Primatology of Science

On the Birth of Actor-Network Theory from Baboon Field Observations

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**Abstract**

This article situates Actor-Network Theory in the history of evolutionary anthropology. In the 1980s, this attempt at explaining the social through the mediation of nonhumans received important impulses from Bruno Latour's conversations with primatologist Shirley Strum. In a rearticulation of social evolutionism, they proposed that the utilization of objects distinguished humans from baboons and that the use of a growing number of objects set industrialized human populations apart from hunter-gatherers, enabling the formation of larger collectives. While Strum's and Latour's early work presented baboons as almost human and suggested that we moderns had never been modern, the Anthropocene has reawakened curiosity about the original question of anthropology: how do modern humans, including modern scientists, differ from premoderns and animals? This eighteenth-century question is gaining new significance and urgency as we recognize our transmutation into a super-dominant species. But the answer might not solely lie in the use of more objects.

**Keywords**

Actor-network theory, science studies, objects, primatology, cooperation, Anthropocene

Actor-Network Theory was born from the spirit of a momentous transformation of primatology. Bruno Latour's bold attempt at opening up the social sciences to nonhuman actors surely drew from other important sources as well, but his intellectual exchange with the baboon researcher Shirley Strum left its mark on the underlying philosophical anthropology. As Strum discovered an exuberant complexity of social interactions behind the dominance rank hierarchies of baboon society*,* Latour sought to demonstrate how the highly-organized forms of knowledge and the corresponding social order produced by modern humans grew out of a turmoil that reminded him of the monkey troops he had observed in Kenya. If these animals already lived in complex societies rather than a state of nature, then what distinguished *Homo academicus* from *Papio anubis*?

At a time when many posthumanities scholars and a few natural scientists are eager to leave behind the two-cultures divide and the science wars that pitted social students of science against the sciences, this historical case study looks at a successful collaboration between a primatologist and an anthropologist of science. Its purpose is polemic. Much scholarship in science studies is based on the assumption that we have never been modern. In multispecies studies, the rampant deconstruction of differences has hyperbolized this mantra: now we haven’t even been human (Haraway, 2008: 1–157). At the same time, many prominent voices in primatology, including Strum’s, have presented other primate species as almost human. In a more conciliatory moment, I will happily concede that the proponents of such continuity between humans and animals as well as premoderns and moderns have good political and ontological reasons for the promotion of their popular agendas. In the humanities and posthumanities, however, the excessive dissolution of differences has made it impossible to even ask the question of how behaviorally and historically modern humans have come to colonize the globe and why, instead of living on a planet of the apes, it is we who drive most, maybe all other primates and a vast number of other life forms extinct.

While many posthumanists take anthropogenic climate change and the epochal loss of biodiversity as moral incentives to overcome the arrogant anthropocentrism supposedly responsible for this ecological cataclysm, the natural historical tragedy we are currently experiencing in real time should also reawaken curiosity about the original question of anthropology: How do modern humans, including modern scientists, differ from premodern people and animals? Writing for an audience, which has sought refuge from this politically troubling question in the assurances that we have neither been modern nor human, I will make my case by reading Latour and Strum against the equalizing grain. From the start, these authors have maintained an often overlooked human exceptionalism and a lasting interest in the anthropological problem of modernity. By reconstructing their primatology of science, which situates *Homo* and *Papio* on the same taxonomic tableau, but postulates a categorical difference between these speciesand a gradual difference between hunter-gatherers and industrialized humans, including modern scientists, I hope to rearticulate the eighteenth-century questions of human nature and modernity as the pressing twenty-first century question of how we have transmuted into what Strum called a super-dominant species.

The point at which I diverge from Latour’s narrative is that I don’t share his conviction that the changing relationship of *Homo sapiens* to objects and other nonhuman entities can sufficiently account for this development. While Latour emphatically dismissed cognitive explanations of science, this article will end with a plea for taking into consideration how the dominance of modern humans has also been enabled by the evolution of specifically human understandings of the social and physical world as well as the historically recent restructuring of cognitive passions such as curiosity and wonder and the cultivation of epistemic virtues like objectivity. At a time of mushrooming object-oriented philosophies, we need to bring them into a conversation with an anthropology, nay primatology of thought.

In sum, this article pursues three goals. First, it contributes to the history of the sciences and humanities a case study of a collaboration between a primatologist and an anthropologist of science, which situates Actor-Network Theory in relation to late twentieth-century evolutionary anthropology. Second, I want cultural anthropologists, science studies scholars, and posthumanists to engage again with anthropology's original question of what makes modern humans exceptional. This incorporates the article in a highly varied family of restorationist projects in cultural anthropology: the one thing they share is a sense of urgency to bring back the broader disciplinary and public significance anthropology enjoyed before it began to deconstruct its key categories and approaches in the 1980s (e.g., Boellstorff, 2008; Bunzl, 2004; Col and Graeber, 2011; Viveiros de Castro, 2004). Third, the article offers a critique of Actor-Network Theory, which suggests that the unprecedented powers of domination exercised by modern humans, largely mediated by science and technology, cannot be fully explained by the proliferation of more and more effective instruments but also requires attention to what distinguishes modern scientific thought.



## 1. Fighting over Peaceful Monkeys

In the late 1970s, still a vulnerable early career researcher, Shirley Strum exposed herself by promoting a new image of baboon troops. It arose from a series of profound theoretical and methodological transformations of primatology. In the previous decade, both scientific and popular books about human nature such as Konrad Lorenz’s *On Aggression* (1966), Robert Ardrey’s *Territorial Imperative* (1966), and Desmond Morris’s *Naked Ape* (1967) had emphasized the male drive to defend territory and compete with other males for social status (Milam, 2015). Savannah-dwelling baboons seemed a promising model to shed light on our evolutionary ancestors’ social behavior and early field observations suggested that male rivalries and hierarchies dominated their societies as well. Strum challenged this view. Sensitized by cues from the primatologists Thelma Rowell and Tim Ransom, her almost ethnographic description of the Pumphouse Gang presented a form of collective life shaped by female rather than male hierarchies and by reciprocity and collaboration rather than aggression and competition – and when the males occasionally did fight, victory did not translate into greater access to prized foods or fertile females that had previously been associated with higher status. Baboons lacked a male dominance hierarchy, Strum (1978, 1982, 1987: 75–81) concluded.

This almost serene image of *Papio anubis* starkly contrasted with the attacks, which the human “silverbacks” in her own field launched against Strum as she questioned their accounts of baboon sociality. They accused her of not having enough data and even of having invented it altogether. To Strum (1987: 158, 163) the "cutthroat politics" of her scientific community made the monkeys "seem 'nicer' than people." When she organized the conference *Baboon Field Research: Myths and Models* in 1978, she anticipated a continuation of this controversy and invited the anthropologist of science Bruno Latour – as the primatologists' "conscience" – to study this conflict over a new truth claim and an established academic dominance hierarchy (Strum, 1987: 161–2). Why, Strum wanted to know from him, did scientists ask certain questions but not others and why did they find some answers more satisfying than others.

It did not escape Strum’s ethological gaze that sex differences played a significant role in this dispute. The shift of scientific interest from competition to cooperation had often been attributed to the growing importance of women in primatology in the 1970s and 1980s, Strum (2000: 487) noted. But, while her colleague and collaborator Linda Fedigan (1997) asked whether primatology was a feminist science, as a researcher Strum kept her distance from what she took to be the political cause of the women's movement. She boiled down the diverse interests of feminists in primatology to the desire to find evidence for a primeval matriarchy (Strum, 1987: 82). Instead of an inversion of roles, Strum (1987: 149–50) found social reciprocity and complementary equality of males and females in most social domains, including politics and care taking, which, she believed, would disappoint both feminists and their opponents. This discovery, however, Strum did not want to attribute to her standpoint as a woman or her political convictions. Instead she emphasized that it was scientific practice, not gender, which set her approach apart from those of her mostly male senior colleagues.

Methodologically, long-term fieldwork, following a broad and representative sample of individuals for many years, had enabled Strum’s novel insights into the behavior of *Papio anubis*. While nineteenth-century naturalists mostly collected specimen and the first fieldworkers of the 1930s did not follow monkeys and apes through the wilderness for more than a few weeks, Japanese primatologists around Kinji Imanishi had established the new gold standard of primatological research in 1948 when they began to record the life and family histories of Japanese macaques across generations. From the 1960s onward, modern means of transportation and the availability of antibiotics allowed a growing number of students, including Shirley Strum, to conduct research in remote tropical locations for prolonged periods of time (Kappeler et al., 2012: 3–6; Sperling, 1991: 207). Previously, brief observations of the baboon group as a whole had all too often been captivated by tumultuous but often inconsequential skirmishes between males. Strum, on the other hand, followed each member, making sure to dedicate as much time to the highest ranking female as to a marginal male. During four decades of fieldwork, she discovered that the females rarely fought over their pecking order, but when they did these conflicts could paralyze the entire group for days. It was the resulting female dominance hierarchy that gave the troop a stable social structure (Strum, 1987: 137–42, 2012: 12). Her individualizing approach was in line with the sociobiological assumption that society is not the cause but an effect of individual decisions. But, unlike sociobiologists, she did not explain an animal's tactics and strategies in terms of its genotype. And, in contrast to structural-functionalists who had shaped mid-twentieth century social anthropology and primatology alike, Strum did not presuppose that individuals would fill in fixed positions in preexisting social structures but observed how they constantly negotiated their roles and relations anew.

The methodological and explanatory focus on the individual established in field research and sociobiological theory in the mid 1970s eventually led primatologists to see beyond competition and aggression. Since sociobiologists sought to account for all behavior in terms of reproductive success of individuals and their kin, they were especially puzzled by altruism and cooperation. Simultaneously, ethologists came to see how conflicts within primate groups involved the formation of alliances, consolation of victims, and reconciliation with aggressors (e.g., de Waal, 1982, 1990). What developmental psychologists called prosocial behavior began to attract primatological attention. After post-war evolutionary theory had conceived of the biotic world in terms of struggle between individuals, the pendulum now swung back to visions of nature that emphasized cooperation and even altruism, previously articulated in the 1890s by the Russian biologist and anarchist Piotr Kropotkin and in the 1920s by the left-leaning Chicago School of ecology (Mitman, 1992: 1–9, 64–71). If we were to apply the diagnoses of theoretical turns proliferating in the humanities to the behavioral sciences, the 1980s paved the way for a prosocial turn – although it was really more of a return. It culminated in various attempts to empirically disprove Thomas Hobbes’ philosophical anthropology. That man was a wolf to man now appeared as a politically consequential misunderstanding of the behavior of both canine packs and primate groups (e.g., Benkler, 2011; de Waal, 1997: 98; Tomasello, 2009: 3; for a historical sketch of this development, see Milam, 2012).

Although Strum contributed to this brighter view of primate life, she did not seek to reinstate an image of monkeys and early hominids as noble savages. Her attention to how baboons formed friendships, alliances, and collaborations emerged as a by-product of a much broader and more formal reorientation of Strum’s approach from studying the outcomes of social interactions to the underlying processes. “If you focus on outcome you see reproductive success, with some winners and some losers,” Strum (2017: 163) explained in an interview. “But if you look at the process, the situation suddenly appears much more complex and variable. Even if someone emerges as a winner from interactions that involved aggression, that aggression now appears to swim in a soup of all sorts of social interactions.” Very soon this intervention in primatology would converge with a parallel development in the sociology of science.

## 2. Fighting over Belligerent Scientists

In the eighteenth century, Hobbes's *bellum omnium contra omnes* had inspired the philosopher Pierre Bayle to imagine the Republic of Letters as an intellectual commonwealth across national and confessional boundaries that doubled as a battleground on which scholars engaged in a fierce epistemic civil war. They 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). In the 1970s and 1980s, this image was adopted by the sociology of science and the history of knowledge. The sociologist Pierre Bourdieu (2004: 45), for example, 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). Michel Foucault (1984: 74) described knowledge formations as the object and outcome of power struggles over rules separating the true from the false. British sociologists like Harry Collins (1983) regarded the analysis of controversies as the royal road to understanding the social construction of scientific knowledge. The most famous book resulting from this so-called Strong Program in the sociology of science was Steven Shapin and Simon Schaffer's study of the debate between Hobbes and Boyle about the existence of the vacuum. It ended with the insight that, although Boyle’s scientific claim that space could be devoid of all matter prevailed, "Hobbes was right" (Shapin and Schaffer, 1985: 344). He was right in that knowledge was the product of human actions. It was not produced by reality but by altercating researchers whose battles would eventually give way to consensus, when one camp emerged victorious.

As if these bellicose accounts of science had not only been descriptive but also prescriptive, they provoked a conflict between science studies and the sciences, which, in the 1990s, came to be known as the Science Wars*.* Scientists not only took issue with the claim that they had fabricated their findings, but also with what Cambridge primatologist Robert Hinde (2000: 105, 115) perceived as an exaggeration of the differences between schools of thought without presenting them against a background of their commonalities and the shared goal of the unification of knowledge.

In this conflict, Latour (2002) eventually came to present himself as a promoter of peace. But his constructionist metaphysics continuously delivered divisive concepts and philosophemes into this interdisciplinary crisis zone. He replaced the comparatively moderate assertion that scientific knowledge was socially constructed by the much more radical claim that its objects were constructed and only came into existence in the course of research activities (Latour, 1988: 88). With no nature out there to serve as a shared point of reference and neutral arbiter of disputes, different collectives of researchers either had to make an effort to build a common world, or they would forever remain apart. Maybe this ontology appeared less scandalous to a primatologist like Strum who observed animals habituated from a white Volkswagen bus that could only be studied because, after long and patient efforts, they no longer ran away from the researchers – a paradigm case of the nature/culture hybrids, which Latour saw proliferating wherever he looked. In any case, whereas the physicists Alan Sokal and Jean Bricmont (1998) came to regard Latour’s metaphysical speculation and engagement with the natural sciences as “fashionable nonsense,” Strum successfully collaborated with him.

## 3. Primates Without Social Contract

After his participant observation of the 1978 conference, Latour (2013a: 12) followed Strum to her Kenyan field site. They co-taught a course on the evolution of technologies and ecology and worked together at the University of California in San Diego almost every spring from 1979 to 1992. And yet no ethnographic study of primatology ever ensued. The publications resulting from their collaboration were no second-order observations of scientific controversies but first-order reflections on the difference between baboon troops and human societies: an empirically informed philosophical primatology, which became a corner stone of Latour's œuvre. While Strum had recruited Latour in the controversy with her colleagues over the social life of baboons, Latour enrolled the primatologist to help him overhaul social theory and social scientific methodology by “unscrewing the big Leviathan.”

Together with the sociologist Michel Callon, Latour wrote an article of this title a few months after returning from Kenya in 1979, which became the founding document of Actor-Network Theory. History, anthropology, and most recently ethology had disproved Hobbes’ idea that a social contract constituted human society and that society had only emerged with man, Callon and Latour (1981: 279) claimed. Since the 1960s, primatological fieldwork had revealed that baboons did not live in the bestial chaos, which Solly Zuckerman had observed in the 1920s in an oversized group crammed into the London Zoo (Strum and Fedigan, 2000: 8–9). If their lives had been nasty, brutish, and short, it was not because they lived in a state of nature, but because their existence in captivity did not at all resemble baboon life in the wild. In Callon and Latour’s (1981: 282) schematic historical narrative, baboons subsequently became a projection screen for a different vision of animal sociality: instead of waging a war of all against all they now lived in an almost totalitarian system of strict dominance hierarchies formed by fighting males. Consequently, the conception of the human changed alongside its other: now the flexibility of his social organization set *Homo sapiens* apart from his primate relatives. Callon and Latour (1981: 282) followed Strum’s revision of these accounts, which emphasized that no rigid system held social relations in a baboon troop stable and endowed the animals with extensive social skills "to repair, accomplish, and ceaselessly consolidate the fabric of such a complex society."

The sociologists compared Strum’s primatological approach to ethnomethodology. In Harold Garfinkel’s (1967) microsociological investigation of common sense methods people use for understanding and producing social order, actors appeared as constantly performing or achieving society instead of entering the fixed roles, classes, and structures presupposed by classical macrosociologies. Latour and Strum (1986: 170) noticed echoes of the political zeitgeist in the ethnomethodological shift of focus from society at large to interactions between individuals: "Much like the political debates in California and in the U.S., ethnomethodology is marked by a strong diffidence toward ‘macro-actors’. Ethnomethodology disputes the construction of macro-actors, much like California tax-payers want Big Government ‘off their backs’" (cf. Langlitz, 2016). Ethnomethodology had been one of the optics through which Latour and Woolgar (1986/1979) had ethnographically studied the nitty-gritty of research processes in a neuroendocrinology laboratory, also located in California, which produced facts from interactions between researchers and their measuring devices – work which had drawn Strum's attention to the young French sociologist of science. In Kenya, this Californian taxpayer taught Latour to view the baboons through a similar lens while he enabled her to conceptualize the approach. Since there was no social order the monkeys could take for granted, they constantly probed their relations and thereby revealed what their human students also sought to know. In one of their co-authored articles, Strum and Latour (1987: 788) pointed to the corresponding symmetry between primates and primatologists:

If, as recent evidence suggests, baboons are constantly testing, trying to see who is allied with whom, who is leading whom, and which strategies can further their goals, then both baboons and scientists are asking the same questions. And to the extent that baboons are constantly negotiating, the social link is transformed into a process of acquiring knowledge about "what society is."

At a time when many American anthropology departments broke apart along the front line between cultural and evolutionary anthropology, Latour and Strum looked at primates as their own primatologists and at primatologists as primates. Like Frans de Waal’s (2005: 53) comparison of the body language of a senior faculty member who had just become the victim of an academic intrigue to that of an overthrown chimpanzee alpha male, their animalization of scientists and the corresponding humanization of monkeys was still tongue-in-cheek – contributing to a slightly frivolous new genre of theory, which no self-respecting social science journal would have accepted twenty years earlier and which they could hardly have published in *Folia Primatologica*. But, in contrast to the scornful caricatures of an apish Charles Darwin a century before, Strum and Latour's humorous analogy served to make a serious theoretical point. By breaking down the great ontological divide between humans and nonhumans, their primatology of science situated baboons and researchers on the same taxonomic tableau, which revealed both commonalities and specific differences. That's what distinguishes a primatology from an anthropology of science that makes no attempt at understanding the place of scientists in natural or, really, "naturecultural" history. This novel approach paved the way for a whole family of related projects, ranging from Dominic Lestel’s proposal of an etho-ethnology or ethno-ethology that observes the shared lives of human and nonhuman animals through the same optic (Lestel et al., 2006) to the current mushrooming of multispecies ethnographies (Kirksey and Helmreich, 2010).

In the cases of Strum and Latour, the withering of structural functionalism in both social and evolutionary anthropology had enabled the convergence of their respective approaches. This theory had explained all behaviors as adaptations to larger social systems. Strum, by contrast, focused on how different animals navigated group life. Her analysis departed from individuals, eventually enlarged to include kin, who sought to maximize their reproductive success. A similar heuristic individualism had led sociobiologists to attribute an organism's tactics and strategies to its genes. But, attending more to outcomes than processes, they had failed to explain how a particular animal came to behave the way it did in a specific situation, argued Strum and Latour (1987: 788): "The sociobiological solution left moot the question of the proximate means by which society could be achieved. Smart gene calculators might be appropriate actors in an 'ultimate' scenario, but whole individuals coexisted, competed or cooperated as real participants in society." Moreover, politically sociobiology had been enlisted by the *Nouvelle Droite* to replace the republican foundation of France's social order by a biological one, noticed Latour and Strum (1986: 180). Thus, Strum kept her distance from sociobiology and instead joined the other intellectual insurgence shaking up the behavioral sciences at the time.

A child of the cognitive revolution, which also departed from observations of individuals but assigned their tactics and strategies in group life less to determination by selfish genes than to flexible minds, Strum saw baboons as intelligent creatures capable of responding circumspectly to changing situations (Strum et al., 1997: 62–3). In the 1970s, the primatologist Nicholas Humphrey (1976) proposed that the intellect of monkeys, apes, and humans had evolved in response to the problems of living in complex groups, especially the increased competition over limited resources, which required to manipulate, deceive, and cunningly cooperate with others. In two respects Strum set herself apart from what Andrew Whiten and Richard Byrne (1989) would call the Machiavellian Intelligence Hypothesis. First, she rejected the negative connotations of Machiavellianism because the term overemphasized exploitation, domination, and deception. In her eyes, primate social complexity amounted to “an intricate tapestry of competition and co-operation, of aggression and reconciliation, of non-aggressive social alternatives” (Strum et al., 1997: 74). Second, she replaced talk of individual tactics and strategies, understood as external manifestations of an internal plan of action, by cognitive anthropologist Edwin Hutchins’s account of distributed cognition. Accordingly, continual coordination and negotiation with other group members and elements of the physical environment extended the mind beyond the brain and solved the problems of social living (Strum et al., 1997: 66–73). For this purpose, baboons did not enter a social contract, but exercised social skills.

## 4. A New Social Evolutionism: From Baboons to Scientists

From his encounter with Strum's Kenyan monkeys Latour (2013a: 10–1) had learned "that an intense social life – that of the baboon troops […] – was perfectly compatible with an extremely limited use of technology. While baboons manifest an unimagined degree of social complexity, […] they still use only their paws and their brain." It was precisely because of these limited means to organize their life together that Strum and Latour (1987: 790–3) described it as highly complex. By "complex" they meant that the baboons constantly juggled multiple and often conflicting issues at the same time: competing with rivals within their own troop while keeping their offspring safe and defending food sources against other groups; negotiating whether the group should stick to its traditional lifestyle, regularly risk their lives raiding human farms that provided such nutritious crops that females reproduced more successfully, or whether raiders and non-raiders should split up, etc. (Strum, 1987, 2012). Latour's investigation of science in action revealed a similar complexity: researchers had to sideline contenders while building the broadest possible support for their truth claims, sought to ensure funding from industry while maintaining academic freedom, and tried to estimate whether a new research object would produce data original and solid enough for a high-ranking publication. All of this got mixed up before facts would become facts. The convoluted associations of elements deemed natural and social became the constant refrain of Latour's popular song of complexity. But this Janus-faced theoretician also sung a very different song, which did not catch on as much: eventually, simplification would turn a complex state of affairs into one that was still complicated, but well ordered. Science didn't always remain in action. Once scientists had closed a controversy, it no longer made sense to Latour (1987: 100) that anthropologists and sociologists would continue talking about their interpretations, representations, and biases: “Nature talks straight, facts are facts. Full stop.”

Despite Strum’s (1987) sense that baboons were “almost human,” she and Latour saw an important difference between humans and baboons: a difference in practical means to organize others on larger scales. While baboons only had their bodies to build stable societies by grooming and beating each other, humans had come to use a growing array of objects and symbols to both complicate and simplify their collective lives. In a recent article, Strum (2012: 11) showed a series of photos of stuffed baboons wearing hats, which twenty-first century Americans use to signal professional affiliations and other social roles:

If baboons had 'hats,' they could begin to shift from complex society where almost everything impinges simultaneously and needs to be continually negotiated toward a complicated society where negotiations are simplified, more focused, and produce more stable outcomes that can travel over space and time.

In Latour's account of science, this transition from a baboon-like brouhaha to an orderly state of affairs happened when controversies came to a close and when novel facts provided thousands of scientists with a solid foundation to built upon. But getting there required mediation by objects. Objects broke the initial symmetry between primates and primatologists: while they might have been asking the same questions about individual interactions, only the scientists saw a social structure emerging from the myriad of social interactions between the monkeys because they recorded and analyzed them with the help of computers, statistical correlations, etc. (Latour, 1996: 231) For example, only their human observers could discern a lasting hierarchy among female baboons while the animals themselves had to do without the stabilizing effects of crowns and other insignia of power.

Apart from scientific instruments, there was a second kind of nonhuman entity that enabled primatologists to order their own relations and affairs: the objects of observation. Setting himself apart from feminist science studies, Latour (2000: 360) emphasized that, in his eyes, it had not been the increase in female primatologists but the primates themselves who were responsible for the changes in how we conceive of them. Their behavior had to support Strum’s claims against her colleagues’ competing claims. Consequently, it would have been her long-term relationship with well habituated baboons that decided the controversy over female dominance rank hierarchies in her favor.

"Baboons we were, baboons we would have remained," wrote Latour (2005: 74), had it not been for material and symbolic means that have enabled human beings in general and the moderns in particular to extend their networks far beyond the size of a baboon troop – or, for that matter, a band of hunter-gatherers. Although Strum and Latour (1987: 792) had neither operationalized these measures nor collected any quantitative data, one of their co-authored articles contained a graph correlating social complexity to the ability to organize others on a large scale, which rose from baboons and hunter-gatherers to agricultural and modern industrial societies. “My colleagues in the faculty almost ostracized me for talking in these terms,” Strum (2017: 161) remembered. “I hadn’t realized how much anthropology was under assault from the postmodern deconstructionists.” At the time of her collaboration with Latour, cultural anthropologists came to unanimously reject any form of social evolutionism, partly for epistemological reasons – after all, late twentieth-century !Kung were contemporaries rather than living fossils – but largely on political grounds: assigning small-scale societies to a different developmental stage had legitimated their colonial and imperialist exploitation. As these groups came to be regarded as our equals, they could no longer represent the proto-human past. Susan Sperling (1991: 208–9) explained the growing use of nonhuman primates as models of early humans in the era of decolonization, which had originally led Strum to study baboons, as an attempt to fill the cosmological slot previously occupied by supposedly premodern or primitive human societies. Strum and Latour momentously reworked that slot into a state that was not a formless state of nature but an always already complex form of sociality.

Yet, just like Strum, Latour did not share the multicultural left’s desire to overcome inequalities of every shade and color. When Johannes Fabian (1983) denounced anthropology’s denial of coevalness to supposedly premodern others as a trope of colonial domination, Latour (1986: 16) defended the asymmetry of their representation: “We map their land, but they have no maps either of their land or of ours; we list their past, but they do not,” and, hence, “harder facts about the other cultures have been produced in our societies, in exactly the same way as other facts about ballistics, taxonomy or surgery.” Apply this logic to baboons and you hit upon the limits of symmetry in Strum and Latour’s primatology of science: although her primates and his primatologists might have asked the same questions about monkey society, no baboon sought to understand the structure of the American Society of Primatologists.

On the face of it, Latour's *We Have Never Been Modern* levels the difference between moderns and premoderns, leaving all of us equally nonmodern. If our last common ancestor with baboons already lived in complex societies, it would be safe to say that human societies have always been complex. But such an egalitarian reading would miss that Strum and Latour also suggested that not all societies were equally complicated and that baboon troops weren’t complicated at all. Latour (1993: 132–6) returned to the social evolutionist distinction between hunter-gatherer bands and industrial societies in the final chapter of *We Have Never Been Modern* where he distinguished between moderns and premoderns (as well as antimoderns and postmoderns). Considering which characteristics of these forms of life should be retained and which should be rejected, he argued that the ethnocentrism and the limited size of small-scale societies was not worth preserving but should give way to the larger collectives that had contributed so much to the “moderns’ greatness” (133). Although the new collectives were no longer divided by large gaps between premodern and modern, natural and cultural, they were everything but equal. Longer and more global networks were stronger and would eventually integrate or dominate the ones that remained small and local (Latour, 1987: 215–57). Ultimately, Latour's (1993: 108–9) symmetrical anthropology did not aim at establishing equality between collectives but at detecting a "new asymmetry," namely differences between their sizes, with the goal of "respecting the efforts collectives make to dominate one another." *Science in Action* and *We Have Never Been Modern* provide an alternative explanation for “why the West rules – for now” (but for how much longer?). In contrast to the archeologist Ian Morris’ (2010) book of that title, Latour emphasized the growing quantity and power of technological objects rather than geography. In the exceptionalist reading that I propose, Latour not just analyzed but affirmed this reconstruction of asymmetries.

Strum and Latour believed that the social evolution from baboons to modern scientists was enabled by the human use of objects and other extrasomatic resources, which enabled *Homo sapiens* but not *Papio anubis* to reduce social complexity and build stable structures. By simplifying negotiations these structures eventually enabled the emergence of larger and more complicated societies. Hats, for example, let everyone know who was responsible for what and thereby facilitated a division of labor and cooperation between people performing different functions. While the nineteenth-century biologist and sociologist Herbert Spencer (1862: 148, 350) had postulated developmental progress from simple to complex and from small-scale and unorganized to large-scale and cooperative forms of social life, Strum and Latour did not exactly invert this process, but proposed an evolution from complex to complicated societies.

When Latour (2013a: 10–1) saw first-hand the complex and messy sociality of Strum’s hatless baboons, he found Callon’s and his intuitions about the technological fabrication of human societies confirmed: “What characterizes humans is not the emergence of the social, but detours, translations, the enfolding of all courses of action into more and more complicated (but not necessarily more complex) technological arrangements.” The discovery of the ability of objects to mediate social links and broaden alliances became the foundation of Actor-Network Theory.

The attention which sociologists of science now paid to how researchers cooperated, however cunningly, to consolidate and extend their networks mirrored primatologists’ growing interest in prosocial behaviors. The point at which Latour’s anthropology diverged from the prosocial turn was that he did not imagine *Homo sapiens* or any other primate species to be “good natured” (de Waal, 1997). An unrepentant Hobbesian misanthrope, Latour (2004a: 456) continued to imagine “men of ill will” who were ­– and here he diverged from biological humanism – “possessed by super- and subhumans of ill will.” At the end of the day, however, it is not the inherent behavioral proclivities of humans and nonhumans that matter, but the overall makeup of the networks, in which they operate (Harman, 2014: 146). Like other socially intelligent primates, Latour’s scientists work with each other in competing groups not out of selflessness but for their mutual benefit: “The easiest means to enroll people in the construction of facts is to let oneself be enrolled by them. By pushing their explicit interests, you will also further yours.” (Latour, 1987: 110)

## 5. Back to Modernity, Back to Human Nature

In the three decades that followed their initial collaboration, both Latour and Strum reaccentuated the theoretical orientation they had developed side by side. In 1987, Strum had called her first book on baboons *Almost Human;* in 2012, she published an article subtitled “Why Baboons Can’t Become Human.” While her emphasis shifted from continuity to discontinuity between humans and other animals, Latour (1999) began to distance himself from Actor-Network Theory, more recently on the grounds that thinking in terms of networks was “very good at multiplying connections, but not for listening to differences” (in: Tresch, 2013: 306). Reducing Eduardo Kohn’s (2013) exploration of how every shrub and vegetable was a thinking self that made meaning through transspecies communication to an attempt at tracing more and more connections and continuities between humans, animals, and plants, Latour (2014: 265) exclaimed: “I want my discontinuities back, not the old ones to be sure, I don’t care about abandoning nature/culture, object/subject, world/words, but still, I don’t wish to confuse a seed with a dream or a predator with a prey.”[[1]](#footnote-1)

Just as Strum had reversed her center of attention, highlighting anthropological difference rather than human-animal continuity, Latour (2013b) no longer foregrounded his denial that we had ever been modern, instead presenting his *magnum opus* as *An Anthropology of the Moderns.* By definition, the question of modernity is a question of difference: what historical rupture separated us from the premoderns? In the eighteenth century, the questions of what distinguished man from the animals and what distinguished modern from primitive man constituted anthropology as a project of empirical philosophy. In the decades leading up to the American and French Revolutions, it was the quest for a new political order that broke with a monarchy ordained by God that motivated reflections on man and modernity. At the beginning of the twenty-first century, these questions have returned with a vengeance, but against a radically different background. What Latour and many of his contemporaries perceive as “the ecological crisis” of anthropogenic climate change and the rapid decline of biodiversity have renewed his interest in the peculiar path, which has led the moderns to the edge of planetary catastrophe: “Suddenly, it’s quite important to know what has happened to us, and what’s the difference between us and the others” (in: Tresch, 2013: 306).

In primatology, this question of historical modernity is paralleled by the question of behavioral and supposedly cognitive modernity. Latour’s concern could be translated by asking what happened to early *Homo* that enabled this rather inconspicuous ape among apes, which neither stood out in terms of population size nor ability to transform its environments, to become what Strum (2012: 19) called a “super-dominant species.” Or, as her colleague Joan Silk (2016: 176) put it: “Anthropocentrism […] is not the only reason researchers are eager to understand what is distinctly human; some are driven by curiosity about how humans came to dominate the planet." First they drove Neanderthals and Denisovans extinct, now orangutans, gorillas, bonobos, and chimpanzees as well as 60% of all other primate species (Estrada et al., 2017; Shipman, 2015). Zoo animals aside, chances are that, by the end of the twenty-first century, we will be the only hominoids left.

Baboons have fared slightly better than apes because of their adaptability to human-modified landscapes. However, just like other primate species, their integration into associations of humans and nonhumans has failed all too often, thwarting the kind of “biocultural hope” for more inclusive societies beyond the human programmatically fostered by multispecies ethnographers (Kirksey et al., 2014: 18). Commenting on the steadily aggravating conflicts between baboons and humans in South Africa, Strum called for major efforts to deter the monkeys from approaching and feeding on human food as the only option short of eliminating most or all of the baboons (Nicholson, 2012). Her heavy-hearted plea for killing no longer controllable individuals did not diverge from Latour’s (2004b: 124) vision of a politics of nature since his understanding of a progressive composition of a common world always involved the exclusion of certain entities, “beings that the collectivity has decided to do without, for which it has refused to take responsibility – let us remember that these entities can be humans, but also animal species, research programs, concepts.”

The question of how *Homo sapiens sapiens,* the doubly wise ape, has come to make decisions about the life and death of baboons rather than the other way round, as imagined in Pierre Boulle’s (1963) science-fiction novel *Planet of the Apes*, might well have to do with our ability to use a historically increasing number of objects, which forge and stabilize social ties in wider and wider networks. Yet it is technologies of deterrence such as electric fences, paintballs, and bear bangers, which featured most prominently in discussions about the Cape baboons’ fate, reflecting the need for “cutting the network,” as anthropologist Marilyn Strathern (1996) called it in her response to the hybridizers’ excoriation of boundaries. Latour (1996: 231) had already pointed out that human sociology differed from primate sociology in that human interactions not only occurred in an extended network connecting the here and now with remote dates, places, and people, but were also framed in ways that interrupted the network: while Strum’s olive baboons dealt with each other in the “continuous presence of all,” human material culture allowed for “closed-door interactions.” Of course, the fission-fusion societies of hamadryas baboons, spider monkeys, or chimpanzees allowed for such temporary detachment and social compartmentalization without any backrooms, envelopes, telephone lines, or private Facebook groups. But objects certainly helped to truncate an otherwise limitless web of relations. This double work of localization and globalization distinguished human from simian sociality, Latour (1996: 234) maintained. Is that why we don’t find ourselves in a Chimpocene or a Papiocene but in the Anthropocene?

## 6. Latour's Moratorium on Cognitive Explanations Has Long Run Out

To this day, Strum and Latour’s (1987) thesis that objects have driven the social evolution from baboons to scientists has remained speculative in character, representing a style of thought more reminiscent of eighteenth-century philosophical anthropology than late twentieth-century field primatology. This is all the more surprising since they had previously called for a science of our social origins that was not only coherent the way Hobbes’s and Rousseau’s just-so stories had been, but that was also based on the latest scientific facts (Latour and Strum, 1986: 186). And these facts would best be supported by quantitative data generated in experiments and long-term observations, Strum (2012: 19) maintained. It seems as if Latour and Strum’s claim that objects had enabled primate groups to decrease the complexity of their social interactions while increasing their ability to organize themselves on larger scales could well be operationalized. For example, the number of material objects (or kinds of objects) used by different human and nonhuman primate populations could be correlated to their group size (or the number of individuals cooperating on any given task). Considering significant advances of knowledge about the material cultures of different species of monkeys and apes since the 1980s, the primatological premises of Actor-Network Theory could well be put to the empirical test.

The literature on the evolution of prosocial behaviors hardly ever mentions objects. In the case of Strum’s peaceful baboons the reason is simple: she never saw them use any tools. Nor did she see them "cooperate" in the strict sense of the term, although they did "collaborate," as Strum (2012: 17) put it. That is to say, instead of working together to achieve a predetermined common goal, they merely acted in conjunction with each other, pursuing similar goals individually. For example, when hunting gazelles they effectively chased the prey in a certain direction because each monkey wanted the meat, but the successful captor would keep everything for himself. Lacking tool use and genuine cooperation, the baboons served as a strong contrast to Latour’s scientists who did both.

However, chimpanzees had been described as cooperating, possibly even through division of labor, when hunting monkeys (Boesch and Boesch, 1989; Goodall, 1971: 198). Alongside several other primate species, chimpanzees had also been observed to use a broad range of tools (Boesch, 2012; Goodall, 1971: 35). But their termite- and honey-fishing sticks and nut-cracking hammers and anvils were used for the purpose of extractive foraging and were not known to play any part in the apes' cooperative endeavors (which led the philosopher Barry Allen (1997) to claim they weren't genuine tools). Thus, in primate species other than *Homo sapiens*, prosocial behavior and object use might not go hand in hand. This raises the question of whether the absence of objects rather than cognitive differences is the primary reason why the social life of baboons is more complex – if it actually is – than that of humans. It also casts doubt on whether the difference between foraging monkeys and foraging humans can be reduced to a quantitative difference between groups sizes and numbers of objects used by these groups.

In their work on human scientists, however, Latour and Woolgar (1986: 280) called for a 10-year moratorium on cognitive explanations of science: "We hereby promise that if anything remains to be explained at the end of this period, we too will turn to the mind!", they wagered. At first glance, their reasons support the leveling of demarcations of science from non-science because Latour (1986: 3, 12) could not see anything special about the way scientists thought. The researchers in the neuroscience laboratory he and Woolgar had observed ethnographically "stuttered, hesitated, and talked nonsense, and displayed every kind of political or cultural bias" – unless they could rely on their instruments, Latour (1986: 4) reported. Although modern scientists' cognitive structures were quite ordinary, their objects – from the printing press to the latest chromatograph – endowed them with their "superiority" and enabled them to "dominate at a large scale" (Latour, 1986: 13, 21).

Just as the purported prosocial functions of objects should be tested primatologically, they also deserve closer scrutiny in the anthropology of science. Two decades after Latour and Woolgar's 1986 moratorium ran out, the philosopher Karen Frost-Arnold suggested that scientific collaboration did remain to be explained – and she explained it in cognitive terms. If scientists indeed behaved like "Hobbesian rational egoists," she wondered, why would junior researchers ever enter into collaborations with higher-ranking individuals (Frost-Arnold, 2013: 304)? After all, young scholars lacked the resources to sanction breaches of trust on the part of dissertation advisors and other senior figures in their field who might easily scoop their results. While a self-interest approach suggested that scientists only trusted each other because they believed that sanctions for untrustworthiness made it in their colleagues' own interest to be trustworthy, Frost-Arnold argued that, in reality, many victims of plagiarism did not appeal to university administrations or journals to enforce community-level sanctions for fear of retaliation. Or they did complain but found their grievances dismissed in favor of unscrupulous colleagues with greater credibility, or they worked in communities with poorly functioning sanctioning mechanisms. To explain why such relatively powerless scholars nevertheless did not shy away from collaborations, Frost-Arnold pointed to cognitive research on prosocial behavior providing evidence for intrinsic moral motivations, which scientists shared with other members of our species. While Latour (1987: 124–5) had argued that decisions about alliances cutting across the boundaries between human beings and things were a matter of "Machiavellian strategies," Frost-Arnold's scientists largely trusted in their colleagues being good natured.

However, presupposing that we have always been trusting, such cognitive explanations of prosocial behavior in scientific collaborations can no more elucidate how modern science came to develop its unparalleled reach and power as the insistence that we had never been modern. To understand why contemporary science differs so drastically from earlier and other contemporary ways of knowing the world we might turn to Lorraine Daston’s historical studies of the emergence and restructuring of cognitive passions and stances such as wonder, curiosity, and objectivity. Although the invention of new objects such as measuring instruments undoubtedly played a key role in this story, the rise of objectivity in the nineteenth century also required its cultivation and thus internalization as an epistemic virtue (Daston and Galison, 2007). Nothing has more effectively pacified ideologically opposed parties in the trench warfare of modern knowledge production than this will to will-lessness. Objects alone cannot account for how objectivity facilitated prosocial behavior among scientists, enabling their facts to travel far and wide. Nor is it for lack of scientific instruments that chimpanzees won't become physicists, as Daniel Povinelli (2000) suggested in *Folk Physics for Apes.* Latour and Woolgar’s moratorium on cognitive explanations was rash and the underlying object-oriented primatology of science might profit from reconsidering what distinguishes human and especially scientific thought from nonhuman and nonmodern forms of thinking.

Much remains to be explained about how modern humans came to change the face of our planet. The cognitive capacities and material cultures enabling large-scale cooperation and thus science and technology appear to be very important pieces in the puzzle. The question of what set the course for the curious evolutionary success and possibly doom of this hominoid species can recreate common ground between cultural and evolutionary anthropology as well as primatology and science and technology studies. Yet the egalitarianism underlying both Darwinian attempts at rethinking humankind asan animal species among animal species and the Latourian mantra that we had never been modern obfuscate the original, still unresolved, and newly pressing questions of anthropology regarding our behavioral and cultural singularity. What cognitive capacities, social skills, and material cultures allowed modern humans to outcompete less concerted forms of life and to extend their networks across the globe? It is not easy to suspend the anger that most of us feel about the recklessness of this process and to overcome the sadness about the resulting losses, but these moral emotions should not suffocate our curiosity about that bewildering stream of life, in which baboons and humans are but moments.

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1. A closer reading of Kohn (2013: 133) reveals that he does propose the symbolic and the moral as quite classical *differentiae specificae* setting humans apart from other forms of life. [↑](#footnote-ref-1)