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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-

ogy undermine the authors' underlying argument that knowing the research structure by itself solves epistemological issues? Yes, I believe so. The authors uncritically adopted Lakatos's philosophy as a methodological assumption to get on with the project, and so they do establish a well-articulated and accurate connection between Lakatos and evolutionary psychology. I argue that this connection will not rebut the falsifiability charge as the authors seem to think.

Why Choose Lakatos?

Ketelaar and Ellis argue that the explanatory system in evolutionary psychology "adheres to the well-established Lakatosian philosophy of science" (this issue). Why pick Lakatos as the solution to the problem of falsifiability? The authors do not present a rationale for choosing Lakatos rather than other philosophers or historians of science as their intellectual resource for dealing with falsifiability. Let's consider one line of reasoning, starting with the simplest version.

First, Lakatos speaks explicitly to falsification. However, philosophers and historians of science of all stripes have something to say on falsification. There were medieval disputes over whether our best theories are true or just "save the phenomena." These disputes dealt with the problem of whether all theories can be adjusted so that they can be made consistent with empirical phenomena (which would make falsifiability in principle unachievable). Thus, the problem of falsifiability has a long history extending back to medieval time. So, speaking about falsification leaves wide open a historical array of potential candidates, not just Lakatos.

Second, Lakatos speaks explicitly to falsification as stressed by Popper. But Lakatos is not the only one whose methodology was touted as "a necessary addendum to Popper's method of falsification" (this issue). He is not even the only one whose methodology responds to the falsifiability problem by appealing to scientific development. Kuhn said that he learned most of what he knew about science from Popper. Kuhn used his finding from the history of science, that scientists are not always trying to test their theories, to create his own theory of scientific development in terms of paradigms. He thought that scientists only try to test and falsify fundamental theories during times of crisis and revolution, whereas in normal science they take the reigning fundamental theory for granted and dogmatically apply it in a kind of mopping-up operation after the major fights were won. Kuhn even alleged that Lakatos's talk of research programs could be translated into Kuhn's talk of paradigms. Further, Laudan (1998) dissected Kuhn's holistic picture of theory change, dividing a paradigm up into elements. Unidirectional change from one paradigm to another could

be understood as a series of rational, gradual changes in constitutive theories, aims, and methods. So, why choose Lakatos rather than Kuhn or Laudan? Or even Popper? Popper replied to Kuhn that Popper knew about paradigms all along (Popper, 1970, p. 51), described as a (possibly multilevel!) structure of assumptions that allow scientists to get to the heart of their particular research problems and discuss their work rationally. Or why not pick one of many more recent figures who developed this line of philosophy further?

Third, Lakatos speaks explicitly to falsification as stressed by Popper, who responds that falsification is overrated and who appeals to scientific development in a way that both illuminates scientific development in psychology and gets rid of the falsification problem. The one thing new in this answer is that Lakatos illuminates development in psychology. However, look at who is doing all the work in the target article. It is not Lakatos's philosophy, but the authors' own model of multiple levels of analysis. They said,

> Whereas the Lakatosian model of science describes two levels of analysis (the hard core of assumptions and its protective belt of auxiliary hypotheses), the evolutionary psychology model breaks down the protective belt into three levels of analysis (middle-level theories, hypotheses, and predictions). (Ketelaar & Ellis, this issue)

This statement exemplifies my first point that applying philosophy of science to psychology is no mere application of preexisting ideas without modification, but requires active construction of a bridge between the philosophy of science chosen and the particular science at hand. I am all for a more fine-grained analysis of the structure of research when needed to solve problems about science. However, the authors could have added this fine-grained analysis to the models of science espoused by Popper, Kuhn, Laudan, Quine, or contemporary philosophers.

Given the extensive disagreements among these philosophers of scientific method and in philosophy of science today, it does not make much sense to try to evaluate "whether the procedures for developing and testing evolutionary psychological models are consistent with contemporary philosophy of science" (Ketelaar & Ellis, this issue). There is no point in trying to make something consistent with one in an array of models of science that describe research structure and scientific change in similar ways but draw mutually inconsistent epistemological conclusions. Thus, one has to argue that Lakatos's model of science is superior to these other models at the outset of a project like that of Ketelaar and Ellis. Otherwise, one invites the reply that if Ketelaar and Ellis are right—evolutionary psychology is consistent with Lakatos—who was wrong or incomplete about the important issues

of scientific method, and so much the worse for evolutionary psychology?

What Is the Meaning and Point of Falsifiability?

When we apply a philosophy of science theory to evolutionary psychology, the problem of context arises. If we ignore the philosophical context and rip the philosopher's views out of his context, then two things may be lost: the very meaning of the philosopher's view and what is at stake in it.

Let adaptationism be the thesis, call it T, that for most traits in most species, there exists a selectionist explanation. This thesis is present in the central assumption of evolutionary psychology that "the human brain is comprised of a large number of specialized mechanisms that were shaped by natural selection" (Ketelaar & Ellis, this issue). Adaptationism, and thus the central assumption, is not falsifiable, according to what Popper meant by falsifiability. Popper held that a proposition P is falsifiable if and only if P deductively implies at least one observational sentence. If that observation sentence is found to be false, then by using the deductive inference form of modus tollens (If T, then P; not-P, so, not-T) we have a deductively sound argument that T is false. If that observation is found to be true, then deductive logic licenses no inference to the truth of T (which would be the fallacy of affirming the consequent, if T, then P, P, so, T). Now, adaptationism is not falsifiable in Popper's sense. First, existence claims as such are not falsifiable; if we have not found the adaptation we are looking for, that does not mean it does not exist, but just that we have not looked in the right place or way for it. Second, qualitative probabilistic claims are not falsifiable; terms like *most* or *large number* are too vague to be falsified by finding some mechanisms shaped by natural selection and other mechanisms not so shaped, as has been done. Third, there is no crucial experiment that could possibly refute adaptationism, even as compared to the alternative thesis of pluralism, which sees natural selection as one of many forces of evolutionary change (recall population genetics) whose effects are roughly coequal.

Ketelaar and Ellis say that evolutionary psychology is indirectly falsifiable because judgments from evidence can be transmitted up the levels of generality from the empirical test to the multiple levels of the protective belt. Here, they rely on an undefined concept of falsibility, which may suffice for rebutting the authors' version of the unfalsifiability charge (2a) on the grounds that evolutionists do reject models based on negative test results. But an undefined concept will never suffice for rejecting their version of the unfalsifiability charge (2b) on the grounds that scientifically defensible criteria

are used in such rejections. The authors need to define indirect falsifiability so that it functions similarly to Popper's falsifiability criterion of science, namely, to explicate what it means for a theory to be empirically testable and thus vulnerable to empirical refutation. None of the authors' examples of evolutionary models speak to the issue of adaptationism versus pluralism. Adaptationism is unfalsifiable in Popper's sense because there is no single test such that we can specify an observational outcome prior to the test that would provide a deductively sound argument against adaptationism. Adaptationism is unfalsifiable in Lakatos's sense, which requires that every test that counts be a test of alternative theories within a research program or of rival research programs, because evolutionary psychologists do not test adaptationism against rival research programs, such as pluralism. Adaptationism is lodged in the hard core, a metaphysical position in the heart of evolutionary psychology that is immune from revision by convention, even though its pros and cons have been much debated. Hence, a key guiding assumption of evolutionary explanations is unfalsifiable both in Popper's sense and in Lakatos's sense with respect to the methodologies used in evolutionary psychology.

So, we have found out that the central assumption of evolutionary psychology is unfalsifiable, in virtue of its adaptationist character. So what? What lessons follow? According to Popper, there are lessons about science, truth and falsity, and empirical knowledge. Falsifiability was proposed by Popper to provide a demarcation criteria between science and nonscience. From the unfalsifiability of adaptationism, it follows that evolutionary psychology is not a science, and that the many tests of hypotheses in evolutionary psychology do not indicate that its central assumption is true or false, and that evolutionary psychology cannot deliver empirical knowledge. These lessons can be found only in the role falsification plays in Popper's philosophy of scientific method, conceived as a method for generating scientific knowledge.

What lessons follow according to Lakatos? Let's assume that Ketelaar and Ellis have persuaded us that evolutionary psychology fits Lakatos's methodology of research programs, and that it is a progressive research program. Lakatos probably regarded progressiveness, as defined in terms of corroborated novel predictions, as a demarcation criterion. Most interpreters think he was wrong. Here is a sketch of their argument, using Kuhn's own comments on Lakatos (1970a). First, Lakatos's model of scientific development in terms of research programs can be translated into Kuhn's (1970) description in terms of paradigms: "Though his terminology is different, his analytic apparatus is as close to mine as need be: hard core, work in the protective belt, and degenerative phase are close parallels for my paradigms, normal science, and crisis" (p. 237). Second, on both Lakatos's and

Kuhn's models, research programs (paradigms) are not adequately judged instantly one test at a time, but are appraised according to their long-run successes and failures in testing. Sometimes researchers credit failures to low-level (within-paradigm) claims that can be revised so as to retain the research program (paradigm) and other times to higher level claims (the paradigm itself). Third, there is a consensus that Kuhn's model does not provide a demarcation criterion; "If, as my critics point out, my further description fits theology or bank-robbery as well (as science), no problems are thereby created" (Kuhn, 1970, p. 245). So, even if Ketelaar and Ellis have shown that evolutionary psychology is rationally progressive, they have not shown that it is a science.

Moreover, according to Lakatos's model neither true nor false observational consequences will warrant any judgment regarding the truth or falsity of the theory from which they are deduced (Kourany, 1998, p. 163), for example, evolutionary psychology's main assumption. So, the rational progressiveness of evolutionary psychology does not imply that its hierarchically arranged assertions count as knowledge. Furthermore, Lakatos's model does not make it more rational to choose to do research in a progressive rather than a degenerating research program (Lakatos, 1970b, p. 68), so we cannot infer from progress in evolutionary research that psychologists should convert to evolutionary psychologists. Finally, Ketelaar and Ellis have only recorded the successes of evolutionary psychology, whereas Lakatos requires that progressiveness be determined by counting both successes and failures (Lakatos, 1970b, p. 68). So, the authors have not yet shown evolutionary psychology to be rationally progressive.

I would guess that the intense debate among advocates and detractors of evolutionary psychology would make it hard for both sides to agree on what counts as an unbiased tally of successes and failures. For example, prior to Silverman and Eals's hypothesis in 1992, nonevolutionary psychologists conducted studies of people remembering collections of objects in a room. Had Silverman and Eals heard the empirical reports of gender differences in remembering which items were in which locations before they theorized about how ancestral women's gathering and men's hunting might explain and predict gender differences in spatial recognition? If so, part of the credit for their novel predictions goes to evolutionary theory and part to facts uncovered without evolutionary theory.

Lakatos analyzed situations in which the issue of post hoc explanations versus tests verifying excess empirical content was an issue pertaining to rival research programs. Lakatos (1970b) wrote, "If a research programme progressively explains more than a rival, it 'supercedes' it, and the rival can be eliminated (or, if you wish, 'shelved')" (p. 68). What is the rival research program to evolutionary psychology? Antievolutionary

psychology or nonevolutionary psychology are not genuine research programs, just labels for research not based on recent evolutionary theory. Even the standard social science model is not a research program, just something Tooby and Cosmides (1992) found helpful for identifying the false presuppositions of critics of evolutionary psychology that give rise to many wrong-headed objections.

Although Ketelaar and Ellis want to retain the centrality of falsification and appeal to Lakatos as "a necessary addendum" to Popper, both cannot be done. Popper argued from a logical asymmetry between verification and falsification, the unsolvability of Hume's problem of induction, and examples from physics to the conclusion that theories cannot in principle be verified but they can be falsified. On the contrary, Lakatos (1970b) decided that

> Within a research programme a theory can only be eliminated by a better theory, that is, by one which has excess empirical content over its predecessors, some of which is subsequently confirmed. And for this replacement of one theory by a better one, the first theory does not even have to be "falsified" in Popper's sense of the term. Thus progress is marked by instances verifying excess content rather than by falsifying instances; empirical "falsification" and actual "rejection" become independent." (p. 65)

Ketelaar and Ellis have not given us examples of such progress, so they cannot conclude that evolutionary psychology is rationally progressive.

In sum, you can only get out of an application what you put into it. Ketelaar and Ellis have not dealt with the epistemological issues Popper and Lakatos were dealing with when they took the views of each philosophy out of context (presumably, to focus on evolutionary psychology). As a result, they have prevented themselves from being able to justify the kinds of assertions they would like to make about the epistemological import of applying Lakatos to evolutionary psychology. The severely limited epistemological significance of their article derives from their method of applying philosophy of science to evolutionary psychology. They took the first step. The next step is to get into the philosophical fray.

Note

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How to Keep Our Metatheories Adaptive: Beyond Cosmides, Tooby, and Lakatos

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If Popper had never existed, would we need Lakatos to set us straight? Probably not. Good philosophy of science is useful mainly in inoculating scientists against bad philosophy of science, so we can get on with our business. Such preventative medicine is necessary only when pathological memes such as falsificationism are endemic in the academic population. Nevertheless, Popper did exist, and remains the only philosopher of science known to most scientists. Ketelaar and Ellis (this issue) do us the important service of providing an equally venerable philosopher we can invoke to protect our metatheories against the ghost of Sir Karl.

My quibbles with Ketelaar and Ellis are that (a) they overlook some important features of evolutionary psychology's metatheory, and some of these features are ripe for rethinking and extending; and (b) the Lakatosian framework seems inadequate to describe evolutionary psychology's scientific success, popular appeal, or institutional challenges. These quibbles do not undermine the power of Ketelaar and Ellis's anti-Popper antidote. Their target article usefully introduces the concept of metatheory to mainstream psychologists, many of whom have never encountered a useful metatheory before, so cannot distinguish between metatheory and ordinary hypothesis.

For a young science barely a decade old, evolutionary psychology has achieved a remarkably strong metatheoretical consensus. In part, this is because the metatheory was imported wholesale from contemporary adaptationism in evolutionary biology. As Ketelaar and Ellis point out, standard biological adaptationism includes many psychologically relevant ideas such as kin selection, reciprocity, and sexual selection. Yet evolutionary psychology's metatheory was also shaped very strongly by a series of ambitious, persuasive, and visionary articles by Cosmides and Tooby in the late 1980s and early 1990s that showed how adaptationism could be applied to the human mind (Cosmides & Tooby, 1987, 1994; Tooby & Cosmides, 1990a, 1990b, 1992). The Cosmides–Tooby vision of evolutionary psychology profoundly influenced the thinking of other leading researchers such as Buss (1995), Gigerenzer (Gigerenzer & Todd, 1999), Pinker (Pinker & Bloom, 1990), and Thornhill (1997). It was also adopted as the conceptual framework in the most influential popular accounts of evolutionary psychology (Buss, 1994; Ridley, 1993, 1996; Pinker, 1994, 1997; Wright, 1994).

The Cosmides–Tooby metatheory was a special form of adaptationism that stressed (a) functional efficiency criteria for identifying adaptations shaped by natural selection, (b) the context-sensitive psychological adaptation (rather than the "instinctive behavior") as the appropriate level of analysis for human nature, (c) a highly modular view of the mind as comprising hundreds of domain-specific psychological adaptations, (d) a computational metaphor for the mind imported from cognitive psychology, (e) the universality of evolved