

the social sciences in the looking glass

edited by didier fassin
& george steinmetz

*STUDIES IN THE
PRODUCTION
OF KNOWLEDGE*

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cooperative primates & competitive primatologists

Prosociality &
Polemics in a
Nonhuman
Social Science

NICOLAS LANGLITZ

EVOLUTIONARY THEORY HAS ITS reflexive moments. For example, in 2010, when the journal *Nature* published a comment on a resurfacing controversy over how selfless behavior could have evolved, the philosopher of science Samir Okasha (2010, 653) demanded that “altruism researchers must cooperate.” Instead they once again fought over whether natural selection only worked on individuals and their kin or whether it also operated on the group level, where even help to non-kin would benefit all group members, including the generous helper. *Nature* illustrated this state of affairs with a picture of a honeycomb, each cell inhabited by a bee in a white lab coat, either sulking or suspiciously eyeing its coworkers. Okasha warned that by descending into tribalism the warring camps risked causing serious damage to evolutionary biology. Financial support would be cut if funding agencies perceived the field to be in massive disarray. And, as had already happened during the “sociobiology wars” of the 1970s and 1980s, once more creationists might seize on and exaggerate the differences in opinion between biologists for their own ends. Okasha’s evolutionary anthropology of science clearly favored group

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selection theory and suggested that altruism researchers would be selected against unless they worked *with* rather than *against* each other.

This chapter examines the social behavior of behavioral researchers who contributed to the surge of interest in prosociality during the past three decades. It focuses on a primatological controversy over cooperation in humans and chimpanzees between two codirectors of the Max Planck Institute for Evolutionary Anthropology in Leipzig, Germany. As they jointly ran this international center for the study of our natural history, the American comparative psychologist Michael Tomasello and the Swiss-French field biologist Christophe Boesch did not cooperate but instead argued over the question of whether the ability to cooperate set apart or united *Homo sapiens* and *Pan troglodytes*. Thus primatologists reanimated an old philosophical quarrel, imagined as a debate between two Enlightenment thinkers. “So is the devilish Hobbes or the angelic Rousseau correct?” asked Tomasello (2009b, 44). “Are humans by nature kind or mean-spirited?” The debate between the experimenter and the fieldworker was as much about primate behavior as it was about primatological methods. And it was about the ethos of science: Should it be an essentially collaborative or a competitive endeavor?

My essay contributes to the social science of social science a study of primate sociology in a broad sense (neither Boesch nor Tomasello self-identifies as a primate sociologist, but both have studied the social behavior and cognition of primates, and Boesch [2012] describes his approach to chimpanzee societies as ethnographic). I take Okasha’s intervention as an incentive to examine a rarely vocalized reflexive dimension in primatology as research that primates conduct on primates. Qualified by socio-cognitive differences between primate species, conceptions of competition and cooperation among primates should also affect how primatologists understand their own behavior, including their scientific research. Thus far, I don’t practice reflexivity but make it an object of social scientific observation and reconstruction.

Yet findings in primate sociology also pertain to research on human sociality, even if sociologists rarely compare and contrast their subjects or themselves with members of other species. One exception that proves the rule is the use that Bruno Latour made of Shirley Strum’s primatological research on baboons, which led me to interpret his early Actor-Network Theory as a “primatology of science” (Langlitz 2019; Strum and Latour 1987). At a time when a growing number of social researchers have discarded the opposition of nature and society and consider the two-cultures divide between the natural and the social sciences an anachronism that new interdisciplinary methodologies should overcome, the social science of primate social science

can facilitate a conversation about what both sides share and what keeps them apart. In this essay, I focus on issues that both students of human and primate societies have struggled with: whether or not to think of humans and other primates as primarily cooperative or competitive and whether cooperation or competition is the better behavioral strategy to advance knowledge and understanding in our respective fields of scholarship. So the essay will conclude with the question of what ethos would be appropriate to a reflexive social science beyond the human.

Primate Sociology

Since its inception in the early nineteenth century, social science had never been an exclusively human science. Although Auguste Comte, who had given sociology its name, had still speculated that only humans formed societies, the first doctoral thesis in sociology, defended in 1877 at the Sorbonne, took animal societies as its object (Heilbron 2015, 63). Critics of Alfred Espinas's *Des sociétés animales* (1877) dismissed it as a mere zoology, but the *soutenance de these* took place at the Faculté des Lettres rather than the Faculté des Sciences (D'Hombres and Mehdaoui 2012, 33–34). By the 1870s, Charles Darwin and Herbert Spencer had sufficiently established the continuity between human and animal societies for a reviewer of Espinas's book to curtail the author's claim to a new science: the ground of "animal sociology" was "all but unoccupied" (Collier 1878, 105). The controversial question was not whether animals lived in societies but what that meant for "the limits of the Social Science." "How far down the animal scale are we to go?" asked the reviewer. "Are not plants societies too? . . . If animals and plants are societies, are not masses of inorganic matter also societies?" (108). What was the scope of a sociology beyond the human?

Émile Durkheim referred to the literature on animal societies to compare them with human society. To be sure, the gulf was vast. But the differences in kind had grown out of a difference in degree: human groups tended to be larger. "Even the smallest we know are more extensive than the majority of animal societies," Durkheim (1964, 345) claimed. "The more people there are in association, and the more they react upon one another, the more also does the product of these reactions pass beyond the bounds of the organism." This quantitative difference gave rise to a qualitative difference. Colonies of lower animals could only act collectively by doing the same thing at the same time. But social evolution had created more complex and differentiated societies. Modern humans, especially, practiced a division of labor, in which individuals

acted independently while also depending more on each other to get by. Based on such cooperation, their “organic solidarity” contrasted with the “mechanical solidarity” of more primitive forms of life (Durkheim 1964, 283–84).

Sociological studies of primate groups constituted a core component of primatology as the discipline began to emerge in the 1930s. Solly Zuckerman (1932) argued for a divorce of human and animal sociology. Describing the social lives of nonhuman primates in the same vocabulary as that of humans amounted to a variety of anthropomorphism known as sociomorphism, which was anathema to many European and American primatologists (Asquith 1986; Daston 2005). He pleaded for a description in purely ecological and physiological terms. However, at a time before long-term field observations (Zuckerman only knew wild monkeys from shooting them on hunting expeditions), little was known about primate ecology, so Zuckerman presented primate sociality as “determined primarily by the mechanisms of reproductive physiology” (1932, 29). Since this behavior was “blind” and “reflex in character,” what had appeared to observers as cooperation, mutual aid, and altruism could only be superficially and misleadingly cast in such anthropomorphic terms (Zuckerman 1932, 304–5).

This short essay is not the place for a history of primate sociology, which gradually shifted from physiological to ecological and genetic explanations (for a brief sketch, see Rees 2006). Suffice it to say that the controversy over whether human and nonhuman social behavior could be explained in the same conceptual framework did not come to an end. Nor did disagreements about how exceptional the human capacity for cooperation and altruism were. The evolution of behaviors that benefit other individuals or the group has been debated and politicized ever since, in the late nineteenth century, the English naturalist Charles Darwin and the Russian geographer and anarcho-communist Pyotr Kropotkin provided two very different images of nature: was it primarily shaped by competition over scarce resources or by mutual aid? Since the sociobiology debate of the 1970s, interest in prosocial behaviors has flared up again as a wide range of disciplines—from anthropology, economics, and population genetics to developmental psychology and primatology—have challenged the idea that humans naturally behave in predominantly selfish ways and can only be brought to care for and collaborate with others through a precarious process of enculturation.

Since the mid-twentieth century, these debates have received important impulses from Japan (Asquith 2000; Asquith 1981; Langlitz 2020, chaps. 1, 5, and 6; for a historical account of the human sciences in Japan, see Kingsberg Kadia, this volume; regarding the globalization of the social sciences in

general, see Heilbron, this volume). From the 1940s onward, Kinji Imanishi's Kyoto School developed an anti-Darwinist biosociology that emphasized harmonious coexistence in a hierarchically organized world of living things (Imanishi 2002). His student Jun'ichirō Itani (1985) developed his mentor's evolutionary theory into a research program for a comparative primate sociology. The Buddhist tradition, which held that souls transmigrated between humans and other animal species, helped monkey and ape researchers to overcome "anthropodenial," the false negation of humanlike traits in animals, which the Dutch primatologist Frans de Waal (1999) considered as grave an epistemological error as anthropomorphism. He spoke of a "silent invasion" of Japanese primatology, which helped the discipline at large to put into perspective the cultural biases of free-market capitalism that had informed primatology's overemphasis of competition (de Waal 2003, 2001).

Historically de Waal's claims are questionable, since many European and American animal sociologists, from Espinas (1877) to Carpenter (1942) and Kummer (1971), had also highlighted the importance of solidarity, cooperation, and a peaceful and well-coordinated coexistence for the survival of non-human primate groups. But more than anyone else in late twentieth-century primatology, de Waal (1997, 1990, 1982) moved center stage the idea that so-called prosocial behaviors such as cooperation, helping, reconciliation, consolation, empathy, and so on, are no thin culturally learned veneer under which nature is red in tooth and claw.

The bottom line of the historical narrative of a prosocial turn in the behavioral sciences, which de Waal and others have spun, is that, for at least four centuries, Western thought has been led astray in presupposing an essentially egoistic human nature that needs to be restrained by the state or let loose to serve the common good on free markets—or, I might add, in academic controversies (Benkler 2011; de Waal 2010; for a historical sketch of this development, see Milam 2012; Sennett 2012). This somber human self-conception is currently brightened up by researchers showing prosocial behavior to be as deeply rooted in our biological constitution as purely self-serving and aggressive conduct. That man was a wolf to man now appeared as a politically consequential misunderstanding of the behavior of both canine packs and primate groups.

Cooperation Controversy

The comparative psychologist Michael Tomasello and the field primatologist Christophe Boesch would both fit into the narrative of a prosocial turn. As far as human nature is concerned, they share some common ground. As

codirectors of the Max Planck Institute for Evolutionary Anthropology in Leipzig, both understood our social behavior as shaped by natural history, and neither of them doubted that we were born with the cognitive capacity for cooperation, mutual aid, and altruism.

Experiments in Tomasello's laboratory painted a sympathetic portrait of *Homo sapiens* that bears little resemblance with the Hobbesian *Homo homini lupus*. Even one-year-old human infants Tomasello's group had tested were eager to help without having learned to be so. Later considerations of reciprocity, reputations, norms, and so on would mediate their cooperativeness (usually based on mutualism rather than altruism; Tomasello 2009b, 4, 45, 52). By and large, this mediation even increased the human potential for collective action.

Tomasello's French Swiss colleague Boesch did not deny our ability to cooperate. As a former rugby player, he had learned that a team could only win if everybody followed the motto of the Three Musketeers: "All for one and one for all." Yet Boesch (2012, 92) had also experienced the limits of human cooperation: "The demon of selfishness lurked around every corner and whenever one player had the impression that he could succeed alone, he would invariably forget the team and sadly be knocked to the ground."

Tomasello had also experienced the dark side of human prosociality. Having grown up in the American South in the 1950s and '60s, which he conceived of as an apartheid system, he knew firsthand how the group-mindedness in cooperation could lead to aggression against other groups, corroding the social integration of multiethnic societies. In the face of an increasingly globalized world inhabited by a fast-growing human population of unprecedented size, he wondered whether our evolved capacities for cooperation in small groups scaled up successfully to large-scale modern civilization: "We are still here. But of course we are only a few nuclear bombs or a few more decades of rampant environmental degradation away from not being here" (Tomasello 2009a).

At the height of the Obama era, however, Tomasello spun a surprisingly optimistic narrative. He drew political hope from natural history. While evolutionary psychologists had worried that our modern skulls housed a Stone Age mind, which had not evolved to cope with the exigencies of a globalized industrial world that was home to more than 7 billion people, Tomasello's antireductionist account of cultural inheritance and social coordination suggested that *Homo sapiens* had acquired mechanisms of behavioral adaptation many orders of magnitude faster than organic evolution (Tomasello 1999). This made him

confident that the very capacities that had led to the problems humanity was now facing also enabled the political practices and institutions that would solve them: “New prosocial norms for being careful with our environment and for recognizing the dignity and value of all peoples from all ethnic groups seem to be spreading in influence, not receding, and we are continually finding new ways for creating more cooperative and open arrangements for communication and coalition-building in large-scale societies,” Tomasello (2009a) claimed a few years before a surge of right-wing populism in Europe and the United States put his belief in historical progress to the test.

Tomasello considered the socio-cognitive capacities underlying such progress the prerogative of *Homo sapiens*, while his experiments with apes in the Leipzig zoo suggested that they could not cooperate with each other, nor did they altruistically teach or assist others to get food, even if such help came at no cost to themselves. Their egocentrism went so far that mothers competed and refused to share food with their own children (Tomasello 2009b, 21–28). “Great apes are all about cognition for competition,” Tomasello (2014, 31) claimed. “Human beings, in contrast, are all about (or mostly about) cooperation.”

The field primatologist Boesch (2012, 2002, 1994), on the other hand, had described how wild chimpanzees in Taï Forest, Ivory Coast, cooperated when they hunted monkeys. A sophisticated division of labor between a driver, a blocker, a chaser, and an ambusher increased their chances to make a catch. He had not been able to observe such a division of labor when he visited Gombe Stream National Park in Tanzania. Boesch (2012, 86, 91) interpreted this geographical difference in behavior as a difference between two hunting cultures. Its cultural nature did not free hunting from ecological constraints: while the open woodland of Gombe enabled lone hunters to capture a monkey in an isolated tree, the continuous forest canopy of Taï provided ample escape routes and forced the chimpanzees to hunt in a well-coordinated team. He took this evidence to be compatible with Tomasello’s demanding definition of collaboration as based on shared goals and intentions (Boesch 2005, 692). Of course, such cooperation would only be sustained if the captor did not keep all meat to himself but shared it with the other hunters—not based on each individual’s place in the group hierarchy but on their contribution to the joint endeavor. That was exactly what the chimpanzees of Taï Forest did, Boesch claimed. Thereby, he directly contradicted Tomasello, whose experiments in the Leipzig Zoo suggested that chimpanzees did not cooperate or voluntarily share with each other (Tomasello 2014, 35; Tomasello et al. 2005).

Laboratory versus Fieldwork

This disagreement about whether cooperation distinguished humans from other apes led to a heated methodological controversy over controlled experiments and field observations. Boesch (2005, 693) accused Tomasello of ignoring forty-five years of field studies on wild chimpanzees: “Observational data are dismissed as mere anecdotes or are discredited as not conclusive because alternative scenarios could always be constructed.” Comparative psychologists of Tomasello’s ilk reminded Boesch of “the old philosophers” who pronounced on what made humans unique based on their personal convictions and intuitions rather than experience and empirical data (2005, 691).

Tomasello and colleagues (2005, 722) countered that his group knew the field data which Boesch sought to leverage against them but that these data “have many interpretations in terms of the cognitive processes involved.” On the basis of mere observations Boesch could not rule out leaner and less anthropomorphic interpretations of chimpanzee behavior. For example, what appeared to Boesch like a collaborative activity with a shared goal might have been a motley of opportunistic tactical choices. An individual might respond to the relative positions of prey and other hunters without coordinating with the latter: maybe everybody just hoped to “get lucky” (Tomasello et al. 2005, 722). And if the hunt was really collaborative, why did the captor still get to keep a larger chunk of the meat instead of handing out equal shares to all participants? Only controlled experiments could determine the underlying cognitive processes.

Thus Tomasello proposed scientific cooperation based on a division of labor between experimental psychologists and field biologists: fieldworkers reported *what* animals did in their natural habitat; experimenters revealed *how* exactly the animals did it cognitively (Tomasello and Call 2008, 451). As far as Boesch’s observations of cooperation among chimpanzees was concerned, Tomasello’s lab had provided evidence that the animals did not share intentions and adopt different roles. Pace Boesch, they did not collaborate with each other to achieve a common end, experiments by Tomasello and colleagues (2005) suggested.

Boesch, however, called into question whether these experiments actually allowed Tomasello to make valid claims about the difference between how humans and chimpanzees thought. Since the early days of comparative psychology when, in the 1910s Louis Boutan tested human children against his gibbon *Pépée*, the validity of cross-species comparisons rested on the similarity of the respective experimental and psychological conditions (Thomas

2005, 443). But Tomasello tested human children and adult chimpanzees under different conditions. While the children were “free-ranging,” as Boesch (2008, 453) put it, and came to Tomasello’s laboratory at the Max Planck Institute in the company of a parent, the chimpanzees lived in captivity and, for most experiments, they were isolated from their group. The children were tested by conspecifics, chimpanzees by another primate species. Since the experimenters did not want to take the risk of entering into an experimental booth with a potentially violent chimpanzee, they interacted with test animals through a thick Plexiglas wall, while no such wall separated experimenters and children. While toddlers were told what to do in their native language, verbal instruction could not be given to the apes. Such differences between experimental conditions allowed critics to call into question the proposed causal relationship between experimentally manipulated variable and observed effect. Every uncontrolled variable enabled alternative explanations and compromised the so-called internal validity of the experiments. As a consequence, laboratory data turned out to have as many interpretations as field data.

Boesch also challenged the external validity of Tomasello’s findings by calling into question whether they applied beyond the walls of his laboratory. “The ability to care for the welfare of others has been denied to chimpanzees by some authors on the basis of experimental results obtained from captive chimpanzees,” he wrote. “This difference should not come as a surprise as we should not expect that individuals would care for others without any prior experience of group solidarity, and such solidarity will develop only if external pressures favoring it exist” (Boesch 2012, 102). Living in the Leipzig Zoo, Tomasello’s test subjects did not have to fend off predators, nor did they have to cooperate to obtain meat. They relied on keepers to provide the necessities of life. In Taï Forest, by contrast, the chimpanzees were regularly attacked by leopards or neighboring chimpanzee groups competing over scarce resources. In such situations of life and death, they would rush to each other’s defense (Boesch 2012, 96–97). If an infant lost its mother, Boesch and colleagues (2010) had observed, the orphan frequently got adopted by a genetically unrelated foster mother or father who extended group solidarity to children even though they could not reciprocate. Considering that such adoptions had been observed less frequently at East African field sites where leopards had either been eradicated or did not attack chimpanzees, Boesch (2012, 100–102) presented the altruism of the Taï chimpanzees as cultural. “Well-tuned captive experiments incorporating socio-ecological circumstances equivalent to those seen in nature might elicit cooperation more readily in animals,”

Boesch (2012, 105) concluded. “Sadly, though captive conditions are not ideal for this and engineering experimental situations mimicking group hunting, predator attacks, or territory defense are probably not possible.”

Even if the Leipzig chimpanzees could be tested under more natural conditions, Boesch doubted that they would behave like their wild conspecifics. Many had been obtained from a Dutch biomedical research institute. To provide a sense of how abnormal Tomasello’s test subjects were, Boesch recounted that when they first arrived at the zoo, these chimpanzees did not dare to enter the outdoor compound because they had never seen any grass. “Now it’s more than fifteen years and the zoo director has just told me that some individuals still don’t go on the grass,” he said to me in an interview in 2013. “Can you imagine what these chimpanzees must have gone through?” (see also Boesch 2012, 203–4).

Underneath Boesch and Tomasello’s methodological disagreements loomed the old theoretical rift between universalism and particularism, which primatologists had inherited from anthropologists. Against the background of what Boesch (2007, 233) considered the single most important finding of the last two decades of chimpanzee fieldwork, namely the behavioral diversity within the species, he asked how the behavior of psychologically deformed animals could represent *the* chimpanzee while Leipzig kindergarten children represented humankind. In a bellicose tone, Boesch (2012, 41) alleged that “such captive studies would be akin to studying the culture of the Aka Pygmies in Central Africa with Nigerian prisoners in German prisons!”

Science: Cooperation or War?

Although neither Boesch nor Tomasello wrote in a reflexive key, the distinctive ways in which they engaged in this controversy reflected their anthropological positions. While Tomasello highlighted the exceptional place of *Homo sapiens* in natural history by painting an almost black-and-white picture of cooperative humans and competitive apes, Boesch argued for human-animal continuity by presenting humans as less cooperative and chimpanzees as more cooperative than his opponent would admit. In their debate, Tomasello fashioned himself as a highly cooperative human, while Boesch adopted the persona of a competitive hominoid.

When Boesch accused him of disdain for observational data, Tomasello pushed back in a cool tone that betrayed nothing of Boesch’s thymotic anger. He denied that there even was a “debate on the relative importance of field observations versus controlled captive experiments,” as Boesch (2007, 227)

had claimed: “Both are necessary, and their functions are complementary” (Tomasello and Call 2008, 451). Why not work together?

In an interview with me, on the other hand, Boesch paraphrased the French philosopher Bernard-Henri Lévy (2010, 33–56) as saying: “Philosophy is war. When I discuss with another philosopher my goal is not to compromise, my goal is to convince him that he is wrong and should accept my opinion.” Boesch added:

I feel science should be like that. Science should not be about doing politics. It should not be about reaching compromises, but about finding the one solution to go forward. Some people in the field of cognition say we need all the different approaches to understand culture and cognition. I disagree. . . . If some people argue that field and captivity are complementary, that’s wrong. Removing animals from the wild and putting them into totally artificial situations, sometimes for generations, and then to test them in equally artificial experiments to claim that this was representative of what they could do in the wild is wrong.¹

Boesch doubted that *Homo academicus* should generally comport himself as a cooperative primate.

This bellicose vision of science can be traced back to the eighteenth-century philosopher Pierre Bayle, who had imagined the Republic of Letters as torn by a Hobbesian *bellum omnium contra omnes*: scholars would fight until all contradictions would perish and only incontrovertible truths would survive, with no Leviathan assembled from the multitude of conflicting researchers to trade academic freedom for security (Daston 1991; Koselleck 1988, 108–13). Throughout the nineteenth and twentieth centuries, the pendulum swung back and forth between combative and collaborative conceptions of knowledge production. Most relevant to a social science of primate social science is the fact that the sociological framework in which we are currently thinking about science was created by a generation of scholars who would have concurred with Boesch rather than Tomasello. In the 1970s, Pierre Bourdieu (2004, 45) turned against Robert Merton’s “irenic image” of a “scientific community” as a “world of generous exchanges in which all scientists collaborate towards the same end” (see also Bourdieu 1999, 31). Instead he sociologized Bayles’s vision of “a truth that has undergone the test of discussion in a field where antagonistic interests, and even opposing power strategies, have battled over it” (Bourdieu 2004, 84). Harry Collins (1983) regarded the analysis of such controversies as the royal road to understanding the social construction of scientific knowledge. Bruno Latour’s (1987) account of

science in action was teeming with war metaphors: only facts constructed so robustly that no scientific adversary had the resources to unscrew them could pacify select domains of research as they congealed into textbook knowledge. Despite all their internal differences, the social studies of science coalesced around their opposition to the positivist ideal of a unified science.

Many scientists perceived these social scientific descriptions of their work as hostile and struck back. The Science Wars of the 1990s broke out over many things—epistemology, ethics, politics, style—but they were also about what some scientists perceived as the sociologists’ overemphasis on competition and controversy. The Cambridge primatologist Robert Hinde (2000, 105, 115), for example, complained about an exaggeration of the differences between schools of thought that failed to present these different approaches against a background of their commonalities and the common goal of unifying knowledge.

The Hungarian philosopher of science György Márkus (1987, 36–37) noted that the natural sciences developed practices to contain dissent and establish a widely shared background understanding. These normalizing practices make challenges to the conceptual and practical foundations of scientific research in open polemics and controversies the exception, not the norm. By contrast, Márkus argued, the humanities and social sciences came to be articulated in a “polemic-dissensive manner” (34–35). Different traditions have been organized around theoretical alternatives, which can be traced back to texts considered classical because each provides a paradigmatic formulation to one or another of these alternatives.

If we followed this account of the two epistemic cultures, Boesch’s scientific warrior ethic rearticulated the ethnography of chimpanzee societies in the polemogenic manner that Márkus presented as the *modus operandi* of the human and social sciences. Although the primatologist opted for polemics to triumph over competitors, the history of knowledge seems to suggest that dissensive approaches are more likely to add perspectives than to eliminate them. Whatever the actors intend, the result is rarely a cognitive monoculture; rather, it tends to be an epistemically disunified intellectual space.

Conclusion

Bourdieu (2004, 91) sought to institute and collectivize reflexivity as the common law of the social scientific field, in which a “sociological critique of all by all” would intensify the truth-producing effects of the “epistemological critique of all by all.” This conception of science contrasts sharply with the ideal of a cooperation of all with all, which inspired Okasha’s plea for less contro-

versy and more esprit de corps among altruism researchers. If I end this essay on a reflexive note, it is not to second Bourdieu's rationale for a social science of social science or to warn that such calls for total critique might have self-defeating consequences for a scientific field, as Okasha prophesied. Instead of making any such spirited appeal, I would like to raise some genuine questions.

As research on social cognition and behavior extends beyond the human (Fassin, this volume), it creates an ontological borderland in which social and natural scientists encounter one another more frequently again. Calls for interdisciplinary cooperation abound. Both sides largely agree that the epistemic divide between the two cultures is an anachronism that owes more to the social organization of the nineteenth-century university than to the makeup of the world (although we saw that even in the nineteenth century, animals had been objects of sociological study). What natural scientists and posthumanities scholars disagree over is whether research practices should be hybridized under the sign of the natural or interpretive social sciences. While the sociobiologist Edward O. Wilson (1998) called for consilience between the two great branches of learning within the epistemological and ontological framework of the natural sciences, as he understood them, multispecies ethnographers tried to model the study of nature and our place in it on the humanities, replacing naturalistic observation by morally engaged witnessing and controlled trials in the laboratory by artistic experiments in exhibition spaces (Kirksey 2014; Kirksey et al. 2016; Kirksey and Helmreich 2010; van Dooren, Kirksey, and Münster 2016). But the dividing lines are not always clear-cut. The case of Christophe Boesch, a staunch natural scientist who operates in the polemic-dissensive manner of many humanists and social researchers, suggests that epistemic virtues and practices from both sides can be remixed in numerous ways and to different ends (Langlitz 2020, chap. 3; see also 2015).²

As the social sciences return to their animal origins, they need to decide which elements to adopt from the natural and the human sciences and how to assemble them. Do they want to collaborate on a shared vision of human and nonhuman sociality or engage in a pluralization of available alternatives? Do they want to implement and maybe collectivize reflexivity? If so, would it amount to a critique of all by all, or would less agonistic forms of scholarly life enable more consensus-oriented forms of knowledge? Should a reflexive sociology beyond the human bridge the gap between social and natural sciences by inventing new prosocial norms and forms for science or by fostering controversy over the hominoid condition, which great ape researchers share with the great apes?

Notes

- 1 For an ethnographic and historical case study of the integration of laboratory experiments, field experiments, and naturalistic field observations, see my article “Synthetic Primatology” on Tetsuro Matsuzawa’s chimpanzee research in Japan and Guinea (Langlitz 2020, chaps. 5–6; 2017b). In the human sciences, field experiments have also come to serve as a bridge between bench- and fieldwork. Behavioral economists, for example, use experimental games to study prosocial behaviors across cultures and species, both in the laboratory and in the field (Camerer and Fehr 2004; Henrich et al. 2005; Henrich and Henrich 2007; Jensen, Call, and Tomasello 2007).
- 2 A similar point could be made about Michael Tomasello, but it would require more space than is available here. See Langlitz 2020, chap. 4.

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