**Continuity as Crisis in Comparative Cognition:**

**A Critical Genealogy of Human Exceptionalism**

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**I. Statement of Thesis**

My dissertation is motivated by the question as to why contemporary philosophers and scientists are widely resistant to attributing human-like cognitive capacities to non-human animals, particularly great apes, for reasons that do not appear to be based on compelling empirical or theoretical grounds. With respect to the current state of the comparative cognition literature, Rollin (2013: 15) rightly questions the tendency of “empirically-oriented philosophers and biological and psychological scientists to be agnostic if not downright atheistic about animal mind.” Indeed, decades worth of experimental and ethological research has failed to mitigate widespread skepticism under the guise of the so-called “logical problem,” the proponents of which state that *all* approaches—past and present—that have been used to evaluate cognitive capacities such as the presence of theory of mind in animals “cannot provide evidence for this ability even in principle” (Halina 2015: 474). What’s more, a problematic form of rhetoric has long remained popular in arguments for human uniqueness wherein it is claimed that apes and other “higher” taxa merely behave *as if* they possessed X, (where X is a cognitive capacity long thought to be exclusive to our species), but only humans exhibit behavior that is “truly” indicative of “genuine” or “real” X. My contention is that both these skeptical and rhetorical means of reinforcing traditional boundaries between the cognitive capacities of humans and other species owe less to naturalistically-minded approaches to inquiry, and more to underlying normative assumptions that have deep, tractable histories of prejudging the grounds for a rigid human-animal division in philosophy and science.

My project appraises the current state of the literature by providing a critical genealogy which charts the development, accretions, and sedimentations of these contemporary attitudes in order to understand why it is that hypotheses suggesting cognitive continuity have been marginalized and met with skepticism for centuries despite vast improvements in the available evidence—especially*,* I argue, in a post-Darwinian intellectual climate where we would expect lessopposition to continuity-hypotheses, rather than *increased* opposition (Boakes 1984; Singer 1994; Rollin 1989, 2013). While Rollin and Boakes attribute these current “agnostic” and “atheistic” trends to sedimentations of positivistic values propagated by behaviorism, my dissertation aims to demonstrate that there is a great deal more to this story, thereby casting new light on contemporary attitudes about animal cognition and undercutting dominant arguments in defense of human uniqueness.

A critical genealogy will demonstrate how challenges to long-dominant world views have led contemporary scholars such as Penn, Povinelli, Heyes, Vonk, and Lurz to place a great deal of emphasis on the so-called logical problem.[[1]](#footnote-1) It will also trace the history of how problematic forms of rhetoric—prominent today in the writings of Tomasello’s Leipzig group—have been traditionally used to undergird discontinuity arguments wherein the most complex of human faculties, *e.g.,* recursive mindreading, are used as the “gold standard” (Bekoff and Peirce 2009) to define the “true” or “real” meanings of core cognitive concepts, thereby denying their application to the behavior of non-human species. For instance, despite the fact that “we are far from being the most cooperative species on the planet” (Skyrms 2009: 145), an argument repeatedly touted by the Tomasello group is that humans are the only species that “truly” cooperates (*e.g.,* Tomasello 2006, 2008, 2009), or that engages in “*truly* *joint* joint attention” (Carpenter and Call 2013).[[2]](#footnote-2) A critical genealogy will reveal that such means of reinforcing pre-established notions of human uniqueness are based on outmoded and/or prejudicial assumptions about animal minds and thus have no place in the contemporary literature. While the presence of a healthy, responsible skepticism about animal minds is necessary, I argue that prevailing defenses of human uniqueness are neither healthy nor responsible.

My genealogical project will show that, since antiquity, discourse over animal minds has broadly followed two paths, both of which are representative of positions in the recent philosophical and scientific literature. By far the more dominant path has been to defend a fundamental discontinuity between features of the human mind and the rest of the natural world. I argue that this dominant tradition is bound up with certain kinds of arguments, rhetoric, and values that prejudge debates in comparative cognition in favor of discontinuity, and that we seem to be in this position at the moment—particularly, I argue, with respect to debates over mindreading in chimpanzees. My project historically situates oft-cited discontinuity arguments and skeptical attitudes emerging from the Leipzig and Louisiana labs, led respectively by Michael Tomasello and Daniel Povinelli, alongside outmoded and anthropocentric forms of rhetoric and argumentation found in Aristotle, the Stoics, Aquinas, Descartes, Kant, and Wallace (1870), among 20th century figures such as Wilhelm Wundt (1912), William James (1890), Albrecht Bethe, Theodor Beer, and Jakob von Uxeküll.[[3]](#footnote-3)

I also present a genealogical account of an alternative, marginalized tradition that takes the possibility of cognitive explanations of animal behavior very seriously. These figures emphasize and defend conceptions of a fundamental kinship and/or continuity between human cognitive capacities and those of other species, embracing—I argue—a more open-minded and empirically grounded approach to evaluating evidence from a wide variety of sources and toward a wide variety of taxa. In line with my rationale for investigating the dominant tradition, the impetus for critically exploring this alternative history is my contention that long-standing epistemic stalemates and dogmatic positions prevalent in contemporary debates can be ameliorated and avoided by drawing from ideas commonly embraced by figures from this historically marginalized group.

Unlike the trajectory of views embodied in the works of canonized philosophers in the dominant tradition, this alternative history is far less linear in its varied defenses of cognitive continuity. Nonetheless, I suggest that there are intriguing conceptual similarities that link these figures. The key players here range from Plutarch, Porphyry, La Mettrie, Condillac, Gassendi, Hume, Voltaire, Schopenhauer, Darwin, Romanes, Schweitzer, Washburn, Mills, and Yerkes to—more recently—Griffin, Gruen, Bekoff, Racine, and de Waal. The writings from this history of ideas are indicative of a more naturalistic approach to cognition (or, as de Waal [2016] stresses, cognition*s*) that does not evoke a cognitive hierarchy with human abilities situated at the top. My project explores potential explanations for why this tradition has not received due attention in the current literature, and, again by means of a critical genealogy, I utilize attitudes and arguments from these figures to outline and promote a healthy skepticism toward animal cognition. This genealogical project culminates in a chapter that aims to undercut the force of contemporary discontinuity arguments regarding—and generalized skepticism about—the socio-cognitive capacities of chimpanzees.

In sum, contrary to the largely ahistorical perspective from which recent debates over human uniqueness are most often undertaken, I argue that the key to achieving clarity and resolution on seemingly intractable issues such as the logical problem rests, importantly, on the recognition that there is actually very little that is *contemporary* about these ways of reinforcing traditional species boundaries. I am interested in the question as to why it is that the former path has remained the prevailing ideological background for discussions of animal minds, while the more naturalistic approach to comparative cognition has been largely relegated to the sidelines, constantly being met with heightened skepticism, accusations of anthropomorphism, and indictments of flawed analogical reasoning (anathema to many in the modern literature).

While the logical problem has been a bone of contention for decades, a critical genealogy of this problem has never been undertaken. My decision to approach this topic from an historical perspective is motivated by my contentions **(1)** that there *is* room for responsible skepticism about animal minds in the current literature, **(2)** the skepticism of the Povinelli camp is *not* responsible, and **(3)** thatattractive avenues for formulating a responsible skeptical stance can be uncovered through such a project. Furthermore, the exceedingly common (and problematic) use of terms like “real” and “true” to reinforce what I call “*as if* arguments” for human uniqueness[[4]](#footnote-4) remains profoundly under-discussed.[[5]](#footnote-5) Put simply, my genealogical project seeks to answer the question, “Why this almost desperate insistence on human uniqueness?” (Steiner 2005: 94), and my reconstructive project turns to the alternative history in order to suggest avenues for removing traditional prejudices from contemporary debates in comparative cognition.

**II. Outline of Project**

The bulk of my dissertation will be divided into two parts. The first part will contain two, internally related chapters of ~25 pages in length; the second part will be comprised of two chapters of roughly the same length. The goal of Part One is to provide a critical genealogy of thedominant tradition. These chapters challenge conventional ways of reinforcing human exceptionalism that remain present in the current literature, as well as offer explanations as to why the alternative naturalistic path has been marginalized. In Part Two, I turn to the history of ideas that comprise this alternative tradition, critically exploring, distinguishing, and consolidating various naturalistic modes of inquiry in comparative cognition, and drawing from the wide variety of disciplines of which this relatively nascent field is comprised. My aim here is to argue that achieving clarity and consistency in comparative cognition requires that we take very seriously these naturalistic approaches.

The final chapter applies the content of the first four chapters to critique the current state of debates over mindreading in chimpanzees. Chapter Five defends the following two theses: **(1)** the rhetoric of “real X” or “true X” has no place in comparative psychology, and **(2)** the logical problem does not need to be ‘solved’ in order to attribute cognitive capacities such as a theory of mind to non-human animals; this problem is not unique to comparative cognition and its tenacity in this literature is indicative of longstanding double standards regarding the perceived gap between humans and animals.

I open my dissertation broadly by highlighting the fact, put nicely by Rollin (2006: 28), that “despite the ideological belief that science changes only by empirical or logical falsification,” values (whether epistemic, moral, or indicative of a cultural zeitgeist) have played significant roles in “what counted as scientific legitimacy” in engendering certain scientific and philosophical ideologies and presuppositions in ethology (Burkhardt 2005), psychology (Greenwood 2015), and science more generally (Kitcher 2001). I raise the crucial question as to which epistemic, ethical, and social values have motivated 21st century ideological stances in *comparative* psychology, as well as to what historical contingencies modern attitudes regarding human uniqueness may owe their origins. The idea here is to stay broad in my approach and raise questions that will be responded to critically in the genealogical chapters.

The overarching purpose of the Introduction is to present the *problem*, *i.e.,* why evidence suggestive of cognitive continuity is so strongly contested today, as well as my *method* for engaging it, *i.e.,* critical genealogies of two opposing traditions. To explain what I mean by a critical genealogy, I reference philosophers such as Nietzsche (1887), Dewey (1925, 1929), Williams (1985), Rollin (1998), and Kitcher (2011). At least two legitimate criticisms of this genealogical method must be dealt with: **(1)** the risks involved in using anachronistic sources as means to critique contemporary positions, and **(2)** the risk of distorting history (as well as present views) by forcing linearity into ‘the record,’ *i.e.,* just because both Aristotle and Tomasello both use *as if* arguments to defend claims of human uniqueness, this doesnotnecessarily imply any historical influence.

In comparing the dominant and marginalized traditions, I highlight a fact that I take to be of some significance: prior to the 20th century, the opposing ideological positions regarding animal minds in both histories arose in concrete relationtoopposing positions in animal ethics; figures from the dominant history advocating differences in *kind* were strongly opposed to moral consideration for animals, while figures in the marginalized history purporting differences in *degree* were far more amenable to consideration of their moral status, *e.g.,* from Plutarch’s, Porphyry’s, and Schopenhauer’s arguments from animal capacities to vegetarianism, to Darwin’s (1871: 126) claim that the “disinterested love for all creatures” is “the most noble attribute of man.”In the 20th century, however, contributors to the animal minds literature tend not to make claims about ethics. In this regard, my genealogical project may cast light on the following question: since ancient and medieval discontinuity arguments were motivated by ethical and ontological assumptions about human exceptionalism and the *scala naturae*, and accretions and sedimentations of these arguments remain present in the contemporary literature, then to what extent might the current agnosticism and atheism about animal minds owe its foundations to these normative positions from the history of philosophy?

**Part One** of my project provides a critical genealogy of the dominant tradition. I begin by elucidating and expanding upon Sorabji’s (1993) claim that a “crisis” occurred in ancient philosophy, which arose in response to *the difficulty of reconciling pre-existing notions of human uniqueness with empirical evidence and/or reasoned arguments strongly suggestive of species continuity.* Animals often behave in ways strongly suggestive of explanations evoking higher cognition—explanations that are often taken for granted when humansbehave in analogous situations, *e.g.,* group hunting in chimpanzees. The “crisis” to which Sorabji is referring is the following: if we deny cognitive capacities to animals, we must then find another way to convincingly explain the presence of *cognitive*-*looking* behavior without evoking cognition. In the case of Aristotle, for instance, “To compensate for the denial of reason and belief to animals, perceptual content must be expanded” (Sorabji 1993: 17). This was no easy task, leading to tensions throughout Aristotle’s own—as well as subsequent—explanations of complex forms of animal behavior.

I argue that Aristotle’s dogmatic positions on animal minds were, in fact, antithetical to his long-dominant theory of the proper conditions for obtaining empirical knowledge, thereby suggesting other normative factors at play. This conjecture is given additional weight considering the fact is that while Aristotle’s ethical and political writings are full of claims that animals merely behave “as if” or “as though” they possessed capacity X (Sorabji 1993), his mammoth zoological texts rarely contain rhetoric of this nature (Steiner 2005). In terms of influence, it was ultimately the former, more dogmatic, approach that abounds throughout the canonized history of discussions of animal minds, *i.e.,* from the Stoics, Aquinas, Descartes, Locke, and Kant, to Darwinian reactionaries of the late-19th to early 20th centuries, *e.g.,* Wallace (1870), Bethe, Beer, and von Uexküll (1899). Importantly, figures from this dominant tradition did not suspend judgment at this juncture; they did not question whether or not the *appearance* of human-like cognitive capacities might possibly reflect an underlying *reality*. Rather, they reacted to this “crisis” by further digging in their heels in favor of human exceptionalism.

As I conceive the structure of my project, the first chapter of Part One explores the accretions of overriding philosophical reactions to this “crisis” from the history of philosophy up to Kant, while the second chapter explores the emergence and aftermath of what I argue to be a very similar crisis that occurred whencomparative psychology arose as a “proper” science at the turn of the 20th century.[[6]](#footnote-6) Scholars such as Thorndike, Pavlov, and Watson reacted to the anecdotal anthropomorphism of Darwin and Romanes by taking up the “challenge” of Morgan’s Canon “to develop explanations of animal behavior without reference to mentality or consciousness” (Greenwood 2015: 225). In linking these two chapters, I suggest that there are strong parallels between the crisis to which Aristotle and his intellectual progeny were responding, and the dominant attitudes underlying the “controversial milieu” (Murray 1990) from which the scientific discipline of comparative psychology emerged.

Indeed, Washburn (1917: 16-7) was rightly concerned with what she described as the “opposite tendency” to the optimistic, open-minded—though, due to over-reliance on anecdotal anthropomorphism, admittedly flawed—approaches of Darwin and Romanes to animal cognition, namely, the reactionary “tendency to make purely biological concepts suffice as far as possible for the explanation of animal behavior and to assume the presence even of consciousness in animals only when it is absolutely necessary to do so.” While the parallels to the initial “crisis” described by Sorabji are, I suggest, quite clear, a key difference lies in the fact that unlike the largely dogmatic attitudes towards animal minds from the history of philosophy, the discontinuity hypotheses that prevailed throughout much of the scientific literature of the 20th century did so largely in the face of rapidly increasing evidence for “higher” or more complex forms of animal cognition. Washburn (1917: 24), for instance, notes her discomfort with dogmatic opposition to the very idea of cognitive continuity at a time when evidence for animal cognition had “wonderfully advanced within the last twenty-five years.”

With respect to the current state of the literature, I suggest that it is evident that the same “crisis” or “challenge” to human uniqueness is likewise recognized and engaged with today. Gruen (2011: 9, 12) rightly describes a clear “bar-raising dialectic” throughout debates over human uniqueness in the late-20th and early 21st centuries as “misguided attempts by those who cling to the idea of an insurmountable divide between humans and other animals to establish human exceptionalism—even in the face of clear evidence establishing continuities between human skills and the skills used by some non-humans.” The final section of Part One thus situates the hugely influential commentary to Premack and Woodruff’s (1978) landmark paper, “Does the Chimpanzee Have a Theory of Mind?” in the context of this historical trajectory. It is here, in the critically persuasive responses of Dennett (1978), Harman (1978), and Bennett (1978), where the logical problem as it is commonly known today emerged.

Furthermore, it was at this moment in history when the notion of *theory of mind* arose as a core concept in a myriad of debates over human uniqueness, where it has since served as the linchpin for defining the “true” meanings of concepts in comparative social cognition. In tracing predominant reactions to this “crisis” throughout these historical iterations, Part One therefore provides the critical groundwork for demonstrating how the rhetoric of *as if* arguments and the skeptical basis of the logical problem are usefully conceptualized as contemporary manifestations of long-dominant attitudes toward preserving a rigid human-animal division in the study of animal minds.

**Part Two** is tentatively titled “Comparison without Crisis: The Marginalized Discourse and the Way Forward.” Here, I provide a critical genealogy of the second*,* more naturalistically mindedtrajectory of ideas regarding the study of animal cognition. I note that the proponents of the wide variety of views that make up this alternative history—reaching back as far as Plutarch—do not recognize any challenge or “crisis” inherent in the fact that non-human animals behave in ways indicative of cognitively complex human behavior. By explicitly placing the marginalized figures and ideas of which I am sympathetic into dialogue with their contemporaries in the dominant tradition, the chapters that comprise Part Two adopt a more *constructive* tone than those of Part One. I maintain, for instance, that the approaches to comparative cognition at the beginning of the 20th century from figures such as Washburn, Mills, and Yerkes were more empirically substantiated, facilitated more productivequestions about the similarities and differences between humans and other species, and offered better grounds for a responsible skepticism about animal minds than was the norm during their time.

In a similar vein, I point out what I take to be nascent forms of the logical problem in figures ranging from Philo to Chanet, whose writings I place in tension with the “constructive skepticism” of Mersenne and Gassendi (Popkin 1979). These early-modern skeptics explicitly challenged Descartes’ dogmatic position on animal cognition, adopting a “positivist-pragmatic conception of knowledge” that accepted findings from the natural sciences, while likewise fighting the “quest for certainty” (Popkin 1979: 131) that provoked many figures of their age to raise skeptical challenges akin to what is today called the logical problem. Unlike the vast majority of their contemporaries, I show how Mersenne and Gassendi remained cautiously optimistic on questions of animal cognition. They were careful to avoid anthropocentric double standards by explicitly identifying the study of animal minds as a science like any other.

As Popkin notes, “Beginning with Mersenne, a new type of scientific outlook had arisen, a science without metaphysics, a science ultimately in doubt, but for all practical purposes, verifiable and useful” (1979: 140). In Part Two, I am interested in exploring the question as to why, when it came to the topic of *animal minds*, this scientifically-oriented skepticism remained marginalized during this period, only to be embraced by figures such as Washburn and Morgan centuries later—and, in the latter case, consistently misinterpreted and utilized to undergird claims of human exceptionalism emerging from the dominant tradition throughout the 20th century. Interestingly, as both a proponent of cognitive discontinuity, as well as of open-minded, responsible skepticism about animal minds (Greenwood 2015: 225), I explore how Morgan had one foot in both traditions.

The chapters in Part Two also emphasize the curious fact that the figures from these two traditions very often *spoke past one another*—a point which, I aim to show, applies to the contemporaryliterature as well. As such, I suggest that placing their attitudes and ideas into critical dialogue is imperative for identifying the strengths and weaknesses of both sides. In this sense, I conceptualize the critical genealogy in Part Two as somewhat of a dialectic, wherein each tradition may be understood as keeping the other in check, and from which a more judicious approach to inquiry into animal cognition and human uniqueness emerges in the process.

As a result, these chapters will occasionally embrace features of the dominant tradition in order to adopt a critical attitude toward less-attractive features of the marginalized tradition. I explore, for instance,how it is arguably the case that figures ranging from Aristotle to Tomasello tend to *overestimate* facets of human cognition and *underestimate* facets of non-human cognition (Boesch 2007; Buckner 2013; Andrews 2015), but how the *converse* is arguably true of many figures from the marginalized tradition. This area of focus will be equally important when discussing the study of animal minds in the 20th century as it will when examining classic philosophical attitudes on the subject. Hume’s claim, for instance, that “no truth appears to me more evident, than that beastes are endw’d with thought and reason as well as men” (*T* I.3.16) is clearly as dogmatic as the dominant Aristotelian-like views of Descartes, Locke, and Kant that animals do not possess thought or reason at all. Likewise, while Huxley and La Mettrie posed valuable challenges to the potential double standards involved in Descartes’ mechanomorphic conception of animals, thereby introducing a valuable point of contention into traditional arguments for human uniqueness, such views were arguably more reactionarythan they were substantive (despite both having a neurological basis).

The overarching aim of Part Two is therefore to consolidate the most attractive features of this critical genealogy into a responsible, multi-faceted attitude toward the study of animal cognition. Part of this attitude consists in contemporizing “Gassendi’s Challenge” to the “strict either-or between full rationality and inert passivity” (Steiner 2005: 93)—a feature of debates over animal minds that remained present in “the survival of only two extreme parties” (Washburn 1908: 17; see also Mills 1905) during the birth of the discipline and that, I argue, is still largely present in the literature. Another part consists in critically exploring what Andrews (2015) has called “Morgan’s Challenge,” namely, “To interpret animal behavior one must learn also to see one’s own mentality at levels of development much lower than one’s top-level of reflective self-consciousness. This is not easy, and savors somewhat of a paradox” (Morgan 1930: 250). Andrews recently uncovered this passage from Morgan’s *Autobiography*, commenting—fairly, I argue—that “While Morgan’s Canon is taught to all students of comparative cognition, Morgan’s Challenge is not, though meeting it is a requirement for doing good comparative work in psychology” (Andrews 2015: 44). Besides Andrews’ brief mention, the meaning and implications of Morgan’s Challenge have yet to be explored.

What’s more, I examine how many figures from the marginalized tradition place great emphasis on the necessity of drawing from a wide variety of evidential sources and multiple forms of criteria (including field data and, occasionally, anecdotal evidence), rather than focusing on crucial experiments and single populations as representative of an entire species (a common criticism of the Tomasello and Povinelli groups[[7]](#footnote-7)). Yerkes (1905), for instance, “emphasize[s] the importance in comparative psychology of the use of all available signs or criteria of mind rather than the selection of any one as the sufficient and final proof of consciousness”—a position suggestive of what Washburn calls the “ideal method” (1917) and what Andrews (2015) calls the “calibration method.” As I suggest in the concluding section of Part Two—and elaborate upon in Chapter Five—the methodological approaches embraced by the marginalized tradition in many ways stand in stark relief with those of the Louisiana and Leipzig groups.

Finally, I explore how Washburn (1917) and Yerkes (1905) professed views indicative of de Waal’s (2016: 158) recent call for “a moratorium on human uniqueness claims” in favor of a “unitary theory that covers all the various cognition*s* found in nature” (see also Bekoff and Pierce [2009]). I contend that the grounds for such a view were present as early as Gassendi.[[8]](#footnote-8) In this sense, what distinguishes many of these figures is their interest in hypothesizing cognitive capacities in a wide array of species, often conceptualizing the very notion of *human uniqueness* as no more significant than the “uniqueness” of any other species. Whenever this notion *is* entertained, it is treated with a cautious, healthy skepticism perceptively aware of our historic tendency to prejudge such questions in favor of human beings.

**Chapter Five** shows how sedimentations of the dominant tradition are currently shaping the structure, skepticism, and prevailing discontinuity hypotheses embodied in contemporary debates over mindreading in chimpanzees, while also critically applying the naturalistic attitudes discussed in Part Two to this literature.

First, Chapter Five defends the thesis that, while the rhetoric of “real X” or “true X” has proven useful in human psychology, it has no place in comparative psychology. I describe how this sort of rhetoric has been used to draw important, unbiased, distinctions in psychology, *e.g.,* there *is* a reasonable discussion to be had between what constitutes altruism vs. altruistic-looking behaviors. In *comparative* psychology, however, I suggest that something markedly different is going on. In this comparative context, when adjectives such as true*,* real*,* genuine*, etc*. are attached to cognitive capacities, it is evident that the intent of the prefix is to be synonymous with the expression of a given behavior as *humans* perform it. Chapter Five critiques the implicit normativity that has entered the picture when similar rhetoric is used—particularly in the writings of the Tomasello camp—in arguments which draw distinctions in *kind* between the human-looking behavior of other species and the like-behavior of our own.

Chapter Five then stakes a claim on the necessity of ‘solving’ the logical problem. Ioutline two positions here. The Povinelli group, along with philosophers such as Lurz, believes that the antidote to the logical problem is to design more appropriate experimental paradigms. Others, such as Andrews, Halina, and Buckner—with whom I ally myself—suggest that *no* experimental paradigm could ever solve the problem and, besides, “science doesn’t usually proceed by relying on a single groundbreaking experiment to prove a theory true. Instead, a research program arises, involving many different researchers, studies, and approaches, and which aims to investigate the phenomenon from a variety of angles” (Andrews 2015: 149). This*,* I suggest, is the appropriate naturalistic approach to studying mindreading in chimpanzees. Chapter Five considers and builds upon a number of other specific criticisms of the logical problem likewise based on a naturalistic basis of inquiry (*e.g.,* Halina 2015) which, I argue, owe their foundations to attitudes from the marginalized tradition.

**III. Selection from Dissertation**

**Varieties of Skepticism in Comparative Cognition:**

**The Logical Problem**

**I.**

Discussions amongst philosophers and primatologists have long been conducted under the influence of what has, in recent years, been referred to as the *logical problem* (Hurley and Nudds 2006) or *Povinelli’s Problem* (Lurz 2011). In the mindreading debate, the logical problem rests the difficulty (perhaps impossibility) of designing experiments that can convincingly distinguish between “two very general and opposing theories” (Lurz et al. 2014) of the mechanisms underlying social cognition: behavior-reading hypotheses and mindreading hypotheses. With respect to the former, agents make predictions about others’ behavior solely on the basis of non-mentalistic representations formed by past experience with various societal cues.[[9]](#footnote-9) Animals navigate their environments without representing these social cues being caused by—or in any way associated with—underlying mental states in themselves or others. It is crucial to emphasize that behavior-reading hypotheses do not amount to a stimulus-bound conception of the causes of animal behavior. For instance, Penn and Povinelli (2013: 63) argue that while chimpanzees are “fully cognitive creatures with a rich suite of representations at their disposal,” nonetheless, the “comparative evidence strongly suggests that nonhuman animals possess a variety of top-down heuristics, ploys, and biases for picking out the causal features of other agents’ occurent behaviors and for reasoning about other agents’ future behavior in terms of their goal-directed relation to the world.” While there exist a number of distinct accounts of behavior-reading hypotheses,[[10]](#footnote-10) there is no reason to assume that they are mutually exclusive.

In contrast to explanations of this nature, the mindreading hypothesis states that certain animals are capable of predicting behavior on the basis of a *theory of mind*. According to Premack and Woodruff (1978: 515), “An individual has a theory of mind if he imputes mental states to himself and others. A system of inferences of this kind is properly viewed as a theory because such states are not directly observable, and the system can be used to make predictions about the behavior of others.” To infer another’s *mental states* means that one is interpreting that individual’s behavior in terms of their underlying intentions, beliefs, doubts, knowledge, as well as perceptual states such as hearing and seeing. Mindreading hypotheses also come in various forms[[11]](#footnote-11) and, as with the alternative conceptions of the behavior-reading hypothesis, it entirely possible that mindreading animals—including, of course, humans—possess and utilize multiple mechanisms to predict the behavior of others (Mitchell 2009: 1309).

With this distinction in mind, we can now clarify our understanding of the logical problem with the help of a specific example. Liebal *et al.* (2004) found that, when gesturing to both humans and conspecifics, chimpanzees will reliably exercise the following process: Attempt one gesture, monitor the receiver’s response, and if necessary, walk around the receiver and repeat the gesture or try a different one. The fact that chimpanzees appear to employ “practical reasoning” in gestural communication strongly suggests that they possess a theory of mind, *i.e.,* that they attribute mental states such as *attention* and *inattention* to others (Tomasello 2008). The mindreading skeptic would then respond that there is no need to posit an “intervening variable” in the form of the chimpanzee attributing these mental states to humans and conspecifics. Their behavior can just as well be explained in terms of their previous experiences in like-situations where they formed “behavioral rules” based on the success and failure of certain gestures to achieve their goals. It is assumed that these two explanations, *i.e.,* theory of mind and behavioral rules, are “functionally equivalent” (Cheney and Seyfarth 2005: 138). In other words, they both achieve the same result, which, in Liebal’s study, is receiving food. As such, Andrews (2011) rightly claims that, “The logical problem is a descendent of Skinner’s worry about intervening variables: if we can predict future behavior based on environmental stimuli, there is no need to postulate a mental state in order to predict that behavior.”[[12]](#footnote-12)

The logical problem is widely considered the most formidable impasse in the various disciplines engaging not only questions of comparative *social* cognition, but also questions regarding the appropriateness of applying cognitive explanations to animal behavior more generally. Often the logical problem becomes aligned with arguments from parsimony and bears normative weight, *i.e.,* because there is no functional difference between theory of mind and behavioral rules, and because the latter are assumed to be far less cognitively taxing,[[13]](#footnote-13) it is scientifically irresponsible to conclude that non-human animals possess a theory of mind (*e.g.,* Shettleworth 2012). Meketa (2014:) usefully describes this additional normative claim in terms of *The Principle of Cognitive Simplicity* (PoCS): if the behavior of a non-human animal *can* be explained in terms of a ‘lower’ or ‘less complex’ cognitive capacity, then the behavior *should* be explained in terms of that capacity. Since the PoCS may well be said to apply to humans, scholars have legitimately questioned whether there is a double standard at play (*e.g.,* de Waal 1999; Sober 2005; Fitzpatrick 2008; Buckner 2013; Andrews and Huss 2014). Recently, some have argued that more data is necessary on the comparative behaviors of our *own* species in order to prevent potential bias (*e.g.,* Andrews 2015; Heyes 2015).[[14]](#footnote-14) Clearly, contemporary reliance upon the PoCS is deeply indebted to the influence of Morgan’s Canon (Morgan 1903) and, to a lesser extent, Wundt’s “law of parsimony” (Greenwood 2015: 223).

It has only been in recent years that some have seriously engaged the question as to whether the logical problem needs to be ‘solved’ at all (Buckner 2014; Andrews 2015; Halina 2015). In my estimation, the key reasons for this general lack of discussion are as follows: **(1)** the import of overcoming the logical problem is emphasized in most—if not all—primers and readers on animal minds, marginalizing deflationary accounts as to the very question as to whether it can and/or should be dismissed; **(2)** the logical problem is representative of long-standing concerns over anthropomorphism in the history of discussions over animal minds and is therefore deeply engrained in the discourse; **(3)** since the logical problem is commonly conceived as unique to comparative cognition research, rarely has the question arisen as to whether it may be indicative of more general issues in the natural sciences wherein progress *has* arguably been made; **(4)** the two alternatives offered by the logical problem are widely conceived as integral to the structure of mindreading debates, thereby raising the (perhaps legitimate) concern that evading this epistemic and methodological problem will make the very notion of animal mindreading empirically intractable. Finally, **(5)** the challenge of ‘solving’ the logical problem has generated—and continues to generate—a robust literature of its own, not only among philosophic and scientific commentators, but also amongst the myriad of laboratory researchers whose careers have been built upon the challenge of crafting ingenious experiments to overcome it.

There have, of course, been many benefits that have arisen from the dominance of this problem in terms of motivating more productive dialogue about the precise meaning(s) of core concepts as well as stimulating an impressive amount of creative experiments in the attempt to overcome it. It is certainly the case that the logical problem is largely responsible for the fact that we now know *a lot* more about social cognition in non-human primates than ever before. On the 30th anniversary of Premack and Woodruff’s (1978) landmark paper, “Does the chimpanzee have a theory of mind?,” Call and Tomasello (2008) published an oft-cited companion-piece summing up much of the relevant experimental research to that date. They note that though there is still strong disagreement over whether the logical problem will ever be truly solved, “In a broad construal of the phrase ‘theory of mind’ […] the answer to Premack and Woodruff’s pregnant question of 30 years ago is a definite yes.” By this they mean that, despite the fact that chimpanzees have consistently failed non-linguistic false belief tests (See 2017b), there is currently overwhelming evidence that they understand others’ goals, intentions, and perceptual states. Fletcher and Carruthers (2013) call this “stage 1 mindreading,” which they contrast with “stage 2 mindreading,” namely, the ability to attribute “reality incongruent mental states” such as false beliefs to others.

A natural question then arises: given the fact that we now know so much more about social cognition in chimpanzees, why is it that widespread skepticism about stage 1 mindreading not only still persists, but persists at such an extraordinary level of intensity? Against the wealth of positive support for these abilities, Penn and Povinelli (2013: 68), argue that there is a “lack of evidence for anything even remotely resembling a theory of mind among nonhuman animals.” Indeed, the Povinelli group has not budged in its contention that there is insufficient evidence for mindreading in chimpanzees (Povinelli and Vonk 2003, 2004; Vonk and Povinelli 2006; Penn and Povinelli 2007, 2009, 2013; Penn, Holyoak, and Povinelli 2008; Penn 2011). According to Halina (2015: 474 [emphasis added]), “These skeptics argue that the experimental approach currently used in comparative psychology to test for mindreading in nonhuman animals cannot provide evidence for this ability *even in principle*.”[[15]](#footnote-15) What is going on here?

**II.**

Prior to Povinelli and colleagues’ “early foray into skeptical primatology” (Allen 2002: 698) in the 1990s, it is a curious fact that the term *skepticism* appears sparingly in the history of debates over animal minds. Philosophers by and large held dogmatic attitudes on the subject, electing not to suspend judgmentin cases of animals behaving “as if”they possessed human-like cognitive capacities.[[16]](#footnote-16) Even after comparative cognition became regarded as a legitimate scientific discipline in its own right, the dominant attitudes have been **(1)** *categorical skepticism* from those who “think that researchers in animal cognition are making a category mistake by asking whether animals have certain properties” (Andrews and Huss 2014: 2), and—more commonly—**(2)** *selective skepticism* from those willing to sparingly attribute certain low-level cognitive capacities to animals, while largely eschewing mentalistic explanations of their social behavior (*e.g.,* Shettleworth 2010). In this sense, the mindreading debates of the past several decades should be understood as ushering in somewhat of an epistemic sea change in the field. It is worth discussing why only recently the suspension of judgment following from the logical problem has become a popular option, and—crucially—why this Pyrrhonian-styleattitude should *not* *necessarily* be understood as coextensive with increased *open-mindedness.* Heyes(2015: 313) rightly claims that while the animal minds literature from 1978 to 2000 showed “considerable promise,” more recent debates have become mired with theoretical and methodological problems, stalemating a promising research program that now “seems to be in trouble.”

In the wake of a series of oft-cited experiments strongly critical of Premack and Woodruff’s (1978) original paper—such as Povinelli and Eddy’s (1996) finding that captive apes beg for food indiscriminately from humans with and without buckets covering their heads—the “skeptic” label has since been used with increased frequency. Most often, it is employed loosely as a pejorative term to describe tough-minded stances that a given author takes to be uncharitable to the evidence (*e.g.,* Fletcher and Carruthers 2013). It is notable that those figures most often referred to as “mindreading skeptics” rarely use this term to describe their own attitudes and conclusions. Lurz (2015) is a notable exception. Indeed, the only time the word appears in Povinelli’s work is in the introduction to *Folk Physics for Apes* (2000), where he addresses “the dogged skeptic” who questions the legitimacy of his controversial claim that chimpanzees understand the physical properties of objects in a way that is fundamentally different from humans.

Povinelli’s use of the word is odd and worth dwelling on for a moment because it runs completely contrary to what I have previously described as the dominant tradition in the animal minds literature. Remarkably, Povinelli (2000: 1-2) ascribes “the skeptical voice” to the intellectual progeny of Hume, Darwin, and Romanes who assume that “when it comes to trying to compare the mental lives of humans and other species, analogous behaviors imply analogous minds.” But *who* are these credulous individuals?[[17]](#footnote-17) Povinelli fails to name even one, which is unsurprising given the fact that analogical reasoning is anathema to the majority of 20th and 21st century figures in the discipline (Andrews 2015). Nonetheless, according to Povinelli, “[e]ven today, the invisible tentacles of this assumption run deep and tangled in our efforts to understand the minds of other species.” To be clear, my purpose is not to criticize Povinelli’s understanding of the history of his own field, but to raise questions to better understand the epistemic attitudes of those who take very seriously this so-called logical problem of animal minds.

As I understand Povinelli’s perspective on his place in this literature, he occupies the role of the responsible, sober-minded scientist whose controversial views inevitably ignite the “skepticism” of those who are convinced by the myriad of studies suggestive of basic mindreading abilities in chimpanzees (see Call and Tomasello [2008], Andrews [2015], and See [2017a] for reviews). According to Penn and Povinelli’s (2013: 75 [emphasis in original]) most recent paper, their group’s long-standing “fundamental philosophical and methodological challenge […] has never been acknowledged or refuted by those advocating a mentalistic explanation of nonhuman cognition: to wit, comparative researchers have consistently failed to specify what *unique causal work* is being performed by nonhuman subjects’ ToM system that could not have been performed by a sophisticated cognitive system representing and reasoning about observable behaviors alone.” As mentioned above, there is a legitimate concern that the Povinelli group does not comment on the applicability of their “fundamental” challenge to the human subject.

What’s more, according to Fletcher and Carruthers (2013: 87), the challenge posed by Povinelli and colleagues is “too strong a demand to place on any theory”—a critique that I likewise raise below. This challenge *does* intuitively seem more indicative of what may be called *philosophical* *skepticism* than ordinary scientific doubt, as does the sort of language that Povinelli often employs. For instance, Povinelli (2000: 2) states that his work “takes a fresh look at the mental lives of other species, a look which may one day allow us to see them without the fog of our own way of thinking about the world shrouding their true natures.” Povinelli’s contention is therefore that many in the field are tender-minded towards the study of animal minds, and that more tough-minded, conservative attitudes are necessary to mitigate doubts.[[18]](#footnote-18)

Consider, for example, Heyes’ (1994) critique of Galef’s (1996) field reports on the potato-washing behaviors of a population of Japanese macaques.[[19]](#footnote-19) This particular group of monkeys uniquely washes their potatoes before they eat them—a phenomenon that Galef explains in terms of a species-typical capacity for observational learning and, more generally, *culture*. Heyes believes that a more parsimonious explanation exists: each monkey “went through a similar serendipitous process, perhaps accelerated by the tendency of monkeys to congregate in the water” (Allen and Bekoff 1997: 61). In the absence of controlled conditions, Heyes (1994) claims that *chance* and *asocial learning* offer equally plausible explanations of Galef’s findings. However cynical one might be of this conjecture, and while it hardly needs to be stated that good science necessarily involves varying degrees of doubt, it is also clear that there exists a legitimate difference between varieties of “philosophical skepticism” in epistemology and the types of “ordinary incredulity” commonplace in the natural sciences (Klein 2015).

One reasonable response to Heyes’ doubts about capacities for observational learning in Japanese macaques is that, presumably, her “complementary” explanations are independently testable. To press Heyes *specifically* on this criticism is, however, somewhat beside the point. As Allen and Bekoff (1997: 61) note, “many critics of cognitive accounts mount their criticism without making the relevant empirical case, relying on the fact that for any behavior it is always possible to imagine some (possibly complex) stimulus to which the behavior is bound but that has escaped the notice of the researchers whose cognitive inferences are being questioned.” Turning to Heyes (1994) directly, they claim that “Heyes seems committed to the view that there must be such a stimulus even if we do not know what it is.” If Allen and Bekoff are correct in their assessment—and I suspect they are—it is worth noting **(1)** that this *ad hoc* attitude toward low-level explanations of animal cognition may be “viewed as a questionable methodological bias” (Allen and Bekoff 1997: 61; see also Sober [2005] and de Waal [1999]), and **(2)** that the tough-mindedness of the Povinelli group is by no accounts a recent development in the discipline. Many early comparative psychologists made the same assumption, *e.g.,* Konorski and Miller (1937a), (1937b), and Guthrie (1952).[[20]](#footnote-20) Moving even further back in time, in his response to Montaigne’s claims about strong similarities between human and animal cognition, “Chanet maintains [that] all things that have been attributed to reason [in animals] can equally well be explained by the operation of instinct” (Boas 1966: 75).

Heyes’ dismissal of Galef’s—to my mind—perfectly reasonable supposition that Japanese macaques possess socio-cognitive capacities for observational learning thereby raises important issues that have hitherto been ignored in the philosophy of animal minds: **(1)** To what extent can conclusions like Heyes’ be fairly described as skeptical?**(2)** When does skepticism impedethe progress of this discipline, and when might it contribute to it? Finally, **(3)** to what extent can the *logical problem* be fairly described as a skeptical stance? It is often described loosely in these terms, but no one has analyzed the logical problem from the perspective of varieties of skepticism in the epistemology literature.

As self-described skeptics, Lurz *et al.* (2014) argue that the best grounds for “optimism” about the future of the mindreading research program rests on yet-to-be conducted experiments based on their Appearance-Reality Mindreading (ARM) theory which, they suggest, are likely to solve the logical problem. I conclude this section by challenging their general claim by offering some preliminary remarks on what optimism in mindreading debates should look like. In the following section, I use this discussion as a springboard to critique the ARM theory from a variety of angles.[[21]](#footnote-21) With this groundwork in place, the remainder of this chapter promotes avenues for a more naturalistically minded optimism about the future of mindreading debates, as well as confronts the above questions of whether **(1)** the logical problem is indicative of “philosophical skepticism”[[22]](#footnote-22) and **(2)** whether this problem needs to be “solved” at all in order to advance the mindreading research program in comparative psychology.

**III.**

There is a substantial difference between the attitudes of the Povinelli group and Lurz group (Lurz 2011; Lurz and Krachun 2011; Lurz *et al.* 2014) regarding the logical problem. While Povinelli himself gave up on task of solving the logical problem some time ago (see Penn and Povinelli [2013] for an overview of their evolving positions), Lurz and colleagues remain “optimistic” that it can still be solved, arguing that “What is needed if we are to resolve the issue of animal mindreading…”

…are more sensitive tests that solve Povinelli’s problem. Until such tests are run, the epistemically responsible attitude to take is that of withholding judgment on whether or not animals are mindreaders. What is more, there is reason to be optimistic on this matter. Experimental protocols have been proposed which solve Povinelli’s problem, and there is no *a priori* reason to think that animals will not be able to pass them […] The only way to find out, of course, is to run the tests (2014: 432).

To the contrary, I suggest that there *are* a priori reasons to doubt the success of the experiments that Lurz and colleagues have proposed: they too are open to complementary behavior-reading (CBR) hypotheses (Andrews 2015: 148). What’s more, the impetus for the ARM theory is at once its most promising and more restrictive feature. Lurz and Krachun (2011) wisely suggest that the key to resolving the mindreading debate may rest, first, in considering how theory of mind evolved in our own species. I am in agreement with the authors, as well as Andrews (2012a, 2012b, 2015), that evolutionary accounts of mindreading are lacking in the current literature. However, Lurz’s optimism puts all its eggs in the basket of the Machiavellian Intelligence Hypothesis (Whiten and Byrne 1988), while other evolutionary accounts exist (Andrews 2005). Despite the fact that it is has long been thought that mindreading evolved in tandem with skills for engaging in deceptive behavior, Andrews (2012: 109) has convincingly argued that this may be a red herring, since one can manipulate the behavior of others *without* the use of a theory of mind. As such, I argue that while the ARM theory itself is a step in the right direction,[[23]](#footnote-23) its focus is far too narrow and, as such, it is best understood as part of a larger, pluralistic research program in comparative social cognition.

Before explaining and critically engaging the experiments that Lurz and colleagues’ have proposed, I will make some preliminary remarks on why constructive optimism about the future of the mindreading research program should not be contingent upon their success. What’s more, neither should our confidence in the science of animal cognition be contingent upon ‘solving’ this logical problem at all. Andrews is quite correct that, “The challenge to come up with an experiment that avoids the logical problem may be a red herring” because “science doesn’t usually proceed by relying upon a single groundbreaking experiment to prove a theory true” (2015: 149). Despite the fact that this does sometimes occur, focusing all of our collective optimism here is out of step with the various ways philosophers of science have debated the means by which science progresses, *e.g.,* Lakatos (1970), Kuhn (1962), Popper (1959), and Newton-Smith (1981). Scientific progress is not usually viewed as a *deductive practice*, *i.e.,* “the evidence for a hypothesis cannot guaranteethe truth of the hypotheses” (Andrews 2015: 149). On the contrary, evidence offers varying degrees of *support*. In a similar vein, Fletcher and Carruthers (2013: 87-8) correctly emphasize the fact that, “No theory, in any domain of science, can ever show that the data *cannot* be explained in any other way […] theory choice is never a matter of proof, but of judgment.”

As in any field of study, we need not be concerned that “complementary” hypotheses exist to explain a given phenomenon; we should expect it. What we *should* be concerned with is “which hypothesis best accounts for the overall body of data” (Andrews 2015: 150) such that we might provide reasonable *arguments from the best explanation*. As such, rather than focus on crucial experiments, the mindreading research program should pull congruous evidence from a wide variety of sources and develop what Whewell (1840) called a “concilience of inductions.” As I argue below, there is significant room for optimism here, as the 20th and 21st century literature on animal behavior is comprised of a myriad of distinct disciplines and research paradigms that—in their own ways—weigh in on the mindreading debate. Despite Povinelli’s (2000: 15) claim that “questions about the internal architecture of chimpanzee psychology are virtually impossible to address in the rain forest,” and Heyes’ (1987: 124) suggestion that the time has come for researchers in comparative cognition to “hang up their field glasses,” the comprehensive research program that I propose takes very seriously recent, highly promising developments in field research.[[24]](#footnote-24)

In direct contrast to this more naturalistically oriented approach to the mindreading research program, consider Povinelli and Vonk’s claim that “the problem we face is *not* primarily an empirical one. Instead, the most pressing problem is to come to grips with the fact that the experimental results from the kinds of techniques that are currently in vogue cannot add a single bit of evidence in unique support of the conclusion that chimpanzees reason about mental states—*any* mental states” (2004: 11; emphasis in the original). As such, regardless of whether or not it turns out that chimpanzees do indeed possess a theory of mind, from the perspective of the logical problem, *twenty* extant studies suggestive of mindreading abilities—so long as they all admit of CBR hypotheses—are ultimately as valueless as a single such study. Of course, Lurz and colleagues would likely respond that with each passing study we get *methodologically* closer to zeroing in on that ideal experiment protocol. But this is beside the point. In terms of considering the collective *epistemic* value of all these previous studies such as they might function in an argument from the best explanation, for those who take the logical problem seriously, their skepticism does not budge an inch.

In a similar vein, Fletcher and Carruthers (2013: 84) correctly note that, “What Povinelli and colleagues have ranged against them […] is a regular scientific research program of good standing that generates determinate predictions capable of falsification when combined with auxiliary assumptions (e.g., concerning the animals’ other forms of knowledge). Moreover, it is a progressing research program, issuing in a stream of positive results and increasingly precise theories.” One of the key features of Fletcher and Carruthers’ argument has also been highlighted by Halina (2015: 27), who notes that, “the CBR hypothesis does not have the features of an empirical hypothesis,” “skeptics do little to motivate the CBR hypothesis empirically,” and, “more importantly, […] the CBR hypothesis lacks the content necessary to make new predictions.” The utility of the CBR hypothesis—however one wishes to cash it out—has recently been subject to increased opposition along precisely these grounds (see Heyes [2015] for a review), to which I add that, *if* it is not falsifiable nor capable of making novel predictions,[[25]](#footnote-25) then the role played by the CBR hypothesis in mindreading debates is purely *ad hoc,* and—in direct contrast to Lurz’ *optimism*—may be indicative of what Popper understood to be “pseudo-scientific dogma” in a *degenerating* research program (Thornton 2016). As Lakatos (1974: 34) likewise argued, a research program “is *progressive* if it is both theoretically and empirically progressive, and *degenerating* if it is not,” by which he was similarly referring to potential for falsification and making novel predictions.[[26]](#footnote-26)

In the next section, I explore these criticisms in depth and offer a robust, naturalistic account of what optimism in the mindreading literature shouldlook like. What’s more, the optimistic attitude that I promote is in line with recent deflationary attitudes toward the logical problem. For these figures, there are multiple reasons to assume that the logical is *not* unique to animal minds, but is rather indicative of general issues in the natural sciences, such as Hempel’s theoretician’s dilemma (Halina 2015) and the problem of distal content (Buckner 2014).

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1. *E.g.,* Heyes 1998; Povinelli and Vonk 2003; Penn and Povinelli 2007, 2013; Penn *et al.,* 2008; Lurz 2011. [↑](#footnote-ref-1)
2. *E.g.,* Tomasello and Rakoczy 2003; Tomasello *et al.* 2005; Moll and Tomasello 2007; Tomasello and Carpenter 2007; Schmidt and Rakoczy 2016. [↑](#footnote-ref-2)
3. *E.g.,* Bethe, Beer, and von Uexküll (1899). [↑](#footnote-ref-3)
4. *i.e.,* although chimpanzees behave *as if* they possess X, they do not possess “true” or “real” X, where X is exclusively defined in terms of the highest-level human ability, *e.g.,* apes do not engage in “true cooperation” because they (presumably) lack the capacity for recursive mindreading. [↑](#footnote-ref-4)
5. Gomez (2007: 77) is an exception, criticizing those “tempted to enter into a discussion of whether these differences make the apes’ system a less ‘genuine’ joint attention.” Gomez claims, rightly, that one can state that “the scope of JA in non-humans is more limited than in humans” while arguing that “apes still show genuine JA, in the sense that both some of the functions and part of the machinery may be common or closely related.” He then raises this pregnant analogy: “Are chimpanzees who occasionally walk bipedally engaging in *genuine* bipedal walking? Their bipedal excursions are occasional, relatively inefficient, and subserved by underlying mechanical dynamics that are not exactly like that of humans. However, it would seem extremely arbitrary to say that they are *not really* ‘walking on their feet’. Apes can walk bipedally, even if humans were the ones who developed further adaptations that made of bipedalism a fundamental way of life.” [↑](#footnote-ref-5)
6. This division of chapters is highly tentative. I am not suggesting a significant break in the dominant tradition in the late 19th century. Nor am I suggesting that this handful of figures constitute the whole story of how that tradition influenced the origins of comparative psychology. [↑](#footnote-ref-6)
7. *E.g.,* Allen and Bekoff (1996), Boesch (2007, 2008), and Leavens, Bard, and Hopkins (2010). [↑](#footnote-ref-7)
8. “Gassendi’s point is that subjective experience can take many forms, and that the seeming lack of capacities for language, logic, mathematics, and the like in animals is no bar in principle to their having their *own* forms of communication and ways of reckoning with the contingencies of life” (Steiner 2005: 93) [↑](#footnote-ref-8)
9. For example, it is well documented that subordinate chimpanzees engage in a wide variety of deceptive tactics in order to mate when dominants are nearby (see Roberts and Roberts [2015] for a review). This literature makes it clear that subordinate males (and willing females, *e.g.,* Matsumoto-Oda [1999]), are far more likely to engage in mating behaviors when gaze of dominant chimps is obstructed. For mindreading skeptics, the question is whether apes perform these deceptive behaviors on the basis of their prior successes and failures based the *bodily-orientation* of the dominants (what Lurz [2011] calls *direct line of gaze*), or, whether they are attributing perceptual mental states such as *seeing* to the dominants. [↑](#footnote-ref-9)
10. *E.g.,* Povinelli and Vonk (2003), Perner’s (2008), and Gergely and Csibra (2003). [↑](#footnote-ref-10)
11. See Lurz *et al.* (2014) and Spaulding (2011) for reviews. [↑](#footnote-ref-11)
12. Halina (2015) likewise describes the logical problem as follows: “The problem is […] rejecting the hypothesis that nonhuman animals solve mindreading tasks on the basis of reasoning about observable regularities alone. It is not until this alternative hypothesis is rejected that comparative psychologists are justified in claiming that they have evidence for mindreading. This criticism has been dubbed the ‘logical problem’ because it points to a flaw in the logic of the current experimental approach.” [↑](#footnote-ref-12)
13. For critiques of this common assumption, see Heyes (2012), Dickinson (2012), Fletcher and Carruthers (2013), and Meketa (2014). [↑](#footnote-ref-13)
14. Heyes (2015: 313), for instance, wisely suggests “ways of tackling these theoretical and methodological problems that draw on recent studies of humans, and the resources of human psychology more generally.” [↑](#footnote-ref-14)
15. For elaboration on this point, see Penn and Povinelli’s (2013) “challenge” on pgs. 19-20 below. [↑](#footnote-ref-15)
16. For Sextus Empiricus and the myriad of philosophers his work directly influenced, *e.g.,* Montaigne, Mersenne, and Gassendi (Popkin 2003), since “our judgments about [animals] as objects of perception are problematic,” “we must end in suspension of judgment about the way animals really are” (Annas and Barnes 1985: 46).Sextus’ student Philo, for instance, considered Chrysippus’ claim that his dog showed evidence of grasping basic logic, arguing, “The same could be said of those who gather clams or any other thing which moves. That they seem to follow a definite pattern is only logical speculation,” whereas the truth of the matter is, at present, “inapprehensible” (*qtd.* Annas and Barnes 1985: 48). [↑](#footnote-ref-16)
17. Perhaps Povinelli means figures like Griffin (1981), Premack and Premack (1983), Roitblat (1987). [↑](#footnote-ref-17)
18. As discussed below, it is possible that the Povinelli group’s implicit evidentialbias is, ironically, one way of “fogging” such debates. [↑](#footnote-ref-18)
19. Galef must have formally published these findings after previously discussing them elsewhere. [↑](#footnote-ref-19)
20. See Hearst (1975) for a review of these and other 20th century figures who make the same assumption. [↑](#footnote-ref-20)
21. Outside the scope of this document. [↑](#footnote-ref-21)
22. Andrews (2012), for instance, describes the logical problem as simply “the problem of other minds in sheep’s clothing.” [↑](#footnote-ref-22)
23. I lack space to aptly describe the ARM theory here. [↑](#footnote-ref-23)
24. Elsewhere (See 2014), I offer an extensive survey of laboratory and field research on intentional vocal and gestural communication in chimpanzees, arguing that positive data for theory of mind in laboratory studies is consistent with the conclusions of contemporary field researchers. [↑](#footnote-ref-24)
25. Lurz *et al.* (2014) deny this charge. I consider their rationale below. [↑](#footnote-ref-25)
26. See Musgrave and Pigden (2016) for elaboration on Lakatos’ views. [↑](#footnote-ref-26)