

# COMPARATIVE RELATIVISM

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## Symposium on an Impossibility

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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.

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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.<sup>1</sup> 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.”<sup>2</sup> In the place of these spurious analytical practices, Lowie argued that anthropology should be put on a “broad inductive basis.”<sup>3</sup> 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.<sup>4</sup> 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.”<sup>5</sup> 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.<sup>6</sup>

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.<sup>7</sup> 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.”<sup>8</sup> 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.<sup>9</sup> 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.”<sup>10</sup> 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.”<sup>11</sup> 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.<sup>12</sup> 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,” *Inner 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).<sup>13</sup> 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).<sup>14</sup> 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.”<sup>15</sup> 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.”<sup>16</sup> The aim is not agreement but alliance.<sup>17</sup> 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.