

ings did he suggest that the function performed by an exaptation needs to be biological. As Buss et al. reported, Gould (1991) offered two definitions of exaptation: "a feature, now useful to an organism, that did not arise as an adaptation for its present role, but was subsequently coopted for its current function" (p. 43) and "features that now enhance fitness, but were not built by natural selection for their current role" (p. 47). Thus, although exaptations *always* provide a current function, that function need not be biological. In fact, Gould's (1991) thesis is geared toward exapted functions performed by the brain to serve psychological rather than biological functions.

Additionally, Buss et al. (1998) suggested that Gould (1991) used the term exaptation "to cover novel but functionless uses or consequences of existing characteristics" (p. 539). My reading of Gould (1987, 1991, 1997) suggests otherwise: Structures or characteristics for which there are no identifiable functions, either current or historical, are always spandrels. Thus, the term *spandrel* subsumes what Buss et al. referred to as functionless by-products. When a spandrel is coopted (exapted) for a function, it becomes an exaptation; there is no need to differentiate between spandrels and functionless by-products. Ironically, Gould (1991) coined the terms exaptation and spandrel to avoid this sort of confusion.

Additionally, although Buss et al. (1998) sought to outline the usefulness of the terms adaptation, exaptation, and spandrel for the science of psychology, most of their discussion was restricted to structural rather than psychological levels of analysis. Although understandable given their strictly biological interpretation of exaptations, this approach is problematic because the authors sought to apply their standards of evidence to the list of exaptations proposed by Gould (1991), namely language, religion, principles of commerce, warfare, reading, writing, and the fine arts. All of the items on this list are psychological phenomena borne of a structural complexity (i.e., the human brain) that is not well understood and that was not sufficiently considered by the authors. Here again, Buss et al. may have misinterpreted Gould, who did not use human brain size merely as an "example of an exaptation" (p. 539), as the authors stated. Rather, Gould suggested that the human brain, by virtue of both complexity and flexibility, is "the best available case for predominant exaptation—in other words, for a near certainty that exaptations must greatly exceed adaptations in number and importance" (p. 55). Gould offered the practice of religion as an example and suggested that our enlarged brains force us to confront our own mortality. Because it is quite unlikely that brain enlargement evolved to serve this end, the practice of religion performs an exapted

function by moderating, through a variety of themes, our evolutionarily functional fear of death. Thus, recognition of our own mortality is a spandrel, and the practice of religion is an exaptation.

Moreover, it is a priori unlikely that such complex psychological phenomena can meet the standards of evidence proposed by Buss et al. (1998). Specifically, they suggested that evidence of special design for a hypothesized function be demonstrated before concluding that any structure or behavior is adaptive. As an example, they presented the hypothesis that female orgasm serves the adaptive function of facilitating sperm transport, for which evidence is reportedly lacking. It is thus concluded that female orgasm does not serve the hypothesized adaptive function. Although instructive, this example trivializes the difficulty of falsifying, at the phenotypic level, hypotheses about exapted psychological functions, such as the practice of religion. There are at least two reasons for this. First, as outlined by Buss et al., exaptations "carry the additional evidentiary burdens of documenting both later co-opted functionality, and a distinctive original adaptational functionality" (p. 546). Thus, to confirm the exapted function of religious practice, one would be required to demonstrate (a) that enlarged brains were naturally selected for reasons independent of religious practice, (b) that such brain enlargement resulted in the capacity to practice religion, and (c) that religious practices function to assuage the fear of death (which would itself be required to meet the evidentiary standards of a spandrel). Although the first two of these conjectures may be true, they can at best be confirmed only at the pseudoempirical level. Gould (1991) recognized this and suggested that the term *exaptation* be applied to cases where a lack of historical evidence precludes the determination of whether a characteristic is an adaptation or an exaptation. Furthermore, confirming or disconfirming the third conjecture is far more difficult than establishing the utility of female orgasm as a sperm transport mechanism, because the functional level of analysis is psychological, not structural.

All of the proposed exaptations listed by Gould (1991), because they are specified at the psychological level, are similarly precluded from meeting the strict evidentiary standards set forth by Buss et al. (1998). Yet, it would be unfortunate indeed if psychologists, in an effort to meet such standards, were to reject Gould's distinctions and continue in adaptationist practices. We should recognize the error in logic of inferring evolutionary cause from current consequence, whether or not we can empirically demonstrate the existence of psychological exaptations.

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Noise, Logic, and the Span of Time

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Buss, Haselton, Shackelford, Bleske, and Wakefield (1998) should be commended for a thoughtful article that did the important work of clarifying and elaborating the theoretical terminology of evolutionary psychology. However, I was disappointed in their treatment of an issue that, in my opinion, needs to be more fully addressed before evolutionary psychology can gain further epistemological ground.

Buss et al. (1998) noted the three major conceptual means through which evolutionary theory accounts for our current psychology: (a) adaptation, (b) exaptation, and (c) random noise (surviving mechanisms that neither aided nor inhibited gene survival). The question troubling me is this: Might the possibility of random noise explanations inhibit the potential acceptance of explanations based on adaptations or exaptations?

According to the logic of scientific investigation, unless one rules out plausible alternative theoretical explanations for one's findings, then the proposed theoretical explanation for those findings cannot fully be accepted (e.g., Cook & Campbell, 1979). Given that evolutionary theory provides logical room for random noise, a real difficulty for evolutionary psychologists is how to rule out such noise as an alternative explanation for psychological mechanisms that are proposed as adaptations. Buss et al. (1998) suggested that "as more and more functional features suggesting special design are docu-

mented . . . the alternative hypotheses of chance and incidental by-product become increasingly improbable" (p. 537).

Special design, however, seems a vague standard highly dependent on intuition. Demonstrating special design requires understanding exactly how a psychological mechanism would have been functional to the survival of the genetic code in the distant reaches of human history—for it was survival in *that* history for which the mechanism was purportedly designed. However, it has been pointed out by other scientists that the task of logically estimating what psychological traits would have promoted genetic survival in a given historical context is an immensely difficult one—and one that by no means should be taken lightly. As Richard Dawkins (1976) cautioned in *The Selfish Gene*, "it is a very complicated business to demonstrate the effects of behavior on long-term survival prospects" (p. 5).

One of the reasons it is difficult to logically argue for one particular type of evolutionary explanation (over another) is that Darwinism is a very flexible framework and offers a nearly endless variety of ways that mechanisms could have been adaptations, exaptations, or random noise (see Ornstein, 1991). To be sure, explanations proposing adaptations or exaptations can be logical, but because there are potentially so many logically plausible explanations for a given psychological mechanism within an evolutionary framework, logic alone becomes a hollow argument. If all explanations are logical, then logic itself is useless as a means of determining which one of those explanations is the right one.

To pick an example from sexual strategies theory (e.g., Buss & Schmitt, 1993), it is certainly logical to suggest that a male preference for young (and thus fertile) women would lead to a greater chance of passing on that male's genes, but random noise explanations can be equally logical. Consider, as one example, the following logical progression: (a) The preference for young women would have caused men to get into more fatal fights (over the "rights" to the young women)—an activity surely not conducive to the passing on of genes. (b) Such a disadvantage could potentially offset any advantages of a preference for fertile women. (c) Although not aiding survival, the preference could have survived as random noise, first because survival is a complex thing involving thousands of different trait combinations, and second because the random operation of genetics did not provide a truly adaptive mechanism to compete with it.

The point here is not to argue that random noise explanations are better than adaptationist explanations. Rather, the point is that one can construct random noise explanations

for any given psychological mechanism that, in lieu of direct facts, are as logically sound as adaptationist explanations.

The logical difficulties of evolutionary psychological explanations would be of no great concern except for this additional troubling fact: It seems difficult (at least until a time machine is invented) to provide even an approximate empirical test of whether a particular psychological mechanism is the result of an adaptation, an exaptation, or random noise.

One distinction should be noted: Evidence for the existence of psychological mechanisms themselves is not evidence for the proposed adaptive qualities of those mechanisms. Finding mate-preference differences between men and women across 37 cultures, for example, is powerful evidence of biological differences between men and women. However, how those biological differences got there (in a Darwinian framework) is a separate question altogether, and we are really no further toward answering that question for having discovered the phenomenon itself. Thus, evidence of such biological differences certainly implicitly supports the functionality to psychology of evolutionary perspectives (in that these perspectives generated productive research), but does not support the proposed adaptivity of the mechanisms.

How then does one claim empirical support for adaptiveness? Consider Gould's (1991) hypothesis that religion is an exaptation. Buss et al. (1998) suggested that providing evidence for this involves demonstrating (a) what original adaptation was coopted for religion, (b) what caused the coopting, and (c) the new biological adaptiveness of religion. Buss et al. then alarmingly stated that "these predictions can then be subjected to evidentiary standards of empirical testing and potential falsification" (p. 542).

The trouble is, how does one empirically test hypotheses that are about processes that occurred millions of years ago? Does even extensive testing on current human beings really get you much closer to the adaptiveness in the distant past of the psychological processes discovered now? (As evolutionary psychologists often emphasize, those things that were adaptive then are not necessarily the things that are adaptive now.) In addition, because most psychological mechanisms—within a Darwinian framework—can logically be noise as easily as they can be adaptations or exaptations, how, given this state of affairs, does one ever rule out with even moderate confidence the probability that something is simply evolutionary noise?

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Spandrels in the Consulting Room

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Instead of embracing the complexity of evolutionary theory, Buss, Haselton, Shackelford, Bleske, and Wakefield (1998) coopted Gould's (see Gould & Vrba, 1982) notion of exaptation for their own purposes. Giving only lip service to Gould's critique, Buss et al. ultimately reiterated the need for adaptive explanations for all aspects of human existence and behavior. Buss et al. criticized Gould for not reducing language, religion, and so on to clear adaptive stories. They revealed their own reductionist agenda in stating that "parental care—investing in one's own children—is *merely* [italics added] a special case of caring for kin who carry copies of one's genes in their bodies" (p. 535). We lose so much in this description of human experience, both good and bad. What about the knowledge that we have of our mortality, our desire to be connected to a chain of ancestors and descendants, and our delight in knowing the stories that connect us to our forebears? How do the experiences of adoption and egg donation fit into this scheme? On the darker side of human experience, the rosy implications of kin selection theory deny the reality of child abuse and neglect, as well as the relatively high proportion of homicides committed against kin.

When complex human behavior is in effect reduced to determinants in the genes, many important intervening variables are ig-