***Cognitive Adaptations: Some Conceptual Issues***

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The research program of evolutionary psychology centers upon the attempt to understand human psychology in terms of evolved features of the human mind. More particularly, the aim is to identify cognitive adaptations, features of human psychology shaped by natural selection under the conditions faced by our ancestors.

From within this perspective, it is natural to suppose that "the mind is a collection of domain-specific, content-dependent modules. These modules, in turn, have specific design features that mesh with the adaptive problems they were designed by natural selection to solve. Information about a specific adaptive problem provides important clues to the nature of a given module, the environmental factors that calibrate it during development, and the stimuli that activate it." (La Cerra and Kurzban, 63)[1]

On the whole, I believe this research program is promising and well-founded, both for the sake of theoretical understanding and for its possible practical fruits. However, it has been subjected to a number of criticisms claiming that it is in various ways misconceived. These range from sweeping charges of thoroughgoing confusion to more modest and measured criticisms of specific claims or aspects of the program. If the sweeping charges are correct, then there would be little or nothing to constitute the province of evolutionary psychology, while the more modest criticisms might require the field to be more or less substantially recast.

[1] The evolutionary psychologist need not claim that there is nothing in the mind but domain-specific and content-dependent modules, as this quote might seem to suggest. He does, however, expect such modules to be pervasive and important in explaining human capacities and behavior.

In an attempt to address some of these, I wish to discuss certain conceptual issues with respect to cognitive adaptations. My general claim will be that the criticisms are often themselves misconceived, that at most they succeed in identifying empirical issues that need to be addressed, not conceptual roadblocks to the research program.[2]

## *Terminology*

In order to do this, some terminological distinctions are needed. We need to understand what an adaptation is, how it is related to and distinct from a feature which is adaptive and what, in particular, a cognitive adaptation might be. Adaptations can best be understood in terms of inclusive fitness. The inclusive fitness of an organism is relative to an environment which may and typically does include other organisms. That organism will possess certain heritable traits. The inclusive fitness of an organism is a measure of the contribution it makes, *because* of those heritable traits (not just accidentally), to the presence of other organisms carrying those same heritable traits in subsequent generations.[3] That contribution will typically occur through the organism's own successful reproduction, through what it does for close relatives (who will normally carry at least some of the same heritable traits), through reciprocal altruism,[4] or through some combination of these.[5]

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[2] I do not mean to be claiming, because I do not believe, that a crisp distinction can be erected between the two, that the issues raised are solely empirical and not at all conceptual, whatever that might mean. Rather, I am claiming that the conceptual issues point to problems that are in principle susceptible of empirical resolution. The research program, in short, needs refinement not abandonment.

[3] "Inclusive fitness is calculated from an individual's own reproductive success plus his *effects* on the reproductive success of his relatives, each one weighed by the appropriate coefficient of relatedness." (Dawkins 1982, 186)

[4] One organism may act in a way that benefits another that is not closely related because the other can be expected to reciprocate. At least some cases of symbiosis can probably be explained in this way.

[5] If the relevant information were available, then, for comparative purposes (this variant is more inclusively fit than that), in principle, an index number representing an organism's inclusive fitness could be assigned by determining how many other organisms carrying the same heritable traits there can be expected to be (where the expectation is based on the heritable traits) in subsequent generations because of that original organism. Thus, for example, if there were a population of genetically identical organisms that doubled in size in each generation, then the inclusive fitness of each member of that population could be represented by the index number, 2.

Given this, a heritable trait is *adaptive* if it contributes to inclusive fitness. That is, an organism possessing it is more inclusively fit than an otherwise similar organism (living at the same time and in the same environment) lacking it or possessing some (actual) alternative to it.

If a trait is adaptive and if the environmental conditions under which it is adaptive remain stable, it can be expected that the trait will spread through the population; its carriers will reproduce more successfully and will be less likely to be eliminated under adverse conditions than non-carriers. This is to say that there will be selective pressure in favor of the trait.

An *adaptation* is a trait which is present in a population of organisms because there has been, at some time and in some environment, selective pressure among its ancestors for that trait.[6] An adaptation may be more or less complex and the clearest examples will be of complex traits that must have been shaped out of multiple mutations.

Clearly, given these definitions, it is not necessary that an adaptive trait be an adaptation. First, there may be adaptive traits that are not heritable. A hunter's skill is not genetically transmitted to offspring. Second, there may be traits exhibited in a population which are both adaptive and heritable but the explanation for which does not include selective pressure in their favor. This would be the case for any *new* adaptive mutation and could also be the case for adaptive traits that are in some way byproducts of other processes.[7] It is also not necessary that an adaptation must be adaptive. It must have *been* adaptive when it evolved, but the circumstances that made it adaptive may be quite different from what the organism carrying it faces now.[8] There is no serious doubt, for example, that the human appendix is an adaptation, though it no longer makes any positive contribution to our reproductive success.

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[6] I shall not place much emphasis on what is or is not to count as a trait. Since we are speaking of heritable traits, it will have to be the case that any adaptation referred to has some realization in the body or brain of the organism in question. However, our evidence for the existence of an adaptation will often be largely or entirely behavioral. We may have only indirect arguments that the trait is realized in or supported by some inherited structural feature of the organism.

[7] See Gould 1991. To complicate the story slightly, it should be noted that a trait may *emerge* as a by-product of other processes, but be preserved because it confers adaptive advantages. See Dennett 1995, 238-251.

[8] Steven Pinker puts this nicely in an application to human psychology (1997, 207-208):

> . . . [W]hat about the Darwinian imperative to survive and reproduce? As far as day-to-day behavior is concerned, there is no such imperative. People watch pornography when they could be seeking a mate, forgo food to buy heroin, sell their blood to buy movie tickets (in India),

A *cognitive adaptation*, then, will be an adaptation that affects the behavior of the organism by way of the organism's capacity for information processing. Thus, reflexes would not count as cognitive adaptations but language perhaps would.[9]

*Is There a Case for Evolutionary Psychology?*

Cognitive adaptations have had their critics, but before looking at criticisms, it is worth considering whether there are any reasons, prior to the detailed examination of evidence, for *expecting* to find cognitive adaptations. The outcome of that inquiry can provide us with some guidance in estimating the seriousness of criticisms offered. If there are no prior reasons (or if the prior reasons prove to be weak or insubstantial), then even slight empirical or conceptual difficulties might justify abandoning evolutionary psychology's research program. If, on the other hand, there are substantial or weighty prior reasons, there will be (at least) a presumption in favor of the research program—rebuttable, of course, but only if powerful counter-arguments are offered.

Since the evolutionary psychologist expects to find an array of (many) "domain-specific, content-dependent modules" (La Cerra and Kurzban, 63), the alternative would be that, with

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> postpone childbearing to climb the corporate ladder, and eat themselves into an early grave. Human vice is proof that biological adaptation is, speaking literally, a thing of the past. Our minds are adapted to the small foraging bands in which our family spent ninety-nine percent of its existence, not to the topsy-turvy contingencies we have created since the agricultural and industrial revolutions. Before there was photography, it was adaptive to receive visual images of attractive members of the opposite sex, because those images arose only from light reflecting off fertile bodies. Before opiates came in syringes, they were synthesized in the brain as natural analgesics. Before there were movies, it was adaptive to witness people's emotional struggles, because the only struggles you could witness were among people you had to psych out every day. Before there was contraception, children were unpostponable, and status and wealth could be converted into more children and healthier ones. Before there was a sugar bowl, salt shaker, and butter dish on every table, and when lean years were never far away, one could never get too much sweet, salty, and fatty food. People do not divine what is adaptive for them or their genes; their genes give them thoughts and feelings that were adaptive in the environment in which the genes were selected.

[9] I don't mean to express doubt that language is an example of a cognitive adaptation, just to contrast the sort of case in which some feature of behavior would not be appropriately considered a cognitive adaptation—though it might still be an adaptation—with the sort of case in which it would be plausible to say that the feature is a cognitive adaptation.

perhaps a few exceptions, such as built-in drives, the information processing that controls or modulates behavior is domain-general and content-independent.

Human behavior, both in the ways in which it is cross-culturally similar and in the ways in which it differs, will be explained chiefly in terms of learning. The claim that explanations that rely chiefly on learning are adequate—and, therefore, that there is little reason to appeal to cognitive adaptations—I shall call the Learning Model. (This seems to me simpler and more descriptive than Tooby's and Cosmides' "Standard Social Sciences Model".) That is, human behavior will not have an evolutionary explanation except in the fairly tenuous sense that our evolutionary history has gifted us with large brains which, operating on input from our physical environment and culture, manage to do the rest. To illustrate, if the Learning Model is correct, there will not be a genetic explanation for male promiscuity and female selectivity with respect to sexual partners, even if the pattern is cross-culturally uniform.

Given certain background, there are at least three general reasons for expecting that human behavior, insofar as it depends upon information processing, is not likely to be adequately accounted for simply in terms of learning from experience. The background is constituted by the facts that human behavior (a) addresses cognitively demanding tasks, (b) is versatile and (c) is, in many cases, uniform across cultures.

The claim that it is cognitively demanding is just a way of highlighting the fact that substantial amounts of information processing are required to deal with typical human problems and activities. The claim that human behavior is versatile refers to the fact that the cognitively demanding tasks come in a great many varieties. The last claim, of cross-cultural uniformity, refers to commonalities that are found in all or almost all cultures studied. These may be on a relatively abstract level of description. That, however, does not empty them (entirely or virtually) of content. There are, of course, *empty* cross-cultural uniformities such as "Humans in all cultures typically eat regularly." What I have in mind are uniformities such as "Women in all cultures typically seek high status (as defined by the local culture) in their mates." A uniformity such as this is one to which there are imaginable alternatives: It might be true in some cultures and false in others.[10] Plainly, behavior of this sort imposes cognitive demands. In order to implement it, women must be able to recognize what constitutes high status in their local culture and must be able to recognize signs of high status (or prospective high status) in potential mates.

The first reason for skepticism about the Learning Model derives from well-known arguments that all observation is theory-laden. We do not learn *from* experience if we bring nothing *to* experience, at least in the form of dispositions to interpret or categorize experience in certain ways.[11]

If we term these interpretive dispositions and their more elaborated linguistic and formal counterparts, "theories," then we can raise the question where theories come from. On the Learning Model, they must either somehow be "free creations" not learned from others, not based on modification of prior theories and not justified by experience or they must have developed from earlier theories in conjunction with experience.

If theories are free creations, then cross-cultural uniformities are very puzzling. If the earliest theories (for each person) are not constrained in any way, it is not obvious why all cultures would agree upon them. It is as if everyone on earth selected a number using a random-number generator and almost all of them (since a small-enough minority of deviants wouldn't prevent cultural uniformity) happened to get the same one.[12]

If, on the other hand, all theories are derived from modification of prior theories in the light of experience, there will be for each person some initial theory or theories with which she is equipped genetically. On the Learning Model, these can only be the handful of basic drives that

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[10] Perhaps, the most plausible way in which it could turn out to be false would be if there were egalitarian cultures in which males did not get assigned differential status.

[11] See, for example, Laudan, 33-36. The basic argument can be put simply: Experience does not classify itself. But even the most theoretically non-committal "observation reports" or classifications, e.g., 'red patch here now,' imply further claims that could turn out to be false, for example, that one's senses and memory are in sufficiently good working order to identify the color as red.

[12] There might be some explanation for convergence if the "free creation" of theories were frequent and if there were some selective or evolutionary process that weeded out inferior theories. (And ordinary learning processes could take over once there were some theories in terms of which to classify experience.) I don't think this really helps here. There will have to be *lots* of free creations, at least one for every normal human being. A selective process might lead to convergence on an optimum relative to some environment but is hardly likely to lead to convergence on an optimum relative to all the environments in which cultures actually live.

the model allows. It *may* be that this is enough, that with modest equipment in the form of biologically given drives, a powerful content-independent and domain-general information-processing system, operating on data provided through a sensory system,[13] can do the rest that is needed to account for human cognitive capacities and achievements. However, it is prima facie implausible and, so far, unsupported by the evidence.[14] To say the least, a proponent of the Learning Model has the burden of proof.

The second and third reasons for skepticism about the Learning Model can usefully be presented together. These are the problem of combinatorial explosion and the frame problem.[15] Both can be illustrated with a chess-playing computer program.

Such a program has somehow to play legal chess and, to be of much interest, has to play well enough to challenge—often win against—human players.[16] In chess, from an initial position and the iterated application of a simple set of rules, an enormous variety of positions can be created. There are, for example, 400 possible positions after each player's first move, and each additional move, until late in the game, creates further possibilities at a rate ranging up to orders of magnitude per move. This is the problem of combinatorial explosion. It refers to "the fact that with each new degree of freedom added to a system, or with each new dimension of potential variation added, or with each new successive choice in a chain of decisions, the total number of

[13] I shall not seek to directly engage the question whether our sensory systems are themselves correctly describable as cognitive adaptations. I think that they are, but for the moment I will allow the proponent of the Learning Model the assumption that they somehow convey only uninterpreted data to be worked upon by our information processing capacities.

[14] See Cosmides and Tooby, 94-100. One of the most difficult sorts of evidence to accommodate on the Learning Model is that related to "poverty of stimulus". In learning language, for example, young children achieve substantial mastery of their native languages without the benefit of very much in the way of input to base learning upon. Thus, Pinker and Bloom, on 451, say, "Children are fluent speakers of complex grammatical sentences by the age of three, without the benefit of formal instruction. They are capable of inventing languages more systematic than those they hear, showing resemblances to languages that they have never heard, and they obey subtle grammatical principles for which there is no evidence in their environments."

[15] For an explanation of the frame problem, see Dennett 1984. For brief accounts of both the frame problem and the problem of combinatorial explosion, see Cosmides and Tooby, 102-108.

[16] In the first match (February 10-17, 1996) between Kasparov, then world champion, and a program called Deep Blue, Deep Blue scored one win and two draws out of six games. In the rematch (May 3-11, 1997), Deep Blue scored one loss, three draws and two wins to win the match. So, it appears that at least some programs on some computers are able to challenge even the strongest human players.

possibilities faced by a computational system grows with devastating rapidity." (Cosmides and Tooby , 102)

In order to be challenging to a human player, a selection from among the possible moves must be made in a relatively short time. Since few games are decisively won or lost within the first few moves, it is plain that, given computational speeds presently or foreseeably available, there is no possibility that a chess-playing program can exhaustively consider the possible moves, their possible replies, the possible replies to those replies and so on.[17] If that had had to be done, the first chess-playing program designed, though it could have been switched onto faster machines since the sixties, would still not have made its first move.

The program in question faces two related problems. First, the problem of combinatorial explosion (in conjunction with time limits) insures that move choice cannot be governed by exhaustive calculation. Second, since move choice must be governed somehow, the frame problem arises. It must make moves while ignoring or failing to take into account vast tracts of possibly relevant information. To do that while playing sufficiently well to challenge a human player, there must be some non-random correlation between what it does ignore and what it should ignore. That is, it must in some sense make assumptions, *which are generally correct*, about what tracts of the search-space are worth examining.

For chess-playing programs, these problems have been dealt with by installing in their memories "opening books" that determine early move choice against the most common openings employed by human players, by installing generally reliable but fallible evaluation rules for positions examined in the course of calculation and by limiting search depth.

To put the matter in more general terms, no one has yet found a way, even in the relatively simple world and with respect to the relatively simple set of problems faced by a chess-playing program, to get a domain-general, content-independent, information-processing system to play (or learn to play) a respectable game of chess. But, since human cognitive

[17] Even if, improbably, some computer were fast enough to "solve" chess—to calculate best possible play against all possible play by an opponent—the question would remain as to how any human could ever manage to play a respectable game, for humans surely do not do it in that way.

achievements range over a far more complex and extensive array of problems, which are equally or more than equally affected by frame and combinatorial explosion problems, it is implausible that any general-purpose processor can provide the whole explanation in our case.

Summarizing, the theory-ladenness of observation, the problem of combinatorial explosion and the frame problem provide powerful presumptive reasons for expecting that cognitive adaptations will play an important role in the explanation of human behavior. Or, to put the point the other way around, proponents of the Learning Model have a heavy burden of proof to discharge.

In examining whether the critics of cognitive adaptations have successfully discharged that burden of proof, I shall first consider some general criticisms of functional explanations, and then turn to examining Steven Jay Gould's argument that there is reason to expect other types of explanation (than those in which cognitive adaptations figure) to be the most appropriate to the larger portion of human psychology.

## *Criticisms of Functional Explanations*

Explanations of a feature of human psychology or behavior in terms of cognitive adaptations are functional explanations. A cognitive adaptation is understood to occupy a functional role in the behavior of the organism, ultimately, though normally not immediately or directly, contributing to inclusive fitness (under the conditions prevalent in the ancestral environment in which it evolved).[18]

In *Philosophy of Social Science*, Michael Root has recently surveyed and endorsed some general criticisms of functional explanations. Though his concern is more with the employment of functional explanations within the social sciences, if his general criticisms are correct, they would also (as he recognizes) apply to functional explanations in evolutionary biology.

[18] For some discussion of functional explanations in biology, see Cosmides and Tooby, 53-77.

His criticisms can be summarized as follows. First, functional explanations are empirically empty. Second, they do not solve what he terms "the selection problem". And third, they do not explain why a trait, T, rather than a functionally equivalent trait, T', prevails in a population.

*I. Empirical Emptiness*

With regard to the first criticism, Root says:

> [One] concern over functional theories is whether they are empirically testable. Philosophers of science differ on how theories are to be put to an empirical test; but, to the extent that a theory in the natural . . . sciences is empirical, it must, in some fashion, face the tribunal of sense. In the case of a functional theory, for example, there must be evidence that a cluster of attributes constitutes a trait and that the trait is adaptive. Functional theories seldom satisfy this condition, for the hypothetical design problem is seldom well-defined enough to show that the trait is the best solution. As Lewontin says in his criticism of functional theories in evolutionary biology: "By allowing the theorist to postulate various combinations of 'problems' to which manifest traits are optimal 'solutions,' the adaptationist program makes of adaptation a metaphysical postulate, not only incapable of refutation but necessarily confirmed by every observation". . . . Lewontin's point is that so much is open-ended that a story could be told about any trait that would make it seem adaptive. (Root, pp 82-83)

This criticism appears to fail in two respects. In the first place, "the adaptationist program" may be understood in either of two ways. On one hand, it may be understood as making the descriptive claim that significantly many cases of adaptations will be found if they are searched for. If that is its claim, then it is subject to empirical testing, or at least, the arguments given do not show otherwise. If hypotheses about adaptations, even when those are generally recognized by practitioners of the research program as exemplary models of what they hope to confirm, are regularly discredited and only rarely confirmed, then the descriptive claim would be false. On the other hand, the adaptationist program could be understood as issuing an heuristic injunction to search for adaptations. In that case, it is of course not subject to empirical disconfirmation, but that is because it does not make an empirical claim. Nevertheless, such a research program may prove more or less fruitful, and if, as above, adaptationist hypotheses were regularly discredited but only rarely confirmed, there would be excellent reason for abandoning the research program (or for restricting it to some area in which it had shown more promise).

On either understanding of the adaptationist program, it is not insensitive to empirical evidence unless the case can be made that the specific hypotheses about particular adaptations to which it gives rise are insensitive to evidence.

Making that case seems to be the burden of the claim that "so much is open-ended that a story could be told about any trait that would make it seem adaptive." (Root, 83)

Now, it may immediately be granted that a great deal *is* open-ended and that we are rarely, if ever, in a position to show decisively that a trait under investigation is *the* optimal solution to an adaptive problem actually faced by the ancestors of an organism at the time that the trait appeared. It may also be granted that, in some sense, an adaptive story can be told about any trait that may strike the fancy of an adaptationist.

However, the adaptationist program is not seriously threatened by these concessions. The form of an adaptationist explanation does not just commit its proponents to claims to the effect that a trait under investigation would be an optimal solution to a hypothetical problem. If it did that (and nothing more), then the charge of empirical emptiness would be sustained, since, with a free hand to specify the hypothetical problem, any trait whatsoever could be an optimal solution to some hypothetical problem or other. Instead, the proponents of an adaptationist explanation must also claim that the ancestors of the organism with the trait actually faced that problem at some time in its evolutionary history, that the problem-situation endured long enough for the adaptive trait to be shaped out of random mutations in response to the problem, that there is reason to think that each of the mutational steps involved in the production of the trait was itself adaptive,[19] and so on. Any or all of these further claims to which an adaptationist explanation

[19] Of course, under special circumstances, some one or more of the mutational steps might have been adaptively neutral or even, perhaps, slightly adaptively negative, if there is some further explanation for its spread through the population.

commits its proponents are in principle subject to investigation and testing without presupposing the correctness of that (or any) adaptationist explanation.

In addition, the above is only one dimension along which functional explanations in evolutionary biology may be subjected to empirical confirmation or disconfirmation. Another is provided by the fact that adaptationist explanations are not the only ones possible in evolutionary biology. (This should be obvious since the critics presumably wish to replace adaptationist explanations with others.) Hence, adaptationist explanations of a given trait may be competitive with and defeated by non-adaptationist explanations. This seems to be what happened, for example, with respect to the human chin. Once there was a satisfactory and well-supported non-adaptationist explanation for that bone structure, there was no reason to seek any further adaptationist explanation. (Gould and Lewontin, 585)

*II. The Problems of Selection and of Functional Equivalents*

Root also claims that functional explanations (where they are not mediated by the deliberate choices or intentions of some designer) fail to adequately address what he terms the selection problem – "why of all the solutions that might have been selected [to a hypothetical design problem] T was selected." (1993, 83)

> An answer to the selection question must include a description of how the trait is transmitted from some members of the group to others. The biologist explains how, first by theorizing that the trait is the effect of a gene and next by describing the process by which genes are inherited. However, this answer to the selection question replaces a functional explanation with a causal one....
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> Functionalism faces a dilemma.... If it doesn't offer an answer to the selection question, then it doesn't explain the presence of the trait; but if it does provide such an answer, then the answer, rather than the functional fact, explains the presence of the trait. Add the causal facts needed to explain the selection of the trait, and the fact that the trait is functional is trimmed from the explanation; for the causal rather than the functional fact now explains the presence of the trait.[20]

[20] Root 1993, 86.

In a closely related criticism, Root maintains that functional theories in evolutionary biology do not deal adequately with the problem of functional equivalents. For any given trait, T, for which a case can be made that it is adaptive with respect to some problem faced by an organism, there is some other possible trait, T', which would be equally adaptive with respect to that problem. However, the only explanation that the biologist has to offer as to why T appears in the population rather than T'is that genes for T and not for T'appeared among the organism's ancestors. Once again, a functional explanation is replaced by a causal explanation. (1993, 86-88)

I am addressing both of these together because it appears that they both rest on the same mistake – roughly, upon the assumption that causal explanations are invariably competitive with functional explanations. A useful way of showing that they are not is in terms of the model of fitness landscapes.[21]

One imagines the ancestors of an organism at some time in their history located upon an abstract landscape, a fitness landscape, where ascents represent increases and descents represent decreases in inclusive fitness. Where exactly the organisms ascend (or not) depends on causal factors – what adaptively favorable mutations occur. These will be hills (or mountains) on the adaptive landscape. So, in one sense, there is a causal explanation for every adaptively favorable mutation that spreads through a population. However, if there had been a mutation producing a different but functionally equivalent trait present (then and there) on the fitness landscape, that trait would have spread through the population instead. The functional explanation focuses on what is common to the two or more different causal stories, namely, that they both contribute (or would have contributed, had they occurred) to inclusive fitness.

[21] For some discussion and further references, see Dennett 1995, 190ff. Further useful discussion may be

The point can be illustrated with a non-biological example: What explains the stable and non-interfering orbits of planets in the solar system? Why are there not many bodies in unstable orbits? How, in short, were the stable orbits selected (the selection problem) and why *these* stable orbits (the problem of functional equivalents)? Now, there is, of course, for each planet, a causal account that explains why it is in the particular stable orbit that it is. Apparently, what Root would recommend is that we be satisfied with taking the conjunction of the separate causal accounts as the explanation for the overall order of the solar system.

But there's a simpler explanation, obvious once it has been suggested, that operates on a different level of generality. To wit: the planets we see in stable orbits are *survivors*. There may have been any number of bodies in the solar system in unstable orbits. But, given time (and in the absence of any regular or large-scale influx of bodies in unstable orbits from outside the system), they either fell into the sun, achieved escape velocity from the system, or collided with something else, thus producing one or more new bodies which then, if the collision did not result in its product(s) achieving stable orbit, either fell into the sun, achieved escape velocity, or collided with something else ... and so on.

Eventually, any solar system, in whatever state of order or disorder it may have begun, can be expected to be either empty or to be occupied primarily by bodies in stable orbits.[22] That is, though there is a detailed causal explanation for each stable orbit, there is also a functional explanation for the fact that almost all the orbits are stable. Since the explanations are not competitive with one another, the correctness and relevance of the detailed causal account does not, as Root presupposes, necessarily undermine claims that a functional account is *also* correct

found in Nagel 1968, 80-96.

[22] I am still assuming the absence of any large-scale or regular influx from outside the system.

and relevant.[23, 24]

## *Gould's "Exaptationist" Alternative*

Steven Jay Gould has long been skeptical about the range of adaptationist explanations in evolutionary biology. In the classic 1979 article which he co-authored with Richard Lewontin, "The spandrels of San Marco and the Panglossian paradigm: a critique of the adaptationist programme," we find:

> We feel that the potential rewards of abandoning exclusive focus on the adaptationist programme are very great indeed. . . . Too often, the adaptationist programme gave us an evolutionary biology of parts and genes, but not of organisms. It assumed that all transitions could occur step by step and underrated the importance of integrated developmental blocks and pervasive constraints of history and architecture. A pluralistic view could put organisms, with all their recalcitrant, yet intelligible, complexity, back into evolutionary theory. (Gould and Lewontin, 597)

The point being made is not, of course, to deny that adaptationist explanations have any role, but rather to claim that other types of explanation also play an important role. Indeed, at the time of that article, Gould apparently regarded adaptation as "the most important of evolutionary mechanisms." (Gould and Lewontin, 589)

In a more recent article, however, he has claimed that what he calls "exaptations"—a term coined to refer to another type of evolutionary mechanism—"are neither rare nor arcane,

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[23] The qualification, "not … necessarily," is important. Some causal accounts would undermine some functional explanations. If, for example, we had reason to think that basic laws of celestial mechanics ruled out unstable orbits, then the functional explanation would be an unnecessary fifth wheel.

[24] It might be wondered whether, if we can find functional explanations appropriate for the explanation of non-biological systems, there remains any interesting normative punch to the claim that natural functions are important to understanding living organisms. Are we saying any more when we say, e.g., that the heart is *supposed to* pump blood than when we say that the planets are "supposed to" be in stable orbits?

I think we can identify a difference between the two cases. The most promising suggestion in this direction that I know comes from Nozick. His suggestion is that, for full-fledged natural functions, we need a two-level etiological account: "Z is a function of X when Z is a consequence (effect, result, property) of X, *and* X's producing Z is itself the goal-state of some homeostatic mechanism [or process, such as conscious design or natural selection] ... and X was produced or is maintained by this homeostatic mechanism M (through its pursuit of the goal: X's producing Z)." (1993, 118) In other words, some organ or process will have a function if it is "designed" to have that function and the "designing" itself can be understood as a homeostatic process. See also Nagel 1968.

but *dominant* features of evolution." (Gould, 43, emphasis added)[25] He is especially concerned to claim that they play or should play a dominant role in the explanation of human psychology. The burden of the article is to maintain that the concept of "exaptation . . . cries out for recognition as a key to evolutionary psychology." (Gould, 43)[26]

*I. Gould's Terminology*

Gould begins the article with a generally sensible explanation, which I shall not rehearse, of the need for the term (and concept), "exaptation". Exaptations, he says, are "useful structures coopted from other contexts—for such structures are fit (*aptus*) not by explicit molding for (*ad*) current use,[27] but as a consequence of (*ex*) properties built for other reasons." (Gould, 47) Elsewhere, he adds that the structures coopted may have evolved "for no purpose at all". (Gould, 46) As a neutral term to apply to those cases where there is fitness for some current use but where we haven't got available the evidence to decide whether we are dealing with an adaptation

[25] It may be that Gould does not mean to be making quite so strong a claim as appears in this quote from the abstract. Later in the article, he refers to a "retained Darwinian core" of evolutionary biology (52)—which suggests that adaptationist explanations still have considerable importance—and, in the passage which most nearly echoes the words of the abstract, is talking directly about the human brain as "the chief exemplar of exaptation". (55) On the other hand he also says there that the brain is "*prima facie*, the best available case for predominant exaptation"—which might be understood in either the stronger or weaker sense.

[26] Leaving aside pointless attempts at legislating verbal usage, it has to be agreed that if Gould is correct about the extent to which exaptations are important in explaining human psychology, then their recognition is important to any evolutionary psychology. That is, the features called exaptations would be important in explaining human psychology, and there is a non-supernatural, non-miraculous evolutionary account to be given that explains our possession of those features.

That is not, however, the way that I or that most of those involved in what I have called the research program of evolutionary psychology have used the term. Instead, it has been employed to identify the research program that seeks and expects to find pervasive evidence for cognitive *adaptations*. If there is, as Gould thinks, not much to be found in the way of cognitive adaptations, the evolutionary background of the features will have little to do with explaining the psychological effects. If, for example, language is a "spandrel of the mind", then knowing that our linguistic capacities have an evolutionary explanation gives us little guidance in the formulation of testable hypotheses about language that cannot be gotten more easily from the study of the capacities themselves.

[27] I have a quibble with Gould's use of "adaptation". Note that the emphasis on current use differs somewhat from the account given above: "An adaptation is a trait which is present in a population of organisms because there has been, at some time and in some environment, selective pressure for that trait." My definition leaves open the possibility of describing a trait—such as the eyes of blind fish who pass their lives in complete darkness—as an adaptation for sight.

On his account, they are . . . what? Lacking a current use, perhaps they are nonaptations. But then, if their descendants should be exposed to a different set of selective pressures and regain their sight, should we say that the eyes of those descendants are exapted for their new role? To say the least, the emphasis on current use in defining adaptation seems awkward.

or an exaptation, he suggests “aptation”. For the cases in which a structure or feature is not evolved for any purpose, he uses “nonaptation”.

This terminology seems useful to make some important distinctions. However, at times, Gould risks employing the category of exaptation to eliminate any application for adaptation. (He doesn’t actually go so far, of course, but it’s not altogether clear what stops him.) For example, he says, “coopted structures will probably undergo some secondary modification . . . for the newly seized function. . . . But such secondary tinkering does not alter the primary status of such a structure as coopted rather than adapted.” (Gould, 47)

Well, why exactly doesn’t it? What if the secondary modification amounts to massive and intricate retooling of the prior structure for its new use? Are we to say, for example, that the wings of a bat are *not* an adaptation for flight because they evolved from fore-limbs that were used for other purposes? More generally, every adaptation relies in various ways upon pre-existing features, traits and structures that were not selected for the current use. But, if we were to take Gould’s trivializing remark about secondary tinkering quite literally, we would have to conclude that every feature of an organism had the “primary status” of an exaptation.

*II. Are Exaptations Predominant in Human Psychology?*

The foregoing has been mostly terminological, and I have no doubt that, purged of occasional rhetorical excesses or verbal infelicities, there are exaptations which may be usefully contrasted with adaptations nor do I have any doubt that whether a feature is a case of an adaptation or of an exaptation will make a considerable and potentially theoretically fruitful difference to the explanations we can offer in evolutionary psychology. The real question, which Gould begins to address towards the end of this article, is as to the relative predominance of one or the other type of feature in human psychology.

Gould’s claim is that the human brain provides “*prima facie*, the best available case for predominant exaptation—in other words, [it is] a near certainty that exaptations must greatly exceed adaptations in number and importance.” (Gould, 55) I shall not examine all of his claims in detail but will limit myself to the more modest project of showing that he has substantially overestimated the evidence if he thinks that what he offers supports “near certainty”.

Consider what Gould offers as a “prime candidate”—language. (61f) Many of the features to which he points or alludes, such as the complexity and universality of the generative grammar, seem to call out for some adaptationist explanation. Presumably, he is willing to allow that there may have been such an explanation, just not that it was language for which there was selective pressure. Instead, there was *something* for which there may or may not have been, at the time it arose, an adaptationist explanation—which had all the complex structure, was universal among our ancestors, and got exapted in the service of language.

Well, it can hardly be denied that it’s possible that Gould is right. But one would like to know: What is the “something”? Why has it got the complex structure that we now find exapted into language? Where do we look for evidence of it? It is hard to imagine that he could seriously think—even if it were much more difficult than it is to construct and test an adaptationist account of the origin of language—that this nebulous hypothesis (Language was caused by something or other, but not by natural selection) has much standing as a likely alternative to an adaptationist account.[28] Surely, it is the sort of hypothesis one would resort to only after repeated failings of attempted adaptationist explanation, perhaps slightly ahead of “God did it.”

Many of Gould’s other examples offered as evidence for predominant exaptation[29] are infected by one or both of two further problems. First, there is a question about the appropriate characterization of an adaptive problem. “Stereoscopic vision probably arose for precision of locomotion in the three-dimensional world of trees. . . .  not for the purposes so central to modern

[28] I consider the argument of Pinker and Bloom in “Natural Language and Natural Selection” to amount to a decisive rebuttal to the sort of position Gould is adopting here. (Gould, of course, could not have read that article at the time of his own paper since his was published earlier. I do not know if any versions of that article were circulated in draft form or if Gould had a chance to examine one.)

[29] Some of Gould’s examples seem rather odd. He says that he is concerned with “human universals and cultural predictabilities”, but among his examples are reading and writing. (59) In one sense, reading and writing may be described as exapting linguistic capacities, which, however they arose, were not shaped by selective pressure to subserve reading and writing. But they hardly seem to be human universals or cultural predictabilities. Probably, most of the people who have ever lived have been illiterate (or non-literate). To the extent that reading and writing are widespread—but not universal—among human cultures, this seems like just the sort of case that an explanation in terms of general nonstupidity and Good Tricks is called for. (Dennett 1995, 485f.)

human life." (Gould, 60) Thus, its current use appears to be an exaptation. Or does it? On a different level of generality, as Austin Dacey has pointed out (6), "the adaptive problem could be cast as something like 'navigation', in which case stereoscopic vision in hominids remains an adaptation."

So, what determines whether a feature is really an adaptation or an exaptation? How free a hand do we have in specifying the context that sets the adaptive problem? The problem is difficult and I don't have any general answer to offer, but we need to appeal to some level of generality not tied to all of the details of the environment in which a feature evolved. Otherwise, every feature that has an evolutionary history will turn out to be an exaptation, for there are always *some* differences between the environment and circumstances in which a feature evolved and those of its current use. Adaptations would be eliminated by redefinition.

Second, Gould says "that distinction of adaptation from exaptation requires knowledge of historical sequences—and that such evidence is often, probably usually, unavailable." (Gould, 47) But, if this is so, what happens to Gould's listing of likely psychological exaptations? He rarely says anything directly about the historical sequences in which they arose. Instead, he relies mainly upon intuitive arguments and challenges to imagine adaptationist explanations. The passage just quoted suggests, however, that in most cases he couldn't provide the necessary evidence to show that we were dealing with exaptations rather than adaptations and states that, without the historical evidence, we are not justified in drawing conclusions that some feature is (or is not) an exaptation. Thus, by his own account, most of the evidence he cites is at best inconclusive in favor of the large claims he made on behalf of predominant exaptation.[30]

[30] I don't think that matters are as difficult to settle as he does. True, historical evidence is needed, but complexities of structure universal in a species, call out for adaptationist explanation. Then, the hypotheses that are formulated to provide these explanations can themselves suggest where to look to find confirming or disconfirming evidence. See also the discussion in Cosmides and Tooby of "what adaptations look like" (55-61) and of the "central elements of evolutionary functional analysis" (73-77).

## *Conclusion*

In the early part of the current paper, I claimed that the critics of cognitive adaptations in evolutionary psychology had a heavy burden of proof to discharge. Now, having examined a number of those criticisms, I think it can be said that where there are problems, they are in general problems that we can expect to resolve through further and more careful empirical investigation within the research program. It is salutary to be reminded that the research program rests on theoretical assumptions which we should be willing to reconsider and test, but adequate grounds for dismissing the search for cognitive adaptations in evolutionary psychology as either misconceived or unlikely to bear fruit have not been presented. The burden of proof has not been discharged. The theoretical credentials of evolutionary psychology appear to be in order.

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