

Evolutionary Psychology

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Evolutionary Psychology

Volume II

Edited by

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Series Preface

The theory of evolution is one of science's great achievements. Though to those outside science, it may seem that the theory is controversial, within science there is no controversy at all about its basic form. Moreover, the theory of evolution plays a pivotal role in guiding new research. 'Nothing in biology makes sense except in the light of evolution', Theodosius Dobzhansky famously wrote; the theory of evolution unifies disparate subfields of biology and generates testable predictions for each. The success of the theory and its explanatory fecundity for biology cannot be doubted. But might the theory also be capable of illuminating phenomena outside the direct purview of biology?

The volumes in this series are dedicated to exploring this question. They bring together some of the best writings of the past two decades which explore the relevance of evolution and evolutionarily-inspired thought to arenas of human life beyond the merely biological. Volumes focus on whether it is productive and illuminating to attempt to understand our most distinctive achievements and our most intimate features as evolved phenomena. Is the content of moral systems explained by evolution? To what extent are the processes of selection and reproduction that explain changes in gene frequencies also at work in explaining the reproduction of ideas? Can evolution shed light on why we think as we do, perceive as we do, even feel as we do? Might even our idea of God – and perhaps with it the perennial temptation to reject evolution in the name of religion – be explained by evolutionary thought?

Answering these questions requires not only a detailed grasp of the phenomena we aim to explain – the contours of religious thought, the features of morality, and so on – but also an understanding of the theory we aim to apply to the field. Though the theory of evolution is not itself controversial within science, there are lively controversies about its details. One volume of this theory is devoted to writings which illuminate these controversies and deepen our understanding of the mechanisms of evolution. It is only if we have an appreciation of how evolution works that we can begin to assess attempts to extend its reach to culture, to the mind, to morality and to religion.

The volumes are edited by experts in the philosophy of biology and include sensitive and thoughtful discussions of the material they contain. Naturally, in selecting the papers for inclusion, and given the large amount of high quality thought on the philosophy of biology, and on each of the topics covered by these volumes, it was necessary to make some hard choices. Each editor has chosen to focus on particular controversies within the field covered by their volume; on each topic, a range of views is canvassed (including the views of those who deny that evolution can contribute much to the understanding of non-biological features of human beings).

Evolution is our story; in coming to understand it, we come to understand ourselves. Readers of these volumes should be left with a deepened appreciation for the power and ambition of evolutionary thought, and with a greater understanding of what it means to be an evolved being.

NEIL LEVY

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Introduction

There is widespread agreement among social scientists that evolutionary theory can and should inform the study of human mentality and behaviour. It is no longer controversial that humans are evolved organisms. Evidence of our ancestry is written all over human anatomy. Aspects of our dentition (Leonard and Robertson, 1992, 1994), cranial morphology (Lieberman *et al.*, 2002) and brain organization (Barger *et al.*, 2007) are just a few of the traits that bear markings of the environments in which our species evolved. Presumably human cognition, emotion and behaviour are no different. These traits also exhibit heritable variation that can impact reproductive fitness. If natural selection shaped human morphology, it should have shaped mentality and behaviour as well. The success of evolutionary theory in explaining the psychological traits of other animals further suggests that this framework can be profitably applied to humans (De Waal, 1982; Krebs and Davies, 1993; Alcock, 2005).

Yet, psychologists (and social scientists generally) have been slow to adopt an evolutionary approach. Most undergraduate psychology courses and textbooks contain only a passing mention of evolutionary concepts (Buss, 2003). Within the psychological research community, evolutionary hypotheses are often considered highly speculative and not a proper subject of empirical investigation. This predicament is somewhat puzzling. Why has an evolutionary framework failed to take hold in this discipline given that most psychologists recognize its potential importance?

One answer can be found in the writings of contemporary evolutionary psychologists. Evolutionary psychology is one of a handful of recent attempts to bring the human mind within the purview of evolutionary biology. This research programme has gained notable momentum in recent years (for example Daly and Wilson, 1988a; Barkow, Cosmides and Tooby, 1992; Buss, 2005; Dunbar and Barrett, 2007). However, its reception within the social sciences at large has been far from welcoming, inspiring such titles as *Alas, Poor Darwin* (Rose and Rose, 2000) and *The Mind Doesn't Work That Way* (Fodor, 2000). Evolutionary psychologists attribute this resistance to a biologically implausible view of human nature, bequeathed to psychology by seventeenth-century empiricism (Cosmides and Tooby, 1992; Pinker, 2002). According to this view human beings are infinitely malleable creatures. We do not come into the world with innate dispositions towards behaviours or towards certain ways of thinking and responding; we come into the world as *blank slates* upon which experience impresses itself. If we are aggressive (say), then it is because we have been taught to be aggressive (wittingly or not); and if we wish to eliminate aggression we need to discover what in our environment disposes us towards aggression and alter it. The blank slate view is taken to underlie what evolutionary psychologists disparagingly call the Standard Social Science Model (SSSM) of human nature. This framework privileges culture and individual learning over evolutionary and genetic explanations of human psychology. Leda Cosmides and John Tooby, two leading evolutionary psychologists, explain the problem as follows: 'The conclusion that human nature is an empty vessel, waiting to be filled by social processes,

removed it as a legitimate and worthwhile object of study ... Why study paper when what is interesting is the writing on it?' (1992, p. 229).

Cosmides and Tooby deny that the SSSM enjoys any theoretical or evidential support. Instead, they ascribe its popularity to moral and political factors. In the last century, nativist explanations of human nature became associated with such offensive ideologies as eugenics, institutionalized racism and sexism. Meanwhile, the blank slate model became associated with such optimistic messages as human malleability and the unlimited potential for social reform (Cosmides and Tooby, 1992). Evolutionary psychologists cite these political and moral associations as the primary reason why their discipline, and evolutionary ideas generally, have encountered such resistance within the social sciences.

If this explanation is correct, then evolutionary ideas have indeed been unjustifiably neglected. An evolutionary approach to psychology is inherently no more pernicious (nor laudable) than viewing the mind as a product of culture (Pinker, 2002). Nor does an evolutionary perspective imply that human behaviour is immutable. The view that the mind does contain genetically specified rules and concepts that have been modified by natural selection allows for the possibility that different social and ecological inputs will generate very different cognitive and behavioural outputs. An understanding of these psychological biases could in fact benefit humanity through the creation of better informed public policy. For example, evolutionary psychologists are well poised to identify the psychological mechanisms that generate racist and sexist convictions. Knowledge of how these mechanisms function could potentially lead to social policies that dampen their expression. Granted, the evolutionary psychologists' picture of human nature does not allow for *infinite* malleability. There are bound to be constraints on the kinds of behavioural institutions that humans will accept. However, this is not a justification for rejecting an evolutionary approach to the mind. It is an obvious mistake to reject any scientific hypothesis on the grounds that it has moral or political implications that one finds unpalatable. Regardless of its potential moral or political appeal, the blank slate model is of little moral consequence if it is false. It is important to be clear that we are not suggesting that all hypotheses in evolutionary psychology are morally neutral. Some of these proposals have had the potential to be quite damaging (Thornhill and Palmer, 2000). Rather, our claim is that moral considerations should not serve as a justification for rejecting this perspective. In fact, some of the most morally questionable evolutionary psychological proposals turn out to be flawed for purely scientific reasons.

This brings us to a second explanation for evolutionary psychology's controversial status. The label 'evolutionary psychology' refers to a particular research tradition associated with John Tooby and Leda Cosmides, David Buss, Martin Daly, Margo Wilson and a handful of others. This research programme has attracted the criticism of many who otherwise endorse an evolutionary approach to psychology. These critics rarely invoke moral or political considerations in their objections. Where they take issue is with the theoretical and methodological commitments of this particular research framework. A brief overview of the core tenets of evolutionary psychology will help frame these debates and provide context for the essays in this volume.

Evolutionary psychologists view the mind as a collection of computational mechanisms or 'modules'. Each module is thought to execute a distinct algorithm that is specialized for solving a particular adaptive problem. Evolutionary psychologists further assume that these modules evolved under ancestral conditions that differed markedly from contemporary environments.

A methodological upshot of this view is that one cannot identify the evolutionary function of a module by measuring its current impact on reproductive fitness. Instead, evolutionary psychologists attempt to match particular modules with adaptive problems that humans encountered in the ancestral (hunter-gatherer) environment. One of their primary research strategies is to look for psychological mechanisms that are found universally across individuals and cultures. If a module is universal, they argue, then it is most likely an adaptation. Likewise, if a module is an adaptation, then it should be possible to 'reverse engineer' the ancestral conditions that shaped it.

Critics of this research programme tend to reject one or more of these assumptions. For example, some theorists reject the picture of a 'massively modular' mind. An alternative view regards the mind as containing a domain-general processor that employs a small set of algorithms for a wide range of tasks. Note that this is not a reversion to the blank slate. It is possible that the mind contains some innate rules or concepts which are supplemented by a domain-general reasoning mechanism. If this hybrid model of the mind is correct, it is unlikely that all cognitive functions can be matched with particular adaptive problems. Unlike a massively modular system, a domain-general processor is constrained in its ability to specialize on particular information processing tasks. Another criticism of evolutionary psychology concerns the methods used to identify putative psychological adaptations. Evolutionary psychologists attempt to match current psychological mechanisms with ancestral adaptive problems that may no longer obtain. One problem with this approach is that ancestral environments are difficult to reconstruct. Evolutionary psychologists are frequently accused of adopting overly relaxed standards of evidence to support their speculations about which evolutionary processes shaped the mind. A third set of criticisms concerns the assumptions that psychological modules are developmentally buffered against environmental influences and therefore invariant across individuals and cultures. There is considerable evidence that human psychology is structured to a large extent by culture and individual learning. Once again, one need not view the mind as an entirely blank slate in order to hold this view. As Elizabeth Lloyd remarks in Chapter 8,

It is one of the most pernicious aspects of the present climate of discussion, that the situation is often set up as a forced choice between accepting the particular theories and oversimplified principles of evolutionary psychology, or retreating to a pre-Darwinian denial of the fact that we are evolved animals. (p. 155)

Many theorists would agree that it is possible to study the mind from an evolutionary perspective without embracing the assumptions and methods of evolutionary psychology. Yet, to date, evolutionary psychology arguably remains the most developed research tradition when it comes to Darwinian thinking about the human mind.

The essays collected in this volume focus on the theoretical foundations of evolutionary psychology. All of these essays appeared in peer-reviewed journals and have made important contributions to debate over this field of research. The following sections in this introduction correspond in subject matter to the five parts of this volume. Part I considers whether evolutionary psychology provides a unifying framework for the social sciences, as its proponents often claim, and also attempts to outline the debate between evolutionary psychology and its sister discipline, human behavioural ecology. Part II evaluates the theoretical and empirical evidence for the 'massive modularity' hypothesis. Part III investigates the methods used by evolutionary

psychologists to identify psychological adaptations. In Part IV we consider how evolutionary psychologists propose to reconstruct the 'Environment of Evolutionary Adaptedness'. Finally, in Part V we consider some ethical and theoretical considerations surrounding the search for human cultural universals.

Theoretical Background

To some extent, the heat surrounding the debate over evolutionary psychology is a little puzzling. What can all the fuss be about? No one seriously doubts that human beings are the product of evolution and that the traces of our history are to be discerned in our minds as well as our bodies. What, then, is different about the group of psychologists who call themselves evolutionary psychologists? How do these researchers differ from others who take an evolutionary approach to psychology?

In Chapter 1 David Buss suggests part of the answer: evolutionary psychologists see themselves as offering a single unified framework for psychology, a discipline that he sees as currently in a state of disarray. All psychological mechanisms are the product (directly or indirectly) of evolution, Buss argues, and all psychological theories entail the existence of psychological mechanisms. Hence all psychological findings and theories, if they have at least a grain of truth to them, can be incorporated into an overarching evolutionary account of human nature. Buss suggests that existing theories are already, implicitly, evolutionary theories; it remains only for them to recognize that they cohere with one another in an evolutionary framework.

Yet saying all this does not seem to get to the heart of the matter, since most psychologists would accept that the mechanisms they study are all (directly or indirectly) the product of evolution, and that it is therefore true that insofar as a mechanism or disposition exists, it can be slotted into an evolutionary framework. The overarching framework does not get at the basic difference between evolutionary psychologists and their rivals.

Buss has a second suggestion: he suggests that evolutionary psychology alone gives us means of individuating mental processes in a way that is non-arbitrary. He points out that it is common to individuate the parts of the human body by reference to their function – the heart is a distinct organ, separate from the lungs, because the heart has the role of pumping blood, whereas the lungs have the function of respiration. There are indefinitely many ways of taxonomizing the body but a functionalist taxonomy has a good claim to carving up its nature at its joints. Similarly, Buss suggests, an evolutionary framework provides us with the only non-arbitrary means of taxonomizing psychological mechanisms: by reference to the functions they evolved to perform. Arguing along similar lines, in Chapter 3 Martin Daly and Margo Wilson complain that theories in mainstream social psychology have, 'risen and fallen more like a succession of fashions than like the building blocks of a cumulative science' (p. 48). The reason that animal behaviour researchers have made genuine progress, Daly and Wilson suggest, is because this discipline partitions its subject matter – the animal mind – into evolutionarily informed categories like mate value assessment, kin recognition and parental investment allocation.

The suggestion that evolutionary theory provides the one true mental taxonomy would, we think, be controversial among conventional psychologists. One obvious response is this: the function for which a mechanism is adapted is not necessarily the function it performs now,

in modern societies. Hence the functionalist criterion does not pick out a unique taxonomy. Of course, if we can identify the evolutionary function of a mechanism we could individuate it on that basis, but if today the mechanism has fractionated, such that it no longer serves its original function and *together with some other mechanism* or parts of a mechanism, it serves some different function, then we have at least as good a reason to individuate it on the basis of its new function and not its old (see Smith *et al.*, Chapter 4, this volume).

It may be that Buss's confidence that the evolutionary account can be the basis for a uniquely non-arbitrary taxonomy is the product of a more basic confidence that the function of our mechanisms will not significantly diverge from the functions they are adapted to perform. As much is suggested by his claim that processes other than selection, such as mutation and genetic drift, are unlikely to 'fashion complex, precise, efficient, well-sculpted mechanisms' (p. 4, n. 1). He seems to believe that either our mechanisms will be adaptations or there will be nothing interesting to explain at all. This confidence, in turn, is the product of a particular view of what evolution can be expected to produce. Whereas other psychologists think that among the products of evolution are likely to be mechanisms that are flexible and general enough to produce novel functions in new environments, Buss denies that this is the case. Instead, he argues that evolution will produce mental *modules*: discrete and independent mechanisms dedicated to particular tasks, which lack the flexibility to be turned to new tasks. Because adaptive problems are many and distinct, and require quite different solutions, mechanisms for their solution are likely to be many in number. Buss believes that evolutionary psychologists have already discovered many such modules, and are likely to discover more in the future. In the following section we investigate the theoretical and empirical evidence for the 'massive modularity' hypothesis in more detail. Let us first consider a theory that rejects this assumption.

An alternative evolutionary approach to human psychology attempts to avoid any such questions about psychological architecture. The field of human behavioural ecology (also called Darwinian anthropology) focuses instead on behavioural strategies or 'decision rules' as the primary unit of analysis. Eric Alden Smith describes decision rules as, 'abstract and somewhat metaphorical ways of conceiving of the covariation of behavior and socioecological environment, having the general form "in context x , do α ; in context y , switch to β "' (2000, p. 30). Somewhat confusingly, decision rules are not meant to describe the operation of psychological mechanisms *per se*. Rather, these rules simply describe behavioural patterns and their fitness consequences in (usually) contemporary environments. A simplified example might help to clarify this distinction between psychological mechanisms and decision rules. Suppose that a group of hunter-gatherers practise reciprocal food sharing. A behavioural ecologist might observe that their sharing behaviour conforms with the rule: after a kill, share with each group member the same amount of protein they offered to you on previous occasions. These individuals need not perform this exact calculation (either consciously or subconsciously) to comply with this rule. Perhaps they base their calculation on some other variable besides protein reciprocation, such as: share most of your food with the hunter who is usually most successful, give a little less to the second most successful hunter and so on. This example points to the fact that decision rules are 'multiply realizable' – they can be psychologically implemented by a variety of different mechanisms. Behavioural ecologists assume that decision rules are more experimentally tractable than underlying psychological

mechanisms, hence they attempt to restrict their investigation to this level of analysis. As Alden Smith explains,

This assumption, which takes a ‘black box’ approach to the actual mechanisms involved, is part of what Grafen ... calls the *phenotypic gambit*. This means taking a calculated risk to ignore the (generally unknown) details of inheritance (genetic or cultural) cognitive mechanisms, and phylogenetic history that may pertain to a given decision rule and behavioral domain in the hopes that these don’t matter to the end result. (2000, p. 30)

Although behavioural ecologists claim to be agnostic about proximate mechanisms, it is unclear whether this ideal is consistent with their research practices. Paradigm examples of this approach involve field studies of modern populations where researchers measure the actual reproductive costs and benefits of particular decision rules (Hill and Hurtado, 1996; Smith, 1998). The default assumption appears to be that even in modern contexts humans will usually behave adaptively. However, this assumption is far from mechanistically neutral. Only certain kinds of psychological or biological mechanisms could generate flexibly adaptive behaviour. The question, then, is what sorts of psychological and biological mechanisms does this perspective presuppose?

Donald Symons (1990), a critic of this approach, argues that behavioural ecologists assume that fitness maximization is an explicit psychological goal of most humans. Why else, he asks, would these researchers expect people to maximize fitness in novel environments that do not resemble the ones in which humans evolved? Such a commitment would seriously discredit behavioural ecology. Humans just don’t base their explicit decisions on fitness calculations.

A more viable possibility is that decision rules are culturally inherited. Since cultural evolution is potentially more rapid than biological (genetic) evolution, this view would allow for behavioural adaptation to contemporary conditions. Smith appears to have something like this in mind in suggesting that inheritance mechanisms might be genetic *or* cultural. However, theoretical developments in dual inheritance theory reveal that not just any psychological architecture will allow for the evolution of adaptive cultural traditions (Boyd and Richerson, 1985). Indeed, the psychological requirements for this outcome are quite demanding (Sterelny, 2006). Once again, the neutrality assumption is violated.

A third possibility is that the human selective environment has changed only minimally since many of our psychological mechanisms evolved, so that there is still a fairly good fit between decision rules and the conditions that shaped them. This would increase the likelihood that decision rules are currently biologically adaptive. This view is endorsed by the human behavioural ecologists Eric Smith, Monique Borgerhoff Mulder and Kim Hill in Chapter 4. These authors note that the fitness value of a behavioural strategy often depends on local circumstances. For example, an individual who has already produced a higher than average number of offspring would probably do better investing in parental care than in mate-guarding. Another individual who has produced few or no offspring would be better off adopting a different strategy. These authors argue that the most plausible proximate mechanism is one that calculates trade-offs among these sorts of fitness-relevant variables. It is these general factors, they claim, that have remained constant over the course of human evolution: ‘only the environmental details are novel, not the fundamental tradeoffs they present, nor the ability to recognize and appropriately react to those tradeoffs’ (p. 62). This is an interesting suggestion that calls for more detailed theoretical and empirical investigation. At the very

least, behavioural ecologists must identify precisely which factors the mind recognizes as salient when calculating trade-offs. Only then will it be possible to begin evaluating the extent to which contemporary environments contain genuinely novel adaptive problems.

Perhaps behavioural ecologists are in fact no less interested in psychological mechanisms than are evolutionary psychologists. Where these two approaches differ is over the extent to which those mechanisms are likely to produce adaptive behaviour in contemporary environments. Let us now consider in more detail the model of mind that informs evolutionary psychology.

The Massive Modularity Hypothesis

Evolutionary psychology is committed to (something like) the massive modularity hypothesis (MMH). According to MMH, the mind consists of a large number of modules, where a module is a discrete functional mechanism dedicated to a single task. This is sometimes called the Swiss army knife model of the mind. Just as the Swiss army knife contains a set of different tools, each designed with one particular task in mind, so the mind contains a set of tools, each one adapted for a particular task.

Evolutionary psychologists oppose the MMH to the view of the mind they attribute to most social scientists, the Standard Social Science Model (SSSM). According to the SSSM, the mind consists of a set of general-purpose mechanisms, which can be put to different uses as the need arises. Mental mechanisms are *domain general*: they are not specialized, like the *domain-specific* mental mechanisms hypothesized by evolutionary psychology.

The idea that the mind is modular is due to Jerry Fodor (ironically, Fodor is one of the most vociferous critics of the MMH and of evolutionary psychology more generally). Fodor (1983) argued that some mental mechanisms have some or all of the following features:

1. Encapsulation
2. Shallowness of output
3. Mandatoriness of operation
4. Cognitive impenetrability
5. Swiftiness of operation

To say a module is encapsulated is to say that it is cut off from most or all other mental processes – that is, information generated by these processes cannot affect its operations. The classic example here is visual illusions. You may well know, say, that the lines in the Müller-Lyer illusion are the same length, but they still *seem* different lengths. The mechanism which produces the seeming is cut off from the information about the illusion. The illusion also nicely illustrates shallowness of output: the output of the module is not a full-blown judgement (you *don't* judge that the lines are different lengths) but a seeming; as Fodor puts it, the output is non-conceptual.

To say that a module is mandatory in its operation is to say that given the appropriate stimulus it is automatically triggered. You can't inhibit modular mechanisms. Cognitive impenetrability is often confused with encapsulation, but whereas encapsulation refers to the inability of a module to get information from other mental mechanisms, impenetrability refers to the inability of other resources to access the workings of modules. From the point of view

of the rest of the mind, a module is a black box: it produces an output, but it is a mystery how it produces it. Finally, modules are swift. They operate in parallel, rather than serially, processing their inputs as soon as they are presented.

There is a great deal of debate concerning which features of Fodorian modularity evolutionary psychology is committed to. Clearly, a mental system need not have all five features to be interestingly modular; however it is plausible to think that once too many of them are dropped from the definition of a Darwinian module, the label is merely functioning as an honorific, not picking out a distinctive mental mechanism. We shall return to this question. First, though, let us set out some of the arguments in favour of the MMH.

Cosmides and Tooby (1994) present four distinct arguments for the MMH, which, following Frankenhuys and Ploeger (2007), we will call the *engineering* argument, the *error* argument, the *poverty of the stimulus* argument and the *combinatorial explosion* argument.

The engineering argument turns on the claim that modules are better designed solutions to the kinds of problems that human beings needed to solve in the 'environment of evolutionary adaptation' than would be domain-general alternatives. The general idea is clear and intuitive. Think of the Swiss army knife metaphor again: clearly, if you need to tighten a screw you would prefer to use the specially designed Swiss army knife attachment than attempt to use the blade of an ordinary penknife. As Cosmides and Tooby put it,

different adaptive problems often require different solutions and different solutions can, in most cases, be implemented by different, functionally distinct mechanisms. Speed, reliability and efficiency can be engineered into specialized mechanisms because there is no need to engineer a compromise between different task demands. (1994, p. 89).

This claim is surely true; specialized mechanisms will typically be better at performing particular functions than unspecialized. But is this a reason to expect evolution to have produced such mechanisms?

Despite originating the term modularity and being responsible for its centrality to cognitive science, Fodor rejects the MMH. For him, only peripheral systems – those involved in perception, essentially – are modular. Fodor (2000) has levelled a number of criticisms against the MMH. Here we shall touch on some of the more interesting. Fodor rejects the engineering argument because he thinks it is false that we ought to expect evolution to optimize along the dimensions Cosmides and Tooby (1994) select – that is, we have no reason to think that evolution will optimize speed, reliability and efficiency. Evolution *always* involves compromises between various desiderata: developments along one dimension always come with costs along others. For instance, bigger brains might be adaptive, insofar as they allow for better solutions to problems confronting an organism, but brains are hugely costly in energetic terms. So there is no reason to expect evolution to optimize speed, reliability and efficiency, even though were it to do so it would produce better designed mental mechanisms, *other things being equal*. Other things are not equal; whether evolution will select for modules over general-purpose mechanisms depends on how costly the trade-offs are (as well as what phenotypic variations were available). We discover these costs by looking, not by a priori argument. Since the engineering argument is a conceptual claim, it fails.

The error argument claims that modular systems are required to solve adaptive tasks because adaptiveness requires that our mental processes and dispositions are fine-tuned to the tasks confronting an organism. If an organism attempts to apply algorithms appropriate for working

out how to divide its time between hunting and gathering, say, to attempting to work out how much time to devote to helping kin, it is likely to misallocate resources badly. Since accepting a single rule across all domains would be maladaptive, we ought to expect criteria of success and failure to vary across domains. And this entails the existence of modules dedicated to domains.

As several people including Fodor (2000; see also Samuels, Chapter 6, this volume) have pointed out, this is a very weak argument. Domain-general systems can apply domain-specific rules. Such a system need only to possess different rules and be sensitive to triggers switching it from applying one set of rules to another. Again, the error argument gives us no reason to expect modularity of mind.

The poverty of the stimulus argument was made famous by Noam Chomsky. As Chomsky developed it, it was an argument for the innateness of generative grammar. Roughly, the argument holds that children must come into the world already possessed of such a grammar, because linguistic proficiency far outruns the range of stimuli to which infants are exposed. That is, since children learn all the essentials of their native language despite the poverty of the data they are exposed to, learning cannot be doing all the work. Learning must be scaffolded on innate structures.

Cosmides and Tooby advance a similar claim:

Adaptive courses of action can be neither deduced nor learned by general criteria alone because they depend on statistical relationships between features of the environment, behavior, and fitness that emerge over many generations and are, therefore, often not observable during a single lifetime. (1994, p. 91)

Consider incest. The knowledge that such incest is maladaptive (because it raises the likelihood of serious birth abnormalities) could not easily be learned, because it takes several generations for the effects to become clear. Of course, cultures could acquire such knowledge and transmit it intergenerationally, but were it necessary to learn about the maladaptiveness of incest, cultures could not arise in the first place: relatively primitive organisms need to act appropriately for an organism complex enough to be able to support such a culture to arise. So evolution will need to build such knowledge into organisms directly.

Unfortunately, as Fodor (2000) and others have pointed out, this is not an argument for modularity at all. It is an argument for the innateness of information. As Fodor notes, *prima facie*, at least, innateness and modularity are doubly dissociable: a domain-general system might have access to innate knowledge, and a module might not. In response to this argument, Frankenhuys and Ploeger (2007) have conceded the point but argued that it is only telling against *Fodorian* modules. Evolutionary psychology, they claim, is not committed to anything nearly as demanding. For them, domain-specific innate knowledge suffices for modularity. They justify ignoring the distinction between domain-general mechanisms with domain-specific knowledge bases and domain-specific mechanisms by noting that either can drive – the same – adaptive behaviour.

Surely, however, this is to weaken the notion of modularity to the point where it loses all interest. Suppose that the mind is somewhat modular, but that in addition to these modules there is a general-purpose system capable of acquiring new knowledge. Suppose, further, that this system was able to categorize this knowledge such that it tagged specific items of information as belonging to particular (novel) domains – for example the domain of

automobiles or modern art, or whatever it might be. It seems that Frankenhuis and Ploeger would be committed to saying that functionally, at least, such a system would be able to generate an indefinite sequence of new modules: one for each new category to which items of information are assigned. It is true that domain-general mechanisms with domain-specific knowledge bases and domain-specific mechanisms can generate identical behaviour, but it is not the behaviour that is (directly) at issue in the debate between proponents and opponents of evolutionary psychology, it is the mechanisms involved.

Cosmides and Tooby's final argument for the MMH is the argument from combinatorial explosion. Roughly, the argument is this: without some way to prune back the options, any decision-making mechanism will find the sheer number of possibilities overwhelming. Even considering just a few possibilities, say, the options available on a chess board, and just a few moves ahead, the number of combinations grows exponentially and quickly outruns the capacity of any computational mechanism. As Cosmides and Tooby note, the more flexible a system, the more options it confronts and the worse the resulting combinatorial explosion. Domain-specific architectures neatly deal with this problem, however, by radically curtailing the number of options the organism has available for consideration.

Fodor has two responses to this argument. The first is to argue that though combinatorial explosion is a problem for computational mechanisms, it is false that central (and, putatively, non-modular) systems are computational. Exploring this response would take us too far afield. We shall limit ourselves to considering his second response, which is essentially identical to his response to the argument from the poverty of the stimulus: the combinatorial explosion argument is an argument for innateness, not modularity. Innate content can guide a domain-general system to ignore many of the available options, thus avoiding combinatorial explosion.

Okasha (2003) argues that on this point Fodor is wrong: innate content is not sufficient to forestall combinatorial explosion. He argues that a (computational) mechanism must be encapsulated to avoid this problem: since encapsulation cuts the mechanism off from other information, the problem of combinatorial explosion cannot arise for it. But while it is true that encapsulation is one way to avoid combinatorial explosion, it does not seem to be the only way. As Okasha rightly points out, merely having innate knowledge does not solve the problem: more information, whatever its source, is likely to contribute to combinatorial explosion, not solve it. Something is needed in addition to information: the information needs to be weighted in some way. If the mechanism is biased towards some options then combinatorial explosion can be avoided, even though the ignored options are not unavailable to it. The difference between weights biasing a mechanism and true encapsulation is this: if a mechanism is encapsulated with respect to some information, there are no circumstances under which that information can affect its processes, whereas if a mechanism is merely biased against certain information, that information can affect its processes when it becomes tagged as significant. The question whether information is weighted in this way as a solution to the problem of combinatorial explosion seems orthogonal to the innate–learned distinction, though it may be that some weightings must be innate to get a domain-general system to the point where more can be acquired by learning.

It seems, therefore, that the conceptual arguments advanced by Cosmides and Tooby for the MMH fail. In the end, however, the MMH is an empirical hypothesis; it succeeds or fails on

the basis of the evidence from psychology, not on the basis of a priori arguments. Let us turn to that evidence. Does it support the MMH?

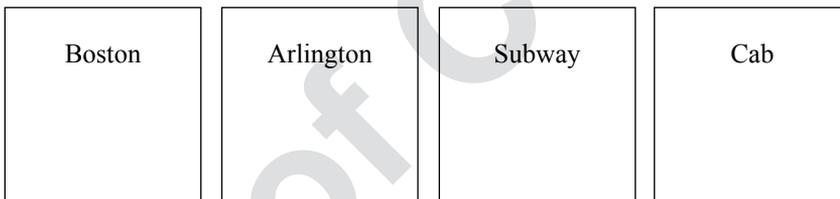
Consider one of the best-known hypotheses advanced by evolutionary psychologists, the hypothesis that we have a dedicated mechanism for engaging in cheater detection. Cosmides and Tooby adapted a pre-existing tool, the Wason selection task, in order to argue for the existence of relevant mental modules. Cosmides and Tooby compared the performance of subjects on two Wason selection tasks.

Wason Selection Task 1

You are employed by the City of Cambridge (Massachusetts) to study the use of public transportation in the local area. According to a previously published report on this topic, the following statement is true:

If a person goes into Boston, then that person takes the subway.

You are required to discover whether this is indeed so. You are presented with a number of cards, which record the transportation habits of Cambridge residents. On one side of each card is printed the destination to which they travel, and on the other is recorded their means of transport. Here are four such cards:



The Wason selection task, version 1

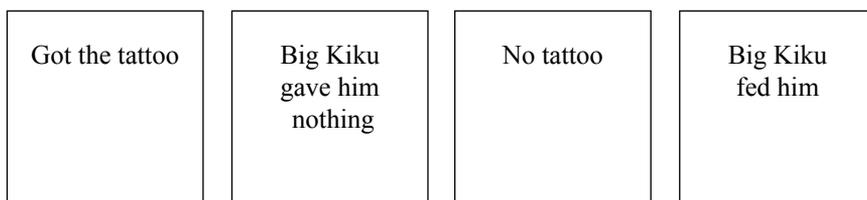
Is the rule true for the people whose transportation habits are recorded here? Which cards *must* you turn over to test the rule?

Wason Selection Task 2

You are an anthropologist studying a Polynesian people called the Kaluame. The tribe you are studying is ruled over by a dictatorial chieftain called Big Kiku. He makes all those who swear loyalty to him tattoo their faces as a sign of their allegiance. Thus, all and only Big Kiku supporters have tattooed faces. Since Big Kiku is very unpopular with the members of other tribes, being caught in another village with a tattooed face is certain to get the unfortunate Kiku supporter killed.

One day, four men who have been kicked out of their own villages come to Big Kiku starving and exhausted. They beg him for food. Big Kiku agrees to feed them, so long as each has his face tattooed. They must get their tattoos that night, and in the morning they will be fed. Each man agrees to the deal.

An informant tells you, however, that Big Kiku hates some of these men, who have previously betrayed him. You therefore suspect that he might cheat them on this occasion. The cards below record information about the fates of each man. On one side of each is recorded whether or not the man had his face tattooed; the other side records whether or not Big Kiku fed him the next day.



The Wason selection task, version 2

Which cards *must* you turn over to discover whether Big Kiku cheated any of these men?

These two tasks are structurally identical. In each case, the rule to be tested is of the form known as *modus ponens*: If p then q . Thus:

If you travel to Boston, then you take the subway.

If you have your face tattooed, Big Kiku will feed you.

If we want to discover whether this rule is violated, we can ignore cases in which p is not the case. If you travel anywhere *other than* Boston, it does not matter what means of transport you employ. The rule does not apply to your action, and you cannot violate it. Similarly, if you did not have your face tattooed, the second rule does not apply. We can also ignore cases in which all we know is that q was the case. The rule does not state that ‘everyone who travels by subway goes to Boston’; in fact, of course, people travel by subway to all sorts of destinations. We can therefore ignore cards which tell us that the person travelled by subway. Turning them over cannot disconfirm the rule, no matter what they say on the other side. Similarly, we can ignore cards which tell us that Big Kiku fed the man concerned. Plainly Big Kiku did not cheat this man, whether he fed him as a result of his tattooing his face or for some other reason.

We need, therefore, only to turn over cards which tell us that p is the case, in order to test whether q indeed follows, and cards which tell that not- q is the case, in order to see whether, in violation of the rule, p was the case. When we think of the selection task in this manner, the solution is obvious. In the first task, we need turn over only those cards which tell us that the person travelled to Boston (in order to see whether they indeed travelled by subway) and that which tells us that the person travelled by cab (in order to see whether, in violation of the rule, they went to Boston). In the second task, we need turn over only the card which tells us that the man got the tattoo (in order to see if Big Kiku fed him, true to his word) and the card which tells us that Big Kiku gave him nothing (in order to see if the man got the tattoo). We can ignore the others.

Logically, the two tasks are identical. However, performance is far worse at the first task than the second. Cosmides and Tooby (2008) found that between 65 and 80 per cent of subjects gave the correct answer in Wason selection tasks like the second, whereas only about 25 per cent of subjects were able to give the right answer in selection tasks like the first.

Why is it that most of us have a far harder time giving the correct responses in the first version of the Wason selection task than in the second? Cosmides and Tooby argue that an

important difference between those selection tasks we find difficult and those we find relatively easy is that the second kind are all concerned with free-riding or otherwise cheating in social situations. We are much better at solving this kind of logical problem when cheating is at issue than when it turns upon some other, more abstract, question. Yet the logical structure of the problems remains the same.

The fact that the substance of the problem makes an important difference to our ability to solve it constitutes further evidence that the brain is not an all-purpose learning device, Cosmides and Tooby argue. Just as studies of perception and illusion show that we have informationally encapsulated modules for judging distance and depth, so the study of the ease with which we comprehend social situations reveals that we have modules in the brain dedicated to the tasks of social life. In particular, the Wason selection task reveals that we each possess a cheater-detection module. The explanation of the origin of this module, just like the explanation of the origin of the module or modules involved in perception, is evolutionary. Our ancestors faced recurrent problems to do with depth and distance perception in the environment of evolutionary adaptedness, problems the solution of which was literally a matter of life and death. So important was it to get the answers to these questions quickly and accurately that people with a dedicated brain module had an advantage over their conspecifics, and therefore had more numerous descendants. But they also faced recurrent and important questions to do with social interaction and the detection of cheats. A brain module dedicated to this task therefore evolved alongside those involved in perception.

Is the MMH the only, or the best, explanation of the data generated by evolutionary psychologists? In Chapter 6 Richard Samuels proposes an alternative explanation of the experimental data, which he calls the *library model of cognition*, according to which the mind consists of domain-general mental mechanisms, but contains innate domain-specific bodies of information which guide these mechanisms. We have seen that there is no conceptual reason why the library model could not be true, but is Samuels right in holding that his model comports at least as well with the experimental evidence as does the MMH?

Some experimental results, for instance the double dissociations observed with regard to theory of mind abilities (that is, the ability effortlessly to attribute mental states to others), comport well with Samuels' model. Sufferers from Williams syndrome have low IQ but preserved theory of mind, while autism presents with no (necessary) deficit in IQ, but pronounced deficits in theory of mind. This evidence is compatible with a theory of mind module, as proposed by Baron-Cohen (1995). But as Samuels shows, the same results might be expected if theory of mind required access to an innate body of information that could be selectively spared or impaired.

For other experimental results, however (and assuming, if only for the sake of argument, that these results bear up to other criticisms, aimed for instance at the methodology used to establish them), the library model seems less convincing. Consider mate-preferences (Buss and Schmitt, 1993). On this theory, men and women assess potential mates based on criteria which are sensitive to their reproductive value. For instance, men are sensitive to cues of fertility and health in women: what males perceive as attractive in a woman (clear and smooth skin, clear eyes, symmetrical features and so on) are reliable indicators of low parasite load and high reproductive value. Women, on the other hand, are sensitive to cues that male will invest resources in any offspring they produce. Clearly much of this kind of assessment can be guided by reference to innate bodies of information. But the hypothesized assessment

mechanism does seem to exhibit many of the classic signs of modularity. It is *encapsulated*: men are aroused not only by actual women, but also, notoriously, by representations of women. The information that a picture is not real has no effect on the operation of the mechanism. Its output is shallow: not a judgement, but a feeling or an affective state. It is mandatory in operation: given the appropriate stimulus (for example a picture of an attractive person), it automatically generates its output. It is cognitively impenetrable; other systems do not have access to its workings, and it is swift in its operation.

Perhaps the provisional conclusion we should reach is that the case for the existence of some modules is persuasive, but the case for massive modularity is still inconclusive. The MMH is an empirical claim, and we may need to await further evidence before we can confidently rule on its truth. Let us turn now from the general hypotheses of evolutionary psychology to some of its more detailed claims.

Adaptationism

In Chapter 1, his introduction to *Evolutionary Psychology*, David Buss identifies Darwinian selection as the only natural process capable of generating functionally complex traits. Since most human psychological mechanisms are functionally complex, he argues, it follows that they were produced by selection and are therefore adaptations. Much of the theorizing that takes place in evolutionary psychology involves positing specific ‘adaptive problems’ to which our psychological mechanisms are the purported solutions. Critics of this approach view adaptationist thinking as unconstrained and poorly corroborated: adaptationist scenarios are ‘easy to generate but difficult to test’. Evolutionary psychologists are accused of applying inadequate evidential standards to adaptationist hypotheses, accepting them on the grounds that they make ‘good adaptive sense’ (Griffiths, Chapter 9, this volume). Evolutionary biologists, by contrast, require more rigorous forms of evidence before signing off on an adaptationist proposal (Williams, 1966; Gould and Lewontin, 1979; Endler, 1986; Grant, 1999). Thus, evolutionary psychologists are criticized for embracing a methodology (some would call it an ideology) which has been denounced by evolutionary biologists as unscientific.

In what follows we remain neutral on whether these accusations are well grounded. Our aim here is to identify the central moves in this debate. First, we provide a brief overview of the adaptationism debate in evolutionary biology. We then describe two methods that evolutionary biologists use to test adaptationist hypotheses and consider whether either of these approaches can be applied to the human mind. Finally, we critically evaluate two alternative strategies that evolutionary psychologists have proposed for generating and testing adaptationist hypotheses.

A few terminological clarifications will help navigate these issues. First, it is important to distinguish between *biologically adaptive* traits and *adaptations*. To identify some trait as biologically adaptive is to claim that it promotes survival and reproduction in some environment (usually the organism’s current environment). Not all biologically adaptive traits are adaptations. To be an adaptation a trait must have been modified by natural selection for some specific function. Adaptation is an historical concept, referring to selection events in the (sometimes distant) past. Hence it is often more difficult to determine whether some trait is an adaptation than it is to show that it is adaptive. A second important distinction is between *functional capacities* and *proper functions*. Any given artefact or trait will have indefinitely

many functional capacities. For example, a computer mouse has the capacity to function as a doorstop, as a paperweight or as a piece of computer hardware. Obviously, the fact that an artefact or trait has some functional capacity *F* does not imply that it was designed or selected to perform *F*. The functional capacity for which an artefact or trait was designed or selected is called its proper function. A final distinction is between *adaptationist hypotheses* and *adaptationist explanations*. An adaptationist hypothesis putatively ascribes a proper function to some trait: it tells a story about how a given trait possibly evolved by natural selection. An adaptationist explanation is an adaptationist hypothesis that has been confirmed or adequately supported by some body of evidence. Much of the debate over the use of adaptationism in evolutionary psychology concerns the nature of the evidence required to convert adaptationist hypotheses into adaptationist explanations.

The Adaptationism Debate in Evolutionary Biology

It is not entirely surprising that most psychologists have shied away from an adaptationist perspective on the human mind. Adaptationist thinking has come under fire from some of evolutionary biology's most influential thinkers. In 1966 George Williams identified adaptation as, 'a special and onerous concept that should not be used unnecessarily, and an effect should not be called a function unless it is clearly produced by design and not by chance' (1966, vii). At the time Williams was writing, evolutionary theorists were gaining an appreciation for the role of alternative processes, besides natural selection, in determining evolutionary outcomes. Genetic or developmental linkage, for example, can cause selectively neutral or even maladaptive traits to persist in a population. Genetic drift can lead to the chance fixation of neutral or maladaptive genes. The kind of hyper-adaptationism that Williams was resisting ignores these processes, treating adaptation as the default explanation for every trait. Without adequate evidence to rule out competing hypotheses, Williams cautioned, adaptationist claims should be regarded with scepticism.

Adaptationism suffered another blow in 1979 with the publication of a highly influential essay by Steven Gould and Richard Lewontin. These authors compared adaptationism to an ideology. Like Voltaire's Dr Pangloss, who held the unshakable conviction that everything happens for the best, advocates of the 'Panglossian Paradigm' in evolutionary biology supposedly view every trait as the optimal solution to some adaptive problem or other. Gould and Lewontin argued that no amount of evidence could falsify this perspective: 'If one adaptive argument fails, try another ... assume that another must exist ... [or] attribute failure to imperfect understanding of where the organism lives and what it does' (1979, p. 586). In addition to linkage and drift, Gould and Lewontin identified several other processes that are also ignored under the adaptationist paradigm. Phylogenetic inertia, for instance, is the persistence of traits in a lineage due exclusively to inheritance, without the help of natural selection. Phenotypic plasticity is the capacity of a trait to accommodate to environmental conditions over an organism's lifetime. These latter processes can generate traits that are functionally complex and adaptive, effectively mimicking the effects of selection. Thus, Gould and Lewontin argued, one cannot legitimately infer that some trait is an adaptation from evidence of adaptive complexity alone.

Within evolutionary biology these issues have remained controversial. Not everyone agrees that adaptation is the hypothesis of last resort when faced with an apparently functional

or complex trait. Ernst Mayr (1983) and Daniel Dennett (1995) have offered particularly influential defences of adaptationist thinking. More recently, Peter Godfrey-Smith (2001) has distinguished three distinct brands of adaptationism within the literature. *Explanatory adaptationism* maintains that the phenomenon of ‘design fit’ between an organism and its environment is the most important topic of investigation in biology. Proponents of this view might acknowledge linkage, drift, phylogenetic inertia and phenotypic plasticity as prevalent albeit much less interesting processes than natural selection. *Empirical adaptationism* holds that natural selection is by far the most potent of these processes. On this view, for any given trait it is likely that selection played a role in shaping it. *Methodological adaptationism* simply views adaptation as a useful organizing concept for biology, without making any claims about its significance or potency.

It is not entirely clear which brand of adaptationism evolutionary psychologists are committed to (see Ferguson, Chapter 11, this volume). At times they appear to hold all three. In what follows we review two of the most defensible methods for testing adaptationist hypotheses and consider their prospects for human psychology. We then consider more closely the forms of adaptationist reasoning evolutionary psychologists employ.

Two Methods for Testing Adaptationist Hypotheses

Two of the most defensible strategies for evaluating adaptationist explanations within evolutionary biology are the *optimality approach* and the *phylogenetic method*. The optimality approach involves measuring the degree of ‘design fit’ between a trait and an adaptive problem. For example, many species face the adaptive problem of how to maximize foraging efficiency in an environment where prey items vary in quality. A detailed optimality model can predict exactly how a maximally efficient forager should operate under a given set of conditions. If organisms foraging in the wild conform to the predictions of an optimality model, one can reasonably infer that the mechanism producing this behaviour was optimized by natural selection (Parker and Maynard Smith, 1990). Importantly, this methodology requires a precise quantitative description of the adaptive problem and its optimal solution (Orzack and Sober, 1994). One must be able to determine unequivocally whether the trait under analysis is an *optimal* solution to a particular problem, not just a solution that works. Otherwise it remains a viable possibility that the trait evolved for some other purpose. Remember that every trait has multiple functional capacities. Sometimes, solving a particular adaptive problem is something a trait can do ‘for free’ without having been selected to do so. The optimality approach assumes that a trait will not serve as the *optimal* solution to some problem unless it was specifically selected for that purpose.

An alternative approach uses phylogenetic information to evaluate competing adaptationist hypotheses. Employing this method requires identifying how some focal trait is distributed among related species (see Griffiths, Chapter 9, this volume). This approach is essential for distinguishing cases of biological analogy (where a trait evolved independently in multiple lineages) from cases of homology (where a trait was inherited from a common ancestor). By comparing a group of related species, some who possess a trait and others who lack it, it is also possible to estimate when in that lineage (or how often) that trait emerged. Such information is extremely useful for evaluating adaptationist proposals. For example, in Chapter 10 Russil Durrant and Brian Haig illustrate how the phylogenetic method was used to

decide among alternative hypotheses for the evolution of concealed ovulation in primates. One possibility, they explain, is that concealed ovulation evolved to promote paternal investment in monogamous species. On this view, a male who is unaware of the precise point at which his partner reaches her peak fertility will be forced to 'stick around' and guard his reproductive interests against potential rivals. If the parental investment hypothesis is correct, concealed ovulation should tend to give rise to monogamy. An alternative hypothesis views concealed ovulation as a strategy for minimizing male aggression towards offspring. In species where ovulation is not concealed, males are more capable of identifying which offspring they have sired. Non-related offspring are then at greater risk of infanticide (Hrdy, 1981). By contrast, when ovulation is concealed a female can raise paternity uncertainty by mating with multiple males. In this scenario, one would expect concealed ovulation to be associated with multi-male mating systems. Using phylogenetic information, Sillén-Tullberg and Møller (1993) were able to decide between these alternatives. These researches provide evidence that the ancestral condition for primates is a multi-male mating system combined with slight signs of ovulation. They further discovered that signs of ovulation disappeared between eight and eleven times in a non-monogamous context. This pattern would be extremely unlikely under the parental investment hypothesis. Such examples illustrate how phylogenetic information can narrow down the range of adaptationist possibilities. If certain ecological conditions were not present when a given trait appeared in some lineage, then that trait is obviously not an adaptation to those conditions.

Are Adaptationist Hypotheses about the Human Mind Testable?

We are now in a position to understand why some critics are pessimistic about the prospects for testing adaptationist hypotheses about human psychology (Lewontin, 1990; Richardson, 1996). As Paul Griffiths argues in Chapter 9, the optimality method is difficult to apply in the human case. For one thing, constructing an optimality model requires getting the costs and benefits just right. Even a slight change in the model's assumptions can have large effects on the expected optimal trait values. Humans, however, are a long-lived species inhabiting a wide range of environments. So setting the parameters of an optimality model for human behaviour is no easy task. Another problem arises in the attempt to test optimality models. A trait that is selected to function optimally in one environment is unlikely to continue doing so when the environment changes significantly. Most evolutionary psychologists assume that the human psyche evolved in an environment that was quite unlike the one we inhabit today (see below). Thus, the appropriate environment for evaluating models of the human mind is one that we, by hypothesis, no longer inhabit.

Further problems arise when applying the phylogenetic approach to our own species. Most evolutionary psychologists are interested in traits like language (Pinker, 2003), romantic jealousy (Buss, 2000) and social exchange reasoning (Cosmides and Tooby, 1992). These traits are supposedly unique to humans. However, unless a trait is distributed among several species the phylogenetic approach is impossible to apply. Only by comparing related species, some who possess the trait and others who lack it, can one pinpoint when that trait evolved.

One possible way around this predicament is to select traits that vary within the human species. One could potentially conduct a phylogenetic analysis by comparing cultural lineages, provided that their ancestral relationships are well known and that they have

remained relatively isolated (Griffiths, 1997). However, this option is ruled out by another assumption endorsed by most evolutionary psychologists. It is generally assumed that human psychological adaptations are universal to our species (Cosmides and Tooby, 1992; Pinker, 2002). The conjunction of these two assumptions – uniqueness and universality – precludes the possibility of any sort of phylogenetic analysis for human psychological traits. The point couldn't be put more succinctly than this:

It isn't that adaptive hypotheses are untestable, but rather that it takes a certain kind of data set to put them to the test. If the dichotomous trait T is universal within species S, then one needs a data set in which some species have trait T while others do not. In one sense, the explanation of impoverished data is easy – it is easy enough to invent a story that fits the data – but, in another sense, the explanation of impoverished data is impossible – the data do not permit the adaptive hypothesis to be tested properly. This, we suggest, is what it means for adaptive storytelling to be 'too easy'. (Lang *et al.*, 2002, p. 667)

Is there any other way to avoid this predicament? One possibility is that the human mind is not as distinctive as some researchers have traditionally assumed. In fact, some sophisticated psychological abilities, like our sense of justice, have been identified in other primates (Brosnan and de Waal, 2003). In Chapter 10 Durrant and Haig suggest that homologues for language and social exchange reasoning might also be found in some of our closest living relatives (see Deacon, 1997). Another possibility is that DNA evidence from extinct hominids might one day provide the necessary information for a phylogenetic analysis. If human psychological mechanisms are genetically encoded (as evolutionary psychologists assume), then it should be possible to identify genetic markers for particular psychological abilities in extinct species of hominid. One could then reconstruct the relationships among those species to determine when particular psychological mechanisms emerged (Rosenberg and Linquist, 2005).

As intriguing as these possibilities might sound, evolutionary psychologists have not explored them. Instead, most evolutionary psychologists remain wedded to the assumptions of universality and uniqueness. This, of course, has not deterred them from favouring certain adaptationist hypotheses over others. Instead of employing the optimality or phylogenetic approaches, evolutionary psychologists offer two more controversial strategies for justifying adaptationist claims: *reverse engineering* and *adaptive thinking*. The remainder of this section critically evaluates these proposals.

Two Alternative Adaptationist Strategies

The first controversial method infers the adaptive problem from its solution. Tooby and Cosmides (1992) describe this strategy as 'reverse engineering'. Just as one might infer the proper function of a computer mouse from a close inspection of its functional capacities, so, they propose, one can infer the selective pressures that shaped psychological mechanisms by investigating how they function. Critics of this strategy point out that reverse engineering rests on an inference to the best explanation. However, it is argued that, 'Argument to the best explanation is not valid when the "best" explanation is one of several that are equally good' (Sterelny and Griffiths, 1999, p. 243). In Chapter 9 Paul Griffiths illustrates this point with a vivid example. He cites several alternative hypotheses for why humans evolved a large neocortex. One proposal – the 'radiator hypothesis' – suggests that the neocortex evolved

as a cooling mechanism, preventing heat stroke when humans transitioned onto the African savannah (Falk, 1990). Another hypothesis states that the neocortex evolved to facilitate social navigation as human group sizes increased (Byrne and Whiten, 1988). Others propose that this structure evolved for ecological problem-solving (Clutton-Brock and Harvey, 1980) or to enhance manual dexterity in throwing objects (Calvin, 1993; see Gray *et al.*, 2003 for an expanded list). The point here is supposed to be that none of these hypotheses stands out as the best available explanation. In cases where reverse engineering has hit upon a single explanation, Griffiths argues, this is only because researchers have not bothered to generate a list of equally plausible alternatives.

In reply, in Chapter 10 Durrant and Haig offer a guarded defence of reverse engineering. They begin with the admission that inference to the best explanation is always provisional in character. However, they argue that reverse engineering is rarely mired in a proliferation of alternative hypotheses. Durrant and Haig question the generality of Griffiths' brain size example. Not all traits can be so easily framed in multiple, equally plausible adaptationist terms. One could further develop this line of argument by noting that, as a general rule, the range of alternative adaptationist explanations for any trait is a function of how precisely that trait is described. A trait that is characterized in general terms, like 'the human neocortex', admits of many plausible alternative explanations precisely because the description is so vague. On this view, characterizing the neocortex in more detailed functional or structural terms could significantly reduce the range of viable alternatives. (This is not to suggest, however, that evolutionary psychologists do in fact provide adequately detailed characterizations of the traits that they reverse engineer.)

Durrant and Haig further defend this methodology by offering a nuanced account of inference to the best explanation. Drawing on the work of Paul Thagard, they argue that reverse engineering can be supplemented by such additional considerations as coherence with background information and analogical reasoning. For example, the 'radiator hypothesis' does not cohere well with what we know about the developmental and regulatory costs of brain tissue. Surely there would be a more efficient way to construct a cooling system than to build it out of the most energetically demanding tissue in our body. Similarly, Durrant and Haig propose that analogical reasoning can support reverse engineering. Some adaptive problems have generated analogous solutions in a wide range of species. For example, in both chiropteran bats and cetaceans, social group size is positively correlated with brain size (Byrne and Bates, 2007). These findings favour the hypotheses that an analogous selection pressure was operative in humans. Such inferences do not conclusively demonstrate that one particular adaptationist hypothesis is correct – perhaps no foreseeable amount of data could. However, Durrant and Haig argue that when background information and analogical considerations are taken into account, not all adaptationist hypotheses stand on a level playing field. Notice, however, that analogical reasoning can be used to supplement reverse engineering only if the focal traits are found in other species besides our own. This is something that many evolutionary psychologists would deny about some of our most interesting psychological abilities.

The second controversial strategy for evaluating adaptationist hypotheses involves inferring the solution from an adaptive problem. This strategy, called 'adaptive thinking', begins with a hypothesis about the likely selection pressures that ancestral humans encountered. One then frames a prediction about the sorts of psychological mechanisms that would have

been adaptive under those conditions. If the prediction is confirmed, and evidence for the expected mechanism is obtained in modern humans, evolutionary psychologists infer that the mechanism is an adaptation to those hypothesized selection pressures.

In principle, adaptive thinking can avoid the fallacy of hyper-adaptationism. As Sally Ferguson points out in Chapter 11, the 'predictive approach' (as she calls it) makes no commitments about the proportion of psychological mechanisms that call for an adaptationist explanation. Only those mechanisms that correspond with known adaptive problems are assigned a proper function on this view. So, if it turns out that humans encountered only a handful of adaptive problems over the course of their evolutionary history, many of our psychological mechanisms will require non-adaptationist explanations. Notice, however, that this method requires an accurate understanding of the selection pressures that humans encountered. When knowledge about the ancestral environment is highly speculative, adaptive thinking is inadequately constrained. In practice, this method can potentially lead researchers to posit many more 'solutions' than there were actual problems to solve.

Another challenge for adaptive thinking lies in the accurate specification of adaptive problems. As Ferguson notes, the number and types of adaptive problems an organism encounters depend partly on how it perceives and categorizes the world. For example, one type of organism might employ two different decision rules for foraging and mating. When foraging (suppose) this organism minimizes the amount of low quality items it selects; when choosing mates it maximizes the number of high quality partners it interacts with. For this organism, foraging and mating are clearly two different adaptive problems. But consider another animal that uses an identical decision algorithm in both domains. Perhaps it follows the rule: choose the first item (prey or mate) that is better than the last n individuals sampled. For this creature there is a single adaptive problem: how to maximize benefits while minimizing search time. Sterelny and Griffiths (1999) refer to this as the *grain problem*: the nature of an adaptive problem depends on the level of grain at which an organism parses its environment. Determining how an organism carves up its environment is a difficult task. This problem is exacerbated by the fact that a species' perceptual and cognitive machinery evolves over time. If a change in adaptive problems does not require a change in the external environment, this makes their precise specification extremely difficult.

Ferguson also notes that not all adaptive problems stand on equal footing. Certain adaptive problems do not guarantee an evolved solution. Consider the plight of the reed warbler whose nest is invaded by the common cuckoo, a brood parasite. Cuckoos lay eggs in warbler nests so that warbler parents will mistakenly raise cuckoo chicks as their own. This is clearly an adaptive problem for the warbler, but it is not one that demands a solution. Although warblers would be better off with a mechanism that distinguishes cuckoo chicks from their own offspring, the absence of such a mechanism does not drive this species to extinction. Other adaptive problems are more urgent. Most animals must find mates, locate food and avoid predators. For these 'must solve' problems one can legitimately infer that an adequate solution exists. Ferguson argues that evolutionary psychologists tend to treat all adaptive problems as if they were 'must solve' problems. These researchers tend to infer from the fact that humans encountered problem X that humans must have evolved a specialized mechanism for solving X. As the warbler example illustrates, this inference is not always warranted. This issue underscores an earlier point, that in order to legitimately apply adaptive thinking to

human psychology, one requires a detailed understanding of the environment in which our minds evolved.

In the following section we consider in more detail how evolutionary psychologists propose to reconstruct the environment of evolutionary adaptedness (as they call it). Before doing so, it is important to point out an alternative defence of adaptive thinking. This argument states that adaptive thinking has a 'heuristic' application that requires no precise knowledge of human prehistory. Presenting this argument will require a brief detour through the aims and methods of cognitive psychology.

Up to this point, we have considered whether adaptive thinking provides an adequate method for testing adaptationist hypotheses. This is a question about whether adaptive thinking can generate *ultimate explanations*. Ultimate explanations answer a certain kind of 'why' question: they explain why organisms possess the traits that they do. For example, the ultimate explanation for why zebras have stripes is because this pattern made it difficult for lions and other large cats to distinguish individuals from the herd. By contrast, *proximate explanations* answer a certain kind of 'how' question: they explain how a trait functions. For example, a proximate explanation of zebra camouflage would identify limitations in the feline visual system, describing how a striped pattern manages to exploit these weaknesses. Evolutionary psychologists are interested in developing proximate explanations in addition to ultimate explanations. The former goal is one they share with cognitive psychologists generally.

One way that cognitive psychologists (in general) develop proximate explanations of the human mind is by a method known as *functional analysis* (Cummins, 1983). Functional analysis usually begins with some known psychological capacity, for example humans' capacity to identify familiar faces among a crowd of strangers. The next step involves positing some underlying computational mechanism. Perhaps humans recognize familiar faces by encoding the distance between an individual's eyes, nose and mouth. Functional analysis proceeds by refining this mechanistic hypothesis in light of further experimental data. For example, modifying the distance between eyes, nose and mouth of an otherwise familiar face should affect people's recognition performance. Evolutionary psychologists also employ functional analysis to develop proximate explanations of the mind. However, they place an adaptationist twist on this methodology. Evolutionary psychologists propose to functionally analyse the mind by beginning with likely adaptive problems rather than with known capacities (Davies, 1996). This strategy involves the same sort of inference as adaptive thinking: reasoning from adaptive problems to likely solutions. However, in this case the goal is not to test adaptationist hypotheses per se. Rather, the goal is to identify which psychological mechanisms humans possess and how those mechanisms function.

When adaptive thinking is used as a strategy for functional analysis, the epistemic demands are more relaxed. It is not essential for the success of this project that one know which adaptive problems shaped the mind – a reasonable adaptive hypothesis will usually suffice. For example, some evolutionary psychologists propose that humans faced the adaptive problem of distinguishing suitable from unsuitable habitats. This hypothesis has generated specific predictions about our preferences for certain landscapes. Some of these predictions have been successful, generating novel discoveries about our aesthetic judgements (Kaplan and Kaplan, 1989). Notice, however, that these discoveries do not require that the adaptive hypothesis that generated them is accurate. It might turn out that humans were never selected to prefer certain

habitats over others. This would not undermine the utility of adaptive thinking when it comes to generating proximate explanations of how the mind functions.

How convincing is this 'heuristic' defence of adaptive thinking? One recurring problem is that evolutionary psychologists are not always careful to distinguish the two modes of adaptive thinking. When heuristic adaptive thinking results in a successful prediction, there is a tendency to infer that the adaptive hypothesis that generated that prediction has been confirmed. For reasons that have already been outlined, this inference is fallacious.

It would seem that the utility of heuristic adaptive thinking lies in its ability to generate novel discoveries about proximate mechanisms. Some evolutionary psychologists argue that this method has generated insights into the human mind that would not have otherwise been discovered (Cosmides and Tooby, 1992; Daly and Wilson, 1988a). Whether this assertion is correct is not a topic that we shall explore here. However, the most reasonable approach, we think, involves a form of methodological pluralism, where both adaptive and non-adaptive hypotheses are used to functionally analyse the mind.

Returning to the question of how to develop (ultimate) adaptationist explanations in psychology, notice that both reverse engineering and adaptive thinking require detailed knowledge about ancestral environments. In the following section we explore in more detail the current prospects for reconstructing the evolutionary past. How, if at all, can one determine which selection pressures our distant ancestors encountered? Moreover, is this information even necessary for determining how the mind evolved?

The Environment of Evolutionary Adaptedness

Evolutionary psychologists argue that the mind is adapted to conditions that few people would recognize today. Our distant ancestors lacked formal systems of punishment and education, they possessed simple tools and no written language, their social networks were small and tightly knit, hunting and gathering were the primary modes of subsistence. From an evolutionary perspective, these ancestors occupied a different niche from our own. It is therefore hardly surprising that some human psychological dispositions are out of synch with modern environments. A popular example is our craving for sweet and starchy foods. Such cravings were presumably beneficial in environments where these items were scarce, motivating our ancestors to seek out essential vitamins and proteins. In modern contexts where refined sugars and fatty foods are available on every corner, these cravings offer no such fitness advantage.

Evolutionary psychologists take most human psychological traits to be like our cravings for sugar and fat. As Cosmides and Tooby (1997) put it, 'our modern skulls house a stone age mind'. This view, known as mismatch theory, relies on several assumptions. First, the theory assumes that human psychological mechanisms are largely influenced by natural selection acting on genes. This is usually characterized as a slow process requiring hundreds or thousands of generations to produce complex traits. Second, mismatch theory assumes that the development of our psychological mechanisms is buffered against recent changes in the environment. For example, a preference mechanism that develops in a high-fat environment produces the same cravings, on this view, as one that develops in a low-fat environment. Finally, evolutionary psychologists assume that the mismatch between human psychology and current environments is fairly pervasive. Most, perhaps all, of our mental processes

are performed by mechanisms that were selected under very different conditions than those prevailing today.

These assumptions support two methodological morals that evolutionary psychologists have embraced. The first is a proscription against measuring the current adaptive value of psychological traits. This marks an important departure from human behavioural ecology, evolutionary psychology's sister discipline. Behavioural ecologists attempt to test evolutionary hypotheses by observing how certain behaviours impact fitness within living populations. Evolutionary psychologists place little weight on these findings (Cosmides and Tooby, 1992; Symons, Chapter 2, this volume). Even when the focal communities are groups of hunter-gatherers, whose lifestyles and physical environments are presumably similar to those of our distant ancestors, there is the threat of potential mismatch. Modern hunter-gatherers have been severely impacted by colonialism, they are subject to legal and geographic constraints, they have access to technologies that were unavailable to early humans and so on (Stiles, 2001). Such factors potentially place modern hunter-gatherers in a different selective environment than our ancestors. When it comes to contemporary Western societies the potential for mismatch is even greater. Such concerns motivate the second methodological moral: the only reliable way to identify psychological adaptations is by determining how the mind functioned in the environment of evolutionary adaptedness (EEA for short). Simply put, the EEA is an idealized picture of the dominant selective pressures thought to have shaped the human mind for most of our species' existence.

In one respect, this idea of a human EEA is theoretically useful. There had to be some set of environmental factors that influenced the mind's evolution. It is helpful to consider how some of those factors might have differed from the ones obtaining today. On the other hand, there are several problems with the way that evolutionary psychologists have employed this concept. The remainder of this section focuses on some challenges to evolutionary psychologists' understanding of the EEA. We begin by distinguishing two different versions of this concept: the Pleistocene conception and the statistical conception. Though both models can be challenged, we identify several epistemic problems associated with the statistical conception in particular. Finally, we argue that the assumptions on which both EEA concepts are based cohere poorly with contemporary views about the rates of selection and the influence of genes on psychological mechanisms. Relaxing these assumptions places evolutionary psychology in a better position to understand human psychological evolution.

Two Competing Models of the EEA

Evolutionary psychologists have put forward two alternative versions of the EEA concept. To our knowledge these models have not been adequately distinguished within the relevant literature. The first version is associated with John Bowlby (1969, 1973), who regarded the EEA as a specific period of human prehistory. In Chapter 14, William Irons quotes him as identifying the EEA as, 'the [environment] that man inhabited for two million years until changes of the past few thousand years led to the extraordinary variety of habitats he occupies today' (p. 301). Evolutionary psychologists adopt this model in some of their writings, for example when they equate the EEA with, 'the way of life of Pleistocene hunter-gatherers' (Barkow, Cosmides and Tooby, 1992, p. 5). For the purposes of clarity we call this the *Pleistocene model*. Its distinguishing feature is the identification of a specific period or periods,

for example the human Pleistocene, during which most human psychological evolution took place. The Pleistocene model has been criticized (perhaps unfairly, see below) for offering an overly static picture of human prehistory. As Irons put it: 'Saying that human beings were Pleistocene hunter-gatherers for one or two million years creates a false picture of stasis during this period' (p. 302). Since it is widely agreed that the human mind evolved in a variety of different selective environments, some evolutionary psychologists have distanced themselves from this model. For example, in Chapter 12 John Tooby and Leda Cosmides propose an alternative conception:

The 'environment of evolutionary adaptedness' (EEA) is not a place or a habitat, or even a time period. Rather, it is a statistical composite of the adaptation-relevant properties of the ancestral environments encountered by members of ancestral populations, weighted by their frequency and fitness-consequences. These properties are selected out of all possible environmental properties as those that actually interacted with the existing design of the organism during the period of evolution. (pp. 252–3)

This view equates the EEA with the set of adaptive problems that shaped the human mind, regardless of where or when those conditions were encountered. The distinguishing feature of this model is the abstract way in which the EEA is formulated. Tooby and Cosmides explain that the EEA is the 'statistical composite' of the adaptive problems encountered by our ancestors, 'weighted by their frequency and fitness consequences'. We call this the *statistical model* of the EEA. In what follows we argue that the Pleistocene conception offers several advantages over the statistical model.

Objections to the Statistical Conception of the EEA

The statistical model of the EEA has one potential advantage over the Pleistocene conception: it is not committed to a specific period of human prehistory. Some psychological mechanisms are likely to have evolved long before the Pleistocene, others are probably more recent. By allowing for a mosaic picture of human psychological evolution, the statistical conception is in this respect more biologically realistic.

However, the statistical conception suffers from at least three major drawbacks. The first problem is that it overlooks the evolutionary significance of ecological variation as a distinct type of selection pressure. When Tooby and Cosmides define the EEA as the 'statistical composite' of the selection pressures humans encountered across different environments, they presumably have in mind some sort of weighted average. Certain adaptive problems are fairly common across different environments, other adaptive problems are relatively infrequent. The EEA, on this view, is the weighted average of these adaptive problems. Notice that this model factors out the variation among environments, treating variation as a type of noise. This is a serious problem with the statistical model. Ecological variation can serve as its own type of selection pressure (Brandon, 1990; Godfrey-Smith, 1998). Organisms that encounter considerable ecological variation are more likely to evolve general-purpose adaptations than organisms who encounter relatively stable environments. In psychological terms, variable environments are likely to favour general-purpose intelligence mechanisms, while less variable environments favour specialized modules (Sterelny, 2003). It is perhaps not surprising that Tooby and Cosmides endorse a definition of the EEA that supports their

assumption of a 'massively modular' mind (see Part II). However, it would be preferable if this bias was not built directly into their definition of the EEA.

On a related note, it is ironic that the Pleistocene conception of the EEA has come to be associated with a static picture of the hunter-gatherer environment. Contrary to the suggestions of some evolutionary psychologists, the Pleistocene conception is not logically committed to stasis. If it turns out that ancestral humans encountered environmental variation during the Pleistocene, then the Pleistocene model can accommodate this fact quite easily. By contrast, the statistical conception does generate a static picture of human prehistory by averaging across different environments. Hence, the statistical conception is more problematic in this regard than the Pleistocene conception. To our knowledge evolutionary psychologists have failed to appreciate this point.

A second drawback of the statistical conception is that it defines the EEA exclusively in terms of *selectively relevant* features of the environment. There are likely to have been other, non-selective forces that influenced human psychological evolution. For example, living in small isolated groups could have promoted genetic drift and inbreeding depression. These conditions can lead to the fixation of non-adaptive or even maladaptive traits. Such aspects of ancestral human population structure are ignored by the statistical conception. Once again, this shortcoming is avoided within the Pleistocene model. Viewing the EEA as a specific period (or periods) of human prehistory draws attention to non-selective features of the environment that might have prevailed during those times.

Finally, the statistical conception of the EEA is 'epistemologically slippery' or difficult to pin down. Generally speaking, reconstructing ancestral environments is a difficult task for any evolutionary psychologist. One must draw upon multiple, independent sources of evidence including anthropology, comparative animal behaviour and archaeology to get a sense of which selection pressures might have obtained over human prehistory. Evolutionary psychologists make this task especially difficult for themselves. For one thing, they assume that most psychological traits are out of synch with modern environments. This view precludes all comparative anthropology as a source of information about ancestral environments. To make matters worse, evolutionary psychologists assume that most human psychological adaptations are monophyletic, not shared by our closest living ancestors. This precludes the use of phylogenetic comparisons as a strategy for reconstructing the EEA (see Part III). Finally, the statistical conception does not specify a particular time and place as being the most relevant for human psychological evolution. This makes it virtually impossible to use archaeological data to reconstruct the EEA. One can never be sure which archaeological period is most significant or how to weigh different archaeological reconstructions against one another. At least on the Pleistocene conception, our attention is drawn to specific periods of human prehistory that are likely to have been influential in shaping certain features of the human mind.

Evolutionary psychologists are aware of these methodological challenges. In response, they offer an alternative strategy for reconstructing the EEA which does not draw on anthropology, cross-species comparisons or even archaeology. Instead, they maintain that it is possible to 'read off' features of the EEA by looking at supposed psychological adaptations themselves. As Tooby and Cosmides propose in Chapter 12:

The discovery and characterization of adaptations is the single most reliable way of discovering the characteristics of the past, because each species' design functions as an

instrument that has registered, weighted, and summed enormous numbers of encounters with the properties of past environments. Species are data recording instruments that have directly ‘observed’ the conditions of the past through direct participation in ancestral environments ... Observation of the structure of present adaptations and logical deductions from these observations constitutes a system for reading back what these ancient but still operational data recorders have to tell us about the past. (p. 256)

This strategy of ‘reverse engineering’ adaptive problems from the structure of likely solutions was discussed earlier. A central criticism of this approach states that there are indefinitely many possible environments that could have selected for a given adaptation. Therefore it is not possible to determine, just by looking at the trait alone, which adaptive problem was responsible for selecting it. As we noted in the previous section, the viability of reverse engineering is a matter of some debate. Advocates of this methodology argue that analogical evidence can be used to select the most plausible adaptationist hypotheses from a range of alternatives (Durrant and Haig, Chapter 10, this volume). One problem with this proposal, is that the psychological mechanisms in which evolutionary psychologists are interested are (allegedly) unique to our species. Perhaps reverse engineering can be applied, however, in cases where this assumption does not obtain. To our knowledge this method has not been put to the test. It would be interesting to evaluate the accuracy of reverse engineering in species where the operative selection pressures can be independently verified. Until such a study has been conducted, the reliability of reverse engineering remains unknown.

Rethinking Mismatch Theory

The proposal that reverse engineering is the preferred method for reconstructing the EEA resulted from a supposed lack of viable alternatives. Evolutionary psychologists are precluded from drawing on other, more traditional sources of information like anthropology, interspecies comparisons and archaeology. It is important to be clear on why these alternatives are unavailable. Evolutionary psychologists assume that psychological adaptations are produced by natural selection acting on genetic variation, which they regard as a slow process requiring hundreds or thousands of generations. They also assume that most human psychological mechanisms are developmentally buffered against recent changes in the environment. These two assumptions, in conjunction with the idea that the human environment has recently undergone considerable modification, justify the methodological proscription against measuring fitness in modern environments. In what follows we question the plausibility of these assumptions.

Take the view that natural selection acting on genetic variation is a slow process. As William Irons points out in Chapter 14, recent empirical studies on the rates of natural selection undermine this view. Significant amounts of evolutionary change can take place in just a few generations. Irons cites the famous studies by Rosemary and Peter Grant on Darwin’s finches (Grant, 1999) and John Endler’s work on guppy morphology (1986) to support this view. If these findings generalize to human psychological traits, then the assumption of pervasive mismatch needs to be reconsidered. It is entirely possible that some human psychological mechanisms are adapted to recent environments. The methodological implication of this view is that studying the current adaptiveness of certain traits potentially provides an indication of

the selection pressures that shaped them. Irons provides several illuminating examples of how this methodology can be applied.

One might also question the assumption that most psychological adaptations are the result of natural selection acting on genes. In recent years, several alternative frameworks have emerged that identify culture as a second inheritance system influencing psychological evolution. The details of these models are beyond the scope of this introduction (see the companion to this volume, *Cultural Evolution*). One of the take-home messages of these models, however, is that cultural evolution can occur over an extremely short duration. Once again, the possibility of rapid evolutionary change highlights the importance of measuring fitness in current environments.

Nor is it safe to assume that psychological mechanisms are developmentally buffered against changes in the environment. To the contrary, evidence from neurobiology and cognitive science suggests that certain features of the human mind are highly plastic (Panskepp and Panskepp, 2000; Sterelny, 2003). Developmental flexibility is at odds with the methodology of reverse engineering. If psychological development is influenced by the local environment and if modern environments differ markedly from the EEA, then the psychological mechanisms of modern humans are a dim reflection of those possessed by our distant ancestors. Granted, there are some psychological mechanisms, like our preferences for sugars and fats, that appear to be buffered against recent environmental changes. However, it is highly questionable whether most of human psychology, including higher cognitive functions, is similarly buffered.

In this section we have argued that the statistical model of the EEA is problematic on several fronts. This model fails to recognize the evolutionary significance of ecological variation, it focuses exclusively on selectively relevant features of ancestral environments while ignoring other factors like population structure, and it is epistemically 'slippery'. The Pleistocene model has been associated with an overly static picture of the EEA. However, there is no reason why this model should be committed to such a view. The Pleistocene model equates the EEA with specific time periods in human prehistory without making any explicit commitments about the amount of variability that characterized those periods. This model also allows for ecological variation to be considered a type of selective pressure. Moreover, the model can accommodate non-selective factors as a part of the EEA. For these reasons, we see the Pleistocene model as the more viable alternative. Perhaps the biggest drawback of this model is that it seems to restrict psychological evolution to a specific period of human prehistory, namely the Pleistocene. Hence this model seems difficult to reconcile with the likelihood that some psychological processes were shaped prior to this period or subsequently. Indeed, as soon as one relaxes the assumptions that the human mind is largely genetically specified, that natural selection on genes is slow or that the mind is developmentally buffered, then it becomes increasingly likely that some psychological traits evolved in contexts not so different from those one finds today. We think that the Pleistocene model can be expanded to accommodate these possibilities. The relevant window of human psychological evolution can be widened to include environments our ancestors inhabited prior to and after the actual Pleistocene. The essential feature of this model is that it equates the EEA with specific ancestral environments, not that the human mind evolved full blown in just one of those places or periods.

Cultural Universals

One powerful piece of evidence for the claim that elements of human psychology were shaped by evolutionary processes is their cultural universality. If a trait is shared by all or most members of a species, the most plausible explanation (often) is that it was inherited from a common ancestor. Of course, universality does not guarantee that a trait has an evolutionary history. All humans believe that the sky appears blue; presumably this disposition is not biologically inherited. Thus, evolutionary psychologists often focus on traits for which no plausible acquisition story is available. If a universal psychological disposition is unlikely to have been learned, then, arguably, it is most likely an evolved trait. Cultural universality is important for a second reason. The fact that some trait is culturally universal is sometimes taken to suggest that it is 'developmentally canalized' or buffered against environmental influences. The argument for this connection is fairly straightforward. Humans occupy a wide variety of physical and social environments. If some trait is invariant across those contexts, it could be because those environments have little influence on its development.

This connection between universality and developmental fixity brings us full circle on an issue that was raised earlier in this introduction. Critics of evolutionary psychology often resist the idea that psychological dispositions are universal. It seems likely that this resistance is motivated partly by their adherence to the SSSM. To understand why the prospect of cultural universality, and its potential implications for developmental fixity, might be considered threatening, it is important to consider why the SSSM is attractive in the first place. This model is regarded by some social scientists, and many interested bystanders, as motivating a political as well as an intellectual project. Social scientists in this tradition see their work as contributing to the project of fighting inequalities and discrimination. Take, for a signal instance, the inequality of women. Many social scientists believe that this inequality is *socially constructed*, by which they mean that it is the product not of innate dispositions of men and women, but of social structures (Hacking, 1999). Men and women are unequal in Western societies (for instance) due to social and cultural choices that are *optional*. We can (and should) alter social arrangements to bring about a more fully equal society. Social scientists in this tradition sometimes cite anthropological and historical studies of cultural variation as evidence for the contingency of our current attitudes towards women: different, and better, kinds of societies are possible; the proof is that they have sometimes been actual. It is worth noting that proponents of something like the SSSM do not in fact espouse the blank slate view as it is often presented; nevertheless, they do maintain that human social arrangements, conventions and practices are far more malleable than do many evolutionary psychologists.

This reading of the motivation behind social scientists' resistance to universality claims finds support in the debates over the universality of human emotions. Many anthropologists who study human emotions interpret them as socially constructed and culturally specific (Lutz, 1988). Evolutionary psychologists investigating the same emotions typically regard them as culturally universal and biologically based (Cosmides and Tooby, Chapter 15, this volume). As Ron Mallon and Stephen Stich note in Chapter 16, the fundamental disagreement between these two camps is conceptual, not empirical in nature. Social constructionists define emotions in broad terms that include the social contexts in which they are deemed appropriate. Since these norms tend to vary among cultures, emotions are typically regarded

as culturally specific. Evolutionary psychologists define emotions more narrowly as patterns of physiological activity accompanied by a distinctive facial expression. These physiological components of an emotion are culturally universal and more likely to have a biological basis. Hence, each research tradition defines emotions in a way that suits its respective agenda. Mallon and Stich argue that the two positions are not incompatible. It is possible to adopt a wide definition of emotion in some contexts and a narrow definition in others. So long as one is clear about the criteria being used, these perspectives can complement one another. One of the interesting questions that Mallon and Stich do not touch upon, however, is why social scientists are attracted to the broad definition in the first place. It seems plausible that by defining emotions in a way that highlights their malleable nature, social scientists are making room for the moral and political projects that partly motivate their research.

Proponents of the SSSM often accuse evolutionary psychologists of attempting to deny that we can make great strides towards more egalitarian societies, and sometimes evolutionary psychologists concur with this assessment. Because evolutionary psychology is committed to the existence of innate or developmentally fixed traits, it apparently sets limits to the plasticity of human social arrangements. There are two ways of understanding these limits. First, it might be true that given the constraints on human beings, we cannot actually have stable social arrangements that produce the egalitarian results that we might want. Second, the constraints might prevent us from producing the egalitarian results and also achieving or sustaining other important goods such as liberty. It is because proponents of the SSSM see in evolutionary psychology an obstacle to their political project that their opposition is sometimes so strident. They see in evolutionary psychology an ideologically motivated obstacle to political progress, as much as a set of empirical hypotheses.

For some features of human psychology and culture that evolutionary psychologists investigate, it has been claimed that there are 'no known exceptions' (Brown, 1991), which is apparently very strong evidence for the developmental canalization of these features. Donald Brown has identified dozens of such universals, including language and features of language (for example synonyms), family and features of family (for example the social and biological mother is usually the same individual across all cultures), rites of passage and music. Many of these universals are rather innocuous, in that they are described at a level of abstraction which sets few constraints and therefore does not impact upon the political projects of proponents of the SSSM. Others are, apparently, more threatening. For instance, Brown also claims that the dominance of males in the political realm is a cultural universal. If one takes this finding to imply that male political dominance is a developmentally entrenched trait, such that its modification would involve a significant social cost, it is easy to see why proponents of the SSSM might resist such claims.

Though Brown maintains that his cultural universals are genuinely exceptionless, we can sympathize with Tooby and Cosmides' claim that insisting on genuine universality sets the bar too high. Citing a single society in which an otherwise almost universal trait failed to be instantiated might be insufficient to invalidate the evolutionary psychological argument for the existence of significant constraints on social structures, practices and attitudes. Much depends on *why* the trait was not instantiated. Consider Dev Singh's (1993) well-known claim that males the world over have a preference for a certain waist-to-hip ratio (WHR) when judging female attractiveness (because WHR is an indicator of reproductive fitness). As a matter of fact, there is evidence that this preference is not genuinely universal. The men of

the Yomybato tribe, in the Peruvian jungle, have a markedly different preference, preferring women with the highest possible WHR. But advocates of evolutionary psychology do not regard this finding as a refutation of their claims about WHR. Instead, they argue that the same evolutionary perspective which generated Singh's original prediction explains the preferences of the Yomybato. In the harsh ancestral environment in which they lived, obesity was all but impossible. Hence Yomybato preferences altered, to prefer women with relatively large fat reserves (Alcock, 2001).

The right conclusion to draw from the example of Yomybato men might not be that our preferences are malleable in the way required by advocates of the SSSM, but that *given an environment conducive to human flourishing* our innate dispositions will be expressed. It might be only in a harsh environment that the facultative nature of the adaptation is revealed. That is, the moral may be that there are high costs associated with success in suppressing our innate dispositions.

Nevertheless, the argument from (near) universality to the existence of genuine constraints on what we may realistically aim for is questionable. There are two, closely interrelated, reasons why this is so: first that we have not yet succeeded in dislodging a trait does not entail that it is difficult or costly to dislodge that trait and, second, dispositions will have as part of their physical causes not elements of the genome but elements of the environment.

To illustrate the first point, consider Simon Baron-Cohen's claim that the inequality of the sexes is a product of our evolved nature and not of social choices. Baron-Cohen (2003) argues that male brains – that is, the brains that males tend to have – are better at systematizing than are female brains, which are better at empathizing. Because male and female brains have different strengths and weaknesses, men and women have different talents and therefore are suited to different social roles. An alleged implication of this view is that the dream of a society in which men and women play the same social roles is based on an illusion. Just as women are, by nature and not by social construction, fitted to childbearing in a way that men are not, men are fitted by nature to the sciences and other roles that require high systematizing ability. Equality must take a different form, Baron-Cohen argues, where men and women are recognized equally not for their similarities but for their different contributions. The problem with this line of argument is that it relies on a faulty inference from (supposed) universality to developmental fixity. Even if the differences in sex roles which Baron-Cohen takes to be innate are found across (nearly) all societies in history, it does not follow that these roles are 'fixed' by the genome or even that the roles are hard to change; if by 'hard' we mean that the means of change must be exotic (say, genetic engineering), very complex or costly in terms of resources, human happiness or other goods. The most one might infer in such cases is that the roles are resistant to change by the means attempted so far. We may yet find that there is some (intuitively) easy and cost-free way to change the roles. Or we may not. Given the complexity of social structures and of gene–environment interactions, it is difficult to draw any definitive conclusions.

The second reason why we should not draw any strong conclusions from cultural universals to fixity or even difficulty of change is brought out by bearing in mind that any trait is instantiated only given a certain genotype *and* a certain environment. Evolution is a conservative process that tends to discover the simplest causal routes to phenotypes. Very often, the causal route to a trait that is stable across different environments will depend on aspects of the environments that are *not* varying. It may be only because some feature or

features of the environment (perhaps a feature or features that strike us as insignificant) remains stable across the environments that the trait in question remains stable. In general, if the causal route to a trait depends upon the presence of a feature of the environment, and that feature is stable (enough) in the ancestral environment to play the relevant role, evolution will not build in the redundancy of replicating the causal role using internal developmental resources like genes. Once again, evidence of universality is not evidence of fixity, or even of difficulty of change.

These remarks should not be seen as suggesting that the SSSM is preferable to evolutionary psychology. They suggest, instead, that the distance between the two may not be as great as the rhetoric on both sides has suggested. Proponents of the SSSM are right to insist that traits and norms are importantly the product of social structures, and that altering these structures can often have significant effects on their contents. The historical and anthropological evidence to which they point might illustrate just these facts. But the evolutionary psychological project, of explaining cultural norms and practices by reference to their adaptive function, remains viable. It is a more difficult question whether culture universality is evidence that an adaptive explanation is more likely to be forthcoming. Traits that are adaptive are likely to be instantiated in environments that are similar to the environment of evolutionary adaptation; to that extent, cultural universality is evidence for adaptation. If any existing or historical culture regarding exhibited the trait was very similar to the EEA, then universality is evidence of adaptation. But a good part of the reason it is evidence of adaptation is that the trait was instantiated in the EEA-like environment. The evidence does not seem significantly stronger that it is an adaptation if it is also stable across environments that differ significantly from the EEA, unless the variations are in features that might have been expected to form part of the ancestral environment as well (in which case we may have evidence for a facultative adaptation). Proponents of the SSSM are impressed by the diversity of human environments. Whether they are right to insist on this diversity or not, we should be aware that some of the innovations which account for *recent* cultural diversity (that is, over the last ten thousand years), like the invention of agriculture (and the growth of cities it made possible) and the invention of writing, are genuinely new. Perhaps the norms that flourish across these new environments are very distant from those we ought to expect to see in the EEA.

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