# Baboons and the Origins of Actor-Network Theory

## An interview with Shirley Strum about the shared history of primate and science studies

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The social sciences ceased to be purely human sciences as European, American, and Japanese primatologists began to study primate societies in the mid-20th century. Around 1980, the sociology of science also ceased to be a purely human science as Actor-Network Theorists began to account for the role of nonhumans in the construction of facts. Few people notice that these two strands converged early on when, in the late 1970s, the baboon researcher Shirley Strum initiated collaboration with a young French laboratory ethnographer. Bruno Latour (2013b, p. 294) recently noted that “thirty-five years later, the shock of my encounter with Shirley Strum, along with primatology, ethology, the Kenyan savannah, and, above all, monkeys, has not faded.” The foundational text of Actor-Network Theory (ANT), “Unscrewing the Big Leviathan,” Latour had co-authored with Michel Callon under the impression of his first visit to Strum’s baboons in Kenya (Callon and Latour, 1981). The social life of *Papio anubis* contrasted with that of *Homo academicus,* which Latour and Woolgar (1986) had previously studied at the Salk Institute, in that it was not mediated by objects. Strum’s primatological research on the kind of social complexity that arises in the absence of technology has been equally transformed by her encounter with Latour’s theoretical project, as her retrospect on four decades of field research revealed (Strum, 2012). *BioSocieties* interviewed Strum to learn more about the work of a primatologist who, undeterred by the Science Wars of the 1990s, accompanied the project of science studies from the start and to this day.

At the height of the sociobiology wars, which, in the 1980s, ripped apart many US anthropology departments along the nature/culture divide, Strum opened up a new venue for cooperation with the sociocultural camp. She was less interested in how genes determined primate behavior than in how cognition allowed for behavioral flexibility. Focusing on social processes rather than their structural outcomes, she quickly realized the potential of nascent ANT’s denial of the preexistence of a structured society and an order of nature, which awaited their discovery. Latour had learned from ethnomethodology that social structures were not written in stone and did not predetermine what people could do, but emerged flexibly from a multitude of small-scale negotiations between actors. This led Strum and Latour (1987, p. 788) to argue that primates and

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primatologists were asking the same questions about baboon troops. The monkeys did not know the constantly changing structure of their group any better than the monkey researchers and were also busy testing out their own hierarchies and alliances. The difference between primates and primatologists – and this was how Latour and Strum went beyond ethnomethodology – was that, as humans, the primatologists negotiated their own social relations through their associations with the baboons (and other objects such as recording instruments and published papers). By contrast, the nonhuman primates had only their own bodies to beat each other into submission or strengthen their bonds in tender grooming sessions. Thus, the symmetry between baboons and humans gave rise to a new asymmetry: humans managed to very significantly extend their networks through the inclusion of nonhumans, whereas the inability of baboons to include nonbaboons put a cap on the size of their collectives.

In the intellectual climate of the 1970s, when evolutionary anthropology emphasized aggression and competition, Strum explains in this interview, her attention to process rather than outcome made her realize early on how important the formation of friendships and alliances were in the social life of baboons. Her insights prefigured a body of literature explaining important dynamics of primate societies, including our own, in terms of the so-called prosocial behaviors (see the Books Forum of *BioSocieties* 7(1)). ANT also anticipated this turn to prosociality as it opened up the narrow focus on the divisive quality of controversies in early science studies to processes of inclusion and mutual enrollment through which actors construct their networks. Together Strum and Latour devised a primatology of science, which continues to inform current sociological investigations of the life sciences (Langlitz, 2016). Hence, the following interview not only recounts an important chapter in the history of primatology but also in the history of the social studies of science.

Finally, our conversation with Shirley Strum marks the time that separates the concerns of the 1980s from those of the 2010s. Although the projects of Strum and Latour have since parted ways, both got involved in discussions about the Anthropocene. Almost paradoxically, at a moment when the collapse of the nature/culture dichotomy seems total, they have, each in their own way, developed a new interest in difference rather than continuity, highlighted by the titles and subtitles of some of their most recent publications. While Latour's early work made many of us believe that *We Have Never Been Modern* (1993), he presents his latest book *An Inquiry into Modes of Existence* as *An Anthropology of the Moderns* (2013a). At the same time, Strum has shifted her emphasis from presenting baboons as *Almost Human* (1987) to explaining *Why Baboons Can't Become Human* (2012). The reason for this transition is that anthropogenic climate change and the plummeting of biodiversity confronted both the student of science and technology and the student of a primate society devoid of science and technology with the old anthropological question of what distinguishes modern humans – now in the guise of a "superdominant species" (Strum 2012, p. 19) – from other forms of life. These are some of the issues we will address in the following interview with Shirley Strum.

NL: As a primatologist, you started collaborating with Bruno Latour at the beginning of your career, in the late 1970s. How did you learn about his work and why did you approach him?

SS: In 1978, I organized a Wenner-Gren symposium titled *Baboon Field Research: Myths and Models*. From my mentor Sherwood Washburn, I had inherited a sense of reflexivity and a curiosity about the role of science in society. So I was looking for someone who could help us primatologists – at least, I thought 'us,' but it turned out to be only me – puzzle through why some questions get asked and why some answers prove to be more popular and satisfying. I had become interested in these issues before there really was such a thing as science studies because I wanted to understand the opposition provoked by my finding that male baboons did not have a dominance hierarchy correlating with success in consorts, the best sitting places, or position in the troop movement. At the time, Bruno was conducting his first study at the Salk Institute, shortly after published as *Laboratory Life*, and I was working across the street at the University of California, San Diego (UCSD). A very good friend working at the Salk introduced us and I invited Bruno to the conference. In terms of the viability of the meeting, including him turned out to be a terrible mistake. Here was a room full of people who spend their lives watching primates, and they wanted to kick him out of the workshop because they did not want to be watched. And they refused to consider the questions about science embedded in their work. I never published anything from this conference because, as far as I was concerned, we accomplished nothing. But that experience created a tremendous bond between Bruno and me.

NL: Why did the following collaboration not turn into the kind of ethnographer–subject relationship so common in anthropology and sociology of science? How would you describe the form it took?

SS: Bruno came to the field a couple of times to see the baboons, and some of his books are a direct response to the experiences he had in Kenya. The baboons were like a backboard to help bring into focus what he was already beginning to understand about humans. For him they served as a useful instrument to work out his own theories. He was not so much interested in how I studied baboons, but more in asking 'what are baboons and what can we learn from them about us?'

Then I was uncovering how much negotiation went on among baboons in order to create relationships, which then had to be maintained. Bruno had found the same types of negotiation at the Salk. We converged in seeing baboons and scientists as both *performing* society rather than entering into a preexisting structure. The next step was to ask how they differed. That was where tools and technology entered the picture, and we wrote a scenario about the evolution of the ability to manage social complexity. Bruno emphasized the use of extrasomatic resources to stabilize social interactions. In the case of the scientists, objects allowed these processes to be blackboxed and built upon. We made the distinction between a 'complex' society, which we considered baboon society to be, and a 'complicated' society emerging from the ability to build bigger structures out of simplified smaller units. Ironically, we concluded that baboons had more social skills than humans because, without tools to stabilize their social order, they had to manage much more complexity. Not then but now as I carry the social complexity argument forward, I see that group size creates a glass ceiling for baboons as they do not have the material culture necessary to reduce the huge cognitive task of keeping track of so many dynamic relationships. Bruno's ANT grew out of that.

NL: The paper in which you outline this theory contains a graph correlating degrees of social complexity with the ability to organize others on a large scale as it evolved from baboons and hunter-gatherers to agricultural and industrial societies (Strum and Latour, 1987, p. 792). If there was one thing that cultural anthropologists agreed on in the 1980s, it was their unanimous rejection of social evolutionism. How was your article received?

SS: My colleagues in the faculty almost ostracized me for talking in these terms. At the time, I hadn't realized how much anthropology was under assault from the postmodern deconstructionists. So I didn't win any friends then. Today, however, when I teach the graduate biological anthropology course, my affiliation with Bruno gives me some credibility in the eyes of the cultural anthropology students, which I think is very funny. I joke that Bruno is what primatologists would call my 'agonistic buffer' against the anti-science of cultural anthropologists. They really like ANT. Bruno and I still have an ongoing disagreement though about his desire to make everything symmetrical. Although I was fascinated by the idea that things mediated social interactions and that therefore a door-closer was a social actor, I have never believed that it had the same kind of agency as a human who closes the door.
But that didn't keep us from outlining a book that we were planning to write together. One chapter titled "Sherlock Goes to Paris" was about a baboon visiting France and about what he could and could not understand. That book never got written, but for me the collaboration totally changed what I did. It sent me on a ten-year detour into science studies. My perspective on primates has been influenced by the kind of questions Bruno and I discussed. The big shift I've been trying to make with my work is to turn our focus from given social structures to the performance of society, from outcome to process. This, I realize in retrospect was my first step.

NL: Talking about the principle of symmetry: regarding the controversy with your colleagues in primatology over baboon dominance rank hierarchies, you wrote in your book *Almost Human* that you could neither believe that you were wrong and everyone else was right, or that you were right and everyone else was wrong. This kind of relativism might have been acceptable in science studies, but for a scientist it appears to be an unusual way of thinking about your work.

SS: At the time, I did not know what caused the differences in interpretation. Now I understand that they resulted from different methodologies. Of course, there were also differences between the observed groups of baboons. We have discovered more and more intergroup differences since we have been studying baboons in different places. But today I can go into any baboon group and agree with the colleague studying it about what behavior we see, how to count it, and how to analyze the data. The weak link is moving from the results to the interpretations. Whereas beforehand there were disagreements about results as well as interpretations. When my peers doubted my findings, I was not yet reflexive enough to wonder whether I was a relativist or not, but it bothered me that one party had to be right and the other wrong.

NL: Wasn't the controversy eventually resolved in your favor?

SS: It was. And so was the debate over shifting our focus from aggressive competition to social strategies. That has been a huge transition in the animal behavior literature of the last fifteen years, but I already talked about those things in the 1970s when I observed nonaggressive social strategies, which required the building of relationships and higher levels of cognitive skills.

NL: In the course of the 1980s, Frans de Waal got interested in reconciliation and alliance building among chimpanzees. Christophe Boesch began a project on how chimpanzees cooperated when hunting monkeys. What caused the shift of interest from aggression to prosocial behaviors in the Reagan–Thatcher era?

SS: I don't know how to link these trends in primatology to the political currents. That's your job. But cooperative hunting in chimps had already been observed by Jane Goodall in the 1960s. The interest in prosocial behavior, on the other hand, only developed later. No one spoke about *prosociality* when de Waal's *Chimpanzee Politics* came out in 1982. It was just social behavior.

I call his Arnhem chimps "facultative" baboons because, in the wild, chimpanzees live in fission/fusion societies: if they have a conflict, they can and do disperse. But in the Arnhem zoo, they have nowhere to go. That leads them to behave more like baboons who have to stay with everyone else all day and night every day. Baboon troops don't have a fission–fusion society. In the captive setting, the chimpanzees developed baboon social strategies half of which have never been seen in the wild, or at least not at the same rates. That does not mean that they are not part of the species potential, but I think what de Waal described partly grew out of these peculiar circumstances.

NL: Sociobiology has been interested in the problem of altruism and cooperation almost since its inception. Did its popularization in the 1970s foster interest in what's now called prosocial behaviors?

SS: Sociobiology switched everything, especially for primatology. It directed attention to the individual, the metric of reproductive success, and gene strategies. It did not really foster the exploration of cooperation and collaboration, but solved the puzzle by defining them out of existence. Sociobiologists suggested that real group selection, which is neither reciprocal altruism nor kin selection, was impossible. When I received my degree and got my job at UCSD in 1976, group selection was standing on its last legs. I see the turn to prosociality today as a reaction to that. Now, group selection theory is back in vogue. What made it viable again was the recognition that human groups can be in competition with each other, and, therefore, if group selection can work in humans, then it might work in other species, too.

As a result, we are examining mechanisms again. This shift, which the sociobiologists did not make, turned our attention from just looking at outcome to looking at process. If you focus on outcome you see reproductive success, with some winners and some losers. 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 and social ties. The resulting picture no longer privileges aggressive competition. The idea that process matters and that collaboration is part of everyday life popped up in a number of places, e.g., in cognitive science as it made room for distributed and embodied cognition as well as situated action. It didn't start with de Waal's observations of the Arnhem chimps or Boesch's research at Taï in the 1980s, but grew out of a reaction to sociobiological reductionism.

NL: Because of your emphasis on process rather than outcome, Latour described you as the ethnomethodologist among the primatologists. Did the aspects of your work that reminded him of Harold Garfinkel's microsociological studies actually originate from *within* primatology or is that an orientation that you acquired from your exchange with Latour?

SS: Working with Bruno gave me words to describe what I had been seeing in terms of performance and negotiation. But at the time, no other primatologist was talking in those terms.

NL: It seems as if the sociological shift of focus from social structures to the nitty–gritty of social interactions converged with the primatological observations of individuals in long-term studies, which revealed that, by building various kinds of social relations, animals could fill seemingly fixed positions in their group in many different ways.

SS: I don't know what was happening in sociology. In primatology, the identification of individuals really started in the 1970s. In the fifties, American primatologists had still been railing against individual identification, which they regarded as unscientific. When Jane Goodall identified individuals in the 1960s, it was just a personal preference, not a scientific stance. By the time I started, the Altmanns were already doing research at Amboseli, but hadn't identified all the individuals. In those early baboon studies, it was only adult males who were known as individuals and all the rest were categorized in age and sex classes. The quantitative methods that Jeanne Altmann's (1974) reviewed but did not introduce in her classical paper were being used from a concern about how to interpret naturalistic observations. The problem was how to decide who was correct if the description of the baboons in one place did not match the description of the baboons in another place. You need a quantitative base and some way to make your selection of individuals reflect the whole range of actors. For example, low ranking female baboons are often not as easily visible as adult males or even high-ranking females. If you fail to include them, you will get a different sense of what the group dynamics are. I think the shift in focus toward individuals and quantification was motivated by the hope that we could collect data to resolve conflicting interpretations. That goal has still not yet been realized.

However, the important things happening in the life of baboons are often not quantitative, they are one-time events. The baboons have a single encounter with a herd of elephants or experience an incredibly serious drought and that can completely change their home range. We do not have the analytic tools to deal with that amount of complexity, but I can describe it in the manner of natural history. Some people might call these descriptions anecdotes, but they go beyond the anecdotal because I collect them across many troops and circumstances. For me it is a combination of quantitative data, which is crucial but not sufficient, and natural history description, which is necessary because the quantitative data cannot account for the complexity of the system as it exists. Having uncovered an amount of complexity in baboons that goes beyond anything anyone has seen in a nonhuman animal, I find myself in a funny position now as I return to arguing for natural history observations.

NL: The 1980s were a time of strong tensions between cultural and biological anthropologists in American departments. However, at UCSD, Latour and you co-taught a course on the evolution of technologies and ecology almost every year from 1979 to 1992. In your experience, did Latour's attempt to overcome the dichotomy of nature and society enable new collaborations between the anthropological subfields? Do you have a general sense why this reintegration has not worked?

SS: The Science Wars split anthropology departments in half. At UC Berkeley, the biological anthropologists left the department and at Harvard they moved to the museum. At UCSD, we managed to hold our department together, but it wasn't easy. The conflict between representatives of the subfields often boiled down to simplistic stances like 'are you for science or against it?' At that time, Bruno came to San Diego to found a program in science studies, and we co-taught that seminar on material culture for many years. The first couple of times, we were like ships passing in the night. I didn't really understand what he was aiming at and he didn't quite understand my position, and yet we intersected on important points. But what we did was in isolation from the rest of the department(s). Since then, anthropology has become more and more specialized and today it's totally fractured. Most departments are dysfunctional. The parts that work reach out beyond the discipline to do team science.

NL: At the height of the Science Wars, in 1996, you organized another Wenner-Gren symposium in Teresopolis, Brazil, bringing together primatologists and science studies scholars. Did you have better luck the second time around?

SS: By the mid-1990s, science studies had a base in the United States. I could invite an equal number of practitioners from each field to see if we could break down the barriers. I had learned my lesson and tried to invite primatologists who I knew had some interest in reflexivity. The conference turned out a lot more successful than the first one. Personal bridges were built and Linda Fedigan and I published the papers in the edited volume *Primate Encounters* (2000), which includes a wonderful chapter by Bruno. However, the workshop did not actually motivate any of the primatologists to create more integral links to science studies. My colleagues aren't thinking much about the questions concerning science that Bruno has dealt with. It's a shame, but this is not unique to scientists studying primates.

NL: Did these encounters with science studies transform primatology at all?

SS: The primatologists of my generation were not really interested in the relationship between science and society. But Adrienne Zihlman and Linda Fedigan, for example looked at primate studies as a feminist science. This conversation by Adrienne predated my collaboration with Bruno. Feminists took up my baboon research, which they misrepresented as having grown out of a feminist perspective and as focusing exclusively on females. I was indeed a feminist, but my scientific questions were of an intellectual nature, not an attempt to correct the imbalance that feminists pointed out. More recently, a concern for marginalized people in the countries where the primates are found has converged with the realization that we need conservation and not just academic research if the animals are going to make it into the future. Here again these social and ecological issues skirt around the questions of science studies. So, sadly, science studies has only transformed a few individual primate scientists and not the field.

NL: Current discussions about the Anthropocene reframe many of the original questions of both cultural and evolutionary anthropology. How have discussions about the loss of biodiversity and human-caused climate affected your work?

SS: For me there is a positive and a negative to the Anthropocene. On the positive side, the rate of change is so fast that I can actually see the process of adaptation in a time frame that my mind can comprehend and with data that I can collect. In the past, such a process might have occurred over 10,000 or 100,000 years. As a scientist, I consider the Anthropocene a fantastic methodological bonus. For the baboons, on the other hand, it is a real threat. They are so adaptable, so opportunistic that at this point they can outsmart people and take advantage of the new opportunities that arise in a humanized environment. Apes might have bigger brains and are smarter in relation to certain tasks, but baboons are less conservative. You can find baboons in deserts, forests, savannahs, on farmland, and in peri-urban conditions; they are able to adapt to all those circumstances just through behavioral means, while chimps lack that flexibility. Thus, baboons have so far fared much better than the apes, but that is not going to last. More and more, we will end up with situations of massive human–baboon conflict, like the one we currently see in Cape Town where the city spends millions of dollars to mitigate baboon damage to human property. Eventually, someone will decide that it isn't worth the money and there will be no baboons in Cape Town.

NL: In 1987, you called your book *Almost Human*. In 2012, you looked back at your work in an article you titled "Why Baboons Can't Become Human." It seems as if the growing awareness of anthropogenic environmental change has refreshed your interest in the difference between humans and other animals?

SS: When I started discovering the social complexity of baboons, their negotiations, social strategies, and sexual politics, it made them seem much more human than they had ever been portrayed before. But in the following decades, I began to understand their limits, which are related to the social sphere. Compared to humans, baboon flexibility and adaptability are constrained. When humans acquired the ability to control the environment through the domestication of plants and animals, they became a superdominant species, that is until this process reaches its global limits. In this new humanized context, baboons will become the victims and not the victors much earlier than humans.

NL: Will primatology make a difference?

SS: Scientific research can't save the baboons. Their lot depends on community-based conservation, politics, and economics. But we need the best science as one of the tools. This has pushed primate studies into an awareness of the new context and toward pursuing much more applied research. The Anthropocene has made a difference to the new generation of primatologists in terms of their values and interests. Also tackling the problems of the Anthropocene requires that we work in teams – not just as individuals as during most of my career. Those teams increasingly will require both natural and social scientists and investigate both pressing problems and important scientific ideas.

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