How Folk Psychology Ruined Comparative Psychology
And How Scrub Jays Can Save It

Derek C. Penn

Abstract
The cognitive revolution in psychology was founded on the premise that all cognitive processes result from rule-governed operations and that cognizers do not need to understand these rules to act “rationally” or “intelligently.” Despite its intent to replace romantic folk psychological intuitions about how the mind works, anthropomorphism is prevalent throughout much of comparative psychology: claims that animals perform “human-like” feats find broad acceptance in the media and permeate the academic debate, while less anthropomorphic explanations are largely dismissed. To construct a viable scientific theory of nonhuman minds, comparative psychology must aim for a computationally explicit account of cognition—not just folk psychological descriptions. Given the impressive body of data that has been collected on the social cognitive abilities of scrub jays, compiling a functional specification of corvid social cognition would be a great place to start.

Introduction
Recently, a pair of papers appeared in Current Biology claiming that chimpanzees may have a human-like understanding of death (Anderson et al. 2010; Biro et al. 2010). In the first paper, boldly entitled, “Pan Thanatology,” Anderson et al. (2010) describe how a group of chimpanzees living in a Scottish safari park “grieved” over the death of an elderly female chimp named Pansy. The researchers claim that a chimp named Chippie “appeared to test for signs of life by closely inspecting [Pansy’s] mouth and manipulating her limbs” (Anderson et al. 2010:R350). They admit that Chippie also attacked the corpse three times, jumping and pounding on the body. However, the researchers suggest these acts were an expression of “denial,” “frustration,” and “anger toward the deceased” or perhaps an attempt at “resuscitation” (Anderson et al. 2010:R350).
The authors provide no evidence for these colorful suggestions other than the fact that the animal’s behavior was, as they put it, “strikingly reminiscent of human responses to peaceful death” (Anderson et al. 2010:R350).

In an accompanying paper in the same issue of Current Biology, Biro et al. (2010) describe how two mothers, whose infants had died of a respiratory epidemic, carried the bodies of their dead children around for days, even after the infants’ bodies had undergone complete mummiﬁcation. The “fascinating” question, the authors write, is the extent to which the chimp mothers “understood” that their offspring were dead (scarce quotes taken from the original). The authors do not claim, however, that the mothers “understood” that their offspring were dead and, indeed, do not believe that the evidence warrants such an interpretation. “In many ways,” Biro et al. (2010:R352) point out, “[the two mothers] treated the corpses as live infants, particularly in the initial phase following death.” Indeed, in all of her discussions with the media, Biro explains, “[I] made it very clear that we had no idea whether the carrying of the corpses in any way reﬂected an understanding of death...or whether any of the responses the mothers and other individuals in the group showed toward the dead infants had any parallels with human responses to death” (Biro, personal email Nov. 9, 2010).

These two papers were cited by hundreds of mainstream media outlets as if they both supported the same anthropomorphic conclusion. AP Press carried the story under the headline, “Chimps deal with death like humans.” NPR entitled their story, “Chimps May Mourn Lost Ones, Study Suggests.” Like Anderson et al. (2010), the mainstream media expressed few doubts that chimps understand death in a human-like way. Indeed, Discovery News opined that this finding was not even particularly surprising and then published a video clip of a squirrel attempting to “resuscitate” a dead comrade (Viegas 2010). Biro et al.’s more careful assessment of the evidence was hardly mentioned.

It would be nice if one could blame this case on the hyperbole of commercial media outlets or the radical views of a fringe movement in comparative psychology. However, stories such as these are ubiquitous. Hardly an issue of Current Biology or Animal Cognition goes by without some new effigy of human cognitive uniqueness being torn down and dragged through the mud. The authors of these claims rarely bother to elucidate alternative interpretations that don’t convey an anthropomorphic story line (Shettleworth 2010b). Far from being a fringe movement, Anderson et al. (2010) represent the reigning consensus among comparative psychologists when they argue that “the differences between humans and our great ape relatives aren’t as marked as most people think.”

1 In fact, Biro says she favors the hypothesis that the chimps’ behavior was a by-product of the strong mother-infant bond in chimpanzees and acknowledges that the choice of the word, “understand,” was “unfortunate.” It is “too bad,” she writes, “that the article didn’t mention the by-product explanation more explicitly—we hint at it, but having it spelt it out clearly would have been the right thing to do” (Biro personal email, Nov. 18, 2010).
From tool use to metacognition, from deception to death, much of comparative psychology over the last 35 years has been driven by the single-minded goal of demonstrating that nonhuman animals are capable of “human-like” cognition. This “anthropocentric approach” to comparative psychology, as Shettleworth (2010a) calls it, has tended to emphasize the mental continuity between humans and other animals and to treat nonhuman animals as “little furry or feathered people.” Vast numbers of comparative psychologists, of course, quietly continue to pursue more careful, ecologically grounded research projects (for a review, see Shettleworth 2010a). Those claiming that animals are capable of “human-like” feats have dominated the general media, commercial bookshelves, and much of the academic debate as well. Those favoring less anthropomorphic explanations have been shunned and/or ignored. The problem has been particularly egregious among researchers studying “Theory of Mind” (ToM), and it has only gotten worse over the last decade.

For the first twenty years after Premack and Woodruff’s (1978) seminal claim, research on the ToM abilities of nonhuman animals was fraught with controversy (Heyes 1998). Over the last ten years, the consensus in support of attributing at least some aspects of a ToM to nonhuman animals has grown ever more widespread and confident (Suddendorf and Whiten 2001; Tomasello et al. 2003a; Hare and Tomasello 2005; Santos et al. 2006; Tomasello and Call 2006; Wood et al. 2007; Emery and Clayton 2009; Byrne and Bates 2010; de Waal and Ferrari 2010). Indeed, in some researchers’ minds, the debate is all but over. In a recent review of the last thirty years of research on the ToM debate, Call and Tomasello (2008:189), for example, conclude: “We believe that there is only one reasonable conclusion to be drawn from the totality of the studies reviewed here: chimpanzees, like humans, understand the actions of others not just in terms of surface behaviors but also in terms of the underlying goals, and possibly intentions, involved.” “In a broad construal of the phrase ‘theory of mind,’” they go on to write, “the answer to Premack and Woodruff’s pregnant question of 30 years ago is a definite yes, chimpanzees do have a theory of mind”(Call and Tomasello 2008:191).

Povinelli and colleagues have long argued that there is no evidence that nonhuman animals possess anything even remotely resembling a “theory” about the causal role mental states play in modulating others’ behavior (Povinelli et al. 2000; Povinelli and Vonk 2003, 2004; Povinelli 2004; Penn and Povinelli 2007b; Penn et al. 2008; Penn and Povinelli 2011). Nevertheless, we have been spectacularly unsuccessful at changing the tide of anthropomorphism that has swept through comparative psychology, and it would serve little purpose to reiterate our arguments here.

What went wrong? How is it that so many eminent comparative psychologists are convinced that there are good scientific reasons for attributing some form of a ToM to nonhuman animals? Why is it that all the evidence and arguments against this position have been so unconvincing and unpopular?
The Revolution that Comparative Psychology Abandoned

Here is my hypothesis: One of the principal factors explaining the prevailing consensus about animals’ ToM abilities is that the comparative psychologists who defend these claims have largely abandoned the fundamental tenets of the cognitive revolution.

Let us all remember that the goal of the cognitive revolution in psychology, some forty-odd years old now, was not solely to leave behind the limitations of a behavioristic approach to cognition. It was also to replace romantic folk intuitions about how minds work with a materialist psychology of mental states grounded on a computational theory of cognition (Von Eckardt 1993). Our commonsense intuitions—our “folk psychology” (Bermudez 2003; Churchland and Churchland 1996; Stich and Ravenscroft 1994)—often posit an immaterial homunculus inside our heads named, “I,” who does our thinking for us, in sudden flashes of “insight” (Dennett 1991). At the heart of the cognitive revolution was the provocative claim that all mental processes (e.g., even the ineffable experience of human self-consciousness) are entirely material processes and thus ultimately explicable in terms of the mechanical, algorithmic (i.e., rule-governed) operation of a biological device. This does not mean, of course, that human or nonhuman minds bear any similarity to a digital computer or a propositional “language of thought” (see Penn et al. 2008). The cognitive revolution was supposed to refashion our folk psychological intuitions into a viable scientific theory based on a computational account of the mind.

Instead, comparative psychologists regularly claim that animals have an “understanding of” or “insight into” some folk psychological concept in order to falsify claims that the animal’s cognitive processes are rule-governed and unconscious. The very term, “cognitive,” is now typically used as a synonym for “mentalistic,” “conscious,” and “insightful”—as if “cognitive” were the opposite of “mechanical,” “unconscious,” and “rule-governed.” Nearly all the most prominent claims in support of attributing a ToM to nonhuman animals are framed using folk psychological idioms (e.g., “chimpanzees know what their groupmates do and do not know,” “chimpanzees can distinguish between an experimenter that is unwilling or unable to give them food,” “scrub jays can project their own experience of being a thief onto the observing bird”) without any attempt to cash out these claims at a computational, algorithmic, or neural level of explanation (Marr 1982).

To be sure, the cognitive revolution is alive and well in the work of comparative researchers as disparate as Gallistel (2002), Menzel and Giurfa (2006), Kacelnik (2006), Clayton et al. (2001), and Seyfarth and Cheney (2003c), to name only a few notable examples. Even many contemporary associationists

---

2 Whether such a scientific theory is a “representational” account or not is, of course, a matter of enormous debate. In this chapter, I take the necessity and utility of a representational account of cognition for granted. My critique of folk psychological excesses in comparative psychology should not be taken as a critique of the use of intentional predicates in general.
have embraced a more computational, information-processing orientation to learning phenomena such as cue competition effects (e.g., Stout and Miller 2007) and causal prediction (e.g., Blaisdell et al. 2006). Folk psychology has plagued every domain of comparative psychology (Penn and Povinelli 2007a; Penn et al. 2008). Research on animals’ ToM abilities has, however, been held hostage by folk psychology to a degree far beyond any other domain. Effectively, most comparative researchers in this domain are not practicing comparative cognitive psychology but rather “comparative folk psychology”; that is, the study of nonhuman minds from a folk psychological perspective.

The Principles of “Comparative Folk Psychology”

Darwin probably deserves to be credited as the founding father of comparative folk psychology (Penn et al. 2008). He was certainly not the first to interpret animals in an anthropomorphic fashion. Our entire species has an inveterate predilection to interpret the world in anthropomorphic terms (for details, see Dennett 1987). Nor was Darwin the first to emphasize the continuity between human and nonhuman minds (Richards 1987). Darwin gave comparative psychology its modern, scientific justification. According to Darwin, the principles of natural selection necessarily entail that the differences between human and nonhuman minds must be “one of degree and not of kind” (Darwin 1871), and it is this Darwinian formulation that is repeatedly cited by contemporary comparative psychologists to justify describing nonhuman minds by analogy to human ones. Frans de Waal, for example, calls this the principle of “evolutionary parsimony”; that is, if two closely related species act the same, the underlying mental processes are probably the same as well (de Waal 2006).

The methodological principles of comparative folk psychology that have grown out of Darwin’s mistake are simple, well established, and widely practiced:

1. Observe animals in the wild behaving in a clever way or create an experiment that causes animals to behave in a way that appears clever to human observers.
2. Rule out random guessing, innate instinct, and stimulus-bound associative learning as possible explanations for the subjects’ behavior.
3. Point out what humans would be thinking if they were behaving like the subjects in that context.
4. Claim that the subjects are thinking what humans would be thinking or are thinking functionally equivalent thoughts or, at least, possess the “precursors” to those human-like thoughts.
5. Criticize any cognitive explanation of the animal’s behavior that is not consistent with folk psychological intuitions as an example of “behaviorism” or as “unparsimonious,” “rule-bound,” or “unfalsifiable.”

Do Chimps Know What Others Do and Do Not Know?

If there is one recent paper that best exemplifies the principles of comparative folk psychology, it is Hare et al.’s (2001), “Do chimpanzees know what conspecifics know?” Hare et al. (2001) placed two chimpanzees—one subordinate to the other—in separate chambers on either side of a large empty room. On each trial, the subordinate chimpanzee’s door was partially raised while the food was being hidden in one of two cloth bags in the middle chamber. On some trials the dominant chimpanzee’s door was also raised so that he could see where the food was hidden. In other trials, the dominant’s door was kept down and the dominant could not see where the food was placed.

Hare et al. (2001) reported a number of experimental conditions based on this protocol. In only one of these experiments, however, was the critical metric statistically significant (for details, see Penn and Povinelli 2007b, note 3). In the uninformed condition of Experiment 1, the dominant’s door was kept closed while the food was hidden and the subordinate could see that the dominant’s door was closed; in the control condition, the dominant could see where the reward was hidden and the subordinate could see that the dominant was watching. The subordinate “approached” the hidden food more often in the uninformed condition than in the control condition.

Based on this result, Hare et al. (2001:148) concluded that “chimpanzees know what individual groupmates do and do not know; that is, what individual groupmates have and have not seen in the immediate past.” Tomasello, Call, and Hare (2003a) went on to cite this experiment as “breakthrough” (p. 154) evidence that chimpanzees “understand some psychological states in others” (p. 156). Hare et al.’s (2001) protocol launched an entire generation of experiments that claim to show that nonhuman animals understand everything from what others do and do not “hear” to whether others are “willing” or “unable” to perform some act (for reviews, see Call and Tomasello 2008; Hauser and Wood 2009).

Hare et al.’s experiment and ensuing claims are a paradigmatic case of comparative folk psychology in action. First, the authors devise a situation in which chimpanzees appear to act in a clever fashion (i.e., they didn’t approach food when a competitor “knew” that it was there). Second, the authors rule out a stimulus-bound associative explanation (i.e., the subordinate could not see the dominant at the time she made her choice; thus the subordinates’ behavior could not simply result from learning to avoid food in the presence of a competitor). Third, the authors note that a human put in an analogous position would decide whether to approach the food based on what the competitor does and does not “know.” Fourth, the authors make the same mentalistic claims on behalf of chimps.

As Povinelli and Vonk (2003, 2004) pointed out, it is hardly necessary for chimps to reason about the beliefs of their competitors in order to pass this experiment. It suffices for the chimps to reason in terms of the dominants’
How Folk Psychology Ruined Comparative Psychology

Overt behavior in light of the way that other competitors have acted in similar situations in the past: *don’t go after food if a dominant competitor has recently oriented toward food in that location.* Although Povinelli and Vonk’s alternative account requires chimps to reason about abstract relationships with respect to the present and past behavior of their conspecifics—and thus is a fully cognitive and inferential account—it does not require the chimps to represent their competitors’ beliefs or goals qua epistemic, representational states.

Nevertheless, in keeping with the fifth principle of comparative folk psychology’s methodology, Tomasello et al. (2003b) disparaged Povinelli and Vonk’s explanation as an example of “derived behaviorism,” claiming that Povinelli and Vonk’s “behavioral abstraction” hypothesis is “unparsimonious” because it would require animals to learn a large number of “rules.” Those who favor a more human-like interpretation of the chimps’ behavior “clearly have parsimony on their side,” Tomasello and Call (2006) argue. “The number of different explanations required to explain the evidence is sensibly smaller” (see also Call and Tomasello 2008 for a similar critique).

In a chapter aptly titled, “Who Took the Cog out of Cognitive Science,” Perner (2010) points out that Tomasello and Call’s line of argument is an excellent example of the prevailing “anti-cognitivism” in comparative psychology. There is no doubt, of course, that folk psychological explanations are “simpler for us” to understand (Heyes 1998). However, comparing the simplicity of a folk psychological explanation (e.g., “chimpanzees know what others do and do not know”) to the complexity of an algorithmic explanation is like comparing a marketing description of Microsoft Word (e.g., “prints, saves, and edits complex documents”) to a detailed functional specification of the application’s underlying code. The fact that the detailed functional specification runs to thousands of pages and the marketing pitch takes one sentence is not a reasonable metric for comparing the merits of the two descriptions (Penn and Povinelli 2007b). The problem is not that the folk psychological explanation violates Morgan’s infamous canon, but rather the job of comparative cognitive psychology was supposed to be to open up the black box of animal minds to functional and algorithmic specification—not simply reiterate the kinds of explanations the “folk” use.

Notably, Tomasello and Call do not bother to provide their own mechanistic account of the chimpanzees’ behavior. Instead, they eschew any rule-governed explanation of chimpanzee social cognition as being “just a theoretical possibility” for which there is little “concrete evidence.” “It is more plausible,” Tomasello and Call (2006:371) argue, “to hypothesize that apes really do know [emphasis added] what others do and do not see.”

The phrase, “really do know,” is telling. From a comparative folk psychologist’s point of view, rule-governed explanations of any kind feel artificial and reductionist. Here is the crux of the problem: Comparative folk psychologists don’t like the consequences of the cognitive revolution. Any explanation of an animal’s behavior in terms of mechanistic, rule-governed operations seems
to reduce an animal’s thought processes to something less than true “insight.” Animals “really do know” what they are thinking, comparative folk psychologists keep insisting: They have homunculi just like we do.

Do Scrub Jays Have Homunculi as Well?

If comparative folk psychology was only practiced by primatologists studying great apes, there would be less cause for concern. But dozens, if not hundreds, of similar claims have been published over the last decade by prominent comparative psychologists for a wide variety of species, from dogs to scrub jays (Brosnan and de Waal 2003; Call et al. 2003; Flombaum and Santos 2005; Santos et al. 2006; Clayton et al. 2007; Wood et al. 2007; Santos et al. 2007; Emery and Clayton 2008; Hauser and Wood 2009). Folk psychological intuitions have also plagued even the most astute researchers’ hypotheses.

Emery and Clayton, for example, have been responsible for some of the most well-crafted and important work in comparative cognitive psychology over the last decade (for a review, see Emery and Clayton 2009). Yet even they have succumbed at times to their folk psychological intuitions. For example, Emery and Clayton (2001) famously found that young scrub jays only re-cache their food if they have previously pilfered others’ food. Following step #2 of the principles of comparative folk psychology, Emery and Clayton argue that the birds’ behavior cannot be “innate” because “otherwise all scrub jays should re-cache” and they rule out a “simple conditioning explanation” because “the birds never received any positive reinforcement or any punishment for re-caching” (Clayton et al. 2007:519). They cite Humphrey’s (1978) and Gallup’s (1982) work on self-awareness and “experience projection” as examples of how humans might reason in such a situation and suggest that scrub jays might be capable of “experience projection” as well; perhaps scrub jays use their own experiences as a thief to predict how a potential pilferer might think or behave (Clayton et al. 2007:519).

This indeed is an “exciting possibility” (Emery 2004:21), but it is certainly not the only or even the most compelling explanation. For example, all of the birds involved in this experiment had previous experience being pilfered (see discussion in Emery and Clayton 2008). However, Emery and Clayton do not explain why scrub jays have the cognitive prowess necessary to reason by analogy to their own experience as pilferers but do not have the cognitive wherewithal to realize that they should start caching once they have been victims of pilferage themselves. Nor do Emery and Clayton show why “experience projection” is computationally necessary or even, more modestly, why it is the best explanation for the birds’ behavior. Perhaps the experience of pilfering triggers a developmental change in their motivation to re-cache rather than a change in their understanding of why re-caching is strategically advantageous.
(Penn and Povinelli 2007b). There are many other cognitive—but non-folk psychological—explanations that merit consideration.

**Povinelli’s Challenge**

Povinelli and colleagues have challenged comparative researchers to show why representations about unobservable mental states are computationally necessary to account for the social cognitive abilities of nonhuman animals (Povinelli and Vonk 2003, 2004; Penn and Povinelli 2007b, 2011). Perner (2010) has termed this “Povinelli’s challenge.” In a chapter entitled, “How To Build a Scrub Jay That Reads Minds,” Emery and Clayton (2008) provide the most extensive critique to date of Povinelli’s challenge, thus providing an excellent opportunity to understand why Povinelli’s challenge has been so unpopular and unconvincing.

For example, Emery and Clayton (2008:89) claim that Povinelli’s hypothesis is incapable of generalizing to novel behaviors or individuals:

> [Behavior reading] does not allow one to accurately predict the future behavior of an unknown conspecific, as behavior reading is based on computing statistical regularities over the course of a relationship between two individuals.

Although an explanation based on associative learning might be limited to associations between specific individuals, this is certainly not a limitation of all accounts of behavior reading and is clearly not a limitation of Povinelli’s account in particular. Povinelli and colleagues have repeatedly emphasized that animals form “behavioral abstractions” about classes of behaviors and animals and generalize these abstractions to novel situations in an inferentially coherent fashion (Povinelli et al. 2000; Povinelli and Vonk 2004; Penn and Povinelli 2007b). Povinelli’s account seems unconvincing because Emery and Clayton have underestimated its inferential flexibility.

Emery and Clayton acknowledge that Povinelli’s “behavior-reading” hypothesis can account for the results of Hare et al.’s (2000; 2001) experiments and even for certain experiments conducted with scrub jays (e.g., Dally et al. 2005). They argue, however, that other cases are not so easily handled by a “Povinellian [sic]” explanation. For example, Dally et al. (2004) showed that scrub jays prefer to cache food in darker locations rather than lighter locations when being observed by potential pilferers. Emery and Clayton (2008) propose the following “mentalistic” sketch of the scrub jays’ behavior:

1. The storer is in the presence of an observer with two caching trays; one in bright light, one in the dark.
2. The tray in the light is easier to <see> than the tray in the dark, and thus easier to pilfer from.
3. The storer caches in the dark tray (because the observer cannot <see> the caches as clearly as those made in the bright tray).
According to Emery and Clayton, a Povinellian explanation would look like this:

1. The storer is in the presence of an observer with two caching trays; one in bright light, one in the dark.
2. The tray in the light is more visible.
3. The storer caches in the dark tray (because observers do not pilfer as successfully from dark trays).

Emery and Clayton (2008:89) argue that the Povinellian-style explanation is less plausible because without the mentalistic clause that light trays are easier to see the storer would have no reason to infer that it should store food in the dark tray, and the storer’s behavior would have to result from a “series of very flexible conditional rules.”

Here Povinelli’s account seems unconvincing because folk psychological explanations seem so much more “intuitive” and “parsimonious” than rule-governed accounts. But the cognitive revolution in psychology was founded on the premise that all cognitive processes result from rule-governed operations and that cognizers don’t need to understand anything about why these rules work in order to be “rational” or “intelligent” (Kacelnik 2006). As Dennett quipped in reviewing Alan Turing’s contribution to the cognitive revolution: Turing demonstrated that “to be a perfect and beautiful computing machine, it is not requisite to know what arithmetic is” (Dennett 2009:10,061). In other words, Turing showed that computers don’t need homunculi, so neither do brains.

And neither do scrub jays. There is no reason that evolution could not have designed scrub jays to cache food in darker areas (and behind barriers and as far away as possible from thieving conspecifics) when being observed by potential pilferers without thereby endowing scrub jays with any understanding of why these devious acts are effective. The parenthetical “because” clauses in Emery and Clayton’s sketch are unnecessary. Scrub jays don’t need homunculi any more than we do.³

**How to Build a Scrub Jay That Thinks Like a Scrub Jay**

So let us imagine a rosier future in which comparative psychologists decide to abandon folk psychological explanations and study nonhuman social cognition from a cognitive perspective. How might a comparative cognitive psychology of nonhuman social cognition proceed?

The place to start, I think, is with corvids. Thanks in large part to the work of Clayton, Emery and their colleagues, we have a richer and more robust body of data about the social cognitive abilities of scrub jays than we do for just about any other nonhuman species (for reviews, see Clayton et al. 2007;...
Emery and Clayton 2009). Scrub jays have turned out to be mavens of social interactions within the context of food caching. They also have the inimitable advantage of not looking anything like cute human children. Thus, there is every reason to hope that the study of corvid social cognition might lead the rest of comparative psychology out of its folk psychological quagmire and into a more cognitively grounded future.

The first and most important step would be to define the functional specifications that any plausible model of corvid social cognition must fulfill. For example, we know that scrub jays not only remember the “what,” “where,” and “when” information associated with a very large number of discrete caching events in the past, they also remember “who” was present during each caching event. We know that scrub jays select which food to re-cache based on who is present and whether a competitor who is currently present was also present during particular caching events in the past. In addition, we know that, when given a choice, experienced scrub jays prefer to cache food farther rather than nearer to potential pilferers, in darker rather than lighter areas, and behind barriers rather than out in the open.

Now this short list of scrub jay cognitive features barely scrapes the surface of the scrub jay cognitive feature set. Yet already it is obvious that scrub jays require a quite remarkable cognitive architecture. Clearly the scrub jay is not only capable of representing and keeping track of the particular features of particular objects (e.g., the spatiotemporal and physical attributes associated with each cache), but also of encoding the relation between particular constituents (e.g., “who” was present for “what” caching event) such that common inferences can be made across similar relations that have novel constituents (e.g., for any S and C, <if S was present when C was cached, re-cache C when S is absent>). To put this in Emery and Clayton’s (2008:89) terms, scrub jays do indeed possess “a series of very flexible conditional rules which can be applied to different individuals across a variety of different but particular contexts.”

Some basic principles of learning together with species-typical behavior and situation-specific predispositions clearly play an important role in explaining how these rules are acquired, and Shettleworth (2010a) is right to castigate comparative psychologists for glossing over these general learning mechanisms too quickly. But associations alone won’t suffice to explain how the birds encode, update, and use the relevant information. For example, not only are scrub jays updating their representations of who was present for what caching event on a one-shot basis without any immediate reinforcement from the environment, they are keeping track of the relation between numerous cache sites and various potential pilferers in a compositional fashion. In other words, given Obs(S,C)—the “observing” relation between subject S and cache site C—scrub jays are somehow capable of encoding and using Obs(S₁, C₁) without confounding this representation with other similar but distinct relations such as Obs(S₁, C₂), Obs(S₂, C₁), and Obs(S₂, C₂). This may seem like a trivial cognitive feat from a folk psychological perspective, but as connectionists have
repeatedly discovered over the last quarter-century, associatively structured representations have dramatic computational limitations, not the least of which being their susceptibility to “catastrophic interference” when updating similar but distinct representations such as these (for a review of the challenges faced by connectionist models of learning, see Shanks 2005).

Moreover, the ability to act on the compositional relation between numerous cache sites and competitors in an adaptive fashion would be literally unthinkable (i.e., computationally infeasible) without some mechanism for encoding the relation between particular constituents of a representation such that when overlapping constituents have different relationships to each other, the fact that these are different relations is somehow manifest in the structural differences between the representations. That is, the relation \( \text{Obs}(S,C) \) must be distinct from the relation \( \text{Cached}(S,C) \) such that scrub jays don’t confound the fact that \( S_1 \) observed food being cached in \( C_1 \) with the fact that \( S_1 \) subsequently cached food in \( C_2 \) or that \( S_2 \) had previously observed food being cached in \( C_1 \). Scrub jays are able to form syntactically structured representations about the “what,” “when,” “where,” and “who” properties associated with concrete caching events in the past as well as about the abstract statistical regularities that hold across similar relations with different constituents. Horgan and Tienson (1996) argue that this is all it should take in order for a representational system to qualify as “syntactically structured,” and I agree.

Scrub jays are hardly the only relationally intelligent creature on the planet. There are baboons as well, who, as Bergman et al. (2003) have shown, are perfectly capable of keeping track of various kinds of relations between their conspecifics such that rank reversals between family members do not elicit the same response as rank reversals between matriline (see also Silk 1999, 2003b). There are domestic dogs who can keep track of the relation between a “pointing” gesture and a distal object regardless of the featural properties of the particular object being pointed at (Soproni et al. 2002). There are, undoubtedly, thousands of other less well-studied species who deserve mention as well. Thus, as I have argued in the past (Penn et al. 2008; Penn and Povinelli 2009, 2011), nonhuman animals are clearly capable of encoding, updating, and using abstract relational representations in a flexible and adaptive fashion.

Picking out the causally relevant relations amidst all the salient but spurious correlations in the world requires cognitive mechanisms substantially more sophisticated than those postulated by associative models of learning (Penn and Povinelli 2011). As Clark and Thornton (1997) put it, relations are “representation hungry” and biological cognizers require an entire panoply of top-down heuristics, tricks, and ploys to circumvent the limitations of uninformed statistical learning. There are still innumerable unanswered questions about the heuristics scrub jays employ to recognize and encode the relations that matter to them and about how learning modulates the birds’ relational abilities. I suspect that if Clayton and Emery sat down and compiled a comprehensive specification of what we currently know about corvid social cognition,
the resulting document would set off a nuclear explosion in cognitive science by ruling out almost all existing connectionist and associative architectures as plausible models of animal cognition. Only those cognitive architectures capable of approximating the first-order relational features of a physical symbol system will be left as plausible representational-level models of the mind (Penn et al. 2008). Even the best of these (e.g., Hummel and Holyoak 2003, 2005) would have great difficulty, I suspect, in replicating the intelligence of a scrub jay or a baboon.

At the same time, the exercise of compiling a functional specification of corvid social cognition could also expose the speciousness of comparative folk psychology’s principle claims. Once one has defined the extensive vocabulary of first-order relational representations managed by the corvid mind, there will be nothing left for representations about mental states to do. And there will no longer be any need to claim that scrub jays “really know” what they are doing.