between competing theories can be identified and tested.

The total focus on rational inference that characterizes Bayesian Fundamentalism is especially unfortunate from a psychological standpoint because the updating of beliefs entailed by Bayes' Rule is psychologically trivial, amounting to nothing more than vote counting. Much more interesting are other aspects of Bayesian models, including the algorithms and approximations by which inference is carried out, the representations on which those algorithms operate (e.g., the parameters of conjugate priors), and the structured beliefs (i.e., generative models) that drive them. The Enlightened Bayesian view takes these seriously as psychological constructs and evaluates them according to theoretical merit rather than mathematical convenience. This important shift away from Bayesian Fundamentalism opens up a rich base for psychological theorizing, as well as contact with process-level modes of inquiry.

It is interesting to note that economics, the field of study with the richest history of rational modeling of behavior and the domain in which rational theories might be expected to be most accurate, has increasingly questioned the value of rational models of human decision-making (Krugman 2009). Economics is thus moving away from purely rational models toward theories that take into account psychological mechanisms and biases (Thaler & Sunstein 2008). Therefore, it is surprising to observe a segment of the psychological community moving in the opposite direction. Bayesian modeling certainly has much to contribute, but its potential impact will be much greater if developed in a way that does not eliminate the psychology from psychological models. We believe this will be best achieved by treating Bayesian methods as a complement to mechanistic approaches, rather than as an alternative.

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NOTES

1. Formally, $E_{\text{posterior}}$ equals the logarithm of the posterior distribution, E_{prior} is the logarithm of the prior, and $E_{data}(H)$ is the logarithm of the likelihood of the data under hypothesis H. The model's prediction for the probability that hypothesis H is correct, after data have been observed, is proportional to $\exp[E_{\text{posterior}}(H)]$ (cf. Luce 1963).

2. Bayesian analysis has been used to interpret neural spike recordings (e.g., Gold & Shadlen 2001), but this falls outside Bayesian Fundamentalism, which is concerned only with behavioral explanations of cognitive phenomena.

3. Note that we refer here to Bayesian models that address behavior, not those that solely aim to explain brain data without linking to behavior, such as Mortimer et al.'s (2009) model of axon wiring.

Open Peer Commentary

Evolutionary psychology and Bayesian modeling

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Abstract: The target article provides important theoretical contributions to psychology and Bayesian modeling. Despite the article's excellent points, we suggest that it succumbs to a few misconceptions about evolutionary psychology (EP). These include a mischaracterization of evolutionary psychology's approach to optimality; failure to appreciate the centrality of mechanism in EP; and an incorrect depiction of hypothesis testing. An accurate characterization of EP offers more promise for successful integration with Bayesian modeling.

Jones & Love (J&L) provide important theoretical contributions to psychology and Bayesian modeling. Especially illuminating is their discussion of whether Bayesian models are agnostic about psychology, serving mainly as useful scientific and mathematical tools, or instead make substantive claims about cognition.

Despite its many strengths, the target article succumbs to some common misconceptions about evolutionary psychology (EP) (Confer et al. 2010). The first is an erroneous characterization of EP's approach to *optimality and constraints*. Although the article acknowledges the importance of constraints in evolutionary theory, it lapses into problematic statements such as "evolutionary pressures tune a species' genetic code such that the observed phenotype gives rise to optimal behaviors" (sect. 5, para. 3). J&L suggest that evolutionary psychologists reinterpret behavioral phenomena as "optimal" by engaging in a post hoc adjustment of their view of the relevant selection pressures operating in ancestral environments.

These statements imply that a key goal of EP is to look for optimality in human behavior and psychology. On the contrary, the existence of optimized mechanisms is rejected by evolutionary psychologists, as this passage from Buss et al. (1998) illustrates:

> [T]ime lags, local optima, lack of genetic variation, costs, and limits imposed by adaptive coordination with other mechanisms all constitute major constraints on the design of adaptations. . . . Adaptations are not optimally designed mechanisms. They are . . . jerry-rigged, meliorative solutions to adaptive problems . . ., constrained in their quality and design by a variety of historical and current forces. (Buss et al. 1998, p. 539)

J&L argue that "it is not [simply] any function that is optimized by natural selection, but only those functions that are relevant to fitness" (sect. 5, para. 4). We agree with the implication that psychologists must consider the fitness-relevance of the mechanisms they choose to investigate. Identifying adaptive function is central. Nonetheless, natural selection is better described as a "meliorizing" force, not an optimizing force (see Dawkins 1982, pp. 45–46) – and thus even psychological mechanisms with direct relevance to fitness are not optimized. As J&L correctly note elsewhere, selection does not favor the best design in some global engineering sense, but rather features that are *better* than competing alternatives extant in the population at the time of selection, within existing constraints (Buss et al. 1998; Dawkins 1982).

Despite occasional problems with the target article's depiction of EP's views on optimality, we fully agree with J&L that (a) adaptationist accounts place significant constraints on explanation, (b) evolution proceeds by "survival of the best current design, not survival of the globally optimal design" (sect. 5.3, para. 3), (c) human cognition is not optimally designed, and (d) the "rational program" in Bayesian modeling has an overly narrow focus on optimally functioning adaptations.

J&L present a partly accurate and partly inaccurate characterization of the relevance of *mechanism* in evolutionary approaches. They correctly acknowledge the importance of elucidating the specific mechanistic workings of adaptations. However, the target article compares EP to Bayesian Fundamentalism and Behaviorism by claiming that all three approaches eschew the investigation of mechanism. We disagree with this latter assessment.

In our view, it is difficult or impossible to study function without investigating form or mechanism. The central logic of adaptationism makes the inextricable link between form (or mechanism) and function clear: An adaptation must necessarily be characterized by a good fit between form and function – between an adaptation and the adaptive problem it was "designed" to solve .The key point is that evolutionary approaches to psychology *necessarily* involve the joint investigation of mechanism and function. Evolutionary psychology generates hypotheses about "design features," or particular mechanistic attributes, that adaptations either must have or might have in order to successfully solve the adaptive problems that they evolved to solve. Indeed, mechanism is one of Tinbergen's (1963) four explanatory levels – mechanism, ontogeny, function, and phylogeny. Ideally, all should be analyzed in order to achieve a complete understanding of any behavior or psychological phenomenon, and all are central to core aims of EP. Of course, not every scientist explores all four questions; every empirical study has delimited aims; and the field is certainly far from a complete understanding of all of the design features of any mechanism, whether it be the human visual system or incest-avoidance adaptations.

As a single example of mechanistic EP research, adaptationist analyses of fear have uncovered social inputs that elicit the emotion, nonsocial inputs that trigger the emotion, the adaptive behavioral output designed to solve the problem, the perceptual processes involved in detecting threats and reacting fearfully, the developmental trajectory of human fears, and the physiological and endocrinological mechanisms driving the fear response (see, e.g., Bracha 2004; Buss 2011; Neuhoff 2001; Öhman et al. 2001). Analogous progress has been made in understanding other evolved mechanisms, such as mating adaptations, perceptual biases, and adaptive social inference biases (Buss 2011).

Most human adaptations are only just beginning to be subjected to scientific investigation, and many mechanistic details have certainly not yet been elucidated. EP could profitably increase its use of formal mechanistic modeling in this endeavor. Fusing the strengths of mathematical and computational modelers with those of evolutionary psychologists would enrich both fields.

Finally, the target article depicts EP as occasionally falling into "backward-looking" hypotheses (sect. 5.2, para. 3) or engaging in "just so" storytelling (sect. 5.2, para. 1; Gould & Lewontin 1979). By this, the authors mean that evolutionary psychologists sometimes note a behavior or psychological mechanism, and then construct a conceivable function for it and simply stop there. We agree with J&L that this practice would be highly problematic if it were the end point of scientific analysis.

Fortunately, leading work in EP proceeds using both the *forward* method in science (theory leads directly to hypothesis, which then leads to empirical predictions, which are then tested) as well as the *backward* method (observed phenomenon leads to hypothesis, which in turn leads to novel empirical predictions, which are then tested) (see Buss 2011; Tooby & Cosmides 1992). Much of evolutionary psychology uses the forward method, and here it is not even *possible* to level the "just-so story" criticism. When evolutionary psychologists employ the backward method, they typically avoid the problem by taking the additional necessary step of deriving *novel* and *previously untested* predictions from the hypothesis (for numerous examples, see Buss 2011). We concur with the implication that there are better and poorer practitioners of the rigors of science, and that all should be held to the highest standards for more rapid progress.

In sum, we view an accurately characterized modern evolutionary psychology as largely avoiding the conceptual pitfalls J&L note, and we look forward to a richer and more successful integration of Bayesian modeling and evolutionary psychology.

The myth of computational level theory and the vacuity of rational analysis

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Abstract: I extend Jones & Love's (J&L's) critique of Bayesian models and evaluate the conceptual foundations on which they are built. I argue that: (1) the "Bayesian" part of Bayesian models is scientifically trivial; (2) "computational level" theory is a fiction that arises from an inappropriate programming metaphor; and (3) the real scientific problems lie outside Bayesian theorizing.

The Bayesian framework asserts that problems of perception, action, and cognition can be understood as (approximations to) ideal rational inference. Bayes' rule is a direct consequence of the definition of conditional probability, and is reasonably captured as the simple "vote counting" procedure outlined in the target article by Jones & Love (J&L). This is clearly not where the interesting science lies. The real scientific problems for a Bayesian analysis arise in defining the appropriate hypothesis space (the "candidates" for whom votes will be cast), and a principled means of assigning priors, likelihoods, and cost functions that will, when multiplied, determine the distribution of votes (and the ultimate winner[s]).

Bayesian models of cognition begin by asserting that brains are devices that compute, and that it is possible to dissociate *what* they compute from *how* they compute. David Marr's (1982) now infamous dissociation of the computational, algorithmic, and implementation "levels of analysis" is usually invoked to justify this belief, and inspires attempts to "reverse engineer" the mind (Tenenbaum et al. 2011). It is no coincidence that Marr's levels resemble the stages of someone writing a computer program, which are granted some (unspecified) form of ontological status: A problem is defined, code is written to solve it, and a device is employed to run the code. But unlike the computational devices fashioned by man, the brain, like other bodily organs, emerged as the consequence of natural processes of self-organization; the complexity of its structure and function was not prescribed in some top-down manner as solutions to pre-specified computational problem(s). The only "force" available to construct something ideal is natural selection, which can only select the best option from whatever is available, even if that is nothing more than a collection of hacks. As for the "computational level" theory, it is far from evident that brains can be accurately characterized as *performing* computations any more than one can claim that planets compute their orbits, or rocks rolling down hills compute their trajectory. Our formal models are the language that we use to try to capture the causal entailments of the natural world with the inferential entailments embodied in the formal language of mathematics (Rosen 1991). The assertion