
COMMENTARY

Clarifying the Foundations of Evolutionary Psychology: A Reply to Lloyd and Feldman

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Lloyd and Feldman's (this issue) continuing commentary on our recent target article and rejoinder (Ellis & Ketelaar, 2000; Ketelaar & Ellis, 2000) focuses on two overarching issues. First, Lloyd and Feldman claim that our description of the core metatheoretical assumptions of modern evolutionary theory overemphasizes the role of inclusive fitness (i.e., the so-called selfish gene approach) at the expense of underemphasizing important alternative approaches (e.g., multilevel selection models, gene–culture coevolution models). Second, Lloyd and Feldman criticize some of the methods and assumptions that ostensibly characterize the evolutionary psychology research program. These criticisms concern the conceptualization of organisms as inclusive fitness maximizers, the soundness of the epistemology of evolutionary psychology, the modularity of psychological mechanisms, and the universality of psychological mechanisms. In the first part of this rejoinder, we acknowledge that different schools of thought exist regarding the plausibility and importance of various metatheoretical assumptions in human evolutionary psychology. We argue that to date, however, only the gene-centered adaptationist program (consistent with inclusive fitness theory) has demonstrated scientific progressivity by generating a coherent, integrated body of new knowledge and explaining away several apparent anomalies. In the second part of this rejoinder, we discuss several misunderstandings that underlie Lloyd and Feldman's criticisms of human evolutionary psychology.

The Role of Inclusive Fitness Theory in Evolutionary Psychology

Lloyd and Feldman (this issue) criticize our reference to inclusive fitness theory as providing the foundation of modern evolutionary theory:

Inclusive fitness theory comprises a small subset of models used for special purposes in evolutionary understanding. ... There are many other components of evolutionary analysis that address both animal and human behavior, and although they are conceptually more intricate than inclusive fitness theory, they may be more appropriate for the exploration of human psychology.

Lloyd and Feldman go on to provide a technical critique of inclusive fitness theory, concluding that inclusive fitness theory is of limited use and that the foundations of evolutionary psychology are theoretically flawed.

What role does inclusive fitness theory play in evolutionary psychology? As we described it (Ketelaar & Ellis, 2000), the field of human evolutionary psychology primarily focuses on developing and testing middle-level evolutionary theories and their derivative hypotheses and predictions. These middle-level evolutionary theories (e.g., parental investment theory, life history theory, good genes sexual selection theory, reciprocal altruism theory) are consistent with and guided by but are not directly derived from a set of more basic metatheoretical assumptions about genetical evolution through natural and sexual selection (see Barkow, Cosmides, & Tooby, 1992; Buss, 1995). In this light, human evolutionary psychologists have found certain basic assumptions about natural and sexual selection, such as the gene-centered adaptationist program (consistent with inclusive fitness theory), to be more useful than alternative assumptions in guiding the development of middle-level evolutionary theories; that is, in guiding the specific theoretical models that provide a link between metatheoretical assumptions and derivative hypotheses and predictions. The armamentarium of mid-

dle-level theories employed by evolutionary psychologists has proved useful in guiding programs of research that have generated new knowledge of how the mind works (Ketelaar & Ellis, 2000). At the same time, many human evolutionary psychologists are interested in investigating alternative metatheoretical assumptions about natural and sexual selection, such as multilevel selection models and gene–culture coevolution models. These alternative frameworks, however, have not (yet) guided the development of successful middle-level evolutionary theories and thus have not proved useful in guiding research (as discussed later).

Lloyd and Feldman (this issue) go so far as to claim that we did not make reference to the “real foundations of evolutionary thought” (p. 152). At one level of analysis, this claim is a bit like criticizing the authors of a book on how to play billiards as having neglected to discuss quantum mechanics as the “real foundation” of the field of billiards. We imagine that most books on how to play billiards adopt an approach that is more similar to Newtonian physics (the physics of medium-size objects) than quantum mechanics (the physics of very small particles). Although it would be incorrect to claim that quantum mechanics is irrelevant to understanding the behavior of billiard balls, one might argue that quantum mechanics is practically useless when applied to the game of billiards. This is the case because humans do not literally interact with billiard balls at the quantum level. Similarly, Lloyd and Feldman may be correct in asserting that genetical evolution is a bit more complicated than the scenarios sketched out by Hamilton (1964), Williams (1966, 1992), Dawkins (1976, 1982), and Tooby and Cosmides (1992; Cosmides & Tooby, 1987) scenarios that lie at the heart of how many human evolutionary psychologists view evolutionary biology. Nonetheless, we argue that just as an enjoyable (and productive) game of billiards does not necessitate invoking insights from quantum mechanics, a useful investigation of the adaptive design of psychological mechanisms does not necessitate a direct appeal to what we know about the underlying dynamics of genes. We are all in favor of using “the full resources of population genetics and behavioral biology” (Lloyd & Feldman, p.155). However, it does not follow that because the devil is in the details of evolutionary genetics that a more abstract concept such as inclusive fitness is irrelevant to our understanding of the behavior of medium-size biological objects such as human psychological mechanisms.

Along these lines, Lloyd and Feldman (this issue) endorse a different set of metatheoretical assumptions than we endorsed in our original target article (Ketelaar & Ellis, 2000). Specifically, Lloyd and Feldman endorse the use of multilevel selection models and gene–culture coevolution models. Although debate continues about the technical merits of these

different views of the evolutionary process (e.g., Laland, Odling-Smee, & Feldman, 2000; Wilson & Sober, 1994), it is extraordinarily premature to suggest that multilevel selection models and/or gene–culture coevolution models should replace the standard, gene-centered adaptationist program. This is because multilevel selection models and gene–culture coevolution models have (thus far) proven scientifically barren as tools of discovery. To our knowledge, there has not been a single new discovery about human cognition or behavior that has been directly guided by either multilevel selection models or gene–culture coevolution models. Instead, these models have at best provided post hoc explanations of known phenomena. This is problematic because, as Lakatos (1970, 1978) showed, it is relatively easy to stretch existing theories to accommodate known facts. Indeed, we suspect that there are very few empirical findings in psychology that, after the fact, could not be claimed by multiple theories as falling within their explanatory purview. A good evolutionary explanation, therefore, must not only account for known facts but also predict further, yet unobserved, novel facts (Ellis & Ketelaar, 2000; Ketelaar & Ellis, 2000). That is, a good explanation must stick its neck out by predicting experimental results that are not known in advance.

Multilevel selection models and gene–culture coevolution models are complex elaborations of the standard, gene-centered adaptationist program. Whatever their truth value may be, these elaborations have remained at too abstract a level to date to facilitate scientific inquiry. Multilevel selection models and gene–culture coevolution models are likely to be incorporated into the metatheory of evolutionary psychology if, and when, they begin to contribute significantly to the growth of knowledge in psychology by anticipating new facts and resolving old puzzles. Human evolutionary psychology is a strongly empirical field. New theoretical advances must demonstrate their usefulness at the nuts-and-bolts empirical level at which evolutionary psychologists conduct their daily business. It is misleading to claim that human evolutionary psychologists have failed to “take advantage of the full armamentarium of modern evolutionary theory” (Lloyd & Feldman, p.153). The expanded armamentarium that Lloyd and Feldman recommend is simply not yet ready for combat on the empirical battlefield.

Misunderstanding of Evolutionary Psychology

In criticizing our depiction of the core assumptions of human evolutionary psychology, Lloyd and Feldman (this issue) demonstrate several misunder-

standings of the field. In this section, we provide an overview of these misunderstandings and attempt to clarify them.

Misunderstanding #1: “Evolutionary psychology claims that human behavior and its cognitive base should be explicable in terms of enhanced inclusive fitness” (Lloyd & Feldman, this issue) and “Optimization is built into inclusive fitness theory itself; individuals are assumed to maximize their own inclusive fitness” (Lloyd & Feldman, this issue)

This first misunderstanding conflates the historical process of adaptation through natural selection with its products: adaptations themselves. The study of adaptation fundamentally concerns how traits evolved to meet the selection pressures of past, not current, environments. Adaptations are reliably developing characteristics of species that over evolutionarily significant periods of time, served specific functions (e.g., a change in color increasing the ability of an organism to avoid detection by predators) that recurrently promoted the survival of the genes that direct their construction. Adaptationist analyses thus provide historical accounts of the origin and maintenance of traits as a consequence of their past effects on reproduction. The fact that a given adaptation was produced through differential reproduction does not imply that either (a) selection pressures are currently favoring that adaptation or (b) that variation in the phenotypic expression of that adaptation will be associated with current reproductive success (Symons, 1992).

For example, the fact that correlations between socioeconomic status and reproductive success are typically negative in modern Western societies (Vining, 1986) does not challenge the adaptationist hypothesis that status striving is an evolved motivational system. This is because the key issue in the study of adaptation is whether differential reproductive success historically influenced the design of a given trait (e.g., whether high-status individuals reliably outreproduced low-status individuals in ancestral environments) and not whether a trait currently influences reproductive success. The historical link between high status and reproductive success could have been severed by any number of novel features of modern Western environments (e.g., social welfare institutions, effective contraception, links between high status and years of tertiary education).

Along these lines, a central focus of the field of Darwinian medicine (Nesse & Williams, 1994; Williams & Nesse, 1991) is on how mismatches between modern and ancestral environments compromise the effectiveness of adaptations. These mismatches often result in dysfunctional behavior that is inexplicable in

terms of enhanced inclusive fitness. For example, the dopamine-mediated reward mechanisms found in the mesolimbic system in the brain appear to have evolved to provide a pleasurable reward in the presence of adaptively relevant stimuli such as food or sex. In contemporary environments, however, these same mechanisms are subverted by the use of psychoactive drugs such as cocaine and amphetamines, which deliver huge dollops of pleasurable reward in the absence of the adaptively relevant stimuli, often to the user’s detriment (Nesse & Berridge, 1997). In total, evolutionary psychologists (e.g., Buss, 1995; Symons, 1992; Tooby & Cosmides, 1992) strongly reject the notion that “human behavior and its cognitive base should be explicable in terms of [currently] enhanced inclusive fitness” (Foley, 1995–1996, as cited in Lloyd & Feldman, this issue) and that “individuals are assumed to maximize their own inclusive fitness” (Lloyd & Feldman, p.155).

Misunderstanding #2: The methods used to generate and test evolutionary psychological models and hypotheses are not scientifically defensible (Lloyd & Feldman, this issue)

Lloyd and Feldman (this issue) assert that the methods employed by evolutionary psychologists to generate and test evolutionary explanations are “simply not good enough to be regarded as part of the science of evolutionary biology” (p.155). Given that the scientific defensibility of evolutionary psychological explanations was the central focus of our original target article, we are quite puzzled that Lloyd and Feldman revisit the “bad science” criticism without engaging any of our original arguments on the topic. We reiterate that none of the original 16 commentators on our target article challenged our central thesis: that the methods and strategies employed by evolutionary psychologists to generate and test hypotheses are scientifically defensible. In this regard, Lloyd and Feldman offer nothing new by raising this criticism afresh.

A common theme in criticisms of evolutionary psychological explanations is that they are unverifiable because they make inferences about historical processes that cannot be directly observed (e.g., Lewontin, 1998; Lloyd, 1999; Richardson, 1996, 2000). Brandon (1990; see also Richardson, 1996) suggested that one cannot claim to know how a trait actually evolved unless one acquires the following five sources of information: (a) evidence that selection has occurred (i.e., that the trait of interest is not the product of other evolutionary forces), (b) evidence that there is some ecological factor in the environment that offers an explanation for the strength of selection, (c) evidence that the trait has a genetic basis, (d) evidence concerning the structure of the popu-

lation both from a genetic and selective viewpoint, and (e) phylogenetic information concerning primitive and derived characteristics. Lacking such evidence, one is restricted to offering accounts of how a trait might possibly have evolved. Brandon acknowledged that these five criteria are rarely met in adaptationist explanations for any characteristic in any species. Lloyd and Feldman (this issue) cite the recent evolution of the gene for human sickle cell disease in response to the malarial parasite as an example that does begin to meet these kind of verification criteria. No evolutionary explanation of any trait of interest to psychologists, however, is likely to meet all of these criteria. Accordingly, evolutionary psychological explanations are seen as being based largely on claims of plausibility and, as a result, appear unverifiable on evidential grounds (e.g., Davies, 1996; Lewontin, 1998; Lloyd, 1999; Richardson, 1996). The charge is that evolutionary psychological explanations are, therefore, of little scientific value.

As we argued previously (see Ellis & Ketelaar, 2000), there are at least two important qualifications to this criticism that must be appreciated when evaluating evolutionary psychological explanations. First, unverifiability is conceptually distinct from unfalsifiability. As articulated by Popper (1959), although particular statements (hypotheses) cannot be unequivocally verified, they can be refuted or falsified. For this reason, Popper placed corroboration (establishing that an explanation is consistent with the data) rather than verification in opposition to falsification. Popper argued that although it is possible to gather corroborating data to support a particular explanation, no amount of empirical evidence would allow, for example, Sherlock Holmes to unequivocally verify (i.e., prove) a particular explanation for the discovery of a cadaver (e.g., "the butler committed the murder with a pistol in the pantry"). This is because it is almost always possible to identify alternative inductive inferences that are consistent with the existing corpus of data. This is especially true of explanations that specify unobservable entities and processes (Churchland, 1989). Thus, in modern philosophy of science, verification is generally considered too high a standard to be of practical use in evaluating scientific explanations (e.g., Feigl, 1956; Haig & Durrant, 2000; Hooker, 1987; Meehl, 1990; Salmon, 1967, 1990). Nonetheless, a single piece of evidence (e.g., the fact that the maid had been dead for 3 years) could be used to unequivocally reject an alternative explanation (e.g., "the murder was committed by the maid"). In science, then, just as in the judicial system, certain accounts are accepted over alternative accounts, not because they have been absolutely verified (shown to be true), but rather because they alone are the best available explanation that emerges from the set of corroborated alternative accounts.

Rather than emphasizing verification, a more common approach to science emphasizes the process of establishing acceptable claims to knowledge through methods analogous to construct validation (e.g., Cronbach & Meehl, 1955; Hooker, 1987; Meehl, 1990). To establish construct validity, it is necessary for a middle-level evolutionary theory to specify the meaning of a construct (i.e., to specify the functional design of a proposed adaptation), distinguish it from other constructs, and generate clear, testable hypotheses and predictions about how measures of the proposed construct should and should not relate to other variables (see the American Psychological Association's, 1985, discussion of construct validation). If a prediction is supported, this provides indirect support for the hypotheses, which in turn, may provide grounds for favoring one theoretical model over another. If a prediction is disconfirmed, it is back to the drawing board, either to attempt a better translation of the hypothesis (into a specific prediction) or to actually modify or reject the hypothesis altogether (Ketelaar & Ellis, 2000). This process may, in turn, lead to revision of the middle-level evolutionary theory from which the hypothesis was derived or to replacement of the middle-level theory by a plausible alternative. In this manner, well-validated evolutionary explanations can count as acceptable knowledge claims; that is, these explanations can be empirically corroborated (or falsified) relative to other explanations, even though they cannot be directly verified (see Salmon, 1967, 1990, 1992, for a more detailed discussion of verification and inductive reasoning in science).

Second, even if an evolutionary psychological model has experienced predictive failures or falsifications and even if the model fails to meet Brandon's (1990) five criteria for verification, this does not itself constitute adequate grounds for rejecting the model. As many philosophers of science have noted (e.g., Lakatos, 1978; Thagard, 1992), theory evaluation is not simply a matter of gauging the degree of fit between a single theory and the world. Rather, theories are evaluated in relation to alternative theories based on the cumulative weight of the evidence. Acceptance or rejection of a given evolutionary psychological explanation depends on comparison with other explanations of the trait in question. It is not enough to claim that a given evolutionary psychological explanation is "highly unscientific or unverified" (Lloyd & Feldman, p.155). Rather, substantive critiques must demonstrate that alternative explanations (whether adaptationist or nonadaptationist in origin) provide a better account of the trait in question than does the proposed evolutionary psychological explanation. Lloyd and Feldman (this issue) offer little by way of empirically corroborated alternatives to the basic assumptions that they call into question.

Misunderstanding #3: “Evolutionary psychologists assume that human beings have a module (or set of genes) for each task that is imposed by the environment” (Lloyd & Feldman, this issue)

Although there is not consensus among evolutionary psychologists concerning the level of domain specificity of psychological mechanisms, the notion that “human beings have a module (or set of genes) for each task that is imposed by the environment” (Lloyd & Feldman, p. 153) is at best a minority position in the field. It is certainly not a position that we endorsed in our original target article (Ketelaar & Ellis, 2000) or rejoinder (Ellis & Ketelaar, 2000). The assumption that “the human brain/mind is comprised of a large number of specialized cognitive adaptations that were shaped by natural selection over vast periods of time to solve the recurrent information-processing problems faced by our ancestors” (Ellis & Ketelaar, 2000, p. 61) is widely endorsed by evolutionary psychologists. This assumption, however, does not imply a one-to-one correspondence between number of information-processing problems and number of specialized cognitive adaptations. As discussed in our original rejoinder, there is ongoing debate among evolutionary and cognitive psychologists concerning the numerosity and specificity of psychological mechanisms.

Samuels (2000) usefully distinguished between strong and weak versions of the massive modularity hypothesis. According to the strong version, the human brain–mind is composed entirely of Darwinian modules: innate, special-purpose computational mechanisms that were designed by natural and sexual selection. According to the weak version, the human brain–mind is composed largely but not entirely of Darwinian modules. The weak version of the massive modularity hypothesis is consistent with the assumption that different psychological mechanisms differ in their levels of specificity and that there are higher level executive mechanisms that operate on information drawn from a range of more domain-specific lower level mechanisms. Evolutionary psychologists have only committed themselves to the weak version of the massive modularity hypothesis (Samuels, 2000). The weak version, for example, was clearly endorsed by Tooby and Cosmides (1998):

The deepest issues in cognitive science involve the heterarchical and cross-cutting scope of different mechanisms: what domains are native to the human mind, what is the mixture of domain-specific and domain-general devices that compute over these domains, what are the designs of these devices, and how do they pass inputs and outputs back and forth. These same issues apply to human anatomy and physiology:

even tissues of very different organs employ identical processes at some levels (RNA transcription, mitochondrial energy production) and highly specialized processes at others (hemoglobin binding for oxygen transport). ... Sorting this out at an information processing level will be a fascinating task, and the answer will not be reducible to “Everything is domain-specific” or “Everything is domain-general.” (p. 200)

We concur with Lloyd and Feldman (this issue; cf. Shapiro & Epstein, 1998) that selection is unlikely to favor distinct Darwinian modules for the solution of every adaptive problem. This would be inefficient, given that some modules can conceivably contribute to the solution of two or more adaptive problems. Indeed, there is clear evidence of multiplicity of function in certain noncognitive traits (e.g., bird feathers function both to regulate temperature and facilitate flight). There should also be multiplicity of function in certain cognitive traits (e.g., input of frequentist mechanisms to many types of judgments; Brase, Cosmides, & Tooby, 1998).

We also concur with Lloyd and Feldman (this issue) that there are real evolutionary constraints on the design features of adaptations. Rather than describing Darwinian modules as optimally designed (from the standpoint of a rational engineer trying to solve a problem), evolutionary psychologists more typically use terms such as *gerrymandered devices*, *Rube–Goldberg machines*, or *weird contraptions* to describe adaptations (see Dawkins, 1982; Dennett, 1995). This is because evolutionary psychologists acknowledge many possible constraints on optimal design, such as the slow speed of evolution, the lack of available genetic variation, the need to build on pre-existing structures, and the necessity of coordination with other mechanisms (e.g., Buss et al., 1998; Dawkins, 1982).

Misunderstanding #4: High within-population variability on most measured human behavioral traits undermines the assumption of universality of psychological mechanisms, that is, the assumption that there is a species-wide evolved human nature (Lloyd & Feldman, this issue)

High within-population variability on most human behavioral traits does not, in and of itself, challenge the assumption of an innate, universal human nature. Universally shared, functionally specialized cognitive adaptations can generate individual differences as a result of different life experiences. Rather, it is the existence of heritable variation in human behavioral traits that calls into question the idea of a universal human nature (Tooby & Cosmides, 1990). Attempts to explain heri-

table variation is an active area of theory and research in evolutionary psychology, and a number of different viewpoints have been expressed on the topic.

Tooby and Cosmides (1990), for example, acknowledged that vast reservoirs of genetic variability underlie human psychological functioning but argued that this variability is “generally limited to quantitative variation in the components of complex, highly articulated, species-typical psychological mechanisms” (p. 24). According to Tooby and Cosmides, pathogen pressures select for multiple-alternative alleles at a large proportion of loci, introducing as much quantitative variation and noise into adaptive systems as can be tolerated without compromising their functional integrity. From this perspective, the genetic component of phenotypic variation mostly constitutes *genetic noise* (nonadaptive random fluctuations around species-typical design).

Other evolutionary psychologists, however, have challenged the genetic noise argument. Miller (2000a, 2000b), for example, posited that heritable variation in sexually selected fitness indicators is adaptively patterned. A core premise of good genes sexual selection theory is that certain traits have evolved because they are reliable indicators of genetic quality; that is, these traits reliably signal viability and good condition that can be passed on to offspring through genetic inheritance. For a given trait to be a reliable indicator of genetic quality, it must be costly to produce. According to the handicap principle (Zahavi & Zahavi, 1997), sexually selected fitness indicators, such as the peacock’s tail, can only be maintained by individuals who are the fittest in the population, as indicated by their ability to maintain steady growth rates, resist parasites, compete successfully in intrasexual contests, and so forth. Consequently, individuals who more fully display sexually selected fitness indicators tend to be healthier, in better condition, and more preferred as mates than conspecifics who display these traits less fully (e.g., Gangestad & Simpson, 2000; Moller, Christie, & Lux, 1999). Kotiaho, Simmons, and Tomkins (2001) demonstrated that there are high levels of additive genetic variance underlying individual differences in condition (operationalized as the ability to withstand nutritional stress) and that strong positive genetic correlations exist between condition and courtship rates. Pomiankowski and Moller (1995) suggested that adaptive genetic variation in sexually selected fitness indicators is maintained by long-term directional selection.

The genetic noise argument was also challenged by Buss and Greiling (1999), who argued that heritable individual differences in human personality traits are reliably linked to reproductively relevant phenomena such as sexual behavior and status attainment. Over the last 2 decades, theory and research in evolutionary biology

has begun to acknowledge that in most species, a single “best” reproductive strategy is unlikely to evolve (Gangestad & Simpson, 2000; Gross, 1996). This is because the best reproductive strategy varies as a function of the physical, economic, and social parameters of one’s environment (Crawford & Anderson, 1989). Heritable variation in personality traits reliably guides individuals toward different reproductive strategies (e.g., Bailey & Martin, 2000; Eysenck, 1976). Some of this variation may, thus, be produced and maintained by natural and sexual selection.

Along these lines, evolutionary psychologists have begun to develop and test frequency-dependent models of individual differences in personality. Heritable variation in personality can be produced and maintained at equilibrium in populations through frequency-dependent selection (where the fitness of a heritable variant increases as it becomes rarer in the population; frequency-dependent selection, thus, favors parents who produce more offspring of the rare variant). Gangestad and Simpson (1990) have proposed that heritable variation in women’s sociosexual orientation (i.e., women’s orientation toward long-term versus short-term mating) is the result of frequency-dependent selection (see Bailey & Martin, 2000, for a recent empirical test of this model). Likewise, Mealey (1995) suggested that heritable variation in primary sociopathy is the result of frequency-dependent selection.

It is important to note that even if good genes sexual selection produces adaptive variation in fitness indicators and even if some heritable variation in personality traits is maintained by frequency-dependent selection, this does not undermine the assumption that there is a species-wide evolved human nature. This is because universally shared psychological adaptations can coexist with other adaptations that are expressed in alternative forms in different individuals. One does not exclude the other.

Conclusions: Two Schools of Thought in the Evolutionary Sciences

We conclude by placing the debate between ourselves and Lloyd and Feldman (this issue) in the context of a larger, ongoing debate in the evolutionary sciences. Sociologist Ullrica Segerstrales (2000) recently examined the last several decades of the so-called sociobiology debate and concluded that this debate often boils down to a conflict between two main schools of thought: the critical experimentalist school and the naturalist school. Many of Lloyd and Feldman’s concerns appear to reflect the perspective of the critical experimentalist school of thought, whereas much of the adaptationist program in human evolutionary psychology, which we have described, reflects the naturalist school of thought.

According to Segerstrales (2000), the critical experimentalist school is comprised largely of geneticists, biologists, and palaeontologists (e.g., Lewontin, Rose, Gould) whose work is largely confined to the laboratory (thus, the moniker *experimentalists*). These scientists work with actual physical constructs such as alleles, cells, and fossils. By virtue of their extensive focus on laboratory techniques, individuals trained in this school of thought tend to emphasize the complications and caveats involved in defining, measuring, and conceptualizing abstract constructs such as genes and adaptations. From the perspective of the critical experimentalist school of thought, human evolutionary psychologists often take great license in applying our laboratory knowledge of genes, developmental embryology, and the fossil record to the “real” world of minds, muscles, and behavior. By contrast, the naturalist school of thought is comprised largely of zoologists, behavioral ecologists, evolutionary psychologists, and biologists (e.g., Dawkins, Alexander, Williams, Cosmides, Tooby, Buss) who are less interested in studying genes, cells, and fossils in the laboratory and much more interested in studying the behavior of organisms in their natural settings. Although proponents of the naturalist school are generally aware of the caveats involved in defining and measuring basic concepts such as genes, they are not as reluctant (as the critical experimentalists) to posit “hypothetical” constructs such as “evolved psychological mechanisms.” For individuals trained in the naturalist school of thought, the admittedly abstract and onerous concept of adaptation is a much more natural level of description (than cistrons, alleles, and proteins) for understanding the properties of entire organisms observed in their natural environments.

It is not surprising that critical experimentalists, who have made important contributions to our understanding of the microworld of genetics, would call on naturalists to be more cognizant of assumptions at the microlevel when investigating macrolevel entities and processes. After all, a complete understanding of our evolved human nature necessarily entails knowledge spanning from the microlevel of genes and proteins to the macrolevel of human behavior and cognition. True progress in the human evolutionary sciences will occur when these very different levels of analysis are successfully integrated.

Notes

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