The Adapted Mind

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INTRODUCTION

Our evolutionary history must be an important part of the explanation of our being the kind of creature that we are. Our history helps explain what we have in common, and the differences between us. The attempt to apply evolutionary theory to us cannot be just wrong. But how should we use evolutionary theory to better understand human psychology and human culture? *The Adapted Mind* defines an approach to this question through theory and case study. The authors collectively picture the human mind as a complex of special purpose mechanisms, each adapted to some specific function(s).

The case studies vary considerably. Pinker and Bloom on language and Shepard on colour perception are extremely convincing. Other proposals are much less compelling: sociobiology's old vice of casual speculation about costs and benefits lingers on in the corners of this collection. Profet, for example, thinks that pregnancy sickness is an adaptation to avoid ingestion of toxins safe for the mother but harmful for the foetus. I think it remains to be shown that pregnancy sickness is an adaptation. Profet may well be right about the potential benefits of this mechanism but she makes no serious attempt to balance it against its cost. Profet claims in passing that "even severe nutritional deprivation in the first trimester often can be compensated for by adequate nutrition in the second and third trimesters" (p. 333). Perhaps, but we need to know if female foragers could afford the sickness-imposed restriction on their foraging focus, with all of its implications for their time and energy budgets, exposure to predators and the like. Nor can we assume that for them "catch-up" feeding was a practical possibility. Silverman and Eals present some very interesting results on sex differences in spatial skills, but it would be a kindness to describe their adaptive hypothesis as a speculation. Why believe that hunting "entail[s] different kinds of spatial problems than does foraging" (p. 534)? For our forebears 'hunting' may well have been a mix of trapping small animals and scavenging from the carcasses of big ones. The idea that our aesthetic responses to landscape have a particular evolutionary history to call their own does not, I

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think, have the ring of truth. For one thing, I think Keith Thomas has given a compelling account of the recent development of just that aesthetic. For another, the evidence from which these preferences are reconstructed are not paradigms of ecological validity. It includes subjects' self-reports of black and white photos of single trees stripped of their characteristic backgrounds (p. 559).

However, I do not propose to focus on the case studies, as interesting as some of these are, but on the general program defended in two very long papers by Cosmides and Tooby, and a somewhat shorter one by Symons. These in turn build on earlier debates; most notably in a special issue on Darwinian Psychology in *Ethology and Sociobiology*.[1]

The program exemplified by *The Adapted Mind* has developed largely in reaction to the Wilson Program so it is important to understand that program and why it has gone wrong. Wilson and various co-workers originally attempted a fairly direct extension of evolutionary models of animals behaviours to humans. They understood some behaviours as adaptations. Incest avoidance, male promiscuity and female coyness, perhaps even infanticide and rape are shaped by selection. There is nothing wrong in the idea that behaviours themselves might be adaptations. Behavioural differences can make a difference to fitness and there is behavioural variation on which selection can work. Moreover, there is good reason to expect that behavioural differences are heritable. If genetically different organisms with similar experience and in similar environments behave differently, then natural selection can prefer one set of genes to the other. So there can be no general argument against the idea of behavioural adaptations.

The Wilson Program was roughly dealt with. Despite sporadic lapses of its practitioners, it need not be committed to naive versions of adaptationism or genetic determinism, nor to a crude conception of the relationship between individual psychology and social form. Nonetheless, I think Cosmides, Tooby and Symons are right to be sceptical of the Wilson Program. To see why, consider the distinction between *mosaic* and *entrenched* traits. A mosaic trait is one that can evolve relatively independently of the rest of an organism's phenotype. Human skin colour is a mosaic trait; skin colour can evolve with relatively little change in the rest of an organism. When that trait changes as the result of selection, we have a very clear sense in which we can identify the selective forces involved, and the adaptive function of the change. So it makes perfectly good sense to see mosaic traits as evolutionary atoms with specific adaptive characteristics. Consider, however, an entrenched trait: our having two lungs. Why do we have *two* lungs? Quite likely, our having two is a consequence of the general bilateral symmetry of our bodies and of the developmental mechanisms involved in that symmetry. There may well never have been variation in lung number in the chordate line. And change in lung number would involve a cascade of many other changes. So its not at all clear that we should think of lung number as an evolutionary unit, a feature of our phenotype which has a more-or-less independent explanation in virtue of which it has a distinctive adaptive function.

Human behavioural characteristics are typically entrenched rather than mosaic

traits. For simpler animals, the story is different. Bee nest hygiene is a mosaic trait: hygienic species have non-hygienic relatives. Some animals' behavioural repertoires will be bundles of independent specialist modules. They are behavioural lego sets in which bits could be taken out and replaced with variants without disrupting the others. There seems no good grounds for denying function to beaver warnings, herring gull pecks and the like. But our behavioural repertoire is not an aggregation of independent units. Our behaviour is the result of perceptual inputs, our learning history, and very complex interactions between distinct psychological mechanisms. So speculations about the adaptive significance of rape, xenophobia or child abuse seem feeble. Like having two lungs, there will be an explanation involving human evolution of these behaviours. But they are unlikely to have histories to call their own, and hence unlikely to have independent adaptive significance.

Moreover, as both Cosmides and Tooby and Symons emphasise, we now live in an environment very different from our ancestral environment. When an animal's environment changes rapidly, many behaviours that were adaptive become maladaptive. The hedgehog responds to danger by rolling into a ball, but that is not an effective response to the danger of a car. Similarly, we should expect much human behaviour to be maladaptive (e.g. p. 45, p. 147). So if we are to use evolutionary theory to understand human behaviour at all, it is no use trying to figure out just how certain types of behaviour maximise fitness despite appearances. Rather, we must look to the mechanisms which produce behaviour. That is the program defended in *The Adapted Mind*, and advanced not just as a theory within psychology but also as a partial explanation for certain facts about human culture.

II. AGAINST THE AUTONOMY OF CULTURE AND THE GENERAL PURPOSE MIND

Cosmides and Tooby think that the social sciences are dominated by a particular conception of the nature of psychology and its relation to culture. They are opposed both to this conception of the social sciences, and of psychology. So they begin the central theoretical paper of *The Adapted Mind* by isolating a line of argument that decouples human culture from evolutionary history.

> *step 1.* Human differences in developmental potential are not predicted by between-group genetic difference. Such differences are minor compared to within-group differences. To a good approximation, "infants everywhere are born the same".
>
> *step 2.* Since there are no significant differences in biological heritage, adults' very significant differences in thought and behaviour have no biological explanation.

step 3. So the between-culture differences and the within-culture similarities have only one sensible source: the culture itself into which the individual has been born.

step 4. Moreover, this causal arrow is one-directional, since the cognitive set the individual acquires antedates and is external to the individual.

step 5. So that which organises and shapes individual agents is the culture that makes them. It is that culture, therefore, which is worth studying. It is at that level that organisation and complex adaptation is to be explained.

step 6. The evolved architecture of the mind is clearly a necessary condition of culture. But it tells us nothing specific about culture. The mind is a general purpose computer that particular cultures program differently. The job of psychology, on this conception, is to explain the psychological preconditions of any culture, not to explain the specific nature or content of human culture or cultures.

Cosmides and Tooby reject this picture root and branch. They suspect the extent of human difference is exaggerated. The social sciences overstate the diversity of human culture. Anthropologists enjoy exaggerating weirdness. So there is plenty of human uniformity that needs explanation: for example: the cross-cultural stability of species recognition (Atran 1990). But much more important, uniformity is no more explained by biology than difference by culture. Even if humans' cultures were widely divergent, difference can be produced by an evolved universal design interacting with environmental differences to produce very distinct outcomes. So there is no argument from human idiosyncrasy to domain general cognitive mechanisms.

They think many lines of argument converge on rejecting the idea of the human mind as a general purpose device (e.g. pp. 101–112). They suppose that human minds are composed of many special purpose devices rather than a few general purpose ones. First, they argue that general purpose learning devices could not learn what we learn. Second, they float the idea that we have no good generalised concept of learning.

(i) Even if a capacity can be acquired by a general purpose procedure, learning it that way may not suffice. Creatures will often need to acquire cognitive competences quickly and without errors. It would, for example, be dangerous to learn about one's potential enemies by trial-and-error. If an organism used a single learning system to acquire knowledge of likely nest sites, of what is good to eat, and of what to avoid it would be very difficult to restrict its "search space" in error minimising ways.

Moreover, many capacities may not be learnable by a general purpose device at all. A "combinatorial explosion" afflicts all general-purpose computational devices; there are simply too many possibilities to consider. So the domain of potential solutions must be drastically circumscribed early in the learning

process. Domain-general principles that effectively cut down search space are rather thin on the ground, so the prospects for a general learning mechanism do not look good. The combinatorial explosion is not the only bar to learning by a general purpose device. The "poverty of the stimulus" is another. Some capacities depend on a correlation between a perceptually salient feature of the environment and an adaptively significant one. Those capacities will be difficult for an unspecialised device to build when that correlation is not reasonably accessible in the ontogeny of a single organism. Many constancy mechanisms in perception and mechanisms of language learning depend on these types of correlation.

(ii) There may be no *unitary* capacity nor unitary process describable as "learning about your culture" or "learning about your environment". There are many forms of adaptive plasticity. The Mambo Chicken grew thicker bones in response to exposure to higher gravity (Regis 1990). But that was not learning. To think that learning is a useful notion is to suppose that there is some form of adaptive response which does not include such examples, but which does apply to a whole raft of apparently heterogenous changes in skill or behaviour. There is no obvious reason to think that learning has a single biological function, nor that there is a single mechanism that mediates each of the various types of learning we and other animals engage in. There are many differing adaptive responses to the environment, many different information-processing mechanisms involved in those responses and many results: procedural knowledge; quasi-physical skills; declarative knowledge and the like. It is unlikely that enculturation is a unitary process at either the levels of function, mechanism, or result.

Plausibility arguments are never decisive. But I agree that modular conceptions of the mind are more plausible than their rivals. But the distinction between the adapted and the generalist mind not clear-cut, and establishing the existence of a dedicated system is less easy than *The Adapted Mind* collective sometimes suggests. So I shall first explain their positive program, then my reservations about it.

III. THE ADAPTED MIND

The main empirical work of *The Adapted Mind* is the attempt to demonstrate the existence of "Darwinian Algorithms", specialist mechanisms adapted to specialist functions. The collection is largely devoted to a defence of specialist cognitive functions.

The defenders of this conception are very keen to distance themselves from "biological determinism" and its political and theoretical allies. To this end, Cosmides and Tooby insist on a major distinction between "behavioural genetics" and the "adapted mind" project. Behavioural genetics exemplifies two sins. One is the attempt at evolutionary explanations of behaviour not mechanism. The second is a focus on human difference: individual, racial,

sexual. *The Adapted Mind* contributors are not much interested in difference: they accept "the psychological unity of mankind" and aim to give an evolutionary and adaptationist account of that unity. To that end, we get the following overall model:

1. Human minds are sets of evolved and adapted information-processing mechanisms.
2. They are functionally specific, and hence they are domain-specialised.
3. They generate some of the distinctive cultural features of human societies. Cultural features so made available can be and often are modified in response to the specifics of the environment and/or spread by historical and epidemiological processes.
4. These "Darwinian Algorithms" substantially help explain the range of cultures and cultural artefacts that are humanly possible.

For example, Symons (with some support from Buss and Ellis) thinks that there are Darwinian algorithms of sexual attraction. These result in the tendency of human males to find those females attractive that bear the cultural marks of youth and of women to find attractive those that bear the cultural marks of high status. Cosmides and Tooby argue that specialist modules for regulating social exchange ensure that all human groups are aware of and have safeguards against defaulting on deals.

So the intuitive picture goes something like this. First, we need to identify the adaptive problems confronted by humans in their ancestral environments. So, for example, in interactions with their physical world, our ancestors would have to know a good deal about the range and whereabouts of food, and its different values. They would need a good grip on the physical, social and biological geography of a range likely to be extensive, and through which they would frequently shift. Second, we need to discover the stable correlations between those aspects of the environment humans are equipped to sense, and those aspects they need to know about. We can expect natural selection to engineer into task-specific devices implicit knowledge of these correlations. If in semiarid environments in which humans lived for a long time there was a stable correlation between a deeper green leaf colour and an accessible underground water flow, the Darwinian Psychologist would expect this to be engineered into those mechanisms specialised for controlling shifts through a complex and varied environment. Third, we must experimentally test for the actual existence of expected mechanisms, for it will not turn out that every potentially useful adaptation is actually engineered into us. Males are not provided with an ovluation-detector despite its potential usefulness. Of course, this may represent female success in an arms race between ovulation detection and ovulation concealment rather than a mere failure of adaptive engineering.

So they advertise the following picture:

i. Identify an "adaptive target": a problem the environment poses to the

organism which the organism needs/would benefit from solving.

ii. Identify environmental invariants that would provide information-processing support for a mechanism to solve the adaptive problem. In visual perception abrupt transitions in light intensity on the retinal image covary with edges. Thus a contingent environmental fact supports the decomposition on the visual scene into a three dimensional array of objects. Components of the visual systems assume that a moving body does not change shape as it moves. Without such an assumption, inferring shape from motions is typically impossible; with it, possible.

For instance, organisms may be able to augment their fitness were they able to direct appropriate levels of assistance to relatives. But for a "kin help module" to evolve there must be some detectable feature which has decent cross-generational correlation with relatedness. Otherwise the kin detection problem is intractable.

iii. On the basis of i&ii, construct an information-processing design – a homuncular functional organisation – that could solve the adaptive problem.

This constitutes the adaptive hypothesis. That hypothesis is evaluated by (a) working out how well that design would work in actual and ancestral environments; (b) by comparing that to actual performance capacities in these environments (if known); (c) comparing the proposed design to other possible designs.

IV. PROBLEMS: OPTIMALITY, MODULARITY AND UNIFORMITY

I am in agreement with the broad outline of this program. The negative case is largely right. The attempt to secure the autonomy of the social by appeal to a distinction between uniform, genuinely biological but uninteresting biological traits and diverse, interesting and cultural ones must be a mistake. Equally unfounded is the empiricist prejudice in favour of domain general cognitive mechanisms. Behaviours are not typically adaptations, though the mechanisms that generate them often are.

However, I have serious reservations about the positive program of *The Adapted Mind.* The distinction between modular and general mechanisms is not straightforward, and identifying modules is much less easy than some of the contributors suppose. I am not hostile to adaptationism as such, but I think *The Adapted Mind* runs the wrong adaptationist program. I am especially sceptical of its universalism. I very much doubt that there is anything like a single human design.

1. Can The Adapted Mind be Debugged?

Cosmides and Tooby think of selection as shaping organisms to environments: "the evolutionary process determines how the environment shapes the organism" (p. 85). Over time an organism's evolved design will be sensitised to stable features of its environment, and its design will change to exploit those features: "selection will design developmental adaptations that respond to those aspects of the world that have relatively stable recurrent structure, such that the mesh between the two will reliably produce design-propagating outcomes" (pp. 85–86). Thus we can identify the adaptive problems facing an animal together with the saliencies on which a solution depends. This identification points us towards the adaptive specialisations the animal's brain will embody.

I think there is both an empirical and a conceptual problem with this diagnosis of an organism's likely cognitive adaptations. First, the empirical problem. Hardwired mechanisms have a downside; in a malign world, they are vulnerable to deception. The problems that confronted our ancestors did not stay the same, and the regularities in the world on which these solutions depended were apt to change. Traits *sometimes* are adaptations to the preexisting environment. But when evolution is driven by features of the social structure of the evolving species, it transforms the adaptive landscape of the evolving organism. The evolution of language, of tool use, of indirect reciprocity, of solutions to the commitment problem, are not solutions to a pre-existing problem posed to the organism. There are no *stable* problems to which natural selection can grind out a solution. For adaptive mechanisms face "Irishing" in arm's races.[2] As men evolved to detect ovulation, women evolve to conceal it. As we evolve to detect cheaters and others of uncooperative dispositions, emotion-mimics evolve better and better fakes of a trustworthy and honest face. So there will be real troubles in store for a methodology of discovering the mechanisms of the mind that proceeds by first trying to discover the problems which it must solve, and then testing for the presence of the solutions. For that methodology does not reflect the interactive character of social evolution.

There is also a conceptual problem: an issue of individuation. What *are* the problems that exist "out there"? Is the problem of mate choice a single problem or a mosaic of many distinct problems? These might include: When should I be unfaithful to my usual partner? When should I desert my old partner? When should I help my sibs find a partner? When should I punish infidelity and how? I suspect that the count of one problem or many is settled by facts about the organism's response. If there is a single cognitive device that guides an organism's behaviour in respect of these issues, "mate choice" is a single domain and these are all different aspects of the same problem. But not otherwise. If that is right though, it is not the existence a single problem confronting the organism that explains the module, but the existence of the module that explains why we think of this as a single problem.

2. Module Hunting

I think the mind probably does have some form of modular organisation. But the distinction between the adapted and the generalist mind is not clean. Moreover, it is difficult to establish the existence of a module.

(i) The distinction between the adapted and the generalist mind is much murkier than it appears on first glance. First, learning devices can be biased without being bounded. That is one of the insights we can take from connectionist models of learning. Learning biases in the initial weight settings do not constrain or forbid eventual attainment of settings quite distant from the initial ones. Second, there are great ambiguities in the notion of a special purpose device. Consider the following learning rule:

> Do what your parents do!

Is it special or general? The mechanism directs attention to a very specific aspect of the organism's environment. But it is not a content-restricted rule, or a domain specific one. Many of the case studies in this book present ambiguities of just this kind, involving as they do the difficult problems of the role of motivation and emotion in cognition. Buss, Symons, Ellis, Daly and Wilson are surely onto something in thinking that there is something strikingly similar about the emotions of human sexuality across cultures. But it is much less obvious that these constitute a cognitive specialisation, a domain specific mode of thought, in anything like the sense exemplified by vision or language understanding.

The resources of the computational theory of mind allow us to distinguish between data structures and the procedures that operate upon them. So there is specificity of information. A domain specific learning device might be pre-equipped with information which determines the order in which hypotheses are tested (information on prior probabilities) but which has testing procedures which are neutral between all the many possible hypotheses. It was once thought that the language acquisition device worked that way. But modules might have procedures that are designed for, and work well in, particular domains. Heuristics for dealing with statistical problems might be one example. So we can distinguish between domain specific information and domain specific procedures. But these are certainly not the only ideas in the literature: encapsulation and the operational independence of a cognitive mechanism from conscious belief and decision is a central criterion for establishing the existence of a specialised cognitive system (Fodor 1983, Pylyshyn 1984). So one problem in this collection is that many of the papers are very inexplicit; they leave it quite unclear just what they take special purpose mechanisms to be.

Inexplicitness is not the only problem. I think many papers presuppose an essential connection between an evolutionary explanation of some feature of our mental life, and that feature being a consequence of a specialised mechanism. The relationship between evolution and specialisation is much more complex than that. For example, Wilson and Daly write that "Sexual jealousy is a

complex psychological system ... that is activated by a perceived threat ... that generates a diversity of circumstantially contingent responses" (p. 303). The idea that sexual motivations sometimes have evolutionary histories is surely plausible, but a good deal of the paper shows that these motivations do not function like a specialized system. For example, judgments about paternity and responses to it do not share the informational encapsulation nor freedom from central control of paradigm special systems. They contrast with incest avoidance mechanisms. These do seem cognitively impenetrable: being bought up together is necessary and sufficient. That is not true of the tensions of the step-parenting relation: "men who coresided with stepchildren from their births were if anything even more hostile towards them than were those whose step-relationships were established later" (p. 307). This seems evidence against jealousy being a system, not in favour of it.

The emotions show that the special purpose/general purpose distinction is often the wrong way of factoring evolutionary considerations into a theory of mind (Griffiths 1990). For though I doubt that there is a jealousy system, I do think the idea that these emotions have an evolutionary history is very plausible. Indeed, one of the most plausible contributions to evolutionary psychology is Frank's theory of the emotions as commitment devices.

Frank develops his theory of the emotions as a solution to the commitment problem. Commitment problems develop in situations in which an agent would be better off if they could "pre-commit". That is, they would be better off if they could take action, and be known to take that action, *now* which would render themselves unable to take an action then which would *then* be in their interests. One example is deterrence: how can a threat to deter be credible if a victim would be still worse off carrying out their threat were deterrence to fail. How can my threat to burn down your house if you shoot my dog be credible? The dog will still be dead, and legal and other repercussions will leave me still worse off. Frank argues our emotions serve as commitment devices. Emotions serve to guarantee both threats and promises. If that view of our emotions is correct, then we would expect the emotions should not being subject to central control; they are involuntary. For if they were, they would be turned off as central control recomputed the utilities in the new circumstances. We would expect emotions to be easy to read. For to serve their biological function they must be recognisable by others. Otherwise they will fail to deter/encourage.

This view of the emotions undercuts the contrast between a general purpose and a modular mind. For we can integrate Frank's theory with the idea that humans maximise expected utility. There is nothing in the formal apparatus of Rational Choice Theory that requires us to suppose that an agent's preferences are self-regarding. But those who employ rational choice theory typically take agent's preferences to be (i) roughly constant over time (ii) roughly constant over different individuals (iii) to be self-interested – "thick" rational choice theory. Unless one makes those assumptions, taking agents to be rational maximisers has no predictive power. If you can make any assumption about utilities, Rational Choice Theory becomes hopelessly *ad hoc*.

However, Frank's Darwinian theory of the emotions gives us an alternative thick rational choice theory by imposing constraints on preference structures. If he is right an agent's preferences are not narrowly self-regarding. But the theory explains their stability over time. Moreover, these preferences are known, known to be stable and hence can be relied on by other agents who need to predict their behaviour. They are not idiosyncratic; they are fairly constant over different agents. So the appeal to other regarding preferences need not be ad hoc. Suppose this is right. Notice how hard it is to apply a modular/generalist conception to this picture of a central aspect of our mental life. Preference structures can have an evolutionary history without being domain specific.

(ii) The perceptual systems exemplify most vividly a general truth about the modules of human mentality: the information-processing tasks implicit in human action are much more complex and difficult than one would intuitively expect. A "poverty of the stimulus" argument is often very persuasive. The output of the visual systems are determinate and astonishingly reliable representations of what is seen, yet the stimulus to the perceptual mechanisms are typically fragmentary and equivocal. That does support the view that those tasks could only be carried out by mental organs purpose-built for those very tasks. But I think we have to read the morals of *Vision* very carefully.

From time to time, the explorers of the adapted mind seem to lapse into the idea that a very cheap argument for an adaptation works:

> in a given species many of the same psychological adapations are likely to be involved in both food and mate choice... But the central evaluative mechanisms *must be* different because the qualities that make for nutritious food are not the qualities that make for a good mate... just as specialized, distinctively sexual anatomy exists below the neck, so the Darwinian expects it to exist above the neck. (p. 143)

There may be such a distinctive cognitive anatomy, but this argument surely does not show it. For if it were sound, we would have cognitive specialisations for judging both cricket and rugby, since the qualities that make a good cricket most emphatically are not those that make a good rugby. A distinctive capacity does not establish the existence of a specialised mechanism: there is no good cheap argument for a cognitive adaptation.

Even superior performance in certain cognitive areas is not sufficient grounds for positing a Darwinian Algorithm. For we clearly have the potential to *automatize* cognitive skills not subserved by purpose built wetware. Chess, bridge, and other difficult cognitive games provide striking examples. I imagine the cognitive skills involved in car driving are domain general and widely spread through the population. So I do not believe in establishing a distinctive capacity, even one that can be applied to unfamiliar cases, marks the existence of a cognitive adaptation.

Secondly, and very importantly, the "poverty of the stimulus" argument does not sustain some of the central hypotheses of the adapted mind. For example, Cosmides and Tooby argue that we have a module of social exchange. But their reasoning is the *inverse* of "poverty of the stimulus" reasoning. We find a range

of inferentially trivial tasks extraordinarily difficult. But when the reasoning tasks are about social exchange we do much better. So we do an easy task in one domain with less difficulty than the same task in other domains. A "poverty of the stimulus" argument applies where we find a computationally complex task that we do easily.

The same is true of mate selection. His gender-divergent mating rules are not computationally complex. Women find high status attractive whereas men find youth attractive. There is no need for a specialised mechanism to operate these decision rules. It may, of course, be very difficult to determine whether someone is young or of high status. But there are many social interactions in which age and status judgments will be important. Neither the type of problem nor the information used in their solution seems to vary importantly from social subdomain to social subdomain. So there seems no reason to suppose that assessing age and status is the work of a dedicated mate choice subsystem.

The contributors to *The Adapted Mind* emphasise the limitations of general cognitive mechanisms. Many of their points are well taken. But it remains to be shown that many important problems can be solved by specialist mechanisms. For example, in his 1983, Fodor convincingly argued that the pragmatics of language cannot be handled by a specialist device. It is one thing to know what a sentence means; it's another to know what someone means by uttering it. The latter problem is not encapsulatable: everything the hearer knows is potentially relevant and potentially used in decoding the speaker's intent. The same problem seems to arise for many of domains discussed in this book. Could an encapsulated mechanism deliver reliable judgments about a prospective mate's status, spousal infidelity or the probability of a prospective partner's cheating? We need to be shown the equivalents in these domains of the very reliable rules of thumb that our perceptual mechanism exploit. Though there are occasional attempts to do this (for example in the papers on pregnancy sickness and environmental aesthetics) the attempts are occasional and unsustained.

3. What Ancestral Environment?

Cosmides, Tooby and Symons argue that there is no reason to expect most of our behaviour to be fitness maximising even though it is the product of an adapted mind. For we are living in an environment very different from that in which we evolved. Such circumstances often produce maladaptive behaviours. But:

i. Abnormal environment worries cuts against design, too. For the development of adaptation is typically contingent on inputs from an evolutionarily normal environment. In very novel environments we should not expect design to manifest itself (Alexander 1990, Turke 1990). So mal-adaptive behaviours are probably not just the result of a normally constructed and operating mechanism operating in an environment out of the range for which it is calibrated, but of abnor-

mally developed mechanisms. It would not be surprising, surely, if our aesthetic responses to landscape develop in a way quite unlike those of our hunter-gatherer ancestors.

ii. The Pleistocene is not a single environment. There is no one environment in which most of human evolution took place. There must have been very considerable variations in physical environment: climate, terrain and food resources. Even more important were the changes in social environment. If Corballis (1991) is right, first there was the invention of language, then somewhat latter a techno-cultural Great Leap Forward. There may have been important changes in mating systems: male versus female dispersal; degrees of inbreeding etc. When group size reached some critical point, fitness benefits from co-operation shifted from direct reciprocation and kin selection to indirect reciprocation. So there is no single lost Pleistocene environment, in which adaptation matched adaptiveness, and for which we can assume our cognitive machines were built. I am sure that if pressed, none of the contributors to *The Adapted Mind* would defend the ecological homogeneity of the Pleistocene, but they repeatedly argue as if it were homogenous (pp. 110–111, p. 143, pp. 556–557).

iii. Moreover, what counts as a relevantly similar environment is trait-relative. For some but not all traits, current environment is relevantly similar to the ancestral environment; the "adaptive landscape" has not changed. We should note, though, that the "developmental; landscape" may change even though the adaptive utilities do not. Even if the affordances of the environment have not changed, the developmental cues it offers may have. Incest mechanisms offer a possible example.

4. What "Psychological Unity of Mankind?"

I do not think there is any good reason for Darwinian Psychology to accept the hypothesis of invariant design, a hypothesis unfortunately central to *The Adapted Mind*. I think the authors are too confident in their argument from the effects of recombination to the idea of a uniform human design.[3] There are too many polymorphic species for this inference to be good. There are physiological changes that are the consequence of new developmental mixes: for instance, regional differences in diseases. I would guess basic human metabolic processes are more developmentally buffered than some of our more recent cognitive adaptations. So we might expect more cognitive than physiological variation. Who knows how common cognitive equivalents of colour blindness are? The existence of psychological, not merely behavioural, variation between humans is common knowledge, and I see no reason to suppose that it can all be written of as noise obscuring a universal design (MacDonald 1990).

Moreover, there is an important ambiguity in their notion of human design. There is a shift between the notion of the design that *results from ontogeny* and the notion of the design that *directs ontogeny*; the "design of the developmental

programs". So for example, Cosmides and Tooby write:

> there is every reason to think that every human (of a given sex) comes equipped with the same basic evolved design. ... The critical question is not ... whether every human male ... engages in jealous behaviours. ... instead, the most illuminating question is whether every human male comes endowed with development mechanisms designed to assemble ... evolutionarily designed sexual jealousy mechanisms.... (p. 45)

When we talk of the design of the mechanisms responsible for behaviour, we deploy an innocent notion of design: the psychological adaptations driving behaviour. This is the notion suggested by the analogy to Gray's *Anatomy*. This notion is unproblematic but we cannot assume that all human minds have the same design. We now develop in environments in many respects unlike those in which we evolved. Developmental constancies depend on an interaction between genes and other developmental resources. So even if we suppose that Cosmides, Symons and Tooby are right in thinking selection would have driven a single design to fixation in hunter-gatherer communities, the resources available in the development of contemporary humans are now different. For all we know, these are quite varied from culture to culture. So the outcome of development may now differ both from its outcome in hunter-gatherer humans and from culture to culture.

Awareness of this problem induces Cosmides and Tooby to shift to a much more problematic notion, that of the design built into the genetic program. But this is a seriously problematic notion (See Oyama 1985, Gray 1992), and moreover one which they fudge. In considering a child never exposed to language, they agree that the child cannot learn to speak, but still claim "she will still have the same species-typical language acquisition device" (p. 45). There is not the slightest reason to believe this. All that is true is that the child is not genetically distinct from a normal language user. There is no reason to suppose that there is any *actual cognitive device* within the child's mind that resembles normal equipment. Aphasia victims are not genetically distinct from the normal population either. Would Cosmides and Tooby want to say that they have standard language modules?

Even if we granted that genomes really are blueprints from which the phenotype is constructed, and hence a blueprint for the language acquisition device exists in the wolfchild's genome, evolutionary psychology is actually about mate preference, language learning, colour vision and the like. That is, the functional and information processing structures within human minds, not about structures within the genome.

The psychological disunity of man is no idle possibility. One plausible hypothesis predicts disunity. I have sketched Frank's ideas about the emotions as commitment devices to make a point about modularity. They can also be used to make a point about the diversity of human cognitive design. For if he is right, then having emotions incurs a cost: really co-operating, rather than pretending too, and cheating. So we should expect an evolutionary arms race between the emotional, and emotion mimics, who try to parasitise, getting benefits but not

paying costs. We should expect long term survival of both mimic and model. For as the proportion of models goes up, the payoff of mimicry gets very high (for they will almost always interact with models), and the value of anti-mimicry devices goes down (for they are insuring against a rare danger) so mimics should not go to extinction. As the proportion of mimics goes up, the value of being a mimic goes down (for they will often interact with other mimics) and the value of anti-mimicry devices goes up. So models should not go extinct. But we should expect the signs of emotional life to be expensive to fake. So the commitment problem suggests that selection might maintain a diversity of human psychologies. There is no single best design for solving it.

V. CONCLUSION

I think the program of Darwinian Psychology is a good one. But I think it needs a more subtle understanding of the contrast between modular and general systems, and a less crude conception of the relations between evolution, selection, an evolving population and the environment it evolves in. I do not object to the adaptationist perspective. The idea that the mind is largely a structure of adaptations is a legitimate bet. But *The Adapted Mind* collective seems to me to have too simple a picture of the likely outcome of selection. Nonetheless, this is an important collection articulating a powerful manifesto. It deserves wide readership and response.[4]

NOTES

[1] See especially Turke 1990, Alexander 1990, Symons 1987, Symons 1990 and Tooby and Cosmides 1990.

[2] From the Irish Question: when the English came up with the answer, the Irish changed the question.

[3] See for example: pp. 38, 45, 80–81, 139, 211; on p. 61 invariance across the species is proposed as part of the *definition* of an adaptation!

[4] Thanks to David Hull, Ian Ravenscroft and Nick Taptiklis for their comments on an earlier version of this notice.

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