Amazonians attend* into the many forms and voices and sites of agency to which they might relate in modernity: an instantiation for Viveiros de Castro of “field geophilosophy.” In another direction, the effect is of pulling out the stops and going after controlling elements within and outside the inquiry that could foreclose its project of “speculative ontology” and thereby its range of encounter with other ethnographic objects.

In short, I read this article as an invitation to deploy Deleuzian assemblage and Amerindian bricolage against prevailing political programs that would deny what really matters for actors about relations in their material worlds. Without consciousness of recombinant value, Viveiros de Castro argues, there can be no successful political action that Amerindians themselves would recognize as such. Indeed, it is an established tenet of postdevelopment theorists such as Arturo Escobar that Amerindian resistance to Western ideological formations of Right and Left, to orthodox science, and to the state, shapes itself in popular practices (such as vernacular art and ritual performances) that flash “back and forth between historical times, self and group, and alienation from and immersion in magic”—making these the undoing of master structures and narratives.³ A world so shot through with indistinctions can only be attentive to disorientation and, in particular, to the risks posed by aesthetic regimes of disorientation. My thoughts, like Escobar’s, turn to the famous Kayapo resistance movement, which gathered bits of its many traditions and appropriated the visual technologies of its oppressors in order to resist the building of a dam that would have flooded their land—the iteration of their history—and would have rendered the Kayapo unrecognizable to themselves. Understanding such a movement as a fitting together of puzzle parts would be inapt; such movements, their values, are transforming and transformative.

In this context, one wonders why alternative solidarities that form in the event-time of bricolage are apparently so threatening to political theorists and activists. The current prominence of “development” discourse and paradigms are, of course, partly accountable for this situation. But, more deeply, there appears to be resistance to the unsettled complexity of the notion of a sensation-

3. Arturo Escobar, *Encountering Development: The Making and Unmaking of the Third World* (Princeton, NJ: Princeton University Press, 1995), 220.

sensitive “point of view.” It draws attention, for example, to complex locations of agency and to the workings of reflexive awareness in unseen as well as seen realms, much more than to reified constructs such as “poverty,” which people believe they already understand.

Thus, in aesthetico-political terms, Viveiros de Castro’s rearrangement of ethnographic insights so that they are comparatively new demonstrates where field anthropology in a sense (and also through the sensorium) lives. His essay dares to address Western philosophical inquiry in these terms: “You are welcome in Amazonia, of course. Understand yourself differently here. Judge yourself by your moments of comfort here. I/we refuse to adjudicate. Neither will I/we foreclose the possibility of our putting questions to you just as you put them to us.” In other words, his essay proposes to transfer control over people’s futures to themselves. Doing so would involve wresting control from programs of Westernization that do not take seriously, for example, the “multinatural formations”—the nonhuman actors—to which the Amazonians engaged by Viveiros de Castro attribute exchange potential and with which they recognize a mutual caretaking relationship. If we accept that his essay offers a kind of mission statement for “perspectival multinaturalism” and that its mission is one of *conversion reversal* from Western or Westernized to Amazonian ontology, there is a clear and certain value to repeating the exercise at other ethnographic sites, though not only as a comparative statement of what the view is like from somewhere else, but also as experienced from *somewhen* else. This is a point that Giovanni da Col—correcting for the spatializations that Viveiros de Castro’s perspectivism tends to favor over temporalities—makes with regard to Yunnan Tibetans.⁴

A Semiosis of Arrows

On this point, I confess to a deep fascination with the essay’s discourse on arrows. The Amerindian arrow is the “archetypal mediator of action at a distance and one of the most ubiquitous images of effective intentionality. . . . look where the arrow is pointing and you will get somewhere . . . look to where the arrow came from and you will find someone.”

In the Melanesian island region where I have worked, menstrual cramps are an effect of being “pierced” by the moon. Someone listening to a painfully beautiful song or a speech may murmur, “Hatieu ipoi” (“It pierces my heart”). But arrows move away from bodies also, as visible or invisible objects are withdrawn. When islanders watch the rays of sunlight vanishing outward from the horizon at sunrise, what they see is the action of spirits withdrawing their spears

4. Giovanni da Col, “The View from *Somewhen*: Events, Bodies and the Perspective of Fortune around Khawa Karpo, a Tibetan Sacred Mountain in Yunnan Province,” *Inner Asia* 9.2 (2007): 215–35.

from the body of the sun: a resurrection scene. A healer's work is to withdraw from the victim's body the sorcerer's arrows or the poison that the arrows have delivered, the cause of pain and sickness. As in Amazonia, the sign of the arrow is for looking both ways: look backward, and you see someone; forward, and you see somewhere—or again, *somewhen*. The sun and moon are the original timepieces for the islanders I lived with; their separation, as told in mythic narratives, divided day from night, creating the conditions for life on Earth.

Viveiros de Castro's arrows anticipate their being effective. It need only be said, "my aim is true," *and so it shall be*—if not this time, then another time.⁵ Under the influence of the archetype, of the template, other arrows must follow: the arrow promises seriality, portends consequentiality. The gaze follows its launch, lending to the effort of sending the arrow a purpose. No matter if this arrow is ineffective—if there are structural defects, or if it lands wide of the mark, or if it has too little power behind it, or if it has too much power and curves back on the sender: there will be another chance. We are in the realm of the archetype.

This arrow marks a possible world of guided intentionality—of critical optimism, as Deleuze would call it. But the arrow archetype does something other than point (formally and materially) toward a future course refigured from past courses. The arrow alleviates pressures brought by the reality of gaps. There are such things as blinds that harbor planned interference to the arrow's hoped-for course (an ambush is an example). And likewise there are natural contingencies: a bird may erupt from the blind, or the arrow be sucked down to earth by a down draft, or the opposite. (Nature coauthors outcomes.) Further, there is the gap that animates a fear of success, and the one that marks the breach of a social contract. Not least, there is the gap too large, which defeats even the most thoughtful preparations, the best intentions and skills. The perspective from inside the gap, from the ground if you will, is not (in short) to be overlooked: seriality continues or ceases only in relation to this active field. Among its other functions, Viveiros de Castro's arrow makes this ground apparent and memorable.

His essay is memorable for its refusal to structure evenness of attention or to accept that memory is an argument (for, if so, it is a colonizing argument). Against the parataxis of numbering, his thought vignettes stand no chance of being differentially weighted or valued; modern thought structures like "core" or "periphery" stand no chance at all. The gaps between vignettes create breathing space for enjoying the journey through its own archipelago and, it so happens, through close encounters with elements of my own ethnographic past. The stimulation of personal memory cannot be merely an accident.

5. See Karen Sykes, "My Aim Is True: Postnostalgic Reflections on the Future of Anthropological Research," *American Ethnologist* 30.1 (2003): 156–72.

VÚJÀ DE AND THE QUINTESSENTIALISTS' GUILD

Roy Wagner

On a sunny afternoon the Reverend Kenneth Mesplay was riding his motorcycle through Pelia Village at Karimui. An extraordinarily gifted individual (he eventually became a shaman), Mesplay had a muted reputation for being an authority on oversize, ill-defined male beings inhabiting the world above. He was flagged down by a group of men casually lounging about the center of the village and smoking their characteristic long, tubular bamboo tobacco pipes:

Excuse me, sir, but we believe that there is a very large man up there, smoking a bamboo pipe just like this one. He is so large, and the sky so difficult to see through, that all we can see is the lit end of the tobacco-roll inserted in the end of his pipe. Very slowly, as the day progresses, he moves the pipe in an arc across the sky, like this (demonstrating), and we take that motion to be the movement of the sun.

Mesplay said something at once encouraging and dismissive, like “I think that is a very good idea,” and rode on. Still, the main point of relativity has never been put quite so well. (Einstein might have said the same thing about *his* pipe), and the idea floated that day has never been reported on or even alluded to before or since by any person, foreign or indigenous to the area. Pelia is a large community of mixed Daribi and Pawaiian extraction, located near the lower edge of Karimui

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DOI 10.1215/0961754X-2010-048

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airstrip. It has a local reputation for some highly refined intelligences and was once the home of the “fast gun of Pelia,” a man reputed to be able to hit any target with the characteristic unfledged arrows in use at that time.

The Pelia Metacosmology

The more conventional or commonplace Daribi (and possibly Pawaiian) cosmology—or, at least, the one I was told whenever I asked about it—goes something like this: the sky (*takaru*) is a semitransparent and permeable membrane stretched over the earth like a dome; on the other side of it live some beings called the Sky People (*takaru-bidi*), including Iwai and Mawa, the two cross-cousins who originally shot the sun and moon into orbit with their bows. Celestial objects, such as the sun, moon, and stars, “and also the clouds,” come out of the sky and go back into it. Those are their most important motions, the people say, and although it is clear to them that the objects have other motions, those are regarded as incidental (read: “important only to white men”). The facts that the sky grows light in the morning after sunrise and dark again after sunset are taken for granted and almost never connected with the above scenario.

Of course, if there really were a different Pawaiian cosmology, I could compare it with the Daribi one and thus essentialize two different “cultures,” using as a control the information that the Pelia, being a “hybrid” population, were forced to strike a compromise between them—a compromise lasting for one afternoon, at least, with a very self-conscious outsider present, and who knows what they thought of it later or would have thought of it otherwise. And if I did try to elicit some apocryphal Pawaiian cosmology, I would run into bigger trouble, this time with language. Daribi mimic the sounds of Pawaiian speech in calling it (and the people) “Yasa,” and Pawaiians do the same in calling the Daribi “Hari,” a minimalist form of comparative essentialism that “works” (in relative terms). Both Daribi and linguists, and sometimes Pawaiia themselves, mimic the significance of the Pawaiian language by calling it “unlearnable,” a contrastive form of radical essentialism that “unworks” (in absolute terms). Daribi speakers assert that inveterate Pawaiian speakers actually *never stop* talking; unfortunately, no one is able to learn what the Pawaiians have to say about this, apart from the fact that there is a great deal of it.

That is the trouble with both comparativism and essentialism: each is a kind of chiasmatic cross-cousin of the other. Thus, Lévi-Strauss is right on both counts (and ought to have called his masterpiece *The Elementary Kinship of Structures*). I should like to think of Lévi-Strauss, Marilyn Strathern, Eduardo Viveiros de Castro, and myself as “quintessentialists” and rest my case on this chiasmatic double proportion: *As well compare one denominate culture with another as a part of experience, as compare one part of experience with another as a denominate culture.*

All thought is imitation, in other words, but not all imitation is thought (there is also sex, for instance, in which one part of the body imitates another so well that pretty soon there are a lot more of them). Notice that we are coming perilously close to what Richard Dawkins calls the *meme*, that which imitates your efforts to imitate it so well that you have no hope of second guessing it—nor it you, for that matter. So if Pawaiians learned to speak Daribi perfectly well (a thing no Daribi speaker has ever been known to do), they would have not only an unlearnable, but in fact (utterly) unspeakable, language; and through no fault of their own.

Chiasmatic Afterlife

Perhaps there is a sort of indigenous essentialism involved in the Karimui experience, after all. The very first patrol officer stationed at Karimui wrote in his notes that, “Unlike the Pawaiian people living to the east of the station, the Daribi, who live to the west, insist that there is no soul or ghost or other spiritual essence that survives after death, and that the dead ‘simply go into the ground,’ and their faces disappear.” It took me several decades to realize that this statement does not really mean what the patrol officer took it to mean, and that the Daribi were simply being very literal about their unbeliefs. A later patrol officer was to make the candid observation that the Daribi are a basically negative people, which checks out when one examines the details of their rhetoric, syntax, and grammar. Of course, an essentialist *would* put it that way. A *quintessentialist* would hit the nail squarely on the (question) mark and say something like, *All living people speak dead languages, therefore all dead people speak living languages*. This claim would surely get a rise out of the Daribi, who call this kind of syllogistic logic a *porigi* (“the talk that turns back on itself as it is spoken”) and claim it is the secret of all power, social as well as rhetorical.

All I had to do was ask some Daribi informants, “What do you call this non-existent soul, ghost, or other spiritual essence that survives/does not survive after death?” to get the answer: “We call it the *izibidi*.” (The ethnographer James B. Watson, visiting me at the time, witnessed this exchange.) In the Daribi language, the word means, literally, “die-person.” It does not mean “dead person,” which would be a contradiction in terms (though what else is a chiasmus?), since, according to the Daribi, a body is depersonalized after death. The important thing to remember about the die-person is that, although there is absolutely no proof of its existence, there is also no proof of its nonexistence, so that most of the time even the mention or thought of an *izibidi* will send people into a state of near-panic, and they will do almost anything to rid themselves of the possibility. The next question we need to ask is whether this behavior is (a) an absolute proof of relative essentialism or (b) a relative proof of absolute nonessentialism, since both of these alternatives are clearly copresent every time the subject comes up.

In other words, the situation is not so simple that we can say the *izibidi* threatens Daribi with the possibility of its existence, for it also threatens them with the possibility of its nonexistence (“and all of that fear in vain”). A resident of Pelia once put the matter very eloquently: “It is true that I leave little bits of sweet potato around the house for my children who have died, but I can never tell whether it is the children who eat them or the rats.” For my own part, I have often wondered whether the ancient Egyptians did not go through the whole process of mummification (eviscerating the dead, embalming them, wrapping the body and sealing it in a series of Chinese boxlike cases, lowering it into a shaft, and then dumping a pyramid on top of it), just to make sure the corpse stayed dead and did not reincarnate.

I only learned there was a second part to this problem when I returned to Karimui several decades later, in the year 2000, when “going into the ground” after death was explained to me. “Going into the ground” is relegated to a formula among the Australian aborigines, who use it as a standard reference for the fact of death itself. I was told that there are individuals called *hoabidi* (literally “soul-persons”) who not only “go into the ground” but become an actual part (*buru-hoa*, “place-soul”) of the living landscape afterward:

You see that mountain peak over there, the one we call “Kebinugai”? Well, that was once a hunter by that name, and now that he has *become* the terrain that he used to hunt over, it knows him as intimately as he once knew it, and he is able to send the [orientational] body-souls of birds and game animals [*puru-noma*'] to living hunters in their dreams at night, so they will be able to go out with their bows and find the creatures disoriented and easy to kill.

The transformed hunter is like those creator-heroes of the Australian “dream-time,” who show the way to initiates by “terraforming” the track through life that they must follow. (The movie *Field of Dreams* and the Kinsella novel it was based on also come to mind.)

This structure, too, is a chiasmus, or rather it is the figure-ground reversal of the *izibidi* chiasmus, in which uncertainty gives way to the dead certainty of one’s aim(s). If Heisenberg were the particle instead of the observer, he would find himself disturbed by the relativity of his coordinate systems to one another. If anthropologists deal in stories, then stories themselves must deal in nothing but anthropologists: “Back in the beginning, stories sat around the campfire telling people to one another.” After that, of course, anthropologists started telling themselves *apart*.

“The Anthropologist Wants to Be the Figure as Well as the Ground, and That Makes Things Hard to Follow”¹

There is no such thing as a story telling a story, or an anthropologist telling an anthropologist . . . *anything*. There *is* such a thing as people using a language their whole lives and never knowing it is a language: “Shucks, we thought that things just *happened* that way”—Yogi Berra called this trope “*déjà vu* all over again.” There is also such a thing as *vújà de*, which is described in my book *Coyote Anthropology* as essentially the figure-ground reversal of *déjà vu*.² The idea is cognate with what I have elsewhere called “antitwinning.”³ Let me illustrate: anthropology’s first professional articulation of the culture concept, by the so-called evolutionary anthropologists, had to do with the objectification of cultural “traits,” objects like the bow, behaviors like the ritual, concepts like animism, that served as objective indicators (“survivals”) of past evolutionary stages, stratificational proofs, if you will. The consequent imago of “culture” persisted in that way—despite the scathing critiques of Alexander Goldenweiser and others—like one of those memory glitches, in which one is certain one has seen something before, without being able to recall when, or how, or why.

In short, one loses the big picture in the small one, deliberately confusing the subjective with the objective, in the vain hope that one’s uncertain mental faculties will be replaced by some antithetical and truly objective form of memory—the object (say, in a museum collection) that is certain it will see you again sometime in the future. The result is a cross-disciplinary schizophrenia that is “institutionalized” and hegemonic to the profession, like one of those languages that are so adept at using people that they (the people) are never able to emerge from their (the languages’) self-consciousness. No one is able to determine where, exactly, any of our languages is coming from (hatched from the Great Egg of languages?), but we all have a pretty good idea of where it is going (not where we want it to be).

Further, no one is quite certain as to the exact etiology of schizophrenia (one has to be crazy to know about it, and then one really does not care). Coyote, a trickster who is so good at tricking others because he must trick himself worse to do so, can explain:

Coyote: “Well, little man, it goes like this: in relative terms we have two distinct possibilities. Either there are a great many (relative) cultures, located around the world, and anthropology is the only (absolute) standard of comparison (they call this “essentialism”); or else there is only one culture, which the anthropologists have secretly made up among

1. Nancy Sue Ammerman, personal communication.

2. Roy Wagner, *Coyote Anthropology* (Lincoln: University of Nebraska Press, 2010).

3. See Roy Wagner, *An Anthropology of the Subject* (Berkeley: University of California Press, 2001).

themselves, never cease teaching one another about, and project upon other peoples (the tribes of nonanthropologists) through the medium of publications on ethnological fieldwork. You teach the first of these possibilities to undergraduates and the public at large, and reserve the second for graduate students, who are obliged to prove it to themselves in their fieldwork and then get a degree for it as the official sanction of the Quintessentialists' Guild."

The problem with embracing both of these alternatives at once is that doing so makes a kind of *izibidi* of the profession—threatening people with both its existence and nonexistence at the same time. The problem with choosing either one or the other is that it makes a *hoabidi* instead: either the anthropologist becomes part of the landscape, or the landscape becomes part of the anthropologist. This is one of those nasty two-part analogies that can only have a four-part conclusion, in this form: (1 and 2) although the water goes down the drain with a different twist in the respective northern and southern hemispheres, (3) the water at the poles is frozen solid and does not go down the drain at all, while (4) the water at the equator actually comes *up* the drain, owing to the frequency of tsunamis and other tropical disturbances. But since the Daribi do not have drains and detest *even numbers*, we had best content ourselves with a three-part analogy, as in this Vorlon saying (cited by J. Michael Straczinski in *Babylon Five*): "Knowledge is a three-edged sword; there is your side, there is the other side, and then there is *the truth*."

What has the truth to do with *kinship* (pace David Schneider, who only asked what kinship has to do with the truth)? Of course nothing could be more relative than a relative, and cross-cousins would be even crosser if they came in threes, so the third edge of the sword often goes missing. But what is beyond all doubt the finest observation on kinship I have ever heard, one that resonates across all physical, emotional, and sociocultural sounding boards, comes from neither the Vorlon nor the anthropologists, but from the Daribi, and it goes like this: "A child is a wound from within." And so, in its way, is a chiasmus. Since the days of Lewis Henry Morgan, many anthropologists have persuaded themselves, with some help from psychologists and sociologists, that there is a large number of kin relationships that, in their permutations and combinations, configure the known kinship "systems" that may be found around the world. Furthermore, the relationships can be classified—that is, reduced to cultural ordering mechanisms—and turned into kinship diagrams (Morgan's one original invention), which are the stock-in-trade of the comparativist kinship expert.

But some other anthropologists, including myself, insist that there is only one kind of relationship, which consists simply in relating to people in whatever appropriate or inappropriate ways this may be done. Thus, according to one's lights, one may *overrelate* (exaggerate the protocols, as in authority relations),

mis-relate (as in joking relations), or *not relate* at all (as in avoidance relations), yet still only be doing variations on the single theme of relationship. As Gregory Bateson once pointed out, “one cannot not relate.” But anyone can appreciate that the approach I prefer is a matter of making distinctions (telling things apart), not of putting things together, as in a construction (set) or a Durkheimian Social Monstrosity. Least of all do I favor an approach that expects to find a symbolic construction of reality, which is a trick Piaget learned by watching children too closely and then actually believing them. Still, what is the relationship between the figure-ground reversal of culture and its apparent mirror imaging in the equally schizophrenic double take of kinship? Here again, we have one of those self-enacting paradoxes like the *izibidi*, placing the enigma of deceased personhood in inverted parentheses and thereby making the rest of the world seem to be a statement about *it*.

Culture is to kinship as the chiasmus is to the nonsymbolic construction of unreality: this statement fairly defines quintessentialism. Elsewhere I have called its methodical application—its application to practically everything in our arcane symbolic universe—as *obviation*, drawing on the dictionary definition of the word: “to anticipate and dispose of.” Taking this definition back to our original problem of metacosmology, we have the following “third edge” truth about cosmic origins and self-maintenance: George Gamow’s Big Bang hypothesis is to Fred Hoyle’s Steady State hypothesis as the paradox of the deceased person is to divine immortality. The two hypotheses need each other (for their own self-definitions) quite as much as they deny each other (for their mutual obviation).

“The One You See in the Mirror, That Borrows Your Whole Act of Looking, but Only to See Itself”⁴

Is it an expression of memory, the *déjà vu* of knowing you have seen that image someplace before, so that it is familiar enough to identify yourself in it? Or is it something else’s nonmemory, or imitation (*vújà de*) of the not-you, since mirrors reverse the polarity of laterality (sidedness), giving a picture of organic totality that no one else will ever see? Are the right of your left and the left of your right joined at the hip, like Siamese twins, in a chiasmatic counterpose to the way your body feels inside of itself? Are feeling/seeing and seeing/feeling reversed on one another, placing the rest of the world—the part of your vision beyond the edges of the mirror, the part of *its* vision beyond the edges of your reality—in inverted parentheses? Since the human field of vision has evolved in such a way that it cannot see itself directly—not see itself without the mediation of some external object (mirror, pool of water, or camera)—then the *vújà de* that is certain it will

4. Roy Wagner, *Anthropology of the Subject*.

see you again someday, the figure-ground reversal of the you, *is* who you really *are*, the quintessential you (as Jacques Lacan pointed out long ago). Chiasmatic operations of the kind I have been evoking, when they are applied to anthropology, are not passive reflections on the subject of anthropology, but rather things done to the subject. They are active participations that do not “conserve for” (in Piaget’s childlike phrase) the assignments of knower and known, which are merely symbolic. They are not *being* things (digital) but *becoming* things (analogue).

As I once wrote in a poem about Kali, who by her own self-definition is *déjà vu* and *vijà de* simultaneously (four-armed is forewarned): “No metaphor is what it thinks you are / but that it take your word as happenstance.” Here again we have one of those unbelievable figure-ground reversals of the possible and the impossible, like “Ammerman’s Gap” between cultures and anthropologists, or like the poetical conceit asserting its intellectual-property rights over author and reader alike. But if a metaphor could *not* think, as an agency in and of itself, we could not do so either—and if its poetry (or conceit) had no voice, we would be reduced to logical positivism, pseudoethical melodrama, or worse. To deal with this problem at all, even to conceive of it *as* a problem, one has to strip one’s thinking down to a very naive level, the quintessential state that anthropology demands of its poetry, and poetry of its anthropology. The purpose of anthropological writing is not to accumulate more facts, more theories, more critiques of theories, or even critiques of critiques, but, like the *hoabidi*, to move or fixate the reader’s point of orientation (Castaneda’s “assemblage-point”)—to make subject and object be one and the same thing. “Remain foam, Aphrodite,” Osip Mandelstam writes, “let the word be made music.”

ZENOS WAKE

Eduardo Viveiros de Castro

Translated by Ashley Lebner

While reading the comments on “Zeno and the Art of Anthropology,” I could not help but associate each with one with the various mythical arrows that I evoked in my essay. Matei Candea’s is an arrow I did see coming, as it moves within an intermittent dialogue that we have been having for some time now on the relations between endo- and exo-anthropology. And his comments do hit the target as they deftly connect the two margins or worlds between which anthropology deploys itself—showing, on the one hand, how the connection is precisely what *creates* the two margins (insofar as it tells them apart) and, on the other hand, how it makes the distinction relative (insofar as it makes any distinction indefinitely *iterative*). It does not matter which “way” we move, whether inward or outward, anthropology will always bring us elsewhere. Debora Battaglia’s comment reflexively focuses on the gaps: the gaps that open between the four vignettes in my text (four arrows that do not form a continuous trajectory), the gaps that open between intention and effect (epistemology and politics), between space and time (somewhere and somewhen), and within time itself (past and future). It is as if the gaps, Battaglia suggests, were what make the arrow *dis-locate*, in the double sense of moving forward and hitting an unforeseen target (which includes missing the target altogether). After all, contingency is “just” the meeting of two—but of

Common Knowledge 17:1

DOI 10.1215/0961754X-2010-049

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course there is always a third—-independent causal “trajectories.” And finally, there is the vertiginous Roy Wagner’s comment, which besides performing a figure-ground reversal of my metaphors, did to my text what the supernatural frog of the Amazonian myth did to the weapon of the inept hunter—broke it down into microreferential bits and, after rubbing on a quintessential, magical unguent, reconstituted it as a powerfully obviational arrow. (Let us imagine obviation as the semiotic infrastructure of calculus.)

Rereading my article, I must confess that I squirmed a little at my impertinence with respect to Richard Rorty. Of the four “formulas” quoted, his is the only one treated in an aggressively critical fashion. I did not choose that particular statement, however, because it is representative of the author’s philosophy, or of his political credo, both of which, it is true, do not overly excite me; but this, to coin a phrase, is my problem. Rorty’s assertion simply seemed to encapsulate a useful antidefinition of anthropology. As with the other three formulas, this passage was taken out of context.¹ Or better, its original context, for me, was my reading it for the first time in a book that should go unnamed, where it was cited to support certain atrociously ethnocentric positions concerning the “beliefs” of an Amazonian people. Not believing what I read, I went to verify the quotation in the original article, where I discovered that its meaning was somewhat different (in other words, not altogether different). The formula seemed interesting to me, in its radical antirelativism (in one sense), coming from an author normally stigmatized as a relativist (in another sense). Now there’s your “comparative relativism” (in yet another sense)!

As for the substance of the three comments on my essay, I thank Matei Candea for observing how the “ontological autodetermination” of the collectives studied by anthropology does not refer to the ontological status of the world’s *peoples*, but rather to the *popular* origin of the ontologies we study. In other words, the term refers to the analytical decision not to reduce anthropological alterity to so many epistemological (“cultural”) variations surrounding an ontological invariant (“nature”) to which the epistemology of the anthropologist would have privileged access. That decision is the only one that is consistent with Wagner’s definition, according to which “the purpose of anthropological writing . . . [is] to make subject and object be one and the same thing”—or rather (relying on Wagner’s comment once more), to make them *become* one and the same thing.

Candea asks about the means by which endo-anthropology can fertilize exo-anthropology, and his answer is: by constantly questioning the contrast between endo- and exo-, reconceived “not [as] a starting point but an outcome.” This is very well put. I would only emphasize that bringing the contrast into question does not imply its cancellation; if an auto-anthropology is, strictly speaking,

1. Except for Michaux’s formula, which is utterly self-(de) contextualized (see the original textual environment).

impossible, this does not make alterity indeterminable in turn, but merely motile or variational—what it always was, *de jure*. “The line between those visions we *ought* to take seriously and those we ought *not* to is never fixed or self-evident.” Very true; again, though, I would merely add that once the line is “procedurally” fixed, the *outcome* or the *outside* (those visions that we ought to take seriously) becomes pretty much self-evident. It is essentially a matter of tactical (procedural) quintessentialism.

I welcome Battaglia’s reading of my article “as an invitation to deploy Deleuzean assemblage and Amerindian bricolage” in tandem. Doing so permits our tracing an unexpected rhizomatic line to connect the Deleuzo-Guattarian contrast between minor science and royal (or state) science with the Lévi-Straussian contrast between bricoleur and engineer. That line locates anthropology definitively on the side of its “object” (which side are we on?)—an achievement of which only an authentic minor science is capable. Anthropology is a science of and at the margins of conceptual imagination, a knowledge practice that makes concept (subject) and figure (object) become “one and the same thing.” It is the study of bricolage through bricolage.²

Battaglia brings to the discussion themes from my previous work on Amerindian perspectivism. Noting its relationship to the current text is astute; the piece indeed “offers a kind of mission statement for ‘perspectival multinaturalism.’” To me, her idea of a “conversion reversal” seems to be very close to the “controlled asymmetry” that Candea sees in the same text. I also welcome her insightful remarks on the contrast between my tendency to spatialize indigenous perspectivism—reflected in my “Zeno” essay in a metatheoretical emphasis on “somewhere”—and the principally temporal inscription (“somewhen”) of other non-Amazonian versions of perspectivism. Battaglia is perfectly aware that this contrast itself is spatial or ethnographic (“what the view is like from somewhere else”), which raises a number of interesting questions.

Finally, what can I say about Wagner’s text that I have not already said or has not already been radically obviated by it?³ No mystery here, seeing that “Zeno and the Art of Anthropology” is replete with allusions to Wagner’s *The Invention of Culture*. But his current text brings a wealth of new elements for reflection, which demand time for rumination and space for digression. And, as Wagner has said somewhere, “digression, if not relativity, is the very soul of anthropology.” Absolutely.

2. Cf. Roy Wagner, *The Invention of Culture* (Englewood Cliffs, NJ: Prentice Hall, 1975), 34: “We study culture through culture. . . .” On “figure” vs. “concept,” see of course Gilles Deleuze and Félix Guattari, *What Is Philosophy?* trans. Janis Tomlinson and Graham Burchell III (New York: Columbia University Press, 1994).

3. Incidentally, while Coyote the Trickster appears in Wagner’s text in the first person, there is another Amer-

indian character who projects his disquieting shadow on Wagner’s comment: Tezcatlipoca, the Aztec “Lord of the Smoking Mirror”—the patron, then, of speculation, “a thing of *smoke* and *mirrors*,” as the author reminds us. And as Guilhem Olivier remarked, “the coyote was one of the favourite animal doubles (*nabualli*) of the Lord of the Smoking Mirror.” See Olivier, *Mockeries and Metamorphoses of an Aztec God*, trans. Michel Besson (Boulder: University Press of Colorado, 2003), 32.

Common Knowledge is published three times a year by Duke University Press (905 W. Main St., Suite 18B, Durham, NC 27701) in association with Bar-Ilan University.

The *Common Knowledge* community thanks Bar-Ilan University for its support, and especially President Moshe Kaveh, Rector Haim Taitelbaum, Dean Joel Walters, English Department chair Sharon Armon-Lotem, and Faculty of Humanities administrative director Efraim Singer.

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Common Knowledge is distributed by Ubiquity Distributors, 607 DeGraw St., Brooklyn, NY 11217; phone: 718-875-5491; fax: 718-875-8047.

Indexing

Articles appearing in the journal are abstracted and indexed in *Academic Search Premier*, *Arts and Humanities Citation Index*, *Current Abstracts*, *Humanities International Index*, *Literature Online*, *Scopus*, *SocINDEX*, and *Sociological Abstracts*.

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ISSN 0961-754x