Just Do It

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INTRODUCTION

In the target article, I tried to make several points. The most important of these are: 1) adaptations sometimes can be illuminated by studies of current reproductive consequences; 2) studies of current reproductive consequences (adaptiveness) can help us understand the extent to which psychological mechanisms are general purpose; 3) environmental novelty can foul-up the development of psychological mechanisms, just as it can foul-up adaptive outcomes; 4) it is inadequate to simply assert that a current psychological construct, even if universal (e.g., mate-choice preference for average facial features, etc.), must have produced adaptive outcomes during the Pleistocene; 5) the methods extolled for evaluating current adaptiveness are not equally valid; and 6) the most evolutionarily relevant aspects of the Pleistocene were probably social, implying that (a) current environments may have changed much less than is widely assumed and (b) hunting and gathering per se is probably not a crucial aspect of our evolutionary background.

Blurton Jones

Nicholas Blurton Jones points out, and I agree, that there are a number of different ways to use evolutionary theory to gain an understanding of people and other organisms. Specializing in the use of one subapproach does not imply the belief that others are invalid. I also agree that so-called evolutionary psychologists have so far done little of what they repeatedly promise they can do, and that dual evolutionists have been big on modeling but have largely neglected to search for the empirical evidence necessary for demonstrating that their models have anything to do with actual behavior.

There are three arguments I disagree with. First, I don't think the "adaptationists" have divided themselves into the "sociobiologists" and the "be-

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havioral ecologists." Some of the earliest explicitly human sociobiological research was cross cultural (e.g., Gaulin and Schlegel 1980; Flinn 1981; Betzig 1982; Hartung 1982). The point of this research was to understand particular dependent variables in terms of the independent variables that vary from culture to culture. Many such independent variables are ecological; for example, it appears that social stratification is influenced by social circumscription, and it also appears that social stratification is influenced by physical features that can circumscribe, such as mountain ranges (see Betzig 1982; 1986).

I think the distinction to which Blurton Jones is alluding hinges on the extent to which different individuals have sought to measure ecological parameters related to food quests, e.g., Hill and Hurtado have measured calories, Betzig and Turke have not. This difference, however, stems not from differences in philosophy but differences in the specific questions being asked. In any case, what is important is that Blurton Jones and I agree that there should not be a distinction between studies of fitness consequences and studies of variation in response to ecological variables.

Second, although some of us have been accused of being "phenotypically agnostic," I don't think that the "psychologists" see themselves as differing from the "adaptionists" in a way that can be understood in terms of the proximate/ultimate distinction. Tooby and Cosmides, for example, construct their hypotheses out of their understanding of past selection pressures, and therefore their hypotheses are ultimate causation hypotheses every bit as much as those of Alexander, Blurton Jones, Chagnon, Hawkes, or Irons. In fact, Tooby and Cosmides would like to reserve the label, adaptationist, for themselves (Tooby and Cosmides, this volume). The primary source of contention as I see it is that Cosmides et al. argue that information about adaptiveness means almost nothing, while also claiming that Alexander et al. think that information about adaptiveness means almost everything. In contrast, I argue that Alexander et al. believe that information about adaptiveness is properly *one* component of the adaptationist approach, because such information often can contribute to our understanding of adaptations and the phenomena of life shaped by adaptations. Thus, the "psychologists" might properly be accused of being poor adaptationists, but they cannot be accused of not being adaptationists.

Third, the "dual evolutionary" approach, because it is a *dual* would seem to be inclusive of what Blurton Jones calls adaptationist and psychological approaches, and therefore would seem to include all the shortcomings of these approaches, plus the shortcomings unique to the cultural evolution component. Therefore, I don't see how the dual evolutionists can be said to be on a firmer or more logical theoretical foundation (see Flinn and Alexander 1982; Daly 1982).

Silk

Given, as Joan Silk emphasizes, that adoption is complex and variable across cultures, it seems likely that all components of the evolutionary approach will be required if we are to understand adoption. Thus, Silk, in doing more to increase our understanding of adoption than anyone else, has considered—and stresses the need to continue to consider—a number of hypotheses which view adoption as one or another type of reproductive strategy with psychological, political, economic, and demographic components.

For example, one might hypothesize, for reasons that need not be developed here, that adoption of nonrelatives in western societies might be adaptive because of social disadvantages associated with childlessness. Or one might hypothesize, alternatively, that such adoption is a maladaptive effect of maternal and paternal emotional mechanisms gone awry in novel environments. The point is that a first step in distinguishing these hypotheses might appropriately involve trying to discover the impact adoption has on the components of inclusive fitness.

In general, understanding why potentially informs the study of "how" (Tinbergen 1963), and tests of adaptiveness can distinguish hypotheses positing "why" (see, e.g., Grafen 1988: 458). Said another way, an understanding of evolutionary (i.e., past) reproductive significance often must preceed attempts to understand mechanisms, and a first step in understanding evolutionary reproductive significance often must involve understanding current reproductive outcomes (i.e., current adaptiveness). Thus, it probably is not accidental that none of the individuals arguing vehemently against one or another component of the evolutionary approach—and hence against, a priori, one or another class of hypothesis—have contributed significantly to our understanding of complex phenomena, such as adoption, depotism, polyandry, morality, etc.

Irons

William Irons' commentary directly addresses each of the six points listed in the Introduction, and generally agrees with them. Beyond noting here that his commentary contributes many additional, important insights, I will direct the remainder of this reply to the three commentaries by Jerome Barkow, Leda Cosmides, Donald Symons, and John Tooby (BCST). BCST mostly focus on what they believe is an inordinate interest in current adaptiveness, which, accordingly will be the focus of my response.

BCST Acknowledge the Importance of Measuring Current Fitness

First, it is important to recognize that, disguised with much irrelevancy, redundancy, and innuendo, BCST end up agreeing that assessments of current fitness can illuminate adaptations (e.g., Symons, this volume, p. 430; Tooby and Cosmides, this volume, pp. 391 and 398–399). Symons should also recognize that Alan Grafan and Randy Thornhill, whom he cites repeatedly, also defend the view that measurements of current fitness, especially the components of fitness, can illuminate adaptations (Grafen 1988; R. Thornhill, personal communication). Grafen, for example, states that "all methods of investigations have their drawbacks, and in analyzing LRS (lifetime reproductive success) data I would certainly perform the LRS analyses first and unleash my suspicions on the results afterwards" (p. 458; see also p. 456). This admission, coupled with the patent falsity of the claim that I and many of my colleagues believe that measurements of fitness provide a complete description of an adaptation, indicates that there is no substantive basis for the dichotomy implied by Symon's DA/DP schema.

Furthermore, abstract, theoretical claims about the primacy of a particular subapproach are generally invalid. The usefulness of a subapproach must be decided on the basis of the question of interest (Tinbergen 1963; Betzig 1989). For example, findings suggesting that the patterns and rules of adoption play themselves out adaptively throughout Oceania is an important first step towards refuting the widely held belief that the cultural patterns and rules of adoption (or any other aspect of culture) arise and change by a process of independent cultural evolution (Durkheim, Marx, White, Sahlins, Cloak, Dawkins, et al.). The reason is that such results are not expected from independent cultural evolution, but are expected if particular psychological mechanisms are functioning as they were designed to function by individual level selection (e.g., Turke and Betzig 1985:86; Turke, this volume). Obviously, however, tests that discover adaptive outcomes do not deny the importance of additional studies of mechanisms; rather, tests of adaptiveness facilitate the study of mechanisms. Thus, primacy is in the eye of the beholder.

You Can't Take the History Out of Natural History

Tooby and Cosmides begin their commentary with quotations by Austin Hughes and Robin Dunbar. These are meant to demonstrate that many human sociobiologists fail to understand that adaptations are designed by a history of selection and, accordingly, believe that measuring current fitness consequences (i.e., current selection) is all there is to an evolutionary approach. Hughes and Dunbar's position is contrasted with that of George Williams, which then allows Tooby and Cosmides to conclude that "[T]here is a deep though largely unexplored schism in modern evolutionary thought . . ."(p. 376). Incredibly, they then devote their paper to refuting the claim that current selection pressures have designed adaptations. Symons' commentary also finds it necessary to point out that adaptations are designed by past selection, and he also brings the reader's attention to Dunbar's work. Barkow's commentary strives, essentially, to make the same point by asserting (incorrectly) that I maintain that assessing current adaptiveness "is the central question for human sociobiology" (cited from Barkow's abstract).

Although Hughes may have been taken out of context (I think he intended to suggest only that it is inadequate to simply assert that a particular trait must have been adaptive during the Pleistocene), Dunbar appears to stand guilty as charged (although in view of his solid primatological contributions I think it likely that even he has somehow managed to argue himself into a corner in which he does not actually stand). Why, in any case, do BCST think that Alexander, Betzig, Chagnon, Dickemann, Hill, Irons, Kaplan, and Turke (to name some of those singled out in recent papers by BCST) support Dunbar's erroneous opinion? Perhaps they believe that we are guilty by association, since the quoted statements by Dunbar appeared in Chapter 9 of *Human Reproductive Behaviour* (1988), edited by Betzig, Borgerhoff Mulder, and Turke. But read on.

First, it is relevant to note that Dunbar's chapter was commissioned as a replacement for Symons' chapter. Second, Betzig and Turke asked Dunbar to cut the statements in question, but were refused. Pressure from the publisher led, unfortunately, to our capitulation. In short, we were forced to exchange one extreme view for another—current fitness consequences mean nothing, to current fitness consequences mean everything—and regret the whole affair. Third, it is especially relevant to note that Symons, through correspondence with Betzig, was aware of this whole chain of events. Moreover, he has been aware for some time of published statements by Betzig criticizing Dunbar for taking "the history out of natural history" (Betzig 1989). In fact, Symons cites Betzig on this point, but, oddly, he does so in the middle of an attempt to characterize her, and me, as supporters of Dunbar's perspective. How can he justify such behavior? I suppose that he would answer that we say one thing and do another. But read on.

In fact, what Betzig and Turke (and others) have done is *sometimes* found it useful to test adaptationist hypotheses by measuring current reproductive outcomes. For example, my primary research interest has been in understanding fertility determinants—fertility transition in particular (e.g., Turke 1985; 1988; 1989; 1990). A central hypothesis which I developed in this research is that individuals are expected (in light of theories of life history evolution [e.g., Williams 1957]) to have been psychologically designed (by a history of selection, which in terms of current development involves learning solutions to current problems) to invest in their offspring, grandchildren, and other younger relatives throughout life. Thus, at least in existing traditional environments—environments in which social organization continues to be based on kinship—I expected to find that the direction of net resource flows between individuals would be from the elderly to their younger relatives (more precisely, from individuals who are not directly able to convert resources into offspring to their close relative who are). This pattern of resource flow, I hypothesized, can contribute significantly to reproductive output.

Through a variety of measures I found that resources are flowing in the expected direction on Ifaluk Atoll, Yap Island, and in the few other traditional societies for which the appropriate quantitative information had been gathered (e.g., Hadza, rural Trinidadians) (see especially Turke 1989). Moreover, I found that resource flows are having the effect I expected: young adults with elderly parents available to provide resources and services *are* outreproducing their counterparts (e.g., Turke 1985; 1989).

If additional tests on Ifaluk and other societies tend to further confirm the above hypothesis, we will have advanced our understanding in the following ways. In terms of understanding fertility transition, it would seem likely that one of the mainstream explanations of high fertility is reversed: the conclusion that a projected need for old age security stimulates adults in traditional societies to have many children, becomes an offspring-based old age security institution can be expected in many traditional societies because it serves the reproductive interests of the elderly, but the reproductive interests of the elderly generally lie in the well-being of their younger relatives. So, in effect, old age security generally is for the well-being of the young. This is a very different interpretation from what passes as dogma in the demographic literature (e.g., Caldwell 1982; Handwerker 1986), and has profound policy implications. For one thing, it implies that simply supplying the elderly in traditional societies with government pensions will increase fertility and completed family size (if all else remains the same), which is exactly the opposite of what generally is intended. (For the latest details and additional implications for demographic arguments, see Turke 1990.)

In terms of understanding human evolution (including evolution of psychological adaptations), the above hypothesis and tests yield a number of implications. I will discuss two of these. 1) Support is given to my assumptions about which features of the environments of human evolutionary history have been important in the evolution of traits governing demographic outcomes; these include facts and ideas about the nature of kin networks, the vulnerabilities to which children have been subjected, and the particular services and resources which have been reproductively limiting (see especially Turke 1989). That is, my hypothesis and others like it (Turke, this volume) provide a window into the past.

To Tooby and Cosmides (this volume), my having pointed out in the target article that understanding past selection pressures is difficult suggests that I therefore believe that we should give up on trying to do so. In fact, the more impartial reader will find that I argued exactly the opposite. Because it is difficult but nevertheless important to reconstruct the past, I argued that we should use every opportunity to do so, including conducting tests of those adaptationist hypotheses which predict currently adaptive outcomes. BCST, in contrast, argue that X was adaptive in the Pleistocene because Y and Z were selective pressures in the Pleistocene, but they forego one opportunity to evaluate the validity of Y and Z by foregoing the opportunity to determine whether X is currently adaptive, and if not, determining why. Thus, one reason I characterized BCST as accepting a simple, static picture of the Pleistocene is that anyone who thinks she or he knows so much about the Pleistocene that tests of the foregoing type are unnecessary, must also have a relatively simple, static model in mind.

2) My hypotheses on fertility transition have been explicit on the mechanisms involved, to the extent that I believe current knowledge makes this possible. (One should not confuse this kind of restraint for phenotypic agnosticism, nor should one confuse a lack of appropriate restraint with psychological sophistication.) By testing such hypotheses one tests (among other things) statements made about mechanisms, which should, as BCST repeatedly point out, allow further sharpening of the hypotheses. I believe that this cycle of progression is precisely what has emerged from my work on fertility transition (see Turke 1985; 1988; 1989; 1990; this volume).

I emphasize that had I failed to find that elderly parents confer a reproductive advantage on their adult offspring, my explanation for high fertility in traditional societies would have been severely challenged. Moreover, the proximate psychological components of my hypothesis would have been challenged at least to the point of raising questions about why current environments interfere with the development of mechanisms which obligate parents to their offspring for life, or why this sense of obligation, if it has developed, fails to produce the intended reproductive consequences. (To Barkow, I note that if all of the above is not indicative of a vertically integrated approach, I don't know what is.)

In using the above example to illustrate my perplexity at having been labeled a phenotypically agnostic adaptivist, I also could have used any number of examples from the work of colleagues like Alexander and Betzig (see the target article). (Note, though, that papers that are largely phenotypically agnostic may nevertheless be extremely insightful, and therefore should not be condemned on first principles [e.g., Trivers and Willard 1973].) Thus, it seems to me that the approach of Alexander et al. differs little from that of many who have escaped the criticisms of BCST.

Consider the work of Martin Daly and Margo Wilson (1988) on homicide. The important difference between my work on fertility transition and theirs on homicide lies not in me not doing what they have done but rather in their not doing one of the things that I have done. Whereas I have explored the current reproductive consequences of predicted patterns of resource flows, they have not explored the current reproductive consequences of homicide. This is not a criticism of Daly and Wilson (it is legitimate to pass over particular avenues of the evolutionary approach as long as the avenues being pursued continue to bear fruit); nor does it imply that they have made less progress on their chosen topic than I have on mine. Still, an explicit description of the selective pressures under which homicide would have been adaptive in the past can generate predictions about the present circumstances, if any, under which homicide should be adaptive. Support for such predictions would help to substantiate the arguments about relevant selection pressures and mechanisms. Disconfirmation would question these same arguments, or require an explanation as to how current circumstances interfere with the function of the mechanisms that were designed by the selective pressures designated in the hypothesis. In either case, one would be in an improved position for the further study of mechanisms.

Another similarity between my work and that of Daly and Wilson is that we are each trying to understand human nature in order to understand a specific phenomenon which is of interest in its own right. In this respect, I believe that the only person involved in this debate who is doing something somewhat different from the rest of us is Leda Cosmides: answers to the Wason selection task are not intrinsically interesting (e.g., Cosmides 1985). Although I support her efforts, and recognize that her findings may be applied to intrinsically interesting phenomena, I nevertheless believe—as she probably does—that an understanding of fertility determinants, homicide, etc., would come more slowly if we all limited outselves to her experimental methods.

To summarize this point, there is no validity to the claim that I or the colleagues I have mentioned fail to recognize that adaptations are products of past selection pressures. The "deep schism" that Tooby and Cosmides believe they have identified actually separates the vast majority from almost no one. Furthermore, there is no validity to the claim that I or my colleagues believe that understanding current adaptiveness is all there is to understanding adaptations.

General Purpose Mechanisms

I suspect that mistaken assumptions about the evolution of general purpose learning mechanisms (see the target article) are behind Tooby and Cosmides' misguided claim that Alexander et al. do not understanding that adaptations have been designed by past selection. To illustrate, consider a recent article by Robert Smuts (1989) which was cited by Tooby and Cosmides (this volume) as an example of the widespread belief that adaptations are products of current selection pressures. In commenting on Buss (1989), Smuts argues cogently that some of Buss's predictions about human mate choice should be altered to more explicitly recognize that mate choice is a complex phenomenon that has, *throughout human evolutionary history*, functioned to solve an array of often novel economic, social, sexual, and reproductive problems. Once one recognizes this history of dealing with novelty, it becomes plausible to suggest, as Smuts does, that current patterns of mate choice are somewhat novel adaptive solutions to the somewhat novel social, economic, sexual, and reproductive problems posed by current environments.

Smuts obviously is not ignoring past selection pressures. However, Tooby and Cosmides, like Symons (1989) in his misunderstanding of Crook and Crook on polyandry (see Alexander and Turke, this volume; Betzig 1989), fail to recognize this because they deny (or at least restrict too severely) the evolution of general-purpose learning mechanisms (note: the term general-purpose, as I use it, does not imply infinite flexibility or adaptive response in all situations). This overly narrow view of human flexibility leads Tooby and Cosmides, I think, to the conclusion that Smuts is proceeding from the assumption that human mate choice mechanisms have been designed by current selection pressures. It is to this same deficit which Alexander (this volume) is referring when he writes, "[Symons] must believe either that the ability to learn is not an adaptation or that learning ability did not evolve because it could be (was being) applied to multiple or generalized life situations" and "To deny that learning has evolved to deal with novelty seems to me to deny the possibility of the most distinctive psychical attributes of humans having evolved . . .".

In short, by assuming an overly narrow facultative ability for humans, BCST mistake hypotheses predicting facultative adjustment to a wide range of current problems for the absurd idea that such hypotheses posit that the relevant adaptations themselves are somehow designed by these current problems (i.e., ironically, they confuse outcomes with mechanisms). It is reasonable to entertain hypotheses which predict considerable adaptive flexibility because theory and evidence indicate that the environments of human evolutionary history posed a range of novel social problems inclusive of many current problems (e.g., Alexander 1989; Turke 1989; this volume).

Note, though, that the term "novel" as Alexander and I use it does not mean completely unprecedented as Tooby and Cosmides (this volume, p. 44) try to imply. Although Tooby and Cosmides are correct in pointing out that humans cannot be expected to adapt to completely unprecedented phenomena, nothing that I or my close associates have written contradicts this obvious fact. For example, Alexander (this volume), Betzig (1989), and Turke (this volume) all go in to some detail describing why the problems that Tibetan polyandry solves are likely to be closely related to the problems faced by our distant ancestors. I made a similar point in my discussion of the avoidance of fast moving trucks (Turke, this volume).

Perhaps another example is necessary. The ordinary integer 31486779015 has a precedent, which undoubtedly dates to the Pleistocene, in the ordinary integer 2. Similarly, doubling 31486779015 and subtracting it from itself almost certainly has a precedent in these functions having been carried out during the Pleistocene with the number 2 (e.g., during food sharing). Thus, in all of these respects 31486779015 is not novel. However, odds are that no one has ever written out the number 31486779015, doubled it, and subtracted it from itself, and therefore in some sense doing so is novel. The alternative to the belief that arithmetic cognition mechanisms have been designed to solve "novel" problems is the belief that individuals have a distinct mechanism for solving each specific arithmetic problem that occurred in sufficient frequency, and with sufficient importance to fitness, in the Pleistocene. However, this alternative is simply too domain specific to be theoretically plausible, and the fact that humans are able to do arithmetic correctly on numbers that would not have been relevant in the Pleistocene provides empirical refutation. Thus, arithmetic cognitive ability must be representative of a general purpose mechanism that, by design, has infinite range on some dimensions.

Although Tooby and Cosmides appear to be trying to draw a distinction between their own position on novelty and that of Alexander and Turke, they are, in effect, acknowledging the existence of general purpose mechanisms for dealing with novelty when they write "ability to handle a certain kind of variation depends upon selectively significant encounters with cues probabilistically linked to that type of variation in the evolutionary past" (this volume, p. 407). Unfortunately, this style of argumentation (distort a position, correct the distortion) appears throughout Tooby and Cosmides' commentary.

Adaptiveness

Following Barkow (1989) and Symons (1987), Tooby and Cosmides (this volume, p. 401) present a list of human activities which are supposed to represent definitive examples of maladaptive behavior; they list buying pornography, listening to music, donating blood, and more (see also Tooby and Cosmides 1989). One cannot draw firm conclusions so easily. To imagine and then assert that people who buy pornography, listen to music, and donate blood would increase their inclusive fitness by not doing these things is invalid because imaginary strategies are not constrained like the strategies of real people (Turke, this volume). Rules, norms, customs, and limits on available information are only a few of the subtle and not so subtle factors which constrain actual behavior.

Returning to an earlier example (Turke, this volume), Cosmides and Tooby may be able to imagine salmon foregoing the immense struggle to return to their place of birth to spawn, but I don't think they would count on the fact that salmon do engage in this struggle as evidence of maladaptiveness. Rather, like most of us, they would leave this issue open for the time being, because there is a reasonable chance that a better understanding of the constraints will eventually reveal the adaptiveness of this behavior (see Quinn and Dittman 1990). They should be just as careful when it comes to people.

In addition, and probably more important, Cosmides and Tooby seem to think that I (and colleagues) have a stake in proving that people behave adaptively. Perhaps they think that I think evolutionary theory, or at least the adaptationist approach, will be invalidated by finding maladaptiveness. Wrong—we are all aware that past selection pressures mold designs that can yield maladaptive outcomes in current environments, and that understanding past selection therefore is likely to be the key to understanding such outcomes (Alexander, this volume). Nevertheless, although I am certain that some current human behaviors will prove to be maladaptive, I am not certain about any of Tooby and Cosmides' specific examples.

Consider pornography. Tooby and Cosmides' claim that the preoccupation of some males with pornography is primae-facia evidence of maladaptiveness. Apparently they believe this because, as they state, pornography involves the stimulation of a Pleistocene mechanism by non-Pleistocene objects; "artificially created images of females." (Note: Pleistocene males probably imagined female images, and they certainly drew and sculpted them, so artificial images are not as novel as implied.) Thus, in their view, exploring the current reproductive consequences of pornography wastes time that should be devoted to thinking of hypotheses, like pornography might "reflect some underlying adaptation, such as the obviously oversimple decision rule: move towards situations that produce retinal images of naked nubile females and become sexually aroused" (p. 402).

However, to me, what is most obvious about their decision rule is not that it is oversimple (which it is) but that it does not even begin to tell so-called Darwinian anthropologists anything that we did not already know. Of course, males have been designed to have an interest in nubile females, and of course pictures of such are likely to sometimes stimulate and attract. While Darwin and Trivers deserve much credit for providing the insight which makes this now obvious inference possible, the inference itself does not count for much. Unfortunately, although there are exceptions, this has been the standard so far in the work of many who call themselves Darwinian psychologists.

BCST would probably agree that a useful psychological theory of pornography would attempt to explain the ultimate and proximate features of the phenomenon in terms of life history (e.g., why interest peaks at particular ages); it would explain variation in the popularity and respectability of certain categories of pornography across social and economic categories; and it would explain social intracacies like why an interest in pornography is hidden or denied in some contexts (e.g., in the presence of females, in the presence of higher status males) and openly and used for status advancement in other contexts (e.g., among one's pool-room buddies). The problem, of course, is that such a theory would be complex; it would require the description of a large number of rules (adaptations) like "move towards situations that produce retinal images of naked nubile females," "engage in activities which trigger cues to status," "note that status cues are context dependent," and "reflect on past experiences to make probabilistic predictions about context dependency," as well as many additional rules that coordinate this very incomplete list of relevant adaptations.

I know of no better way of building such a theory than by trying to identify and coordinate these rules as components constituting a complex, highly plastic, reproductive strategy. Thus, if anyone can come up with a plausible theory of the reproductive significance of pornography, I want to know about it—whether or not it predicts currently adaptive outcomes.

This tendency to produce overly simple mechanistic explanations for complex phenomena results mostly from the tendency to focus on one or two specific algorithms that limit plasticity to one or two choices. An alternative view of human nature is *not that specific algorithms are absent* but that they are complexly interrelated in a way that yields phenotypes that are much more plastic—much more capable of a general purposeness—than BCST imply. This point is extremely important to the current debate about the relevance of current reproductive outcomes because relatively inflexible organisms are expected to fare poorly in even slightly novel environments; relatively flexible organisms are not. Thus, tests of current adaptiveness delimit the boundaries of human flexibility, and in so doing begin to identify and describe psychological mechanisms. I am astounded to find that Tooby and Cosmides would regard such insights as trivial (see p. 399), whereas in fact, to deny the importance of such tests is to "deny the possibility of the most distinctive psychical attributes of humans having evolved . . ." (Alexander, this volume, p. 32).

Aggregated and Dissaggregated Components of Fitness

As Tooby and Cosmides point out (this volume, pp. 399–400), lifetime fitness results from hundreds or thousands of adaptations, making it problematic to claim an association between fitness and a specific adaptation. However, steps can be made towards overcoming this problem by measuring the components of fitness under partially controlled situations (Grafen 1988). See Turke and Betzig (1985) for an example.

In this same context, Tooby and Cosmides also suggest (p. 400) that adaptiveness can mean "high specific functionality in some particular behavior or phenotypic expression (e.g., the eye achieves the special purpose of seeing well, whether or not in any specific instance that contributes to reproduction)." They intend here to divorce adaptiveness from reproductive success, but cannot do so completely because the function of any single adaptation is constrained by the functions of other adaptations. Thus, ultimately, what constitutes "well" in "seeing well" is not based on absolute criteria but on reproductive criteria. That is, the vision system we have is not the one that, out of the available alternatives, maximized vision, but the one that maximized fitness. Of course, as Symons notes (this volume), the fitness of designs—not of individuals—is what is ultimately of interest. The reason is that "only differences in outcomes which are due to differences in design can direct the evolution of adaptations" (Turke, this volume). Nevertheless, designs manifest their fitness through the actions of individuals, so individual reproduction is not of no interest as Symons implies (this volume, p. 428).

Tooby and Cosmides on Adaptation

Tooby and Cosmides state that "[I]ndividual phenotypes are instances of designs, but not designs themselves" (this volume, p. 395). To illustrate, they argue that in sequential hermaphrodites being male or female is not the adaptation, rather, the adaptation "is the conditional rule." While I agree that conditional rules can be adaptations, such rules are always products of development, and as such are always phenotypic. In other words, both the design for an adaptation, and the instance of the adaptation are phenotypic. So even the development of conditional rules is contingent on environmental continuity. Genetic instructions, in contrast, are the nonphenotypic rules that underly design (Williams 1985), and genetic instructions accordingly are not adaptations.

Moreover, as Tooby and Cosmides acknowledge, there are many adaptations that come between genetic instructions and the rule specifying hermaphroditism, and between this particular rule and the actual male or female outcome. Thus, adaptations unfold in a continuous process of development (Turke, this volume), and in the case of hermaphroditic fish even the process of being male or female (after the decision rule has been activated) is comprised of many adaptations.

In related discussion, Tooby and Cosmides suggest that polyandry, for example, cannot be an adaptation because to practice polyandry or not to practice polyandry is caused by environmental rather than genetic differences (this volume, p. 395). I agree, the multitude of mechanisms (wet and behavioral) that underly the outcome (e.g., polyandry or monogamy) are the adaptations.

However, there almost certainly is not a single switching rule that specifies polyandry under some conditions and monogamy under others. It is plausible, though, to hypothesize that there are specific cost-benefit rules which, when integrated and applied to the social, sexual, and economic problems encountered in Tibet (and a few other places), generate polyandrous outcomes (Betzig 1989; Alexander, this volume; Turke, this volume). In this sense, there may be adaptations for specifying a range of outcomes, and in some instances polyandry is the specified outcome.

Tooby and Cosmides on Emotions

As Tooby and Cosmides suggest (this volume), emotions are adaptations that can guide behavioral strategies to adapative outcomes. For example, as they note, feelings of grief are expected to be cued by occurrences which in the evolutionary past have had maladaptive consequences, and therefore grief can serve as an incentive to avoid such occurrences. As such, I would imagine that grief originated long before hominids. *Homo sapiens sapiens'* grief, however, appears to be much more than a mechanism for avoiding events that were correlated with maladaptive outcomes. In fact, grief by *H. s. s.* may be less important as an incentive for avoiding certain kinds of events than it is as a mechanism for bringing together social support in times of crisis (e.g., grieving at funerals), and for making image-enhancing statements about the kind of caring, committed, loving person one is. In this light, both conflicts of interest and the potential for manipulation are likely to be central to an understanding of human grief (cf. Alexander 1987, on guilt). If this argument is reasonable, human grief is much more than a guidepost for avoiding formerly maladaptive circumstances, and therefore it will be necessary to study grief as a component of complex, plastic, social reproductive strategies. In turn, understanding the current reproductive consequences of grief is likely to be important.

Tooby and Cosmides on Timing

Contrary to Tooby and Cosmides (this volume, p. 391), the most recent 500 generations are likely to have shaped human evolution more than the 500 that preceded them, given, as they claim, that the environment (i.e., selective pressures) has changed more in more recent times. For the same reason, the previous 2 million years are not likely to have been only 1/5th as important to human evolution as the 10 million years which preceeded them.

Hindsight

I have argued, following many others (e.g., Betzig 1989; Alexander 1988; this volume), that understanding "why" informs "how," and that understanding "why" often requires understanding current reproductive consequences. Kitcher (1985) and Symons (1987) have suggested, however, that folk psychology offers a superior guide to the study of "how." Symons also claims that because of his "evolution-mindedness" (e.g., 1989) few of the explanations of "why" offered by Alexander et al. have told him anything he didn't already know. These kinds of claims, I think, are meant to downplay the importance of well designed, and in many cases well supported, evolutionary hypotheses about adapative significance (see also Alexander 1988).

But are folk psychology and evolution-mindedness adequate substitutes for careful evolutionary hypotheses and the data which test them? Consider the following examples. Barkow (this volume) accepts much of what demographers have concluded about old age security and high fertility, whereas I have rejected these conclusions (see above); yet we both are evolution-minded and familiar with folk psychological wisdom. Similarly, while some prominent evolutionists are also prominent Marxists, Symons claims that his evolution-mindedness leads him to expect that Marxist predictions are doomed to failure (e.g., Symons 1989). All of this raises the question of whether Symons' lack of surprise (especially when it comes to Betzig's findings) is due to evolution-mindedness, folk psychology, or hindsight.

Symons' Examples

In the target article, the example of stepping out of the way of a fast moving truck was used to illustrate the existence of general purpose mechanisms.

Symons notes, and I agree, that "correlations between avoiding/not avoiding trucks and reproductive success" does not have much to offer anyone trying to understand the adaptations involved. However, neither I nor my associates have ever claimed that such correlations are always useful. We have argued that knowledge of evolutionary reproductive significance is useful, and is sometimes informed by current reproductive consequences, but in the case of avoiding heavy fast-moving objects like trucks evolutionary reproductive significance is not obscure.

However, what if the behavior in question were much more obscure? What if many cultural anthropologists believed that the mysterious practice of truck avoidance was only superficially related to survival but deeply related to some structural consideration concerning the fact that some foods are eaten raw while others are eaten cooked? What if advocates of Dawkins' (1976) meme theory believed that the idea "truck avoidance" had a nice ring to it and spread through the population of ideas on that account? What if still others argued that truck avoidance evolved to serve the good of the group? In each case these hypotheses would be refuted or at least questioned by evidence demonstrating that this mysterious behavior correlates with fitness. Refuting these types of hypotheses has been important, both for clarifying our understanding of specific phenomena and for challenging non-adaptationist views of human nature (see Turke, this volume). Thus, much of this debate could have been avoided if BCST had recognized the specific goals that individuals such as Chagnon and Irons were trying to accomplish (e.g., Chagnon and Irons 1979).

The foregoing is not to suggest, though, that an understanding of current fitness effects is important only in the refutation of a few odd hypotheses popular in the social science literature. On the contrary, fitness effects can illuminate adaptations in ways that even evolutionarily informed scholars should appreciate. For example, if Chagnon argued that the mysterious truck avoidance behavior was part of a mating strategy and Irons argued that it was part of a parental strategy, we could begin to test their hypotheses by determining the components of fitness most effected by truck avoidance. In turn, as a result of their research we would be in a better position to conduct even more detailed studies of the mechanisms involved. In short, the evolutionary adaptive significance of many of the phenomena of interest to anthropologists, biologists, and psychologists is much more obscure than it is for truck avoidance, and therefore such phenomena have been, or can be, illuminated by studies of current reproductive outcomes.

In another example, Symons (this volume, p. 433) concludes that the amount of sugar, fat, and salt eaten by westerners is maladaptive. On the basis of good evidence indicating that these dietary factors contribute to many degenerative diseases, he is probably right. His point, though, again is that concluding that particular behaviors are maladaptive or adaptive does not inform our understanding of the adaptations comprising behaviors. Here I suggest his case against measuring fitness consequences is not as tight as for truck avoidance. That is, even for this seemingly straightforward example—in which a current phenomenon appears to be fully explained by the malfunctioning of a few specific Pleistocene mechanisms—it nevertheless might be useful to explore the reproductive consequences of a preference for fat, sweet, and salty foods. What if, for example, it turned out that after controlling for likely confounding factors we found that the level of fat, salt, and sugar consumption optimal for Darwinian fitness is significantly higher than the level optimal for physical fitness? I suggest that such a finding would stimulate and guide a good deal of new research on mechanisms.

In regard to the points Symons (this volume) raises about the Yanomamo, work by Chagnon since at least 1975 has sought to demonstrate that the cultural ideals (including fierceness) and corresponding cultural strategies of Yanomamo males are reproductive strategies. This work is seminal, hotly contested by mainstream anthropologists, clearly supported by Chagnon's analyses of differential reproductive success (e.g., Chagnon 1988), and by design, helps to refute the argument that human nature is a nature conducive to independent cultural evolution.

Symons' statements about Chagnon's data not indicating genetic differences, or differences in anatomy and physiology, between the Yanomamo and other people are irrelevant, and suggests that he misunderstands some of the arguments about phenotypic plasticity, learning, and general purposeness, to which he alludes. At the very least, Symons does not understand the nature of the arguments into which Chagnon has entered.

Labels

Labels can be useful, but they also can be abused. Sahlins crossed the line separating use from abuse by implying that some of "us" might be "vulgar sociobiologists" (Sahlins 1976). Similarly, Gould and Lewontin (1979) applied the label "adaptationist program," and thereby attempted to identify us as the group believing that every aspect of every phenotype is perfectly adapted to every circumstance. Kitcher calls us "pop sociobiologists" supposedly to distinguish us from the sociobiologists studying nonhumans—apparently "human sociobiologists" never occurred to him (Alexander 1988). Now Symons would like us to be known as "DAs" (Darwinian anthropologists), "DSSs" (Darwinian social scientists), or as belonging to the "adaptivist program" (Symons 1989; this volume); and Tooby and Cosmides favor the label "correspondence program" (this volume).

Some of these labels are mildly offensive, and thus are obviously meant to cast doubt through ridicule. This *ad hominem* tactic has the advantage of being easier and less risky than directly challenging what is believed to be an opposing position. Symons and Tooby and Cosmides have been more subtle than Sahlins, Gould, Kitcher, and Lewontin, but nevertheless have crossed the line into abuse.

In particular, their use of labels has been liberating. For example, see

Tooby and Cosmides' anonymous, pseudoquotation describing the position of the correspondence program (this volume, p. 377). This is not an isolated incident; the publications of BCST are littered with the claim that DAs believe that humans are infinitely flexible, infinitely general purpose, and capable of adapting to every situation (e.g. Tooby and Cosmides, this volume, p. 403).

Whereas it is difficult to attribute ridiculous arguments to Alexander, Chagnon, and Irons, the anonymous correspondence program says all manner of stupid things; any position can be attributed to the DA approach, because DA cannot defend herself (cf. Alexander 1988). It is too bad that much of the current debate can be traced to individuals using such contrivances to claim a superior position, when in fact we are all very much in the same position.

CONCLUSION

Questions of originality aside, I agree in general with the approach advocated by BCST, even while disagreeing with many of the specific hypotheses they have derived. Because motives and associated behaviors are often veiled and mysterious (probably by design, in hypersocial species such as our own [Trivers 1985; Alexander 1987]), understanding the evolutionary adaptive significance of traits is a crucial but difficult task. BCST recognize that understanding evolutionary adaptive significance is crucial but they seem not to appreciate the difficulties involved. That is, they ignore (in fact deplore) tests which potentially bear on their assumptions about past selection pressures, and in their place rely on assertions about what must have been adaptive during the Pleistocene. In part, this stems from their failure to acknowledge the extreme flexibility and capacity for deception and self-deception that so obviously characterizes humans.

Anyway, at long last BCST have begun to acknowledge that understanding current reproductive consequences can help us in the extremely important and difficult task of understanding the evolutionary adaptive significance of adaptations. With so much agreement in the air, it is time to get on with the task of generating and testing specific hypotheses about human nature, and about the phenomena that result from that nature. So to BCST: just do it.

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