Suspending Belief: Epoché in Animal Behavior Science

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ABSTRACT In an anthropology that has turned, in recent years, toward the study of human–animal relations, scientific skepticism about nonhuman animal minds has more often been featured as a focus of conceptual critique than of ethnographic exegesis. Decried as the sign of a problematic detachment from nonhumans, such skepticism is often simultaneously presented as an ideological stance by which no one who actually works and lives with nonhuman animals could truly live. In contrast, in this article, I examine attempts to live by such skepticism through an ethnography of two very different British-led research projects in which scientists study animal behavior and cognition respectively. I describe researchers' commitment to engaging in intersubjective relations with the nonhuman animals they study while simultaneously detaching from propositional beliefs about the latter's inner lives. This simultaneously engaged and detached attitude, which I describe as "epoché," challenges descriptions of a settled "naturalist" ontology at play in animal behavior science and offers the potential for a comparative anthropology of doubt and operational skepticism. [science, multispecies, belief, skepticism, detachment]

RESUMEN En una antropología que ha girado, en años recientes, hacia el estudio de las relaciones entre humanos y animales, el escepticismo científico sobre las mentes animales no humanas ha sido a menudo presentado más como un foco de crítica conceptual que exégesis etnográfica. Denunciado como un signo de una separación problemática de los humanos, tal escepticismo es a menudo simultáneamente presentado como una posición ideológica por la cual ninguno que realmente trabaja o vive con animales no humanos podría verdaderamente vivir. En contraste, en este artículo examino intentos de vivir tal escepticismo a través de una etnografía de dos diferentes proyectos conducidos por Británicos en los cuales científicos estudian comportamiento animal y cognición respectivamente. Describo el compromiso de investigadores a participar en relaciones intersubjetivas con animales no humanos que ellos estudian mientras simultáneamente se separan de creencias proposicionales sobre la vidas interiores de los últimos. Esta actitud simultánea de involucramiento y separación, la cual describo como "suspensión" reta descripciones de una establecida ontología naturalista que hace parte de la ciencia de comportamiento animal y ofrece la posibilidad para una antropología comparativa de duda y escepticismo operacional. [*ciencia, multi-especies, creencia, escepticismo, separación*]

PRELUDE: DOUBTFUL RELATIONS

23 October 2008. As the midmorning heat sets in, I am walking with Marie, a volunteer at the Kalahari Meerkat Project, through dry Kalahari scrub, while all around us a band of meerkats is getting on with their foraging routine. The soundscape is unmistakable: the "position calls" of adult meerkats provide a background of steady beeps, against which the more high-pitched begging calls of the young pups stand out.

Marie points out one particular sound. After a while I manage to pick it out, discrete but steady, beating time for the whole group. This is the regular chirping of the guard: the meerkat who has taken his turn to stand in the iconic posture, propped up on two hind legs and a tail, keeping

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a watch for predators as the others forage. Marie points to the small upright figure: That's Ningaloo. He's so good, she adds fondly, always guarding. How does he ever manage to put on weight, she wonders. Marie then tells me she really likes the chirping guard sound, it is somehow reassuring: as long as the guard is chirping, all is well. Meerkats' acute sensorium keeps a healthy distance between us and some of the Kalahari inhabitants we do not want to tangle with, such as cape cobras or puff adders.

Like other volunteers at the Kalahari Meerkat Project, and indeed arguably like most English-language speakers most of the time, Marie cursorily refers to the animals she works with as persons with a perspective. And yet, occasionally, this "language of the lifeworld" (Crist 1999:202) is interrupted and challenged in a very different register. Earlier that day, as we sat by the burrow in the crisp and misty dawn, waiting for the meerkats to emerge, Marie told me in hushed tones that I should be very quiet. This is one of the projects' most "habituated" groups, but all meerkats are skittish in the morning. They are worried about what might be outside, Marie explains. And then she catches herself and rephrases: They have a heightened response to stimuli. I query her on this: Why the change in language? This is what they taught her at university, she says, to beware of making these assumptions. Making assumptions about animals' mental states is wrong because we cannot know what the animals think. This is anthropomorphism. By contrast, talk of stimuli and response is more scientific. It keeps things *causal*—she emphasizes the word. Of course, she admits, volunteers at the research station, like people everywhere, speak "anthropomorphically" all the time, but they know they should not and they control for it.

Such self-policing was an equally common occurrence at another field site I have begun to investigate in parallel to the Kalahari Meerkat Project, namely the Comparative Cognition Lab in Madingley, United Kingdom, where scientists study, among other things, the cognitive abilities of corvids (jackdaws, rooks, and jays). There too, the commonplace language of intentionality was frequently interrupted by references to an underlying skepticism.

INTRODUCTION: AN ETHNOGRAPHY OF SKEPTICISM

In this article, I attempt an ethnography of scientific skepticism about animal minds. There is already an abundant literature that engages in philosophical, epistemological, or ethical discussion of such skepticism. Scientific skepticism about animal minds has been celebrated, denounced, deconstructed, or simply propounded as "just good method." We also have in the work of Eileen Crist (1999) an excellent and detailed examination of its linguistic and pragmatic aspects. But we have as yet no thoroughgoing ethnographic exploration of what skepticism about animal minds might look and feel like in practice for the scientists who profess to live by it and the ways in which it affects their relationships with the animals they study. In exploring lived skepticism in this way, the aim of the present article is not just to fill an ethnographic gap, however, but also to hopefully present a constructive challenge to two currently popular ways of thinking, in anthropology and cognate disciplines, about Western (scientific) attitudes to animals.

The first turns on the image of "Western science, inured by Cartesian metaphors of mechanical nature" (Scott 1996:76), which is frequently invoked in anthropology as a foil for the description of other, putatively non-Cartesian, ethnographic realities. Anthropologists who work in non-Western contexts frequently highlight the specificity of their description by contrasting it to the "commonly held assumption in the West that attributes of personhood, with all that this entails in terms of language, intentionality, reasoning, and moral awareness, belong exclusively to human beings. Animals are understood to be wholly natural kinds of being, and their behaviour is usually explained as automatic and instinctual" (Willerslev 2004:629). Most recently, this type of contrastive description has taken a theoretically sophisticated and popular form in arguments about Euroamericans' putative "naturalist ontology" (Descola 2005; Viveiros de Castro 1998). Precise definitions of naturalism (sometimes mononaturalism) vary, but broadly speaking the term refers to a set of ontological assumptions under which Euroamericans purportedly labor. These assumptions involve the radical discontinuity between nature and culture (the former is assumed to be physical and external whereas the latter is mental and internal). Whereas nature is unitary and all entities partake of it, culture, mind, and interiority are the operators of significant discontinuities and differences between humans and other entities (we have culture, they do not) but also between different human individuals and groups ("multiculturalism").

Although authors such as Eduardo Viveiros de Castro or Philippe Descola have developed conceptually sophisticated (and subtly different) accounts of "naturalism," they have not engaged in sustained ethnographic description of actually existing naturalist practices. "Naturalism" remains more of a conceptual shorthand for highlighting, by opposition, the interesting worlds of others. As a result, these contrasts between "our" and "their" ways of encountering nonhuman animals tend to remain rather schematic on the "us" side, even as they are ethnographically filled in on the "them" side (Candea 2011; Candea and Alcayna-Stevens 2012). By tracing the different ways in which scientists live their skeptical attitude to the animals they interact with, I seek in this article to the notion of a stable naturalist ontology. To take scientists' skepticism seriously is to focus precisely on their ability to suspend and abstain from settled ontological schemes.

Crucially, this is not the same as arguing that scientists too are "animists" of sorts, that their skepticism is skin deep, a mere dusting of philosophical scruple, belied by their real and active recognition of animals as intersubjective partners. This form of argument also has its avatars in anthropology and animal studies, and this is the second body of literature to which I hope in this article to provide a contribution. Anthropologists and others who work on human-animal relations "at home"-in Western contexts-have in the past decade or so been calling for an attention to nonhuman animals as significant others entangled in human social worlds (Haraway 2003, 2008; Kirksey and Helmreich 2010; Knight 2005). This "interspecies turn" has produced fascinating studies of the ways in which Western hunters, farmers, zookeepers, gardeners, and even scientists actually engage in meaningful interactions and personalized relations with nonhuman animals. This literature significantly fills in the ethnographic picture of "Western" practices, but on the whole it has little to say about skepticism per se-except perhaps that it is unwarranted. Indeed, in some respects, the very possibility of a multispecies anthropology is premised on and drives home the thought that "the philosophical and literary conceit that all we have are representations and no access to what minds think and feel is wrong" (Haraway 2008: 226).

In other words, a large section of the animal studies literature explicitly takes up a position within an ongoing Euroamerican debate about "anthropomorphism"-and this position is de facto inimical to skepticism about animal minds. This takes off from a philosophical argument concerning doubts about other human minds: the "inexorable existential palpability" of the subjectivity of other humans, it is argued, makes philosophical solipsism "absurd enough not to merit serious attention" (Crist 1999:217-218). Extending the point to nonhumans, a number of animal studies scholars have argued that the daily evidence of people's ability to engage in complex and successful interactions with nonhuman animals challenges "species solipsism," because "the belief that we can know the intentions, goals, and desires of other selves allows us to act in this world" (Kohn 2007:7; see also Cox and Ashford 1998; Midgley 1988).

In sum, scientists who work with animals yet claim not to know animal minds are, from this perspective, rather like Melanesians who claim that other human minds are unknowable. As Joel Robbins puts it, "Our colleagues ... find it impossible to believe that people, even if they make [such statements], might in any sense live by them" (Robbins 2008:422).

Against this broad common baseline, different authors take up a range of positions. Donna Haraway's thoughts on animal mindedness, for instance, are characteristically sophisticated: although, as we have seen, she rejects the possibility of complete opacity, she nevertheless recognizes the heuristic value of behaviorism as a counterpoint to overhasty assumptions of similarity, to help us to "learn to meet dogs as strangers" (Haraway 2003:236). By contrast, other scholars straightforwardly denounce scientists' statements about the opacity of animal minds as part of an ideological ploy to distance nonhuman animals from humans, to misrecognize the real intersubjectivity that obtains between us and them so that "we can use them in many ways without being impeded by moral sensibilities; we can experiment on them, eat them and use them for our entertainment, and exploit them in countless other ways that industrial economies, sanctioned by Cartesian science, have devised" (Milton 2005: 265).

The rhetoric from the other side of the anthropomorphism debate is similarly stark. Thus, biologist John S. Kennedy claimed that to explain an animal's behavior simply by pointing to a purported aim or purpose is "effectively a throw-back to primitive animism" (Kennedy 1992:9). As Elliott Sober (2005) has noted, echoing William James (2003:3), scientists writing on this topic often portray themselves as "tough-minded" rationalists pitted against "tender-hearted" anthropomorphists. These critics of anthropomorphism are unwittingly drawing on a long tradition of scientific polemic that can be traced back to a 19th-century Whiggish historiography of the rise of modern western science (see Daston and Mitman 2005). In this account, the ability to refrain from imputing human thoughts, emotions, or motivations to nonhuman entities is taken as a mark of both epistemic and ethical progress and superiority, which marks out scientists from their contemporaries and on another scale "modern science" from "primitive animism" (Bird-David 1999; Daston and Mitman 2005).

When it comes to scientists and animal minds, we seen, in other words, to be faced with a stark choice between engagement and detachment: either we acknowledge meaningful intersubjective relations between humans and nonhuman animals, which prove the inherent ethical and epistemic wrongness of skepticism, or we denounce these relations as illusory and side with those who bravely carry forward "the process of liberation from the delusions of anthropomorphism" (Kennedy 1992:5).

At this point, of course, we have come full circle toward the image of opposed ontologies: scientific authors such as Kennedy, in their rhetorical pronouncements, provide seemingly irrefutable evidence for the description of a settled Western scientific naturalism, in which animals can be known only as objects, as opposed to an epistemology in which to know is to personify (Viveiros de Castro 2004). On the one hand, recent anthropological accounts of ontological multiplicity too often simply accept-indeed, hypostasize-these extreme characterizations of science. They then merely bracket them as one species of ontological certainty. On the other hand, animal studies authors who are engaged in the anthropomorphism debate too easily dismiss scientific skepticism as ideological posturing. More profoundly, both literatures have been calling for a move away from issues of epistemology toward the purportedly more substantive "ontological" questions of multiple, entangled worlds.

My claim in this article is that taking skeptical scientists who work with animals seriously in an ethnographic sense forces us to rethink both ontological dichotomies and the contrast between the tough minded and the tender hearted. I will show that the animal behavior scientists with whom I have worked do engage in meaningful interactions with the animals with whom they work and yet that they simultaneously also live, in varied ways, by their skeptical principles. More precisely, I will argue that these researchers are striving to suspend the question of propositional belief concerning animal mind, detaching it from the implicit "trust" embodied in their daily interactions with particular animals.

Beyond this specific case, my broader theoretical aim is to draw attention both ethnographically and methodologically to the practice of active doubt. For what is crucially at stake in the discussions outlined above is an exchange of certainties—whether these be the relatively dispassionate certainties of alternative ontological schemas (naturalism versus animism) or the urgent certainties of experiential affirmation and principled denial in the anthropomorphism debate.

In focusing on the experience and practice of active doubt, the present article draws on two sources of anthropological inspiration, which are at some remove from the anthropology of human-animal relations. The first is the burgeoning field of the anthropology of ethics, which puts the focus directly on the question of people's own pursuit of certain forms of virtuous practice (Faubion 2011; Laidlaw 2002; Lambek 2010; Robbins 2012). I will argue that suspending belief about animal minds is akin to a form of "virtuous action," a particular technique of the scientific self. The second, related source is a recent call for a new kind of anthropology of belief (Mair in press). Mair argues that anthropologists of religion have too readily given up on the category of belief in the wake of often-justified critiques. Mair nevertheless points out that the people with whom anthropologists work are themselves often explicitly and actively engaged in "understanding, monitoring, debating and cultivating particular forms of belief" (Mair in press). Mair proposes, quite rightly, that we should study these processes ethnographically and attend to people's own cultivation of particular orientations toward their own beliefs. Mair's suggested "anthropology of belief" would involve "both an ethnographic sensibility that allows for people's reflexive relationship to their own belief to register, and a comparative anthropology that helps us to understand it" (Mair in press; cf. Luhrmann 2012).

Mair's argument, and his own ethnographic work, is located broadly within the sphere of the anthropology of religion. However, these questions are also highly relevant to the anthropology of science and of human-animal relations, which have been turning away from questions of epistemology toward questions of ontology or substantive human-animal worlds. It is precisely such forms of active orientation toward one's own beliefs that are my subject here, even though the present article's contribution to the project of an anthropology of belief is somewhat paradoxical, as I will be arguing that what scientists cultivate is precisely an active abstention from certain types of propositional belief. Here I explore such practices of "active suspension of propositional belief" and the ways in which the scientists who pursue them nevertheless do engage in meaningful interactions with the animals they work with.

TWO FIELD SITES

I will follow Mair's call to treat such cultures of belief (or, in this case, nonbelief) comparatively by contrasting the very different ways in which skepticism plays out in the two distinct branches of the contemporary science of animal behavior. Most of the researchers associated with the Kalahari Meerkat Project (henceforth KMP) are behavioral ecologists, whereas researchers at the Comparative Cognition Lab (henceforth CCL) are primarily pursuing questions at the interface of behavioral biology and comparative psychology. The former's aim, in the broadest terms, is to investigate the (physiological) causes and (evolutionary) functions of meerkat behavior, and their discipline stems from a theoretical tradition that leads back to mid-20th century European ethology via 1970s sociobiology (Burkhardt 2005; Crist 1999). The latter investigates the cognitive abilities of corvids (members of the crow family, including magpies, ravens, and jays), a question that has its roots in 20th-century U.S. animal psychology, in its various behaviorist and cognitivist incarnations.

Although it brings us some way into specifying "Western Cartesian science," this contrast itself is still very schematic because the historical traditions I am outlining have been blended and amalgamated over the past half-century. It should be read as a broad approximation that tries to pick out some distinctive characteristics of a more complexly entangled landscape.²

The practical setup of each site differs significantly. At the CCL, the birds are hand raised and kept in aviaries and cages. The research involves experiments in the form of tests-puzzles set up to assess the birds' abilities-and these often involve habituating the birds to strange objects and implements that they will then have to manipulate. For instance, one of the hallmark experimental setups of the CCL involves ice-cube trays filled with sand. The birds, given food items in the presence of these trays, tend to "cache" or hide them by burying them in the trays, as they would do with surplus food in the wild. By varying the trays, the food items, and the presence or absence of other birds, the researchers can ask diverse questions, such as about the birds' powers of recall (can they remember where they hid the food?), future planning (can they hide food in appropriate places to which they will have access in the future?), and theory of mind (do they know to change their hiding places if they see another bird watching them hide the food?).

Setting up these experiments takes some preparation, not just of the apparatuses but also of the relation between birds and apparatuses. In the caching experiments, the birds simply need to be introduced to the ice-cube tray setup. Other experiments, however, involve more complex apparatuses: communicating tubes to test the birds' understanding of physical principles (e.g., that dropping a stone in water makes the level rise); transparent plastic boxes containing food that can only be opened by pressing a button that is inside a long tube to test the birds' abilities to use tools. As we shall see below, the problem of how to get the birds to interact with this equipment, without purposefully "training" them to do so in a particular way, is a crucial focus of attention at the CCL and one that speaks directly to problems of mind reading, skepticism, and objectivity.

By contrast, the KMP is a field station set up to study large numbers of wild meerkats under conditions of habituation. Meerkats are small beige-brown mammals, members of the mongoose family, much loved by the public due to their large, dark-ringed eyes and endearing habit of standing up on their back legs in groups, as though posing for a group photo. Meerkats live in groups of around 10-50 individuals. In each group, one "dominant" pair monopolizes reproduction, whereas all members help out with a number of tasks, such as watching out for predators, looking after and feeding the young, cleaning the burrow, and so forth. This cooperative behavior is what makes the meerkats interesting to behavioral ecologists. Although in recent years some cognitive research has taken place at the KMP (particularly on meerkat "learning" and "cultural transmission"), the primary focus of the project's research has been on the evolutionary aspects of the meerkats' cooperative way of life: from the classically neo-Darwinian questions concerning which individuals benefit, in terms of their "reproductive success," from different patterns of behavior, to the more traditionally "ethological" questions of the actual physiological and hormonal correlates of helping behavior (see Candea 2012 for further details). Much of the research at the KMP, on a day-to-day basis, involves following meerkats around and recording the behavior and weight of known individuals to build up a large database of detailed life histories for comparative study (Candea 2010).

As this brief overview already suggests, at both sites, relationships between humans and animals are central to the conduct of the research and, in both cases, such relationships have to be limited, managed, and controlled through particular ways of behaving, speaking, and thinking. However, the relationships and the locus and type of control differ in each case, as does the relevance of the question of animal mind. As I will show below, research at both sites relies on a kind of active skepticism, but the way this plays out is rather different. I will now examine each in turn before returning to some comparative conclusions.

MEERKATS: BETWEEN DATA AND INTENTION

Perhaps you agree with Bierens de Haan who once said: you ethologists are like people who look at a painting with colour filters in front of your eyes so as deliberately to miss the most essential thing: colour. [Niko Tinbergen, cited in Burkhardt 2005:437]

Contemporary behavioral ecologists' avoidance of the problematics of mind has a long history. Under the guidance of its founders, Konrad Lorenz and Niko Tinbergen, 20thcentury ethology was bent on establishing animal behavior as a bona fide object of biological science. As Crist (1999) has shown, one aspect of this project was the crafting of a technical language for describing animal behavior that scrupulously avoided implications of intentionality, thus isolating the biology of behavior from commonplace Euroamerican descriptions of animals as minded subjects. Tinbergen, however, stressed that the exclusion of questions of animal experience "has nothing to do with whether I think that animals experience something. I do think that, but it is in my opinion totally irrelevant. Experiences are not perceivable, and thereby not usable in animal ethology" (Tinbergen in letters quoted by Burkhardt 2005:435).

Thus, in principle at least, animal mind was not negated but, rather, bracketed by ethology. In the place of explanations of animal actions in terms of intentions, Tinbergen asked ethologists to answer four different kinds of questions: What are the physiological causes of the behavior? What is its survival value? How, historically, did it evolve? And how does it develop during the lifetime of the animal? The answers to these four questions, although related, constitute explanations of behavior on different levels. Crucially, according to this model, none of these levels of explanations in principle coincide with or indeed conflict with explanations in terms of the subjective experience of the animal.

As a result of this initial division, the contemporary professional behavioral ecologists I spoke with were not particularly sanguine about the question of intentionality. They had no particularly strong feelings about the rise of cognitive ethology, a subdiscipline that explicitly sought to address the purported "fifth question" of animal experience and intentional behavior (see, e.g., Griffin 1984:786). If animals' subjective experiences were made scientifically available, it could become an interesting question, to be sure, but one that could hardly displace that of either physiological causation or evolutionary function.

These general disciplinary commitments are embedded in the very structure of scientific work at the Kalahari Meerkat Project, which was set up as a behavioral ecology field site in the early 1990s. At the KMP, a team of 15–20 volunteers, most of them European university graduates in their early 20s, observe a population of around two hundred meerkats, with one human following one group of meerkats every day. Volunteers engage in nonintrusive observational sampling of the meerkat's behavior (in the form of *ad lib*, *focals*, and *sound focals*),³ and three times a day they weigh the meerkats by coaxing them onto electronic scales with blandishments of water and boiled egg.

Volunteers themselves characterize the former practice ("ad lib" for short) as noninteractive—and, indeed, nonrelational—whereas the latter practice ("weights" for short) is inescapably relational and interactive, as indeed is the practice of habituation by which wild groups are slowly coaxed into accepting the presence of humans in their midst (see Candea in press). During weights, meerkats have to be convinced to climb onto the scales and to stay away once they have been weighed and let others take their turn. To do this properly—indeed, to do it at all—as one volunteer put it, "you need that personal connection to each meerkat" (Sue, personal communication, 21 Oct 2011). Weights is "when you, kind of, get to know them" (Elisa, personal communication, 21 Oct 2011), another said. In such interactions, an ability to read meerkats' intentions is crucial: "If I hold out the egg box and a female looks at it, I can tell if she's interested or not" (Jo, personal communication, 13 Oct 2011), as another volunteer put it. Elisa made the point uncompromisingly: "They are also living animals, they have feelings. So if you work with them like a machine or robot . . . it wouldn't work!" (personal communication, 21 Oct 2011).

By contrast, during ad lib, the data had to be insulated from such intersubjectivity. Thus, in answer to questions about the attribution of intention or subjective experience to meerkats, volunteers would often note that this was fine "unless it affects the data" (Annie, personal communication, 13 Oct 2011).

The data primarily consisted of predefined units of meerkat behavior collected and input into a Psion handheld computer (Candea in press). These units of behavior (such as a "pup feed," "dominance assertion," "predator alarm," and so forth) were understood as objective patterns in the world, the description of which can be agreed upon by multiple observers, independent of any question about what the animal may or may not have intended. Thus, for instance, a "guard up" is recorded when a meerkat stands on its hind legs on top of a raised vantage point for longer than ten seconds. A "foraging competition," according to the on-site protocol, should be recorded when the following occurs:

An individual approaches a food item or hole owner to <50cm, looking at it and either not foraging or scraping ground while looking at the food owner and a defensive action is undertaken by the owner. These actions involving [*sic*] growling vocalisations, moving body to block approach, pushing body against other individual, charging at other individual or biting other individual. [Kalahari Meerkat Project Official Protocol 2010:37]

In neither case is the question of the meerkat's subjective perspective in principle relevant to the description of the behavior (see also Crist 1999).

In sum, whereas they prided themselves on their ability to interact successfully with the meerkats in ways that involved implicit mind reading, the volunteers also distinguished such moments of interaction from the business of behavioral data collection when, as one volunteer put it, "you're there doing science and watching them and questioning what they're doing in a scientific way" (personal communication).

At other times, however, this distinction between "scientific" and "nonscientific" ways of being and seeing was drawn, not between weights and ad lib but, rather, within ad lib itself. Thus, volunteers would often claim that learning to recognize the meerkats as individuals with their quirks and habits helped them to quickly identify behaviors in the field. Elisa, who had some previous laboratory science experience, gave a reflexive portrayal of her own shifts in perspective as she moved from the field to data input:

When I'm out in the group, I see them more as individuals with personalities, and I kind of know . . . "[If it's] *you*, it's a foraging competition." When I'm back at the computer and have their codes in my Excel or Access tables and files, I think and see it more scientifically. (Interview 21 Oct 2011)

On the spreadsheet, Elisa added, behavior becomes "more black and white," and you can "filter for individuals and see what they were doing during your morning session" (personal communication). If seeing is "having the world at a distance" (Willerslev 2007), the KMP required different modes and kinds of sight, each with their own kind of distance and their own kind of authority. At the heart of this distinction between seeing scientifically and seeing in an everyday or nonscientific way was a distinction between seeing behavior as interchangeable, objective units and seeing meerkats as intentional, minded, known individuals.

What we have here is the general principle of separation outlined by Tinbergen, made into an embodied practice for data collectors: both kinds of seeing are legitimate and indeed necessary, but only the former (the collection of objective behaviors, without reference to meerkat perspectives) is directly "usable." The latter (recognition of animals as interactive partners, with implicit mind reading) is merely a useful tool. This ability to interact meaningfully with meerkats may produce all kinds of insights, but such insights cannot be straightforwardly translated into the former kind of objective, scientific knowledge of meerkat behavior as data.

The importance of this separation is clearest in situations where the boundary is challenged—when intention risks rearing its pesky head within the data themselves. There was a single case, one volunteer claimed, in which a question of intention mattered to the identification of behavior. Two separate codes and two separate keys on the handheld computer are used to record a "dominance assertion" and a "submission." In the KMP protocol, "dominance assertion" (keystroke V) is defined as covering a number of subsets of observable behaviors (each with their own secondary keystroke):

Dominance assertion is expressed in a number of ways, from glares to vigorous attacks: *approach*—marches purposefully toward the subordinate while staring at it hard (*A-prch*); *glare*—crouches down low and fixes subordinate with an unwavering stare (*G-lare*); *charge*—runs directly at the subordinate (*C-hrge*). (Kalahari Meerkat Project Official Protocol 2010:41)

By contrast, "submission" (down-arrow key) covers behaviors such as "grovelling" when they are initiated by the subordinate animal. However, in practice, submissive behaviors usually follow almost instantly upon "dominance assertions." Furthermore, these dominance assertions, as we have seen, can be very subtle and, more importantly, in the case of a "purposeful march," for instance, they rely upon an implicit attribution of purpose or intent. The volunteer explained the resulting difficulty:

Is the dominant approaching because she's asserting, or is she approaching cause she just wants to walk in that direction? . . . I think in that sense you have to look at intention and it's a bit of cognition . . . But otherwise most of it's quite straightforward. (Annie, Interview 13 Oct 2011)

As a matter of fact, other volunteers pointed to other such cases in which intention played a troublesome role. For instance, the protocol required volunteers to distinguish real from play fights. The latter were scrupulously excluded from the data. But when is a fight not a fight (when is it "play"?), and what is the difference between a nip and a bite if not a difference in meaning and perspective (Bateson 1958)?

The point of these examples is not just that the injunction to treat behavior as an external object, independent of intentionality, was necessarily violated in practice (Crist 1999). More profoundly, it is that such violations were perceived by the volunteers as violations and avoided as much as possible.

In other words, whereas the complete exclusion of any attributions of intentionality from behavioral data gathering was perhaps ultimately impossible, all sorts of mechanisms were in place to bring such an exclusion about as much as possible. At the most general level, implicitly intersubjective moments of weighing were cordoned off from what I have elsewhere called "inter-patient" (Candea 2010) moments of ad lib. More finely, personal knowledge of individuals in the field was separated out from the data management back at the computer. More finely still, the need to resort to mind reading in identifications of particular units of behavior was identified as a problem and kept to an absolute minimum. Separating data from intention was not a sharp, clear-cut process—like carving reality at the joints. Rather, it was a painstaking work of increasingly fine filtering-more like skimming a stock. The result could never be perfect, but the procedures made a significant difference.

CORVIDS: PROCEDURAL SKEPTICISM

I think that being a skeptic scientifically and a believer personally are not mutually exclusive. [Alice (CCL), personal communication]

Behavioral ecology is not, however, the whole of the science of animal behavior. Comparative psychology, once a distinct and even antagonistic endeavor, has since the 1960s come into increasingly close communication with behavioral ecology, as I have discussed above. However, although behavioral ecologists still tend to feel that they can give the question of mind a fairly wide berth, the study of comparative cognition engages directly in attempts to provide scientific answers to aspects of that question.

A particularly central line of debate in the field of animal cognition—in which the Comparative Cognition Lab was a prominent participant—centers on the possibility of establishing that specific species have particular cognitive abilities. Critics in the field reply to such arguments by showing how specific abilities could be redescribed as the effect of a series of computational responses to stimuli (for an ongoing dispute in the case of chimpanzees and the "theory of mind," see Povinelli and Vonk 2004; Tomasello et al. 2003). The CCL itself is perhaps most famous for research on mental time travel in corvids (i.e., the ability to plan for the future and remember specific instances in the past; see Clayton et al. 2003). CCL researchers have also worked on other abilities such as cooperative problem solving and physical cognition.

It is against this background that Alice, a first-year Ph.D. student at the lab, characterized one key contrast in the field in the following terms:

Simplistically, it's between believers and nonbelievers. It's people who believe that animals have all these cognitive abilities, like theory of mind or episodic memory, and people who do not. Or . . . people who believe that it is parsimonious to accept certain explanations given the data and people who believe that . . . it is not [laughs]. (Interview, 25 March 2010)

Both the initial formulation and the reformulation are telling. Alice's first formulation of the contrast involves a substantive commitment about the fact of the matter of animal mind: some scientists believe animals have it, others do not. This is the level at which polemical arguments tend to operate between researchers and at which findings in the discipline are also usually viewed from the outside-experiments prove that certain abilities exist. Alice's more careful reformulation, however, highlights the underlying abstention behind this talk of belief and disbelief. Cognitive abilities could probably never be established beyond all possible doubt. The most one could ever do, she argued, was to relentlessly test the abilities in question through "endless, endless, endless experiments showing that no matter what you manipulate and no matter in what context, the animal can still do the appropriate thing" (Interview, 25 March 2010).

Thus, to "believe" in corvid cognitive abilities in the sense described above was to trust the accumulated evidence of repeated experiments and, ultimately, the authority of peers in the scientific community who had produced these experiments. It did not involve an intersubjective leap of faith between an individual human and an individual animal or, rather, as we shall see below, it involved the ability to objectify such intersubjective insights. Thus, Alice was adamant that as a scientist she had to be able to be some sort of a skeptic, if only procedurally. Thus, whereas she characterized the CCL as broadly a lab of "believers," the key to their success and to the validity of their work was that they act as nonbelievers:

If you truly want to show that something is there, the way of doing that is to do your experiment as if you're trying to give every opportunity for it to show that it isn't there. (Interview, 25 March 2010)

Unlike the KMP volunteers, researchers at the CCL did not live together and mostly worked on independent projects. They thus differed in the extent and type of their relations to the animals with whom they worked far more than KMP researchers. Thus, Alice, who claimed jokingly that she thought of her birds as a cross between pets and colleagues, would argue with Jon, an older postdoc who claimed that his birds were not particularly tame and that this was very convenient for his purposes. Jon would call out Alice for using anthropomorphic language in describing her birds' behavior when "off duty" (over coffee in the lab common room, for instance); Alice would criticize Jon for "bring[ing] science into everything" (personal communication). A strict adherence, both on and off duty, to scientific terminology, she argued, would make it cumbersome, if not impossible, to speak about one's actual relationship with the animals.

Such disagreements testified to a broader agreement, however, about the existence of distinction between "scientific" and "nonscientific" ways of thinking and talking about the birds. These also related to broader procedural understandings: however they might speak in private, to each other or to me, the researchers at the CCL were committed to investigating the fact of the matter about their birds' cognitive abilities or lack of them, and this in turn committed them to painstaking procedures of control. These procedures of control were just as stringent as the ones in place at the KMP, but they differed in scale and focus.

To begin with, the birds at the lab were all hand raised and kept in aviaries. The birds thus relied entirely on the humans for food and the necessities of life. This setup contrasted strongly with the KMP, where the meerkats' ability to survive and reproduce "in natural conditions" was at the heart of the research. Concerns with interference and experimental control were thus primarily located at the level of survival and of aggregate patterns of social and reproductive behavior. These sorts of interferences were both necessary and unproblematic at the CCL. There, concerns with interference were also present, but they related principally to a much more fine-grained locus: the animals' minds themselves.

Corvids, like most birds, are usually wary of new objects and situations, and it would take days to habituate birds to a new experimental setup in order to simply get them to the point at which they would be willing to interact with and explore new objects in the cage. Getting to this point involved repeatedly exposing the bird to such new objects, sometimes encouraging them to approach it by placing an item of food on the object, and so forth. However, for the experiment to be valid, it was important that this exposure be carefully limited. As the head of the lab, Professor Nicola Clayton herself commented on an earlier draft of this article, "exposing" or "habituating" birds to new objects crucially should not be confused with "training." The point was precisely to see whether and how the birds were able to use specific objects spontaneously not to teach them to interact with objects in "the right way." The abovementioned Ph.D. student Alice described the subtle balance that had to be struck in this respect:

There's always this balance between getting them used to all the things they need to do without giving them prior experience that would, you know, make your experiment pointless, but also without giving them the kind of training that means that they never think again, sort of thing, they end up just doing everything automatically. And I had that problem with my first experiment. I overtrained them, as it turned out ... When I did all the transfer experiments—now I'm not saying it's necessarily because I overtrained them, it might be because they're stupid—but they failed all of the transfer experiments. They'd *learnt* the results, they'd learnt how to do them but they didn't *know* how to do them ... They just weren't thinking at all anymore. (Interview, 25 March 2010)

What this quote highlights, first, is that intentions and motivations are crucial to the research here in a way in which they are not at the KMP. There, intention matters not a jot to the data (in principle at least). Here, intention is what distinguishes a "true" from a "false" positive. Conversely, a lack of intention could produce a "false negative." Thus, Alice thought she could tell when a bird she knew well had failed an experiment simply because he was not trying hard enough:

Sometimes the trial is over in thirty seconds because he comes charging up, does whatever is in front of him, and then leaves again. And those are the trials that he fails. And so that frustrates me . . . He's agood bird who's being shit. Whether or not he understands . . . I can't say "I think he was impulsive on this trial but not on that trial," there's no way of me objectifying that, but sometimes it just looks like he's not thought it through. And that's probably because the experiments that I've given him have a very low cost for error. (Interview, 25 March 2010)

Thus, intuitions about what a bird might really be thinking (or whether they are thinking at all) of course mattered to the work done at the lab. But in order to count, such intuitions had to be objectified through experimental designs that allowed the distinction to be made between valid results and "false positives" or "false negatives." Therein lay the difficulty and the skill: a poorly designed apparatus might not give the bird enough incentive to prove itself or it might actually make the birds stupid—that is, stop them from thinking (Despret 2004a). A positive result was valuable, however, only if it could be distinguished categorically from a false positive—the point was not, in other words, to make the birds "clever," either.

BELIEF, TRUST, AND EPOCHÉ

At this point, I will pause to draw two interim conclusions from this discussion. The first is that—counter to some cursory characterizations of "Western Cartesian science" these examples suggest that questions surrounding animal minds are very much an unresolved ontological matter. The accounts of "naturalist ontology" with which I began, schematic as they may be, are thus quite apposite in one particular respect: they predict with great accuracy the likelihood of epistemological anxiety congealing around animal minds, these problematic entities at the intersection of the material and the spiritual, the inner and the outer.⁴ However, in casting these questions in terms of settled ontological "assumptions," one automatically resolves the anxiety into a set of unproblematic certainties. Agnosticism and skepticism are traduced into mechanism and biological reductionism. By contrast, as we have seen, the KMP volunteers, who do not directly study these questions, refer them to cognitive scientists. The cognitive scientists, who do study such questions, map with great epistemological care the contours of an unresolved scientific debate. In neither case were the researchers operating under the conviction that animals are mere objects or automata—as one might have expected from this purported stronghold of "naturalism."

My second conclusion is that ontological indeterminacy is no bar to meaningful interaction (for a similar argument in relation to robots, see Vidal 2007). In both contexts, embodied knowledge and skills allow careful (and, indeed, although I have not elaborated this in detail here, caring) relations to obtain between the researchers and "their" animals. However, I have also shown that these relations coexisted with a fundamentally ambivalent, skeptical attitude to the factual matter of animal mind, intention, or perspective. If these scientists were not quite settled "naturalists," neither did they inhabit the opposite certainties of "animism."

This double negative (neither naturalist nor animist) marks the difference between this account and a superficially similar one, in which scientists are by turns-or simultaneously—animist and naturalist in their attitude toward animals. Indeed, few anthropologists who describe alternative ontological schemas present these as mutually exclusive in practice—even though they are mutually exclusive in theory. Thus, for instance, Descola (2007:239-240) notes that even someone like himself, although "brought up in a naturalist world," can still occasionally behave like an animist, such as when talking to his cat (see also Candea 2012). The point that people may entertain two mutually incompatible ontological perspectives in quick succession is certainly an important caveat to overly schematic accounts of naturalist ontology. It is a point that Lys Alcayna-Stevens has brilliantly illustrated—in relation to animal mindedness through her notion of "double-think" (2009, 2012). Alcayna-Stevens (2012) writes of volunteers at a Catalan chimpanzee sanctuary who alternatively describe the knowledge of other minds as "impossible" and "obvious." With reference to her own ethnography as well as to that presented here, Alcayna-Stevens points out that ethnographers should be attentive both to their subjects' invocation of unbridgeable dualisms and to the way in which these dualisms are intermittently bracketed, dissolved, or forgotten. Alcayna-Stevens sees in this alternation the tension between people's conscious recognition of a potential multiplicity of ontological options and the need to "enact" one of these options at any particular experiential moment, in the immediacy of a particular set of engagements with other human and nonhuman entities. At this point, people usually plump for one ontological possibility among multiple available oneseven if only to change again instantly (Alcayna-Stevens 2012).

My own interest here is in the complementary ability to suspend such definitive enactment: to pause and remain in a state of noncommitment to any particular ontological option. The difference between moments of engaged commitment and moments of detached noncommitment is perhaps best illustrated by a comparative example drawn from the anthropology of belief: Malcolm Ruel's classic discussion of distinctions between "propositional belief" and "trust." In his foundational essay "Christians as Believers" (2002), Ruel showed the coimplication of belief and trust in the history of Christianity. With the apostolic writings of the New Testament, Ruel argues that the meaning of the term *pistia* (belief) changes:

Christian *belief* . . . begins to part company from Hebrew *trust*. Both refer to a relationship—the confidence that people have in God . . . —but for the Christians there is the added confidence or conviction about an event (the resurrection and all that it signifies) . . . A distinction made frequently today is between "belief in" (trust in) and "belief that" (propositional belief). This distinction may clear our minds today but it confuses history, for the point about Christian belief . . . is that it was both at once. [Ruel 2002:103]

Ruel's characterization of the double nature of Christian belief as simultaneously relational ("belief in") and propositional ("belief that") gives a clear counterpoint to what the researchers in both of these contexts were attempting: namely, to precisely detach the question of propositional belief (belief that nonhuman animals are minded, intentional, etc.) from the practice of relational trust (belief in nonhuman animals as intersubjective partners).

In other words, although there are a number of important contrasts between the two cases, the human-animal relations described here share a similar structure: an implicit mind reading coexisting with an awareness of the limits of one's knowledge. Or one could put the point more starkly: what both cases show is an active refusal to deduce certain facts about animal minds directly from the evidence of interspecies interaction. At the KMP, the kind of cursory instances of intention upon which everyday interaction was based is kept categorically distinct from the objective matter of meerkat "behavior"; in the CCL, these same intuitive perceptions of what the birds are really thinking have to be painstakingly objectified through careful research design before they can serve as evidence. Even when the researchers were actually committed to demonstrating the existence of animal mind, they remained simultaneously committed to doing so without first assuming it to be there.

The contrast between belief and trust also sheds new light on the animosity toward scientific skepticism in some recent work on human–animal relations. Rather like Ruel's Christians, a number of authors see propositional belief and relational trust as indistinguishable or, at the very least, as necessarily coimplicated. For instance, Crist, writing about scientists' attempts to avoid anthropomorphic language, argues that such attempts are doomed to fail. This is because the factual question of animal "inner worlds" is always, already, unavoidably, also a relational question:

In behavioural studies there is no detour from the inner life of animals, no avoidance tactic that can succeed, perhaps because, like the inner life of human beings, there is very little that is "inner" about it. Action either has a face or it does not—and its face is what we call mind. [Crist 1999:185]

If animal mind is a relational matter, then-a matter of "recognition"—the distinction between doubting and denying, between skepticism and mechanism, fades out of view: if mind relies on recognition, to doubt (relationally) is already to deny (propositionally). Hence the claim that objectivist scientific language that avoids mentions of animal mind "erases mentality" (Crist 1999:185) and "extinguishes the lifeworld" (Crist 1999:142). In the same vein, philosopher of science Vinciane Despret (2004b) urges both the scientists working with animals and the sociologists who study the scientists to abandon the problematics of propositional belief altogether and shift to a transformative ethics of trust. If, as these arguments would have it, the "recognition" of animal mind is performative, then claiming, as my informants do, that "the jury is still out" about animal mind is irresponsible because the jury will never be in. The implication seems to be that there is no fact of the matter independent of the relationship. Unmoored from the problem of accurate representation, the alternative becomes, as it were, purely moral: a decision to "foster" or "extinguish" the possibility of an interspecies relationship.

This, in turn, explains the frequency with which doubt and denial of animal mind are associated in critiques of scientific skepticism. This goes with a marked animosity toward and pathologization of doubt itself, which occasionally appears in recent writings on human—animal relations. Milton, for instance, writes that "Descartes is said not to have trusted the evidence of his senses. Most of us, happily, do not suffer from this handicap" (Milton 2005:260).

What I have been seeking to show in this article is that what Milton characterizes as a "handicap" is for many practicing scientists a hard-won and painstakingly cultivated virtue: the capacity to be able to hold at bay, up to a point, the evidence of your senses. In the context of animal behavior science, this often means being able to separate the evidence of interspecies trust from the empirical belief in (certain forms of) animal interiority.

This reformulation helps us to specify precisely in what sense these researchers are skeptical about animal minds. Skepticism, in both cases, is a line drawn between trust and a particular variety of propositional belief. Trusting in animals as intersubjective partners is intimately interwoven with various propositional beliefs about what animals want, intend, or feel. What the researchers in both settings are doing, however, is actively unpicking that weave: keeping the trust while abstaining from committing to the propositional belief. This unpicking is an active commitment that involves a reflection on one's own actions and beliefs. Here I rejoin Mair's strictures against the rather fixed and passive way in which some authors characterize the multiplicity of belief. The point is not simply that people have different kinds of beliefs but also that they actively reflect on this and seek to cultivate certain attitudes toward their own beliefs, and by those means to cultivate some beliefs—and ways of believing—rather than others (Mair in press). Similarly, the image of a passive, situational "ontological flip-flop" that emerges, for instance, from Descola's characterization, fails to attend to people's agentive attempts to keep in view multiple ontological possibilities at once, without committing to any.

One classic attempt to characterize this type of active suspension, which may be of analytical use here, is the notion of epoché. Epoché was a term used by the Greek skeptics to describe the simultaneously ethical and epistemic "spiritual exercise" (Hadot 2002) of suspending judgment or withholding assent to one's immediate perceptions. It is, of course, no coincidence that skeptic epoché was influential on Descartes's own articulations of methodical doubt (Husserl 1949:274–280), one of the modern texts that has been central to the development of the scientific self-image. Although rarely articulated explicitly in these terms, some version of epoché is cursorily invoked as part of general definitions of scientific method (e.g., Bachelard 1989).⁵ In particular, an orientation to epoché has been a central aspect of the training of researchers in animal behavior science, and learning to suspend judgment on seeming evidence of intentional activity is a part of this training. More broadly, historian of science Helen Macdonald has very convincingly outlined the importance of a kind of "professional negative capability" for ethological observers: the ability to remain in a state of doubt and uncertainty and to suspend theorizing about observed behavior (Macdonald 2009).

TOWARD A COMPARATIVE ANTHROPOLOGY OF EPOCHÉ

My aim in identifying epoché—an active abstention from belief—as an ethnographic object, however, is precisely not to rehearse generalities about scientific method. On the contrary, taking epoché as an object, we can begin to ask a host of comparative questions, both within and beyond "Euroamerica."

The above ethnographic discussion, although far from exhaustive, has already raised a number of potential comparative questions as to the application of epoché within Western animal behavior science. The first concerns its point of application: What should one strive to be noncommittal about? The answer to this question differed at the KMP and the CCL because the definition of the "problem" of animal mind was different in each case. Whereas CCL researchers were often actively engaged in reading about and reacting to contemporary debates about mind, intention, and action at the intersection of psychology, animal behavior, and philosophy, the majority of the KMP volunteers have only a general undergraduate training in biology, zoology, or conservation, followed by a specific training in the methodology of the KMP, which excluded mindedness as a matter of course. Thus, whereas the CCL researchers were often able to be quite precise in their delineation of distinctions between intention and conditioned response, debates about the meaningful nature of behavior, and so forth, volunteers at the KMP generally encountered "anthropomorphism" in a more broadly delineated sort of way, as a general problem for scientific interpretation.

A second difference is in the extent to which epoché is a personal, intentional endeavor or a matter of routinized practice. As I noted above, the intellectual history of behavioral ecology de facto tends to exclude the question of intentionality from its terminology and research questions, above and beyond the decisions of its practitioners. An avoidance of the question of meerkats' perspective is built into the categorical definition of what counts as data and into the procedures by which such data are to be collected, assessed, and analyzed. This means that practical intersubjective skills could be cultivated while the whole problem of mind was delegated to other knowers elsewhere-cognitive scientists, for instance. Furthermore, researchers such as Alice are faced with questions of mind and intentionality very directly and individually as elements of their own research design. For them the question of distinguishing an intentional action from an accidental or conditioned one is precisely what is at stake in the design of experiments. By contrast, volunteers at the KMP are data collectors, applying research procedures that were designed by others. In other words, much of the weight of suspending belief about animal minds is carried institutionally rather than individually at the KMP. Epoché is often a matter of routinized procedure. As one volunteer noted above, "Most of it's quite straightforward" (Annie, Interview 13 Oct 2011).

One might imagine a number of other axes of comparison (for instance, concerning the different types of moral, metaphorical, gendered, and other associations of the practice of epoché)—both between different forms of Euroamerican practices and between purportedly "naturalist" and purported "nonnaturalist" contexts. Recent ethnographic accounts of animist practice, for instance, have increasingly highlighted the coimplication of engagement and detachment within relationships with nonhuman animals (Fausto 2007; Kohn 2007; Willerslev 2004): Might these be cases in which a form of epoché or abstention from belief also takes place and, if so, how do the comparative questions above apply in those cases? Similarly, the comparison with the often-reported claims of Melanesians about the opacity of other human minds is a fascinating potential for comparison that I do not have space to undertake here (but see Robbins 2008).

Calling for such comparisons of modes of abstention from belief goes against the grain of a recent move in anthropology and cognate disciplines such as science studies away from questions of representation and epistemology (Henare et al. 2007; Latour 1987; Thrift 2007; Viveiros de Castro 2011). The latter is in many ways a bracing and productive move, as well as an understandable reaction to some of the textual excesses of postmodernism. However, what risks being lost in the process (as Mair argues of the anthropology of religion's own move away from belief) is our attention to people's own commitments to issues of knowledge, belief, and representation.

In particular, attending ethnographically to people's active forms of skepticism and abstention from belief highlights the limits of accounts cast in terms of contrasting ontologies. I have argued in this piece that the ontological framework is insufficient to capture the sense of active suspension of belief cultivated by scientists and enshrined in different ways in their institutionalized practices. What I am calling "epoché" is precisely the mark of these scientists' suspension of the ontological. But the point is more general. One of the aims of the ontological turn was precisely to circumscribe Western scientific activity and its host of representationalist concerns, to mark it out as only one among many ontological possibilities. In proposing that epoché trumps settled ontological schemas does one not risk reinstating the special privilege of Euroamerican science, as though scientists alone had the ability or right to "subvert any settled conception of space, time and matter" (Stengers 2007:12)? I would argue not. For there is no reason to suppose that epoché is exclusively either a scientific or Euroamerican preserve. As Jeanette Edwards and colleagues have recently noted, "experimental attitudes, the search for evidence, coherent argument, openness to refutation, critical thinking, doubt and operational skepticism are all found in life worlds that would not otherwise be described as either 'modern' or 'scientific'" (Edwards et al. 2007:3). A comparative anthropology of doubts and operational skepticisms—both within and beyond Euroamerica-would do as much, if not more, to challenge scientific exceptionalism as the multiplication of ontological certainties.

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NOTES

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- Tanya Luhrmann's book When God Talks Back is not only an outstanding instance of the sort of anthropology of belief Mair is calling for, its argument is also extremely relevant to the one I am articulating here. I unfortunately read this book at a point when the present article was already in production and so cannot do it justice here, but please see http://www.candea.net for a discussion.
- 2. As early as 1966, Robert Hinde's handbook of animal behavior bore the subtitle "A Synthesis of Ethology and Animal Psychology." There is considerable conceptual and literal communication between the two sites, whose "headquarters" are a stone's throw from each other in Cambridge, United Kingdom, in the departments of zoology and experimental psychology, respectively. Thus, a number of researchers at the meerkat project are in fact working on issues relating to cognition (see, for instance, Thornton 2008), whereas a number of papers produced at the CCL have focused on the evolution of cognitive abilities (see, for instance, de Kort and Clayton 2006; Seed et al. 2009). Furthermore, the head of the CCL, Professor Nicola Clayton, who was herself trained in behavioral ecology and has a first degree in zoology, has developed the distinctive comparative cognition approach based precisely on combining insights and methods from behavioral ecology and comparative psychology.
- 3. Ad lib involves noting down every occurrence of a set of behaviors chosen by the project (or "at the project's pleasure"; ad libitum) as they occur within the observed population during the sampling period. In practice, it involves one volunteer watching a whole group of meerkats simultaneously over the course of a morning or afternoon. Focals involve noting down all of the behaviors of a selected individual during the sampling period. In practice, this involves following one meerkat very closely, often for shorter, precisely timed periods. In both cases, behavior is noted down on a Psion handheld computer (cf. Candea in press). Sound focals also involve following one individual, but in this case the meerkat's vocalizations are recorded at the same time as the behavior, and context is noted down by the observer through another microphone, creating a double recording in which vocalizations can be matched up to specific behaviors or situations.
- 4. I thank Martin Holbraad for this insight.
- 5. Epoché is implicit in the principle enounced by Gaston Bachelard: "In the formation of a scientific mind, the first obstacle is primary experience, experience placed before and above the criticism that is necessarily an integral element of the scientific mind" (1989:23; see Daston and Mitman 2005:2 for translation). Even though the phenomenologist Bachelard may have been profoundly influenced

here by Husserl's own rediscovery of epoché as the first step of phenomenological reduction, his description of the scientific mind breaking with primary experience resonates with scientific selfdefinitions.

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