CHAPTER

2 Why Behaviorism Isn't Satanism

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Abstract

The history of comparative evolutionary psychology can be characterized, broadly speaking, as a series of reactions to Cartesian versus pragmatist views of the mind and behavior. Here, a brief history of these theoretical shifts is presented to illuminate how and why contemporary comparative evolutionary psychology takes the form that it does. This brings to the fore the strongly cognitivist research emphasis of current evolutionary comparative research, and the manner in which alternative accounts based on learning theory and other behaviorist principles generally receive short shrift. I attempt to show why many of these criticisms of alternative accounts are unjustified, that cognitivism does not constitute the radical lurch away from behaviorism that many imagine, and that an alternative "embodied and embedded" view of cognition—itself developing in reaction to the extremes of cognitivism—reaches back to a number of behaviorist philosophical principles, including the rejection of a separation between brain and body, and between the organism and environment.

Key Words: animal, cognition, behavior, cognitivism, behaviorism, evolution, learning, psychology

Introduction

As Newton's third law of motion specifies, every action has an equal and opposite reaction. As such, it is a law that can be used (metaphorically) to understand how science itself goes about its business. Comparative evolutionary psychology is no different from any other scientific discipline in this respect and its history is characterized by a series of actions and reactions that have turned on the question of whether purely "mental" phenomena are amenable to scientific investigation. This question in turn hinges on the way in which the mind is conceived and, as such, is part of a wider conceptual and philosophical debate.

It would be fair to say that contemporary evolutionary and comparative psychology is committed to cognitivism as its overarching philosophy (see e.g. Byrne & Bates, 2006; Penn & Povinelli[1], 2007; Pinker, 2003; Shettleworth, 2010). In this sense,[1] it reflects the general trend across the broader discipline as a whole; as Costall and Still (1991) argue, psychology has become *cognitive* psychology. The term "cognitivism" captures a particular approach and set of commitments to the study of psychology.[2] Specifically, the cognitivist views "mental phenomena" as internal, brain-based entities and processes that reduce or elaborate sensory input and then store it so that it can be recovered and transformed into motor output (e.g., Neisser, 1967).[3] The dominant metaphors are that of the computer and "information processing" (Broadbent, 1958; Pinker, 2003); that is, cognition is considered to be a process of computation by which representations (mental states) are manipulated according to a set of rules.

As these representations and cognitive processes are the *causes* of behavior, they are, therefore, not identical *with* behavior. Although internal rules and representations are not available for direct inspection, they can, however, be inferred, via observation and experiment, from the behavior they cause.

The twin ideas that cognitive processes are limited to the brain alone and that experience is mediated by representations of the external world place cognitivism squarely within a Cartesian (and hence Platonic) framework in which two kinds of dualisms are prominent. First, there is the separation of the "cognitive" brain from the "lifeless" body, which is clearly a dualist position. Second, there is the acceptance of the Cartesian tenet that mental states are internal and accessible only to their possessor, such that there is a clear differentiation between an inner subjective world and an outer objective world, and thus a separation between the organism and its environment.

Contemporary comparative psychology is also committed to a Darwinian framework in which the form and function of a species' representational capacities are taken to be the products of natural selection. As with other morphological and physiological traits, we should, therefore, expect to see considerable continuity across species with respect to psychological capacities, as a result of evolutionary descent. Equally, those animals that inhabit similar niches can be expected to show similar psychological capacities as a result of evolutionary convergence.

In an interesting take on evolutionary theory's—or perhaps more specifically, Darwin's—contribution to psychology, Costall (2004) argues that Darwin initially helped to rid psychology of the heavily mechanistic view of science that had prevailed up to that point. Most pertinently from a comparative perspective, Costall (2004) suggests that Darwin rejected the Cartesian view that animals, lacking souls, were simply "automata": biological machines incapable of thought, reasoning and judgment. Moreover, he also rejected the separation between organism and environment by emphasizing their mutual relationship. In his work on earthworms—which was explicitly psychological—Darwin (1881) emphasized how worms altered the nature of their environment through their own actions, and were not simply impinged on by it. Put more generally, Darwin saw that all organisms were firmly embedded in their environments and did not exist apart from them. Darwin, then, did not hold with the idea that organisms adapt to a fixed environment (as most modern treatments would have it) but instead assumed that organism and environment mutually adapted to each other (as recognized by modern-day "niche construction theory"(Odling-Smee, Laland, & Feldman, 2003). Linked to this view, and following directly from it, Costall (2004) argues that Darwin also saw mind simply as another part of a mutualistic nature to be explained scientifically and not as something that stood apart from the physical, mechanical world—again rejecting the arguments of Descartes (Costall, 2004). This kind of mutualistic view was later espoused by those of the "functionalist" school, like Dewey and James, who also took up these evolutionary arguments.

Costall (2004) goes on to suggest that this "mutuality" was lost with the advent of Watson's behaviorism, which strongly criticized both introspectionist (structuralist) and functionalist schools of thought. In so doing, it reintroduced Cartesian thinking into psychology by emphasizing the inaccessibility of the mind to scientific study, thus generating a contrast between the inner mind and external behavior, and thereby reintroducing a separation of organism from environment (see later). Although this may well be true for human psychology, it holds less well for comparative psychology. It is certainly true that Darwin rejected the Cartesian dualisms of animal vs. human and organism vs. environment, but there is also evidence to suggest that, nevertheless, he accepted the Platonic-Cartesian concept of mind. In his M-Notebook he acknowledges implicitly that mental phenomena are private and internal, and that all experience of the world, and action in it, are mediated by representations: "Plato . . . says in Phaedo that our 'necessary' ideas arise from the pre-existence of the soul, and are not derivable from experience—read monkeys for preexistence."[4]

It is also abundantly clear that the Cartesian turn in comparative psychology did not arise with behaviorism but had already been firmly established by George Romanes, Darwin's friend and protégé, who first identified comparative psychology as a legitimate evolutionary enterprise. He argued that it was possible, using an analogy to one's own mind, to draw objective inferences about other individuals' inner mental states—whether human or some other species—from the subjective inferences we make about the relation between our own mental states and how these lead us to behave (Romanes, 1882); a position held by other comparative psychologists with an evolutionary bent, such as C. L. Morgan (1894), who also advocated this "double induction" approach (more of whom later). Although these

points are no doubt open to debate, one can clearly make a case for Cartesian thinking as a feature of early evolutionary comparative psychology. In this reading, evolutionary comparative psychology has been committed to a representational view of the mind from its inception, with the result that the notion of continuity is tied to a particular concept of mind and the nature of psychological processes. Behaviorism did not reintroduce Cartesian thinking, but instead constituted a brief non-Cartesian hiatus.

More precisely, it was radical behaviorism that interrupted the Cartesian flow in comparative psychology. As Malone and Cruchon (2001) put it, there are basically two forms of late twentieth-century psychology: radical behaviorism and "the rest of psychology." Unlike "the rest of psychology," radical behaviorism rejects the idea that intervening variables—mental representations—are useful or necessary in accounting for behavior (Baum, 1994; Costall, 2004; Malone, 2009; Utttal, 2000). In this way, it also rejects a Cartesian concept of mind (Baum, 1994; Malone, 2009). This definition of behaviorism can be parsed in a variety of ways, some of which are commensurate with the arguments actually made by radical behaviorists themselves, and some which are not. In today's cognitivist—often mentalistic—evolutionary comparative psychology, it tends to be construed as a position that flatly denies the existence of mental states, and, as such, is often used as a pejorative (e.g., Call, 2006; Call & Tomasello, 2006; de Waal, 2009; Tomasello, Call, & Hare, 2003), to the extent that one could be forgiven for assuming that behaviorism was a practice akin to Satanism. The words of Bergmann (1962) still ring true today, when psychologists invoke the name of behaviorism "to scare little children in the existentialist dark" (p. 674).

There is, however, a case to be made that such negative responses to the radical behaviorist program derive from a fundamental misunderstanding of its stance on the mental (or more accurately, private versus public experiences), which has its roots in pragmatism, rather than the Platonic-Cartesian grounding that characterizes the cognitivist project (see e.g., Baum, 1994; Malone, 2009). In what follows, I first review a number of behaviorist philosophies before arguing that, in its current incarnation, evolutionary comparative psychology has returned to the firmly Cartesian stance of its founders. Finally, I suggest that new developments in psychology, stemming from work in robotics and artificial life, may themselves signal a response to extreme Cartesian cognitivism and herald a return to a philosophical position and empirical approach that bears similarity to the pragmatism of (some) behaviorists.

The Trouble with Behaviorism?

In the "mythical" construction (Costall, 2006) of the emergence of modern psychology, behaviorism was founded by John Watson, in reaction to the structuralist school of psychological thought, which took the quality of conscious experience as its subject and deployed self-examination of mental events (introspection) as its method (see e.g., Costall, 2004; Malone, 2009). In criticizing this approach, Watson's aim was to turn psychology into a natural science, arguing that the impossibility of an objective examination of an internal, hidden mind necessarily required a focus on behavior. This redirection of attention carried with it the requirement that mind and consciousness be removed from psychological consideration (Watson, 1913; 1919).

There is, however, something of a misconception that Watson, in attempting to eradicate the terms *mind* and *consciousness* from psychology, was also denying the existence of human experience, so it is helpful to be precise here. Watson's view was that *mind*, as defined by the structuralist school, was not observable and that the qualities of consciousness were not measurable. Because any empirical discipline requires measurement, mind and consciousness could not be subjected to analysis. As Watson himself stated, "if you will grant the behaviorist the right to use consciousness in the same way that other natural scientists employ it—that is, *without making consciousness a special object of observation*—you have granted all that my thesis requires." (Watson, 1913, p. 11, emphasis added). Clearly, there is nothing in this statement that denies the reality of everyday human experience.

Indeed, Malone (2009) argues that, far from denying "mind" in any way at all, Watson was attempting to reconceptualize it along Aristotelian lines. Aristotle argued that there were no unique aspects to the soul that were independent of the body, and Malone (2009) suggests that Watson similarly considered thought simply as another form of action (a stance that echoed and followed on from that of Thorndike).[5] From this, Watson could then argue that there was no such thing as "mind" that exists independently of our actions in the world. This should make it clear why Watson argued against the mediating factors posited by the introspectionists—factors like "consciousness," and "mental images," For Watson, these were not real

entities, but handy placeholders that plugged the gaps in our knowledge, and served only to generate our misleading folk-psychological impression that we possess minds that somehow transcend the activity of the living organism.

In similar vein, Uttal (2000) notes that Watson was motivated more broadly by what he felt were the religious connotations of the structuralist concept of "mind." In his later writings, Watson explicitly refers to the "religious background of current introspective psychology" (Watson, 1924, p. 3), before going on to argue against the concept of a "soul", suggesting that the concept of consciousness used by the structuralists was nothing but a new incarnation of this kind of dualistic thinking (Uttal, 2000). Put simply, then, both Uttal's (2000) and Malone's (2009) argument is that Watson was not out to deny our subjective, conscious experience of the world but only the dominant Cartesian concept of mind (with its particular religious overtones) that held sway in psychology at that time.[6]

Costall (2004) and Leary (2004), however, take a wholly different view of Watson. As noted earlier, Costall (2004) argues that, by accepting the impossibility of studying mind or consciousness due to their unobservability, Watson was implicitly endorsing a Cartesian view of mind and so rejecting Darwin's mutualist, non-Cartesian view (although Darwin's stance on this, as we've seen, is somewhat moot). Costall (2004) argues that Watson did not so much reconceptualize the mind as merely relabel it in physiological terms drawn largely from Pavlov's work on the conditioned reflex, with the result that Watsonian behaviorism became an "eclectic mix" of "Russian reflexology, school textbook physiology and hand-waving about practical implications" (Costall, 2004, p. 188). Most notably, the terms *stimulus* and *response*, which previously had been purely physiological terms were imported wholesale into psychology, without it ever being made clear how the latter differed from the former (Leary, 2004). In so doing, Costall (2004) argues that Watson once again made the body into a Cartesian "passive" mechanism (Costall, 2004).[7]

Leary (2004) argues that the heavy emphasis on the conditioned reflex came about because of the pressure under which Watson was placed, following the publication of his 1913 "manifesto," to come up with the goods on how to implement his behaviorist program. The conditioned reflex thus became a "fix-all category or mold into which he poured any and every conceivable psychological function" (p. 21). Leary (2004) notes, for example, how Watson also took the ideas of other psychologists, such as the psychoanalytic theories of Freud, and simply translated these into talk about conditioned reflexes. Consequently, Leary (2004) argues that Watson provided no truly novel insights into human behavior as such, but "simply new ways of expressing old insights, with the unconscious . . . being discussed in terms of '"unverbalized responses' and the results of childhood trauma being described as 'conditioned emotional responses" (p. 21).

These differing views of Watsonian behaviorism are worth highlighting because they reveal the difficulty of reaching a definitive conclusion about the work of even a single researcher. One cannot treat "behaviorism" as a single, monolithic entity that can be held in contrast to a cognitivist approach.

Neobehaviourism After Watson

This becomes even clearer when we consider the work of the "neobehaviorists" that followed Watson. Their work can be viewed either as a continuation of Watson's "dualistic" perspective or as the rejection of Watson's Aristotelian precepts, depending on whether one favors a Leary-Costall reading or an Uttal-Malone approach. For example, although Edward Tolman (1926) embraced the idea that a mental phenomenon, like an animal's goal, could be "pointed to" and so was "out there in the behavior; of its descriptive warp and woof" (p. 355), he was also, as Uttal (2000) describes him, a "cryptomentalist." Indeed, Tolman's definition of his own personal brand of behaviorism, given in the glossary of his 1932 book, is hardly cryptic, and it sounds more like full-blown cognitivism: "behaviourism: any type of psychology which in contrast to mentalism, holds that 'mental events' in animals and human beings can, for the purposes of science, be characterized most successfully in terms wholly of the ways in which they function to produce actual or probable behaviour." (Tolman, 1932, p. 439). Such a definition is really only behaviorist in the sense that it rejects "mentalism" of the introspectionist variety, in which the quality of consciousness itself is the focus of study.

Tolman (1932) further distinguished his approach from Watson's by calling it "purposive behaviourism" (and, in this sense, one could argue that is Tolman who is the true Aristotelian), which he described as: "the specific brand of behaviourism [which] asserts that these 'mental events' are to be described further as a set of intermediating variables, immanent [sic] determinants and behaviour-adjustments which intermediate in the behaviour equation between

environmental stimuli . . . and the finally resulting behaviour." (Tolman, 1932, p. 439); Tolman, therefore, moved away from the conditioned reflex as the basis for psychology, and has rather more in common with Skinner and the operant psychology of radical behaviorism. Tolman was also—and apparently by his own admission—a "cryptophenomenologist" (Tolman, 1959, p. 94), in that he explicitly used his own conscious experience to inform his studies. In this sense, he was also an advocate of the heuristic benefits of a specifically anthropomorphic approach to animal psychology, which has clear links to Romanes' inductive approach to animal mentality:

> . . . there seems to me every advantage in beginning by conceiving the situation loosely and anthropomorphically . . . in my future work [I] intend to go ahead imagining how 'if I were a rat' I would behave as a result of such and such a demand combined with such and such an appetite and such and such a degree of differentiation and so on. (*Tolman*, 1938, p. 24)

Clarke Hull, in contrast to Tolman, was committed to a more mechanical view of mind; a consequence of his background in engineering. He used what he called a "robot approach" in his attempts to devise the "intelligent mechanisms" by which behavior was produced and, like Watson, he took the conditioned reflex to be the basis for all learning (Hull, 1962). Like Tolman, however, he also assumed the existence of intervening variables (although these were of a different nature: whereas Tolman was focused on "mental maps," Hull referred to "drive states." This in turn led to debates with respect to whether animals could learn by making stimulus-stimulus links, as Tolman suggested, or whether learning was a matter of linking stimuli to responses, as argued by Hull). More specifically, Hull's mechanical theory argued that knowledge was built up via serially conditioned response chains (Hull, 1930). In this view, each stimulus initially evokes only the corresponding contingent response, but as the response itself produces its own proprioreceptive stimuli, this then becomes linked to the next external stimulus. Over time, the proprioceptive stimuli alone are able to evoke the next response. Eventually, the entire sequence runs independently of external stimuli following the triggering of the initial response, and consequently ". . . the organism will carry about continuously a kind of replica of this world segment" (Hull, 1930, p. 514). In their own individual ways, then, both Tolman and Hull, although adhering to the methodological commitments of the behaviorist paradigm, were perfectly willing to accept the existence of "mental events" and the production of "replicas" inside the head, respectively. In this respect, it can be argued that they restored many of the elements of the Cartesian concept of mind that Watson had summarily rejected as suitable for study.

Why Radical Behaviourism is Truly Radical

The "cognitivist" leanings of behaviorists like Tolman and Hull help clarify the distinction between these particular brands of behaviorism and that of Skinner's "radical behaviorism," highlighting that, even within a particular school of thought, action and reaction characterize the trajectory through time. Radical behaviorism was Skinner's response to the approach he termed "methodological behaviorism" (because of its central emphasis on methods that could measure behavior objectively). Methodological behaviorism maps onto the logical positivist view that "mind" should be excluded from study in favor of behavior because of the unobservability of the former. Skinner expressly denied the mind-body dualism inherent in this kind of methodological behaviorism—after all, to exclude a private, internal mind from scientific study is to implicitly accept that such a thing exists (Skinner, 1945). By contrast, radical behaviorism simply doesn't distinguish between an inner, subjective world and an outer, objective one: "What is felt or introspectively observed is not some nonphysical world of consciousness, mind, or mental life but the observer's own body" (Skinner, 1974, p. 18). Radical behaviorism has its philosophical roots in pragmatism, and, as such, presented a clear repudiation of the Cartesian concept of mind, and this, perhaps, is why it is so badly misunderstood (Baum, 1994). For example, Skinner's rejection of "mental fictions" should not be taken as a denial of the reality of private thoughts—Skinner considered these both natural and real—but as a rejection of the separation of "mental" things and events from behavioral events. For Skinner, the difference between "private" events and "public" ones was simply the number of people who could talk about them (Skinner, 1945 gives a detailed analysis of "private events"). As Malone and Cruchon (2001) put it, "personal experience is not necessarily "private" experience" (p. 33).

Contrary to popular views, then, "conscious" phenomena are not denied by radical behaviorism because, as behavioral events about which one can speak, they are amenable to study (Baum, 1994). If Watson's behaviorism was the "science of the other,"

Skinner's radical behaviorism allowed people to speak for themselves. Similarly, from a radical behaviorist perspective, the goals and reasons for why a person or animal performs a particular behavior are considered to be components of the behavior itself; they are tangled up in the actions taken in the world (and in this sense, it becomes clear why the basis of Skinner's psychology was operant conditioning—in which the animal takes a positive and active role in learning—rather than the conditioned reflex of Watsonian behaviorism). One could argue, therefore, that radical behaviorism simply construes "mind" very differently to that of Cartesian cognitivism by considering behavior to be fully constitutive of mind, and not merely its "product". As Malone and Cruchon (2001, p. 54) note, the more radical behaviorists who followed Skinner, like Harold Rachlin (1994), for example, argue that if we "construe 'mental' as 'temporally extended, overt, observable patterns of behavior,' we can actually *see* a person's 'mental activity' unroll before us." The thing to avoid at all costs is to assume that the "mind" is a "thing" we possess inside our bodies, rather than simply a way that we have learned to speak about our actions in the world as part of our cultural heritage.

Given this stance, it is interesting to note that radical behaviorism has much in common with recent developments in so-called embodied and distributed cognitive science. Skinner (1987), for example, writes that: "cognitive psychologists like to say 'the mind is what the brain does' but surely the body plays a part? The mind is what the body does. It is what the person does. In other words, it is behavior," (Skinner, 1987, p. 784) and had previously noted that "The skin is not all that important a boundary" (Skinner, 1964, p. 84). In presenting such arguments, Skinner's views are not a million miles away from those of the prominent modern-day philosopher of cognitive science, Andy Clark (Clark, 1997, p. 84), who similarly rejects the Cartesian emphasis of modern psychology: "To thus take body and world seriously is to invite an emergentist perspective on many key phenomena—to see adaptive success as inhering as much in the complex interactions among body, world and brain as in the inner processes bound by skin and skull."

Why Skinner was not a Cognitive Psychologist

One of Skinner's main objections to theories that made use of intervening variables, and more specifically, to mentalism, was that they were prone to the "nominative fallacy"; that the naming of something provides an explanation of it. When we claim that, for example, a chimpanzee throws rocks at a gawping crowd of tourists because he has "autonoetic consciousness" and can "plan for the future" (Osvath, 2009), we have not actually explained the mechanisms involved, but merely labeled them. Our explanation is no more advanced with respect to our ability to understand the behavior concerned, and we have also complicated matters for we now have to account, not just for the behavior itself, but for the generation of the inner mental state that caused the behavior (see also Shettleworth, chapter 28of this volume). Another way to put it is to say that internal mediating mechanisms, as a form of inner behavior, are themselves in need of explanation; using internal events to explain outward events simply shoves the problem up one level.

The other thing to note about Skinner's research program is that it wasn't an anti-anthropomorphic attempt to put other animals in their place, and deny them the capacity for internal decisionmaking, emotions, or intentions, as some have suggested (e.g., de Waal, 1997, pp. 50–53). Although it is true that Skinner worked intensively on rats and pigeons, he considered these to be model organisms that would reveal the principles that governed behavior in all animals, including humans. Skinner's program was, in this sense, not anti-anthropomorphic at all, and it was also much less anthropocentric than many contemporary cognitivist research programs in comparative psychology.[8]

It is true to say, however, that Skinner's program wasn't particularly "ecological," in the sense of accounting for how species adaptations to particular niches might affect behavior. Again, this isn't the same as saying that Skinner failed to recognize the existence and importance of niche-related behaviors. As he noted, "no reputable student of animal behavior has ever taken the position that the animal comes to the laboratory as a virtual *tabula rasa*, that species differences are insignificant, and that all responses are about equally conditionable to all stimuli" (Skinner, 1966, p. 1205). A startling statement, no doubt, to those of us raised to believe that the central tenet of Skinnerian behaviorism is exactly the kind of "blank slate" approach he himself criticizes here.

Indeed, Costall (2004) goes so far as to argue that it was Skinner who reintroduced the "mutuality" of animal and environment that was rejected by the early behaviorists (and I would argue, by the early comparative psychologists, like Romanes) by including in his definition of behavior "…that part

of the functioning of an organism which is engaged in acting upon or having *commerce with* the outside world" (Skinner, 1938, p. 6, emphasis added). Like Darwin, Costall (2004) argues that Skinner saw that organisms acted on their environments and did not simply respond to them. For example, the (in) famous Skinner boxes were designed to "reflect" the animals they contained; Skinner stated explicitly that "the apparatus was designed by the organism we study" (Skinner, 1961, p. 543), with the capacities and limits of the animal determining the kind of manipulandum that was used, the kind of stimulation presented, and the kind of reinforcement given (Costall, 2004).

Despite this, it is clear that one can make a strong case for the overly restrictive nature of a Skinnerian approach to the study of psychology—after all, a Skinner box affords only operant learning and, therefore, one can never discover other potential mechanisms using this method. It is also true Skinner himself frequently extrapolated beyond the bounds of his data in an attempt to garner wider acceptance of his views, and he also presented a "dumbed down" version of his philosophy, which only helped fuel further criticism (Malone, 2009). One only has to take a look at a few experimental psychology journals of the time to see that much work in this area was precisely the kind of atheoretical "rat running" that has been so widely criticized. It should also be apparent, however, that behaviorist approaches in general, and radical behaviorism in particular, do not deserve much of the demonizing they receive in the current comparative psychology literature, and that it makes no sense to lump all forms of behaviorism together—especially given the cognitivist leanings of some behaviorists. The inconsistency that is inherent in the demonizing of behaviorism is thrown into even sharper relief when we consider the events of the so-called cognitive revolution.

The Cognitive Revolution?

As scientific legend and *The Oxford Companion to Philosophy* both would have it (Honderich, 2005), Noam Chomsky's (1959) "devastating review of B.F. Skinner's 'Verbal Behaviour' led to the cognitive revolution and the demise of behaviorism in psychology" (p. 139). Although there is no doubt that behaviorism went into decline following Chomsky's review and that cognitivism prospered, it seems unlikely that Chomsky's review was the sole cause. One reason for being skeptical of Chomsky as the catalyst for the cognitive revolution is that, as Malone (2009) and Leahey (1992) point out, mid-twentieth-century psychology was a highly fractured field, and behaviorism was not, in the Kuhnian sense (Kuhn 1962), the dominant paradigm. The "cognitive revolution" could not be a revolution precisely because there was no paradigm for it to overthrow; it might be more accurate to say simply that fashions and tastes in psychology changed around this time (see e.g., Andresen 1991, who argues that Chomsky's success at Skinner's expense was a matter of "cognitive taste," and that this reflected general societal changes in attitudes at the beginning of the 1960s. Similarly, Leudar and Costall (2004) argue that many psychologists were already prepared for the ideas presented in Chomsky's critique; all he did was amplify and focus them).

In addition, and as Malone (2009) makes clear, Wundt had already been studying sensation and perception using methods comparable to those of modern cognitive faculty psychologists (and was not, therefore, the kind of "introspectionist" that he is often portrayed), while Thorndike's early work on animals, with its use of hypothesized internal S-R links, stood in contrast to Watson's more physiological approach that followed it. As we've seen, it is also true that the theoretical underpinnings of both Tolman's and Hull's research were clearly cognitivist in important ways. In other words, behaviorist methodologies were tied to cognitivist theories from early on in behaviorism's history. As noted earlier, then, the "cognitive revolution" with respect to animal psychology was really just an extension of the nascent cognitivism that existed prior to Skinner's attempts to reconceptualize psychology along pragmatist, rather than Cartesian, lines. It is also reasonable to suggest that, in many ways, behaviorism never really died.[9] Indeed, just as dinosaurs roam among us in the form of modern birds, most "comparative cognitive psychologists" are, quite clearly, methodological behaviorists, who remain committed to a linear stimulus-response psychology (although their focus, obviously, is on what happens between stimulus and response) and who limit their evidence purely to observable behavior (Costall, 2004).

Whether one wishes to characterize the move toward cognitivism as a revolution or simply as the arc of a trajectory interrupted briefly by Skinner, it remains the case that the introduction of an explicitly cognitivist approach reintroduced all those aspects of the Cartesian view that radical behaviorism rejected as both unnecessary and pernicious.[10] This reflects, in large part, the adoption of the "brain-as-computer" metaphor, which characterizes cognition

as successive levels of "information processing." This view established firmly that the brain was the seat of all specifically cognitive processes, so separating it from the body and the rest of the nervous system. As a result, the latter came to be viewed merely as the "message cables" that relayed the brain's instructions to the essentially "lifeless" body (Churchland, 1996), just as Descartes had described.

The embrace of the brain-as-computer metaphor represents a step back from the radical behaviorists' emphasis on the activity of the whole organism and its "embeddness" in the environment and the restoration of the "mutuality" of organism and environment that Darwin initially identified; a deep irony given the standard criticism of behaviorism as a conceptually limited, laboratory-bound endeavor (see later). The other corollary of viewing the brain as the sole locus of cognition, with the accompanying emphasis on internal "information processing," is that it has led to the rigid separation of perception, cognition, and action. Action, in particular, is now relegated simply to the "read out" of the brain's instructions—the end product of a linear processing of information that transforms sensory input to motor output—rather than as a co-contributor to cognitive processes and a modulator of subsequent perceptual events. In this way, we have returned to Romanes' (1882) view that behavior is merely the "ambassador of the mind," from which we infer the internal causes of events.

Accordingly, "cognitive processes" are generally confined to those that mediate between perception and action—the two processes that actually "touch" the world. This may be no accident, given that, if cognition is self-contained in this way, it can be modeled without having to consider either the body or the environment. This, in turn, helps to increase the apparent validity of computer models and simulations of cognitive processes. In so doing, the original metaphor neatly turns back onto itself, and the metaphorical notion of cognition as information processing becomes reified. With this move, the study of cognition becomes not only disembedded from the environment, but it is also disembodied, further reinforcing the Cartesian separation of brain and body, body and world. It is this that leads to the idea that it will one day be possible to reduce cognitive psychology to neuroscience; once we understand how the brain works, the psychological level of explanation will simply fall away as unnecessary (e.g., Churchland, 1996). This, however, is a perspective that can apply only on acceptance of the premise that bodies and environments do not contribute in any constitutive way to cognition (aside from the trivial point that one needs some form of body to behave and so perform the acts dictated by cognitive processes).

As something of an aside, it is worth pointing out that this reification of the brain as computer metaphor is most notable within comparative evolutionary psychology applied specifically to humans; in particular, the school of thought promoted by Leda Cosmides, John Tooby, Steven Pinker, and David Buss. Their views are predicated entirely on the computational theory of mind, which they take to be axiomatic. Tooby and Cosmides (2005, p. 16), for example, state

> The brain's evolved function is to extract information from the environment and use that information to generate behavior and regulate physiology. Hence, the brain is not just like a computer. It *is* a computer—that is, a physical system that was designed to process information . . . The brain was designed by natural selection to be a computer [emphasis in the original].

It is Pinker (2003), however, who perhaps makes the strongest claim for this approach, stating that

> The mind is not the brain but what the brain does . . . the brain's special status comes from a special thing the brain does . . . that special thing is information processing, or computation[and that] The computational theory of mind . . . is one of the great ideas of intellectual history, for it solves one of the puzzles of the "mind-body problem" . . . It says that beliefs and desires are *information*, incarnated as configurations of symbols . . . without the computational theory of mind it is impossible to make sense of the evolution of mind. (*Pinker*, 2003, pp. 24–27)

Accordingly, hypotheses generated by advocates of this approach are tested on the assumption that the brain really is a computational device (not simply a metaphorical one), and that cognition actually is information processing.

Recently, Wallace (2010) has presented a critique of this approach to evolutionary psychology and its links to the computational theory of mind. His argument is that its unquestioned commitment to computationalism-cognitivism came about as a specific reaction to behaviorism and, in particular, the kind of "blank slate" caricature of radical behaviorism, criticized earlier, and its focus on learned, rather than innate, behavior (as captured by Pinker [2003, p. 31]: "The entities now commonly evoked to

explain the mind—such as *general purpose intelligence and multipurpose learning strategies*—will surely go the way of protoplasm in biology" [emphasis added]). Regardless of whether Wallace's (2010) view ultimately is shown to be correct, it is an interesting and thought-provoking perspective, given that the rise of evolutionary psychology is more commonly attributed simply to the insight that psychological mechanisms are adaptations, and, therefore, open to evolutionary analysis. Behaviorism often does not enter the debate because this is seen as an approach to animal psychology, rather than human psychology. More specifically, Wallace's (2010) contention is that, should the computational theory of mind prove to be wrong—which he believes it will be—then this particular incarnation of evolutionary psychological thought will, necessarily, fall with it.

With respect to nonhuman comparative evolutionary psychology, the key point to make about the "cognitive revolution," is that—given the extremely fuzzy line that has long existed between methodological behaviorism and cognitivism—it is very difficult to pin the rise of cognitivist animal psychology on the cognitivist-computational turn within psychology as a whole and its "triumph" over behaviorism. To be sure, the groundwork was laid by the emergence of the computer metaphor and the idea that internal information processing mechanisms in the brain were the proper domain of study. However, as Wynne (2007) notes, all of this was taking place in the 1950s and 1960s, whereas explicitly cognitivist approaches are apparent in the animal literature only from the 1970s onward (a point that could also be used to counter Wallace's argument with respect to human evolutionary psychology). Wynne (2007) suggests, instead, that the return to a more cognitivist (and indeed explicitly mentalist) approach can be attributed in large part to Donald Griffin's (1976) book *The Question of Animal Awareness* (although one might argue that, like Chomsky, Griffin was simply picking up on a more general spirit of the time, producing a book for which everyone was "ready"). What is most notable about Griffin's book, in retrospect, is how his position extended beyond that of most modern cognitivist psychologists and ethologists, emphasizing not only a similarity between mental events or cognitive processes, but also mental experiences:

> The flexibility and appropriateness of such behavior suggests not only that complex processes occur within animals brains, but that these events may have much in common with our own mental experiences. (*Griffin*, 1976, pp. 3–4)

More particularly, Griffin envisaged that conscious awareness could act as a form of "information processing" in and of itself that occurred in addition to the activity of the brain, allowing honeybees, for example, to compensate for their lack of neural machinery. Although these aspects of Griffin's arguments were not widely accepted, the strong emphasis on evolutionary continuity that was used to underpin the scientific claims of Griffin's broader argument has led ineluctably to the kind of explicitly anthropomorphic research strategy, and, hence, strongly Cartesian research strategy, advocated by earlier researchers like Romanes. It is also clear that Griffin's point about the "flexibility" and "appropriateness" of behavior as a result of "complex" processes was intended to undermine the notion that nonhuman animals were capable only of those forms of "associative" learning (whether conditioned reflexes or operant responding) studied by the behaviorists and learning theorists (each in their different ways). As a result, associative learning is now often treated as a "noncognitive" and "simpler" alternative to complex, cognitive explanations, even though—as made clear earlier—any theory that posits intervening variables between stimulus and response legitimately can be viewed as cognitive.

And so we return to the present, where it should now be clear how and why evolutionary comparative psychology takes the form that it does. Specifically, modern evolutionary comparative psychology (as applied to both humans and nonhumans) combines the methods of behaviorism with the theoretical stance of cognitivist-computational information processing, along with a heavy Darwinian emphasis on continuity across species, which also brings with it a strongly Cartesian heritage. This leads us nicely to a consideration of the manner in which comparative cognition is defended against those behaviorist alternatives that are argued to deny the possibility of mental states.

As will become clear, many of the arguments used to advocate an explicitly cognitivist "neo-Cartesian" approach gain their force by misrepresenting behaviorist philosophy, thereby generating an easily crushed straw man. Although Byrne and Bates's (2006) short review article is not the only strong ideological defense of a cognitivist/mentalistic stance, it has the advantage of articulating the main arguments extremely clearly and cogently,

presenting an opportunity to systematically examine each of them in turn. In what follows, I take this article to represent the current consensus.

A Critique of Comparative Cognitivism

Byrne and Bates's (2006) commitment to cognitivism is identical to the one articulated at the beginning of this chapter, and the aim of their article is to justify why we should consider all animals, and not just humans, to be cognitive precisely in this fashion. Their argument hinges on the following main points: (1) in contrast to cognitive explanations, "associationist" (behaviorist) models cannot account for the complexity of animal behavior, especially under natural conditions. This is an argument that is made on the grounds of both implausibility and lack of parsimony. (2) The specific "tools" offered by cognitive science are essential to the process of developing testable hypotheses that can be applied to natural behavior. (3) The value of the brain-as-computer metaphor as a means of demystifying mental processes, by enabling the testing of "mechanistic theories couched in information processing terms rather then phemenology"—a stance that naturally allows it to be applied to nonlinguistic animals that cannot report on their experiences as we do.

The first thing to note is the explicit differentiation between "cognitivist" and "associationist" approaches to animal cognition. Byrne and Bates (2006) begin by describing the results of laboratory experiments on scrubjays' caching behavior in the presence of conspecifics, pitting a cognitivist, mental state interpretation against one based on "a complex web of associations, each association well-understood from laboratory study of learning in the white rat."(R445) As noted earlier, the "associationist" account is treated as though it were completely noncognitive (see later for another example of this with respect to chimpanzee cognition), despite the fact that associative learning is, in fact, a cognitive process.

This clarification, however, only serves to make the contrast even more intriguing because it would then seem to require that "complex webs of association" must differ fundamentally from "mental states." This is an interesting argument for two reasons. On the one hand, there is a long-standing philosophical position, going back through J.S. Mill and Hume to Locke, that all our knowledge of the world, including our most complex concepts, are built up from the associations we acquire through experience. On the other, and more pertinently, complex association was exactly the process by which Hull (1930)—a cognitively inclined neobehaviorist—theorized that animals constructed their "replicas" of the world, while more recent empirical evidence shows that such a process can give rise to knowledge of exactly this kind (e.g., Brooks, 1978; Landauer & Dumais, 1997, and see later). Finally, this definition and contrast does not characterize accurately the radical behaviorist stance, which argues that there are no associations *in* the organism; associations are found in the objects of the world itself (i.e., an animal doesn't associate the smell of lemon with its bitter taste inside its head, but rather, the smell and taste are associated *in the lemon*).

Presumably, the distinction that Byrne and Bates (2006) are really drawing here is between mental-state understanding as some form of explicit, propositional knowledge of the world and webs of association as implicit, procedural knowledge. If so, then the contrast being drawn is nothing more than "Descartes dressed up in modern garb" (Papineau & Heyes, 2006, p. 188), with "mentalistic terms representing the immaterial mind and associative learning representing "brute matter" (Papineau & Heyes, 2006, p. 188). In this fashion, Byrne and Bates (2006) not only remain true to the Cartesian mind of late twentieth-century cognitive science, but also reintroduce seventeenth-century Cartesian distinctions between rational thought and mechanical processes. To be fair, they later back away from the idea that cognitivism requires explicit knowledge, although this does make it difficult to assess exactly what their position is in this respect.

The other problem that allegedly renders associative explanations less than satisfactory is that they are "unduly trusting." To explain complex behavior patterns by associative learning, one would have to take on trust that learning is rapid, and "sharply focused on just those specific features that cue the variables important in explaining how a particular behavior was learnt" (Byrne & Bates, 2006, R445). This, again, is an intriguing comment, because it simply takes for granted that any form of unstructured statistical-learning would result in the formation of irrelevant and spurious associations, unless there were top-down cognitive mechanisms to prevent this from happening. This carries with it the implication that a mentalistic interpretation is both more parsimonious and plausible than one based on associative learning mechanisms—an argument that is often made explicitly in the comparative literature (e.g., Call, 2006; Tomasello & Call 2006; Tomasello, Call & Hare, 2003: Whiten & Byrne, 1991).

There are two points to be made here: first, the manner in which parsimony should guide our interpretation of behavior and, second, the assumption that the world lacks sufficient structure to allow for the statistical learning of its features.

Firing Morgan's Canon: Use Caution

For most comparative researchers, the idea that one should accept the most parsimonious explanation for a given set of findings stems from an adherence to Lloyd Morgan's "canon" (see e.g., Knoll, 1997; MacPhail, 1998; Semple, Higham, MacLarnon, Ross, & Lehmann, 2010):

> in no case may we interpret an action as the outcome of a higher psychical faculty, if it can be interpreted as the outcome of one which stands lower on the psychical scale."
> (*Morgan*, 1894, p. 59)

This is considered to be a modified form of Occam's razor that requires us to accept the simplest possible explanation that can account for the available facts. A careful reading of Morgan (1894), however, reveals that he never intended the canon to be used as principle of parsimony in this way (see also Costall, 1993; and Wozniak, 1993). To understand Morgan's actual intent, we have to consider his view of mental evolution, which constituted a very loose kind of scale, with greater levels of "mentality" (whatever that might mean) seen in humans compared to other animals (Morgan, 1894). For Morgan, there were simple associations (found in humans and other animals), perception of relations (the point at which human and other animal capacities part ways) and perception of abstract relations (that only humans possessed). Morgan also recognized that there would interspecific differences in the distribution of these capacities across the animal kingdom that reflected the design of the animals' sensory systems ("the method of variation"), such that, for example, a dog could possess more in the way of a certain lower faculty, like olfaction, than a human, but less in the way of abstraction. It was this possibility that made the "canon" necessary. In other words, if the "method of variation" means that other animals are capable of possessing abilities in some "lower" faculty that are superior to humans, then we should be careful to explore all possibilities at this level—especially as some of these may not be immediately obvious to us, given interspecies differences—before we move up to consider faculties at a "higher" level. Indeed, Morgan specifically identified the pitfalls of mistakenly assuming that his "basal principle" was one of parsimony, and that the simplest explanation is always to be preferred:

> . . . by adopting the principle in question, we may be shutting our eyes to the simplest explanation of phenomena. Is it not simpler to explain the higher activities of animals as the direct outcome of reason or intellectual thought, than to explain them as the complex results of mere intelligence or practical sense experience? Undoubtedly, in many cases it may seem simpler. It is the apparent simplicity that leads many people to naively adopt it. But surely the simplicity of an explanation is no criterion of its truth. The explanation of the genesis of organic world by direct creative fiat is far simpler than the explanation of the genesis through the indirect method of evolution. (*Morgan*, 1894, pp. 54–55)[11]

The canon is not, then, admonishing us to refrain from attributing mental states to other animals, but to perform studies that would allow the appropriate process to be identified on the basis of behavior, lest we shut our eyes to a simple process that requires more complex explication than the attribution of human-like thought.[12] Moreover, Morgan (1890) had argued a similar point earlier, stating that "we do not know enough about the causes of variation to be rigidly bound by the laws of parcimony (sic)" (Morgan, 1890, p. 174). Parsimony, in other words, is a red herring; it cannot be used to argue the case one way or the other. Consequently, if parsimony is no guide to understanding the cognition of other species, then the argument that "associative" accounts are "unduly trusting," loses much of its force because the premise of this argument rests, at least partly, on the lack of parsimony in the associative account.

This clarification of Morgan's canon is also useful because it is frequently invoked by both sides in any argument over what constitutes the "simplest" psychological explanation of behavioral phenomena. That is, advocates of an associative account and those who prefer a more mentalistic interpretation both claim that Morgan's canon supports their favored interpretation on the mistaken grounds that they are dealing with a principle of parsimony. The recent report of an "awareness of death" among chimpanzees is a case in point, and is revealingly illustrative.

In the report, the social events surrounding the natural death of a single aged female chimpanzee in a captive colony were described and interpreted (Anderson, Gillies, & Lock, 2010a). Although Anderson et al. (2010a) did not go so far as to say that chimpanzees possess an explicit concept of

death, they nevertheless stated that their observations will "help to build a more complete picture of the mental life of our nearest evolutionary neighbors, including how they perceive and cope with death" (Anderson et al. 2010b). The behaviors shown toward the female during her last hours were interpreted as "checking for signs of life" (manipulating the arms and opening the mouth of the dying chimpanzee) and possibly "attempting to resuscitate the female" (a male chimpanzee pounding on the torso of the dead female), in addition to more concrete descriptions of what the animals actually did during the period of the female's demise.

In a commentary criticizing both the anthropomorphic interpretation and anecdotal approach adopted in this paper, Semple et al. (2010) referred to Morgan's canon as a "principle of parsimony", and suggested that, rather than attempting to resuscitate the female, a more parsimonious explanation would be that the male hit the female when she failed to respond normally (i.e., with a submissive response) to his threat behavior. Anderson et al. (2010b) also accepted Morgan's canon as a principle of parsimony in their response to this suggestion by turning the argument on its head. They countered that Semple et al.'s (2010) alternative suggestion was both more "complex and implausible" and so "clearly, focus on finding the 'simplest' explanation can result in over-elaboration and inaccuracy"(Anderson et al., 2010b). So, whereas Semple et al. (2010) mistakenly assume that Morgan was arguing for parsimony as an inherent virtue, Anderson et al. (2010b) make the additional, and perhaps more dangerous mistake, of assuming that the most parsimonious explanation is the one that is simplest for *us* to grasp, rather than one that is simplest in terms of the underlying mechanism it posits. The distinction between simple mechanisms and simple explanations seems to get lost in many of these debates. If nothing else, we need greater recognition of the fact that there need be no simple one-to-one mapping between the complexity of a mechanism and the complexity of behavior it produces.

How impoverished is the stimulus?

The second of Byrne and Bate's (2006) points is the assumption that the world lacks the degree of inherent structure that would allow unguided learning to pick up relevant cues and exclude irrelevant ones. That is, associative accounts are not implausible simply because they lack parsimony, they are also inherently implausible. This is similar to the familiar "poverty of the stimulus" argument, raised by Chomsky (1962, 1965) with respect to language acquisition by human infants. According to Chomsky, the input from adult speech is both too underdetermined (in terms of grammar) and degenerate (due to the use of speech fragments and deviant expressions) for statistical learning to occur. Consequently, he argued for an innate, computational language acquisition device that underpinned language learning.

It has become apparent in recent years, however, that statistical learning plays a much larger role than anticipated in language development, and that the stimulus may be much wealthier than supposed. Soderstrom and Morgan (2007), for example, have shown that 20–23-month-old infants have a preferential bias for fluent over disfluent speech, even when all lexical and grammatical items are removed; that is, the prosodic cues associated with these different kinds of speech are sufficient on their own to focus the attention of infants. As only ungrammatical speech is disfluent in this way, these results suggest that a simple perceptual bias steers children to attend preferentially to fluent speech with the consequence that they expose themselves to stimuli that are not as degenerate as Chomsky supposed. The poverty of the stimulus may well have been overemphasized and the degree of statistical regularity waiting to be picked up in speech utterances significantly underestimated. Other findings in the infant language literature arrive at similar conclusions with respect to the underdeterminacy of language (e.g., Gomez, 2002; Saffran, Aslin, & Newport, 1996). Finally, Ramscar (2010) has recently introduced a new version of the "computer metaphor" that likens the brain to a "search engine" that can learn statistically, rather than a Chomskyian spreadsheet-type model.

The broader point that all this work makes, in conjunction with recent research on infant imitation (Ray & Heyes, 2011) is that we cannot simply assume that the world is too complex and messy to allow unstructured (associative) learning mechanisms to be used effectively. For cases in which this assumption has been put to the test, the results can be surprising. A classic example is Landauer and Dumais's (1997) solution to "Plato's paradox" (how does a learner who doesn't know what to learn, manage to learn anything?). Using a technique called latent semantic analysis, they produced neural networks that were able to comprehend written text, starting only from an association matrix of experienced words and the contexts in which they occurred. In other words, a purely associative

process was able to produce language comprehension, the nonpareil of cognitive processes.

In the social domain, Turesson and Ghazanfar (2010) have shown recently that the implicit, statistical learning of social signals represents a viable domain-general mechanism for monitoring aspects of group structure and behavior (e.g., coalition formation) in humans, and is not vulnerable to spurious associations (thus arguing against the modular domain-specificity posited by some human evolutionary psychologists). Perceptual biases combined with statistical-learning may, therefore, represent a very powerful means of learning about the structure of both the physical and social world. This, in turn, means that the "sharp focus" that Byrne and Bates (2006) require may need to be neither so sharp nor so focused. Natural selection may act to make animals differentially sensitive to certain perceptual aspects of the environment, and the animal's own actions in the world may also facilitate this learning (Barrett 2011).

Byrne and Bates's (2006) argument also contains a misleading insistence on the idea that associative learning deals only with the connection of two events, so that learning a complex sequence requires the chaining together of a series of discrete paired associations in the right sequence. A process that is, again, presented as both implausible and lacking parsimony. It is misleading because associative learning can, and does, involve more sophisticated processes (see e.g., Papineau and Heyes, 2006) but—and this is the crucial point—even if it were the case that associations were only ever formed in a pairwise fashion, requiring long chains of association to be built up over time, this does not justify the rejection of an associative account. Returning again to Morgan, we have no grounds for assuming that evolved processes will be simple in this sense; perhaps long, convoluted, complex chains of associations are exactly how many cognitive skills are learnt. As with other adaptations that reflect the tinkering of natural selection over eons of time, psychological mechanisms may represent somewhat clunky, somewhat messy, often convoluted, solutions to particular problems. Effectiveness not efficiency per se is what matters.

Where does Complexity Lie?

Another major reason given by Byrne and Bates (2006) for favoring "cognitive" over "associative" hypotheses of learning is that that, as associative accounts can only be tested under tightly constrained and simplified laboratory conditions, the results of such experiments cannot be extended to account for the complex and flexible traits seen under natural conditions. Although it is fair to say that more flexible and complex behavior is displayed more frequently under natural conditions, it does not follow that associative learning, because of its simplicity, cannot account for the natural behavior. Although it is possible that researchers may be more likely to overinterpret naturalistic behavioral observations in ways that are ruled out by tight experimental control (although as noted earlier, experiments can also be overinterpreted in this manner), it is also possible that the natural environment scaffolds animals in ways that afford more flexibility; the greater complexity of behavior seen under natural conditions may be underpinned by exactly the kinds of simple mechanisms that are often revealed in the laboratory studies, and it is the interaction of those same mechanisms with environmental features that gives rise to "natural" complexity. It is, as already noted, a mistake to think that the complexity of behavior must (and will) map directly onto the complexity of the mechanisms that underlie it (see Barrett, 2009 for two pertinent examples, and Barrett 2011 for a more thorough review). Environmental resources can be exploited to produce behavior more complex than those produced by "raw brain" alone (Clark, 1997, 2008). For the committed cognitivist, however, this argument is not an option because, from this perspective, behavior is a window through which the structure of the mind might be seen. If we accept, instead, that the behavior we see falls out of the interaction between internal mechanisms and the environment, then behavior cannot point accurately or directly to cognitive mechanisms as processes in and of themselves.

One can also question whether it is true that only a "cognitive" level of description allows hypotheses regarding complex behavior to be formulated and tested on wild animals under natural conditions. Byrne and Bates (2006) claim that it is "unclear how the topics of number and counting could ever have been explored from a standpoint of animal learning theory" (although, methodologically speaking, this is often exactly how they were explored). Such a statement seems to making claims for a cognitivist stance that should properly be attributed to the "ecological approach" in comparative psychology, which is an argument made previously and very cogently by Shettleworth (2010; see also chapter 28 of this volume). Contrary to Byrne and Bates's suggestion, "social comprehension, spatial knowledge and navigation, imitation and teaching and understanding

of physical systems like the weather" are not made possible by the adoption of a cognitive stance, but by taking natural behavior as a starting point and using this to build hypotheses that ask what a particular behavior is designed to achieve from a functional perspective, and the possible means by which this could be realized proximately. One can, of course, use a cognitivist perspective to do this, hypothesizing the existence of particular internal processing mechanisms, but there is no reason why an ecologically informed, noncognitivist approach cannot be equally successful.

Indeed, an approach that broadens the scope of investigation by taking into account both the body and the environment and does not conceive of cognition as a brain-limited suite of internal mechanisms may be even more successful. This approach is better equipped to make discoveries that are, not simply unlikely, from either a "pure" cognitivist or an animal-learning perspective, but actually impossible. For example, there is evidence to suggest that the physical structure of salticid spider eyes is as important in understanding how they locate prey as anything going on in their brain, ditto the manner in which the arrangement of facets in the compound eyes of flies allows them to automatically compensate for motion parallax (see Barrett, 2011). In a similar vein, the physics of crickets' ears explains an enormous amount about how females are able to distinguish, locate, and move toward a male of the right species in ways that obviate the need for any form of cognitive processing (Barrett, 2011). In each case, the physics of the body contributes directly to successful functioning in the world, but a cognitivist approach (of either a learning theory or mentalist stripe) would bypass these perceptual systems as merely providing the input on which cognitive processes could then get to work, and not consider that they have an instrumental role to play in producing adaptive behavior.

Cognitivism cannot take advantage of this broader perspective because of its commitment to the idea that it is cognition alone—internal brain-based representational processes—that slot in between "the massive complexity of the brain and the simple efficiency of adaptive behavior in the world," and account for why animals do what they do. This, allegedly, is where the conceptual "tools" of cognitive science come into play:

> theory of mind, working memory, focus of attention, cognitive map, number concept and counting, procedural knowledge, problem-solving, and many others— allows theories to be developed, simple enough to be comprehended and used to make testable predictions in natural environments, yet tight enough to be mapped onto observed behaviour. (*Byrne and Bates* (2006), p. R445)

Apropos these "tools," Watson's and Skinner's warnings once again become pertinent, as do those of Wynne (2007) and Blumberg and Wasserman (1995): if we hypothesize that an animals abilities to find its way around rest on its possession of a "cognitive map," or that its behavior toward another animal is because it has some kind of "theory of mind," have we explained something or only named it? Are we any better equipped to predict and understand behavior than we are to employ an explanation that doesn't make use of such constructs? And does mental-state terminology, in particular, do any unique work with respect to the kinds of explanations we can provide or the hypotheses we can subsequently generate?

This is precisely the point that Povinelli and colleagues have made about explicitly mentalistic interpretations of comparative psychological experiments (e.g., Penn & Povinelli, 2007; Penn, Holyoak, & Povinelli, 2008; Povinelli, Bering, Giambrone, 2000; Povinelli & Vonk, 2003, 2004; Vonk & Povinelli, 2006). Penn and Povinelli (2007), for example, use a simple formalism to demonstrate that observable task features allow complete explanations of animal performance to be made, without any need to posit that the animals in question are able to reason about the unobservable mental states of others. One thing that is important to note with respect to these critiques is that they are couched in the same cognitive (i.e., representational) terms as the mentalistic explanations that they challenge. Povinelli et al.'s (2000) reinterpretation hypothesis, Vonk and Povinelli's (2006) unobservability hypothesis, and Penn et al.'s (2008) relational reinterpretation hypothesis all conceive of other animals as fully representational beings, and expressly reject the so-called behaviorist view that associative-learning mechanisms can achieve the same results. Their argument is only that the available data do not allow one to infer either that other species possess the ability to represent the contents of others' representations (their mental states), or that other species possess the same kinds of analogical reasoning skills that enable the human brain to emulate a physical-symbol system. Tellingly, however, critics of these hypotheses frequently interpret them either as a form of derived behaviorism or assume that they entail that animals can engage only in some form of sophisticated

cue-based behaviorreading, as opposed to actually forming representations of the behavior of others (e.g., Call 2006; Call & Tomasello 2008; Tomasello & Call 2006; Emery & Clayton 2006; Tomasello et al, 2003). Tomasello, Call, and Hare (2003), for example, suggest that Vonk and Povinelli's criticisms of their work treat chimpanzees as "mindless behaviorists", which is not only inaccurate but perpetuates the fallacy that associative learning has no place in cognitivist-mentalistic theory, when it is, in fact, a perfectly respectable cognitive mechanism. Such responses are also interesting in themselves because they suggest that, even when other species are treated as fully "cognitive," rather than as some form of "associative engine" (Clark, 1993), any form of "non-mentalistic" account nevertheless renders their performance less impressive, presumably because of the (misplaced) comparison to our own cognitive skills.

Byrne and Bates (2006) make a similar point when they suggest that the resistance shown by biologists to cognitive explanations of animal behavior is due to a conflation of the concept of "cognition" with those of "intelligence" and "consciousness." They note that, when behavior is said to be the result of a cognitive process, there is an assumption that more intelligence is being shown than if the account were couched in terms of associative learning. This may well be true, but this seems less likely to be due to a "tacit assumption" on the part of those reading such studies than to the overtly mentalistic interpretations given by researchers who perform them and their own dismissal of associative accounts. Consider the results of studies on scrubjay cache- protection strategies, where birds were shown to vary their caching behavior according to whether they were observed by a competitor (Clayton, Dally, & Emery, 2007). These findings were suggested by the authors to

> raise the intriguing possibility that re-caching is based on a form of mental attribution, namely, the simulation of another bird's viewpoint. Furthermore, the jays also keep track of the observer that was watching when they cached and take protective action accordingly, thus suggesting that they may also be aware of others' knowledge states.
> (*Clayton* et al. 2007, p. 507)

This suggestion is made despite the fact that it is perfectly possible to account for these results on the basis of observable task features alone. As Penn and Povinelli (2007) state, in order for the birds to associate specific competitors with specific cache sites from a cognitive perspective, it requires only that they use the following kinds of reasoning: "re-cache food if a competitor has oriented towards it in the past" or "attempted to pilfer food if the competitor who cached it is not present." The additional claim that the birds act in the way they do is because they *understand* that the competitor *knows* where the food is located does no explanatory work whatsoever (Penn & Povinelli, 2007; Penn et al., 2008). Note that, here, the application of Occam's razor is appropriate; if the mental-state interpretation were able to explain some additional or nonintuitive aspect of the results, then this extra conceptual layer would be useful, but this isn't the case (see also Povinelli & Vonk, 2004). The results themselves cannot speak to the mental- state interpretation one way or the other.

Similar criticisms apply to studies claiming that chimpanzees and monkeys are capable of understanding the mental states of others (e.g., Brauer, Call, & Tomasello, 2007; Flombaum & Santos 2005; Hare, Call, Agnetta, & Tomasello, 2000; Hare, Call, & Tomasello, 2001; Santos, Flombaum, & Phillips, 2006; Tomasello & Call 2006). In these experiments, scenarios are set-up in which dominant and subordinate animals (or monkeys and humans) must compete (explicitly or implicitly) for food, and the choices made by the subordinate animal (or monkey) are taken as indicative of an understanding of what the dominant animals (or human) sees and possibly knows about the situation. Again, it is possible to generate a complete account of the animals' responding in fully representational, cognitive terms based on observable task features, but without the need to posit that the animals are able to reason about unobservable mental states (Penn & Povinelli, 2007; Penn et al., 2008; Povinelli & Vonk, 2003, 2004; Vonk & Povinelli, 2006). Despite this, some researchers go so far as to propose that "primates do reason about unobservable mental states, and that they do so with the same basic cognitive systems that we humans use to reason about mental states." (Santos et al., 2006). In both cases, the insistence on additional metarepresentational capacities beyond those needed to explain the data suggest that it is the researchers themselves who are promoting a link between mentalistic explanations, intelligence, and consciousness, and not the unnamed biologists who infer this and then resist it.

Keeping Our Metaphors Under Control

The final argument in favor of a specifically cognitive stance toward animal psychology rests on the

power of the computer metaphor. In particular, Byrne and Bates (2006), suggest that the computational approach permits the de-mystification of mental processes by viewing all mental operations as information processing; as we saw earlier, this is the same argument used by Pinker (2003) to justify the particular conception of human-evolved cognitive architecture. In and of itself, however, the information-processing metaphor cannot de-mystify anything. A brain does not literally process information, and it is not a device that "converts [information] from one code to another . . . stores and retrieves information . . . and operates upon existing information to compute new 'knowledge.'" Brains are organs consisting of neuronal cells that generate action potentials and produce chemicals that diffuse throughout the brain and thereby modulate neuronal firing in various ways. How neuronal firing translates into any of the cognitive processes referred to by Byrne and Bates (2006) remains anyone's guess. Again, the cognitive approach does not de-mystify mental processes as much as simply relabel them, reifying a metaphorical construct. The computational cognitivism advocated by Byrne and Bates (2006) is not only heavily Cartesian, but also commits the "mereological" fallacy of assigning to the brain the actions and powers of the organism as a whole (Bennett & Hacker, 2003).[13] Of course, if one thinks that cognition reduces to brains, such a stance toward the brain is both inevitable and understandable.

The argument, then, that cognitivism is the only sensible evolutionarily-based approach to animal psychology does so at the price of accepting an essentially Cartesian view of the brain-mind as the sole province of cognition, and of mental states as contents that are stored in and manipulated by the brain. However, cognitivism of this stripe is not the only game in town. Space does not permit a thorough examination of alternatives but, as mentioned in several places, a more "embodied, embedded" approach, one that recognizes the mutuality of organism and environment, is possible. This is a view that rejects the absolute centrality of the brain as the seat of cognition but recognizes the fundamental contribution made by both the body and the environment in a constitutive and not merely causal fashion (using cognition to mean not information processing, but the production of adaptive behavior that is sensitive to context: Killeen and Glenberg, 2010).

Broadening The Concept Of Cognition

In this view, ably expressed by, among others, Clark (1997, 2008), Pfeifer and Bongard (2007), Gallagher (2006), Rowlands (2006), and Noë (2010), cognition is conceived in terms of action—much like the radical behaviorists would view things—and bodily actions can be considered as cognitive as so-called mental actions (and, as such, less metaphorical). In this view, animals can also incorporate various aspects of the physical world into their cognitive systems in a constitutive way because there is no "magic boundary" drawn around the skull (Hurley, 1998) as there is in the classical cognitivist approach. Indeed, there is no separation at all between perception, cognition, and action, because cognition is not construed as a linear process, in which animals sense the world, form representations of it, manipulate these representations, and then act. As such, an embodied approach rejects the assumption, made by both behaviorists and cognitivists, of a strictly linear one-way stimulusresponse psychology by which outside environmental stimuli lead to behavioral responding. Instead, cognition is seen as a "loopy," world-involving feedback process in which sensorimotor coordination is key, and one that is both shaped and constrained by the physical body in various ways (Brooks, 1999; Clark, 1997, 2008; Dewey, 1896; Pfeifer & Bongard, 2007; Wheeler, 2005). The embodied, embedded perspective can, therefore, be viewed as a reaction against the excesses of cognitivism within psychology, much as cognitivism itself was a reaction to the extremes of behaviorism.

More practically, a more embodied-embedded approach can provide competing hypotheses to classical cognitivism instead of inappropriate nullmodels (Barrett, Henzi, & Rendall, 2007). By placing as much priority on an animal's physical body as on its brain, and thereby bringing the animal's umwelt into sharper focus (the world as perceived by the organism given its physical and physiological structure: von Uexküll, 1957), there is a reduced tendency to slip into anthropocentric ways of thinking that, as Blumberg (2007) suggests, constrain as much as they foster the generation of hypotheses. In sum, a more embodied, embedded perspective on cognition generates a completely different view of "cognition" as an active process of engagement with the world, and not a thing in the head. In this way, much as Griffin's cognitive ethology reached back in time to incorporate views that the behaviorists had reacted against, so an embodied-embedded approach reaches back and incorporates certain aspects of pragmatic behaviorist philosophy that were rejected by the cognitivist revival.

This view should not be taken necessarily as a rejection of cognitivism and the need to return to a pure form of behaviorism. As Clark (1997) and Penn et al. (2008) suggest, with respect to human cognition in particular, there are certain "representation-hungry" processes that do not seem amenable to an account grounded in coordinated sensorimotor processes alone. What an embodied view does suggest, however, is that a more questioning attitude can be taken toward representations: what they are supposed to consist of, and what unique work do they do—if any—in accounting for behavior? That is, Penn and colleagues' (2008) skepticism toward representations with mentalistic contents can (and perhaps should) be extended to encompass representational accounts as a whole. The comparative literature discussed by Byrne and Bates (2006) (and also Penn et al. 2008) is entirely framed by, and interpreted within, the computational metaphor that places all its emphasis on internal cognitive structure and information processing; these data can do nothing other than support the conclusion that representational structure is needed to account for behavior (and so similar to criticisms offered to Skinnerian operant psychology).

There are, however, alternative metaphors that can be used to frame questions, and these yield a different picture. Take for example, Thelen and Smith's (1994) work on infant development; research that was pursued using a dynamic-systems approach, rather than the dominant computational-cognitivist approach (e.g., Thelen & Smith 1994; Thelen, Schöner, Scheier, & Smith, 2001). In their classic studies of Piaget's A-not-B test, Thelen and Smith were able to show that an infant's perseverative reaching—interpreted from the cognitivist view as the absence of a stable object concept—could be eradicated by the simply expedient of making the child stand during the delay between seeing the toy being hidden and being allowed to reach for it. Adding weights to the infants' arms during the delay period also caused the error to disappear (Thelen et al., 2001). Why should this be? If representations are static structures inside the head, the physical movements of the child's body should be irrelevant to their success or failure.

We can begin to develop a different view if we consider what the task as a whole entails for the baby: It must reach to the right location in visual space, formulate a motor plan, maintain it over the delay, and then put it into action. Smith and Thelen's radical idea (which remains controversial) is that this motor plan and its enactment are part and parcel of what we could call the infant's "belief" that objects persist in space and time. In their view, one cannot say that, say, a 12-month-old differs from a 10-month-old because he or she has gained a static "object concept" in its "mind" and that this is the single cause of any differences in their behavior. Rather, their argument is that the babies' reaching is "soft-assembled" in the moment with many contributing forces that make the error appear and disappear, and that an individual baby's experience is key to the patterns shown. As such, the "belief" resides in the "dynamical system" as a whole—the baby's tightly coupled body and brain, and the baby's tight coupling to the world. It doesn't exist solely in the baby's head as some kind of symbolic, static representation. As Smith (2005) puts it: "Cognition just is an event in time, the emergent product of many heterogenous systems bound to each other and to the world in real time" (p. 296). This perspective gives bodily action a central role to play, and suggests that whatever representations are generated they will be "action oriented." Metaphorically speaking, they are dynamic "plans" for controlling action in the world, and not "mirror-like," static, copies of the world (Clark, 1997). This alternative view of representational "content" opens up the field of cognition in ways that brings bodily action, and the specific affordances of an animal's environment in relation to its body, to the fore.

Therefore, the point is not to deny categorically that animals and humans have representations, but to ask some pertinent questions about the nature of representations, and whether such representations are, in fact, doing all the cognitive work. If we admit the possibility that there may be more structure in the world than previously thought, and that the morphology of an animal's body may contribute to cognitive processes in a constitutive, as well as causally effective, way, we may find that "representations" are not always needed, or that they need not be as elaborate as current cognitivist approaches suggest. The idea that action-perception loops involving tight sensorimotor coordination can provide a complete account of behavior so that there is no work left for "cognition" to do (Brooks, 1999; Clark, 1997) can be seen as the modern-day version of Skinner's argument that separating "mental activity" from other forms of activity is a false dichotomy. An action-oriented approach also permits us to completely reconfigure representations as part of behavior, as Smith (2005) suggests (see also Rowland, 2006; and Keijzer, 2001), which again is a very Skinnerian approach. In other words, a broader

perspective on cognition as a body-based world-involving activity prevents us from throwing out the valuable pragmatic insights of the behaviorist baby along with the bathwater of dogma with which it became associated.

Conclusions and Future Directions

It is readily apparent that cognitivism and the computational theory of mind currently hold sway in comparative and human evolutionary psychology (particularly the Tooby-Cosmides-Pinker school), and a case can be made that, in both areas, this represents a reaction to the "extremes" of behaviorism. In the case of comparative psychology, the restrictiveness of Skinner's experimental approach to identifying general learning principles was abandoned for the delights of discovering the variety of mechanisms by which animals come to know their worlds. In the case of evolutionary psychology, a similar rejection of "general learning principles" and the promotion of "innate" cognitive processes was made, which, although more usually subsumed under the "standard social science model" (Cosmides & Tooby, 1992), reveals clear echoes of behaviorism (Wallace, 2010).

What should also be apparent, I hope, is that the philosophical underpinnings of many forms of behaviorism are similar to those underpinning modern cognitive psychology, as applied to both humans and animals. Contrasting "cognitive" approaches against "associationist" or "behaviorist" theories isn't the simple matter that one might suppose. It is also the case that the truly "extreme" form of behaviorism—radical behaviorism—is heavily misrepresented as both atheoretical and uninformed by evolutionary theory (e.g., de Waal [2009, p. 175] writes that "This theory-free 'behaviorism' never advanced our understanding of cognition to the degree that Darwinism has"), which belies Skinner's explicit use of Darwin's theory of natural selection as a metaphor for animal learning and the philosophical and conceptual sophistication of his writings.

In both comparative and evolutionary psychology, the explicitly "ecological" approach advocated—whereby we should identify the recurrent selective pressures influencing survival and reproductive success—is used as a means to promote cognitivism on the assumption that "mere" associative processes—and hence a behaviorist stance—could not produce the same results. This ties both approaches to the computational mind in ways that, ironically, serve to sever animals (human and nonhuman alike) from their ecological context, and their mutual relationship with their environments, as well overlooking the role that bodies as well as brains may contribute to cognitive processes.

The more embodied-embedded approach that I have presented as a reaction to the extremes of cognitivism is also presented as a way forward. In the case of comparative psychology, it may help us to escape from the heavily anthropocentric focus that the computational metaphor inevitably imposes (after all, its origins lie in the desire to model and even create an explicitly human-like intelligence in a machine), as well as giving us the means to recognize that internal brain-based mechanisms operate in conjunction with body and environment, not in isolation from them. A focus on whole animals, rather than just their brains, also helps us see how an understanding of the world must reflects the manner in which the world is encountered bodily; should animals prove to have "concepts" and "representations" we must entertain the possibility that they will be grounded very differently from our own.

In the case of evolutionary psychology, a more embodied-embedded approach may further help us to recognize that human inventions, like written language and other forms of material culture, augment and ratchet up the power of our evolved brains (e.g., Clark, 1997). These may well make significant contributions to our psychology, as well as explaining some of the differences between us and other animals. More specifically, a greater focus on bodily and environmental embedding forces a recognition of the sociocultural nature of human psychology—the fact that we develop in a socially and culturally rich milieu that reflects the contingent nature of historical events (as well as evolutionary ones). That is, evolutionary theory is, not only a predictive science, but also a historical one. Understanding modern human psychology requires an understanding of the entanglement of cultural practices and historical events with our evolutionary heritage, and not the reverse engineering of human cognitive architecture alone.

By taking heed of these developments in "embodied-embedded" approaches, we may be able to combine all that is most positive about the behaviorist project—the embedding of an organism in its environment, the emphasis on the activity of the organism as a whole, the rejection of the false separation between mental acts and behavioral acts—into a broader conception of cognition. That concept is one in which whole organisms come to know their worlds and behave adaptively in them, and it is one

in which cognition does not exist as a "thing in the head," nor is it the property of brains alone.

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Notes

1. Cognitive psychology is not monolithic, of course, and there is a wide variety of topic areas and theoretical models employed. Nevertheless it is fair to say that all areas of cognitive psychology are characterized by a particular stance to the "mental" as stated here.

2. In human psychology, the terms *cognitivist* and *cognitivism* are sometimes used interchangeably with the term *mentalism* (e.g., Sober, 1983). In the comparative literature, mentalism tends to be used in a more specialized way to convey either the ability to form plans and expectations regarding future possible events (e.g., Miklosi, 2009), or the ability to represent the contents of other individuals mental states (e.g., Call & Tomasello, 2008).

3. Darwin's comments refer to Plato's argument that our '"necessary ideas'" — those that enable us to understand and judge what had been received by our senses—were gathered together over previous lives. As Darwin indicates, these "previous lives" are constituted by our evolutionary ancestors, and this statement also suggests that Darwin was in agreement with the Platonic-Cartesian position that internal representations mediated and interpreted the input from our senses.

4. For Thorndike, the argument that mental activity could cause action was nonsensical because, as far as he was concerned, mental activity simply was action. One didn't think mental thoughts, one simply thought in the same way that one walked, ran, or jumped—an argument that Ryle (1949) later made on logical grounds. Malone (2009) argues that it was Thorndike's decision to study mind as a form of activity—to study behavior—that represented a truly original break with the past, and not necessarily the publication of Watson's "behaviourist manifesto."

5. In this context, it is also worth mentioning that the European ethologists, like Konrad Lorenz and Niko Tinbergen, similarly rejected the dualist position associated with a mentalist stance, and in their views, they were quite strongly aligned with those of the American behaviorists of the time. Tinbergen, in particular, described his work on the mechanisms of behavior as "physiology without breaking the skin" (MacDonald & Dawkins, 1981) and regarded behavior simply as the top level of a physiological hierarchy, with no requirement for something called "cognition" to step in and explain things from that point (Kennedy, 1992). What is interesting, however, is that both Lorenz and Tinbergen were ideologically opposed to American experimental comparative psychology, apparently both because of what they regarded as the extreme environmentalism and emphasis on learning of the behaviorists (Dewsbury, 1994) and the laboratory-based approach they favored (Wynne, 2007).

6. Malone (2009)—as one might imagine, given his view of Watson's behaviorism—gives a different spin to Watson's focus on the conditioned reflex. He notes that Pavlov also conceptualized mind as the product of the workings of a living body. As a physiologist, Pavlov's metaphorical take on things was to consider the body as a "marvelous machine", but one that, because it was specifically a living machine, did not need a Cartesian "ghost" to animate it. Understanding physiological response was, therefore, integral to psychology because "mind" was a product of the workings of the living body, and not a "thinking thing" in the head.

7. Kennedy (1992) goes so far as to argue that it was radical behaviorism's break with anthropomorphism that cleared the way for the development of behavioral ecology as a discipline; by no longer requiring researchers to attribute human-like "intentional" psychological mechanisms, they could use the "phenotypic gambit" and remain agnostic with respect to underlying mechanism in their accounts of the adaptive function of behavior.

8. A number of authors also make the case that, in terms of human psychology, behaviorism—or "behavior analysis" as it is now called—never really died (see e.g., Wyatt et al,. 1986)

9. As noted earlier, there are indeed many theoretical approaches to cognition and cognitive psychology. As also noted earlier, it seems fair to say that all modern cognitive psychology is philosophically committed to the idea of cognitive processes as internal brain-based mechanisms by which knowledge of the world is acquired, and employs the metaphor of computational information processing to describe these. In this respect, cognitive psychology shows more conceptual cohesion compared to the variety seen among the various schools of behaviorism, which, as we've seen, took a variety of philosophical positions.

10. Despite the clarity with which Morgan made this point, misinterpretation of the canon began almost immediately, so that in a later edition of the same work, Morgan (1900) added an explicit rider to his "basal principle" in which he stated that, if independent evidence to the contrary were available, then it was indeed appropriate to infer a higher psychological faculty, even if a plausible explanation at lower levels was possible. This addendum to the canon is only rarely quoted.

11. In this respect, Morgan's position is similar to that of those modern researchers, like Burghardt and Rivas & Burghardt (2002), Burghardt (2007), and Timberlake (2007), who make a case for "critical" or "ecological" anthropomorphism and "theromorphism," where one attempts to see the world from the animal's perspective while recognizing that their responses to the world will reflect the nature of their particular sensory and motor adaptations. It should also be apparent that Morgan's is not an anti-anthropomorphic principle either. Although Morgan initially argued against the inference of psychological faculties on the basis of behavior (Morgan 1886), he was later convinced by Romanes' evolutionary arguments to adopt the "doubly inductive" method, and his 1894 work is an attempt to place this kind of anthropomorphic method on a sound scientific basis.

12. Or as Skinner (1986, p. 86) said "The brain is said to use data, make hypotheses, make choices, and so on, as the mind was once said to have done. In a behavioristic account, it is a person who does these things."

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