Theory Evaluation in Evolutionary Psychology

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Contemporary philosophy of science is an important source of scientific methodology that is frequently ignored by psychologists in their research deliberations. Ketelaar and Ellis's (this issue) constructive use of resources in the philosophy of science to evaluate the worth of evolutionary psychology is, therefore, to be welcomed. We agree with the authors that the charge that evolutionary theories and hypotheses are unfalsifiable involves an untenable appeal to an essentially Popperian conception of scientific method. Ketelaar and Ellis's deployment of Lakatos's (1970) methodology of scientific research programs enables them to transcend important limitations of Popper's philosophy of science and effectively turn the tables on the unfalsifiability charge.

Although Ketelaar and Ellis's depiction of evolutionary psychology as a progressive research program, in the Lakatosian sense, makes clearer the achievements and the promise of that program, we think that there are additional methodological considerations that should be brought to bear on the assessment of the worth of evolutionary psychological research. In this commentary, we suggest that the Lakatosian methodology of scientific research programs emphasizes the importance of prediction in theory evaluation at the expense of explanation. To illustrate this point, we discuss an important case study in 20th-century science in which prediction figured less prominently than other evaluative criteria. We then suggest that the worth of evolutionary psychological theory can be appropriately appraised in terms of what philosophers of science call *inference to the best explanation.* Finally, we indicate how inference to the best explanation can be used to appraise theories in evolutionary psychology by briefly considering the view that language is a biological adaptation.

On Evidential Adequacy

The claims that evolutionary explanations in psychology are both untestable and unfalsifiable are frequently aired complaints. In their target article, Ketelaar and Ellis focus predominantly on the charge of unfalsifiability. They make two clear and important points in response to this criticism. The first is that research programs themselves are not directly amenable to falsification. Instead, attempts at falsification should be directed at specific auxiliary hypotheses, not at the hard-core assumptions of a research program. The second is that the generation of multiple evolutionary hypotheses in a given domain follows a legitimate strategy of inquiry in science, indeed one that is essential for scientific progress to occur. The claim that evolutionary explanations are untestable in nature receives less attention from Ketelaar and Ellis. Although they make it clear that specific evolutionary hypotheses have different empirical consequences, the basis of the untestability charge seems to be grounded in what critics see as the problematic nature of adaptation explanations in psychology. The concept of adaptation is necessarily a historical one (Brandon, 1990; Wright, 1973). Critics of adaptation explanations in psychology (e.g., Lewontin, 1990; Richardson, 1996) suggest, therefore, that the appropriate evidence required to demonstrate adaptation will prove difficult to accumulate, and in many cases will simply not be forthcoming. That is, because we cannot directly observe natural selection in operation, and thus cannot gather the relevant data concerning genetic differences, reproductive success, and so forth, we can never unequivocally demonstrate that any given characteristic really is an adaptation and hence amenable to an evolutionary explanation.

This line of criticism, we suggest, reflects a commitment to antirealist, specifically empiricist, scientific values. Empiricists (e.g., van Fraassen, 1980) argue that we cannot reliably evaluate claims about unobservable entities and processes. Because all claims that putative psychological characteristics are adaptations are grounded in unobservable selective forces operating in the distant past, we cannot assess the merits of such claims. Thus, it is suggested that all evolutionary explanations in psychology are, and will remain, squarely in the realm of plausible storytelling. Because of this, empiricist critics of evolutionary psychology suggest that we should remain in a position of epistemic doubt regarding the evaluation of adaptation explanations in psychology. They claim that we can never be sure that any putative characteristic really is an adaptation, and further, that we can never be certain that any given adaptation explanation is the correct one. We suggest that a scientific methodology that centers on explanation, rather than prediction, provides a more fruitful way of addressing both the unfalsifiability charge that Ketelaar and Ellis focus on, as well as the broader issue of evidential adequacy.

Deemphasizing Prediction

Although Ketelaar and Ellis claimed that evolutionary psychology, and science more generally, evaluate competing theories on the basis of both their explana-

tory and predictive power, they actually give more attention in their article to the predictive successes of evolutionary psychology. This is not surprising, for in adopting Lakatos's philosophy of science, they thereby endorse a modified hypothetico-deductive conception of inquiry that depicts theory evaluation principally as a matter of testing for empirical adequacy by deriving test predictions that are borne out by the relevant data. Although Lakatos claimed that the explanatory power of a theory is important, his key evaluative criteria of empirical and theoretical progressiveness clearly have to do with prediction: A research program is theoretically progressive if it predicts one or more novel facts; it is empirically progressive if at least one of these novel predictions gets confirmed.

This emphasis on the importance of prediction in theory evaluation is clearly widespread in scientific methodology, but it has probably been exaggerated. A number of recent case studies in the history of science have pointed out that, for scientists, the successful predictions of new facts do not necessarily provide better evidence for a theory than cases in which those predicted facts were already known. For example, Brush (1989) showed that the commonly held view that Einstein's successful prediction of the gravitational bending of light provided strong confirmation of his general theory of relativity was shared neither by Einstein nor the majority of scientists of his time. Einstein (and other physicists) have maintained that the coherence and simplicity of the theory were more important criteria for its acceptance than the relevant empirical tests. Brush noted, further, that it is common in science, particularly in physics, to take predictive success to cover both the deduction of previously known facts as well as the confirmed prediction of new facts, suggesting that the novelty of a prediction is not an important factor in gauging its evidential worth. With respect to the general theory of relativity, the successful deduction of Mercury's known orbit was widely considered to be just about as good a source of evidence as the novel prediction of light bending. Brush concluded that the primary value of a successful novel prediction, when compared with the deduction of a known fact, is to provide favorable publicity for a theory. Such was the additional value of the light bending forecast for general relativity theory.

It is interesting to note that the successful prediction of a new empirical phenomenon can sometimes be taken as less secure evidence for a theory than the successful deduction of an existing fact, just because of its novelty! It is widely recognized that a scientific fact can normally be plausibly explained by more than one theory. Thus, the discovery of a new fact is likely to result in efforts to construct alternative plausible explanations to the explanation offered by the theory that sponsored the relevant novel prediction. In the case of general relativity theory, 10 years of unsuccessful efforts to provide a better explanation of the light bending phenomenon passed before Einstein's supporters could convincingly assert that their theory provided the best explanation (Brush, 1989).

Taking Explanation Seriously

To counter the charge that evolutionary psychology trades in "just so" stories, Holcomb (1996) recently suggested that evolutionary psychology, and science more generally, evaluate theories by employing the method of inference to the best explanation. We agree with Holcomb that this important form of inference provides the appropriate perspective for understanding how scientists go about accepting explanatory theories in science. However, we have some reservations about his understanding of inference to the best explanation. Holcomb suggested that a theory that best explains the facts is true. In contrast, we think judgments of the best explanation only provide grounds for theory acceptance. Also, consistent with our aforementioned remarks on prediction, we think that Holcomb's suggestion that the demand for novel testable predictions normally regulates inferences to the best explanation is inappropriate. Finally, Holcomb chose not to refer to the growing literature on inference to the best explanation and he has consequently failed to give us an informative account of its actual nature.

One important account of inference to the best explanation, which avoids the limitations just noted, was recently developed by Thagard (1992). Thagard took inference to the best explanation to be centrally concerned with establishing explanatory coherence. Explanatory coherence occurs when the propositions hold together because of their explanatory relations. According to Thagard's model, the explanatory coherence of a theory is determined by considering three criteria: explanatory breadth, simplicity, and analogy. Explanatory breadth, which is the most important criterion, captures the idea that a theory is more explanatorily coherent than its rivals if it explains a greater range of facts. The notion of simplicity is also important for theory choice, and is captured by the idea that preference should be given to theories that make fewer special assumptions. With the third criterion, analogy, explanations are judged more coherent if they are supported by analogy to theories that scientists already find credible. Thagard's theory of explanatory coherence provides an integrated account of multiple criteria that are constitutive of explanatory goodness.

It is worth pointing out here that those Lakatosian hard-core commitments of evolutionary psychological research programs that are taken directly from Darwinian evolutionary theory are better thought of as being justified by appeal to considerations of explanatory co-

herence rather than predictive success. In his analysis of major conceptual revolutions in science, Thagard (1992) argued that Darwin defended his theory of evolution by natural selection not so much in terms of its empirical adequacy as its explanatory power. He demonstrated that Darwin's detailed justification for adopting his evolutionary theory in favor of the rival creationist theory was based on an assessment of its greater explanatory coherence. For Darwin, the ability of his theory to explain a large range of different facts in an economical manner, to exploit the analogy between artificial and natural selection, and to apply Malthusian principles, were all important features of its explanatory power.

Explanatory Coherence and the Evolution of Language

The role of explanatory goodness can be illustrated by considering claims about the origins of language. In this regard, two questions immediately arise. First, can we reasonably make the claim that complex language should be considered a psychological adaptation? And second, even if we accept that language is an adaptation, is it possible to make reasonable choices among competing evolutionary accounts? Ultimately, the claim that language is an adaptation is, as critics such as Richardson (1996) are eager to point out, not directly amenable to empirical testing. In the absence of time-travel technology, we cannot gather the relevant data concerning, among other things, the relative reproductive success of language users compared to nonlanguage users. That is, we cannot unequivocally demonstrate that language is a direct product of natural selection. Indeed, a number of prominent authors have suggested that language is best not considered an adaptation, but instead is a by-product of other evolutionary forces, which have led to an increase in brain size, and thus can be explained in terms of general laws of structure and growth (e.g., Chomsky, 1972; Gould, 1991; Piatelli-Palmarini, 1989). By contrast, Pinker (1994; Pinker & Bloom, 1990) argued that language is an excellent candidate for a biological adaptation. How are we to evaluate these alternative positions?

Rather than focusing on the direct testing of these alternative accounts, or examining their relative ability to generate novel predictions, we argue for the application of a multicriterial approach to theory appraisal. Thagard's (1992) theory of explanatory coherence is a methodology that enables us to decide which is the best explanatory theory to accept. That is, we suggest questions of explanatory coherence come to the fore in evaluating alternative theoretical proposals in evolutionary psychology.

Biological adaptations can be reliably identified by a range of specific features. In general, adaptations will be species typical, mesh with relevant environmental parameters, and be characterized by special design features such as economy, efficiency, complexity, precision, specialization, and reliability (Tooby & Cosmides, 1990; Williams, 1966). Pinker and Bloom (1990) made a good case that human language possesses these characteristics indicative of biological design. The complexity of interrelated mechanisms, which mesh together to achieve the unitary function of communication, strongly suggests that language was specifically designed by natural selection for this function. The claim that language is an adaptation explains the coordination of mechanisms underlying articulate vocal control, the auditory processing of speech, syntax, and the specific physiological features of the vocal tract. The strength of the claim here is partly a result of the resemblance of the nature of language to other putative biological adaptations, such as the eye. In this case, both share similar characteristics, such as the coordination of multiple structures to achieve unitary function. Thus, the explanatory coherence of the adaptation hypothesis gains force partly by analogy to other adaptations that possess similar features. The universal character of language, its lack of correlation with cultural or technological progress, and its reliable emergence in the absence of formal tuition are also explained by the suggestion that language is the direct product of natural selection (Pinker, 1994; Pinker & Bloom, 1990).

By contrast, the suggestion that language has a nonselectionist origin suffers in terms of its overall explanatory coherence. First, language does not seem to fit the profile of biological characteristics that are by-products of natural selection, or are idiosyncratic products of culture. Second, nonselectionist accounts fail to adequately explain the full range of phenomena that selectionist accounts can; that is, they have less explanatory breadth. Third, nonselectionist accounts need to invoke multiple explanatory accounts to address the different features of language such as articulate vocal control, syntax, and auditory processing, and suffer as a consequence in terms of simplicity of explanation.

A shift in emphasis from the central role of prediction to a fuller range of evaluative criteria allows us to more clearly assess specific claims made about the evolutionary origins of psychological characteristics. It is certainly true that we have nothing like a complete adaptation explanation for language. We cannot even demonstrate that language is the product of natural selection. However, the fallibility of this claim should not engender a state of epistemic equivocation, for the suggestion that language is an adaptation is our best currently available explanation for its existence.

Having made a strongly plausible case that language is an adaptation leads us to an additional epistemic concern, one that is the focus of the article by Ketelaar and Ellis. Although a strong case can be made

that language is an adaptation, there are multiple alternative explanations for how language has evolved. That is, we need to address the problem of evaluating alternative accounts of language evolution. As emphasized by Lakatos (1970) and Thagard (1992), theory appraisal is an inherently comparative affair. In recent years there has been a proliferation of theories concerning the evolution of language.

Although most accounts assume that language is an adaptation, they differ in terms of explaining why language evolved, when it evolved, and how it evolved (compare, e.g., the different accounts offered by Carstairs-McCarthy, 1999; Corballis, 1999; and Dunbar, 1996). Space precludes providing a detailed comparative analysis of these alternative accounts here. However, we suggest that a focus on a broad array of evaluative criteria is essential if we are to further our understanding of language evolution. Although it is the case that alternative theories of language origin make specific predictions about characteristics of language, these often prove of limited value in evaluating alternative perspectives. For example, Dunbar (1996), in his account of language evolution, argued that language arose as a form of vocal grooming, and ultimately as a means of exchanging social information. On this basis, Dunbar predicted that gossip should be the prominent content of language exchange in contemporary human populations. This prediction is borne out by the evidence; however, consistent with the historical examples presented earlier, this does little to generate favor for Dunbar's theory over alternative accounts. This is partly because there are many alternative potential explanations for the prominence of gossip in human discourse, and partly because there are so many lines of evidence that are relevant in assessing alternative theories.

Our best theory of language evolution will be the one that provides the most explanatorily coherent account of the diverse range of phenomena that bear on issues relating to the origin of language. The various lines of evidence that are relevant in this context include paleontological data regarding the brain size of early hominids, the emergence of bipedalism, and changes in the vocal tract; archeological evidence concerning tool use and other aspects of material culture; neurological evidence relating to language function and dysfunction; comparative evidence concerning the communicative capacities of other species; linguistic evidence regarding the structure of syntax, phonology, and so forth; and developmental data about the way language is acquired in childhood. In addition, theories of language origin need to be consistent with the basic principles of evolution and be coherent with a host of relevant theories across a diverse range of disciplines.

The sheer diversity of relevant lines of evidence may forestall any theory emerging clearly as the most explanatory coherent one. Moreover, there are various disputes concerning how some of the relevant evidence should be construed. However, a degree of explanatory pluralism should be considered an epistemic virtue in the sense that it generates a constant pressure on theories to develop increasingly coherent accounts of the relevant phenomena.

Conclusion

Ketelaar and Ellis's target article has the merit of steering a defensible course between the unacceptable extremes of weak statistical significance testing and strong falsificationism. In dealing more broadly with issues of evidential adequacy than is found in Lakatos's methodology of scientific research programs, our commentary is not intended to downplay the important contribution made Ketelaar and Ellis, or to suggest that evolutionary psychology is irremediably beset with methodological problems. Rather, we believe a more detailed consideration of some of the central criticisms of evolutionary psychology suggests the fruitfulness of an expanded approach to evaluating evolutionary explanations in psychology; one that moves beyond a preoccupation with prediction toward a multicriterial approach to theory appraisal with explanatory coherence at its center. We are heartened by the previous discussion in this journal of Meehl's (1990) neo-Lakatosian proposal for appraising theories in psychology, which makes clear that a concern with predictive accuracy need not rule out appeal to other evaluative criteria. Ultimately, the worth of evolutionary psychology as a research program will be determined, like other research programs in science, by its overall explanatory coherence.

Note

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Lakatos Meets Evolutionary Psychology, or Does He?

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It is easy to underestimate the difficulty of what Ketelaar and Ellis (this issue) are doing. Consequently, readers of their target article should temper any misgivings about what Ketelaar and Ellis have accomplished with an appreciation of the difficulty of the task. What does it take to apply philosophy of science to evolutionary psychology? One needs to know philosophy of science. One needs to know evolutionary psychology. Then one has to find ways of connecting them. In my own work, I have found it very hard to identify a connection without tinkering with the particular philosophy of science to be applied; one ends up constructing the connection itself. The trick is to search among the plethora of possible connections for a connection that is illuminating. My focus is on the factors involved in deciding whether their method of making the connection pays philosophical dividends.

I agree with much in their effort to clarify the inferential structure of the field, stressing the multiple levels of explanation Buss pointed out. To evaluate the performance of evolutionary psychology, we need to understand how inferences across levels of generality are used in both explanation and deciding the significance of test results (Holcomb, 1998). The criteria of validation for scientific explanations are to be clarified by elucidating the multilevel structure of science (Holcomb, 1993). Learning about arguments used to justify revision, at one level of generality rather than another, does take the wind out of a common way of understanding the unfalsifiability objection. Evolutionary psychology has suffered from the relative rarity of predicting things we did not expect from our background knowledge or preexisting psychology as compared to the common practice of predicting the things we think we know. The answer to antiadaptationist charges is improved methodology (Holcomb, 1996a, 1996b). As Lakatos advised, the more novel predictions made and verified, the better.

Ketelaar and Ellis have clearly drawn on my work. Two key differences emerge. One is my use of a fine-grained distinction between theories, research programs, and conceptual frameworks to capture the structure of research. Conceptual frameworks tie research programs to facts by redescribing the phenomena to be explained in terms that make them relevant to a research program; for example, the family must be reconceived via geneological versus collateral kin before it can be explained by evolutionary theory. The other is my advocacy of inference to the best explanation of known facts and test results as an epistemology, in response to the failure of deductive and inductive logic to determine the probability of an explanation given the data (the idea that science must be falsifiable presupposes a deductivist approach to validation).

I could rewrite the authors' target article using my model of science and say much the same things about research structure as the authors do in my favored terminology, but my epistemological conclusions would be different. Does the possibility of rough agreement on research structure but disagreement on epistemol-