~~So even if we assume empirical adaptationism, the trouble with running *a priori* arguments for massive modularity is that there is no single privileged evolutionary prediction as to what kind of cognitive architecture we have. It seems clear that a cognitive architecture constructed with many specialised adaptations is just as plausible *a priori* as one constructed with a few general adaptations. As Lewens puts it, 'perhaps we responded to those problems not by acquiring a set of rigid adaptations, but by acquiring abilities to respond in ad hoc ways to new problems as they arose' (2007: 153). Although I disagree with the characterisation of specialised psychological adaptations as being 'rigid'—they are anything but, as we have seen—the general point is taken.~~

2.7 Methodological Objections

Even if considerations of selection do not guarantee massive modularity in any shape, the possibility remains that there might nevertheless be psychological adaptations and that, accordingly, it might be useful to hypothesise between adaptive problems and possible adaptive solutions. But even this has been challenged. The following objections are activated at different points during the methodological process of hypothesising between adaptive problem and adaptive solutions. And, as we will see, they are legitimate and pressing questions—questions that many have taken to have sufficiently destabilised methodological adaptationism in psychology.

2.7.1 No Stable Problems Objection

Were adaptive problems sufficiently stable to allow selection to engineer stable solutions? What if the relationship between environments, especially social environments that involve interdependent decision problems, and evolving populations is far more dynamic than evolutionary psychologists realise?

Sterelny (1995: 372) claims that there are, in fact, 'no stable problems to which natural selection can grind out a solution'. This argument also appears in Sterelny and Griffiths (1999). As you can see, this kind of argument, if successful, would pull the entire rug from underneath evolutionary psychology. The thought is that evolutionary arm races destabilise adaptive problems. And as evolutionary psychology trades on adaptive problems, evolutionary arm races also destabilises evolutionary psychology.

An arms race involves feedback: a problem arises, a solution emerges, but the emergence of the solution creates pressure on the original

problem, changes it, transforms it, thereby requiring the solution to change, and so on. Sterelny observes that, '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' (1995: 372). So evolutionary psychology methodology seems to trade on an overly simplistic characterisation of evolution, on stable adaptive problems and adaptive problems. How then can that methodology possibly capture this dynamic, this interactive character of evolution?

Do evolutionary arms races destabilise adaptive problems? I don't believe they do. But I shall leave that issue for the next chapter because it's important to continue building the strongest possible case against methodological adaptationism in psychology.

2.7.2 The Fine Grain Problem

So let's assume there are adaptive problems. How coarse are those problems? How do we individuate adaptive problems? How do we characterise them? How do we individuate them?

The problem of how to individuate correctly domains that specialised adaptations are purported to operate on is what Sterelny and Griffiths (1999) call the 'grain problem'. How coarse or fine is the grain of a domain? How specific is the adaptive problem? Sterelny and Griffiths (1999: 328) ask us to consider the domain of 'mate selection':

> Is the problem of mate choice a single problem or a mosaic of many distinct problems? These problems 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 and how should I punish infidelity?

The correct identification and characterisation of adaptive problems is not obvious. Is 'mate selection' one adaptive problem, with several sub-problems? Or is 'mate selection' a reference to many distinctive adaptive problems? There seems to be no principled way of answering this. It seems arbitrary.

2.7.3 No Constraints Objection

But suppose we have fixed the grain of the problem according to our own satisfaction. We can now reason from the adaptive problem to the adaptive solution—or vice versa. But how constrained will our

reasoning be? How elastic? In other words, from an adaptive problem can we straightforwardly read off an adaptive solution (or vice versa)? Or will there be many possible reasoning trajectories—perhaps too many?

A long standing, deeply entrenched worry in the literature is that the reasoning between adaptive problem and adaptive solution is too elastic. Fitness considerations can be so flexible that it seems possible to reason between adaptive problem *A1* and a spectrum of possible adaptive solutions *S1*, *S2*, ... *Sn*. And so too the other way: with sufficient ingenuity, fitness benefits can be imagined for almost any observed behaviour.

This concern is frequently cashed out into a very strong position: that evolutionary psychology hypotheses are unconstrained, that there is a free for all, a Darwinian Wild West of hypotheses. For example, Richardson declares that 'Just about anything is consistent with some evolutionary model or other' (2007: 65). Gray *et al*. (2003) approvingly quote Rosen (1982), who quips that there are only two limiting factors that constrain adaptation hypotheses: the imagination of the theorist and the gullibility of the audience.

So there is indeterminacy with respect to both the grain of the adaptive problem and, more seriously, in the (potentially runaway) multiplicity of possible hypotheses. But let us suppose that we have settled upon a hypothesis. Suppose, despite the indeterminacy, the elasticity, we have arrived at a hypothesis: that adaptive problem *A1* is solved by psychological adaptation *S1*. How good would that be as an explanation?

2.7.4 The Thinness of Evolutionary Psychology Explanation

Richardson (2007) claims that evolutionary psychology explanations are actually nothing more than 'crude speculations' (*ibid*.: 96), 'unconstrained speculation' (*ibid*.: 171), 'empty generalisations' (*ibid*.: 171), and that we should 'reject' them as such (*ibid*.: 38). Richardson does not deny that our psychological systems are the product of a long history of evolution, and that some of these might well be adaptations. Furthermore, Richardson is not claiming that evolutionary psychology hypotheses are false, nor does he offer alternative evolutionary scenarios for their hypotheses (*ibid*.: 139). His primary contention, the gravity of his complaint, is that as explanations they're evidentially unsupported—and unlikely to ever be supported.

Richardson's strategy is straightforward. First, establish what a complete adaptationist explanation looks like. Second, argue that evolutionary psychology hypotheses fall far short of this standard. Third, suggest

that evolutionary psychology will never be able to produce the evidence required to establish complete explanations. From this, Richardson believes, evolutionary psychology hypotheses can be declared as 'little more than storytelling' (*ibid*.: 147), 'idle Darwinizing' (*ibid*.: 183), valuable only in discrediting the method that produced them.

Richardson points out, quite rightly, that adaptation claims are historical claims: to claim that *X* is an adaptation is to make an historical claim. 'Whether a trait is an adaptation thus depends on its evolutionary history; and explaining some trait as an adaptation depends on knowing the evolutionary history that produced it' (2007: 98). A complete adaptationist explanation cannot simply indicate a scenario of how selection might have favoured the evolution of some given trait; it must also specify the history of the trait's selection, reliable historical information concerning under what conditions selection actually occurred. To vindicate such historical claims, we need evidence concerning variation in ancestral populations, evidence concerning their heritability and the like. Richardson draws on Brandon (1990), who lists five categories of evidence complete adaptation explanations should provide: (1) selection, (2) ecological factors, (3) heritability, (4) population structure and (5) trait polarity.

First, there must be evidence that selection has, in fact, occurred; without such evidence alternative evolutionary explanations are viable. As mentioned in Chapter 1, without variation there can be no selection, so we need information about trait variation in ancestral populations, as well as the relative rates of survivorship and reproduction of organisms with varying traits—that is, information on differential fitness. Second, the ecological factors and pressures, whether abiotic or biotic, determining relative survivorship and reproduction should be specified. For example, if a specific predation factor affected relative survivorship and reproduction, this should be specified. Third, there must be evidence that the trait in question is heritable. Without heritability, selection is powerless. Fourth, as the strength and direction of selection crucially depends on population size, gene flow, interbreeding and mutation rates, there must be reliable information regarding population structure. Different population structures will be affected by selection in different ways. Richardson points out that gene flow can reduce the impact of selection in a local population, and that small populations are more sensitive than larger populations to chance. Fifth, phylogenetic information concerning primitive and derived traits is required. Is the alleged adaptation a primitive trait or a derived trait? Is the trait ancestral within a clade or is its presence the result of convergence

among lineages? Answering this requires an independently established phylogeny.

Richardson applies Brandon's criteria to Buss's research on the evolution of sex differences in jealousy (2007: 60–4), Pinker's research on the evolution of language (*ibid*.: 124–9), and Cosmides and Tooby's research on their proposed cheater detection mechanism (*ibid*.: 132–6). In his judgement, evolutionary psychologists frequently and dismally fail to provide the evidential particulars, the actual selectionist history, required of adaptation explanations. For example, 'In Buss's exposition of the case for jealousy, there is no significant appeal to the historical conditions of human evolution, aside from general appears to the conditions of the Pleistocene' (*ibid*.:88). Little detail is cited about the structures of ancestral populations, about immigration and gene flow. Few details are provided as to the variation of traits between organisms, the degree of selective advantage, the ecological mechanisms producing selection, or the trait's degree of heritability. And comparative studies and phylogenetic analyses are seldom, if ever, cited or performed.

No matter how plausible evolutionary psychology explanations might sound, they lack the crucial kinds of information concerning the selective pressures and historical contingencies shaping our psychology.

> It is, of course, plausible that our ancestors engaged in sharing of food and other resources—so, for that matter, do chimpanzees—and it may be true that there was considerable variance in food availability. But even if true, these general facts and constraints are inadequate to capture the kind of historical information we would need to construct an evolutionary explanation, including factors such as the kind and extent of variation present in our ancestors, the actual environmental features that affect survival and reproduction, and demographic factors (*ibid*.: 138).

Although Richardson only considers a few examples, we can grant, I believe without controversy, his claim that evolutionary psychology explanations frequently don't provide the full range of evidential particulars expected of complete explanations. When evolutionary psychology research for some given phenomenon is presented as an explanation for that phenomenon, it's a safe bet we can identify missing details. Indeed, highlighting the incompleteness of evolutionary psychology explanations is a standard manoeuvre in the sceptical literature. For example, Profet (1992) hypothesised that pregnancy sickness

is an adaptation to avoid toxins harmful for the fetus. Sterelny (1995) pointed out that the pregnancy sickness might well have benefits but that Profet didn't balance the purported benefits against possible costs, to perform a proper cost–benefit analysis. Evolutionary psychologists are also frequently berated for not adopting comparative tests for traits across a range of species closely related to us. 'Evolutionary Psychology sometimes gives the impression that new cognitive processes appeared suddenly and fully-formed as a result of lucky genetic mutations and fierce, unimodal selection pressures' (Heyes, 2012: 2093).

Furthermore, Richardson claims are not only evolutionary psychology explanations unsupported as they currently stand, but they're also unlikely to be supported in the final verdict. Unfortunately, Richardson doesn't provide an argument for this, and admits as much: 'I suggest, but do not demonstrate, that in the end we are unlikely ever to have the sort of evidence that would be required to make it reasonable to embrace the hypotheses of evolutionary psychology' (2007: 38). Beyond one or two statements expressing this pessimism, he doesn't push the thought any further.

As evolutionary psychology research stands woefully deficient, in Richardson's judgement this means evolutionary psychology simply consists of 'empty generalisations' and 'unconstrained speculation' (2007: 171)—just so stories of what could have been, speculation dressed up in scientific jargon. Just so stories like Kipling's *Just So Stories for Little Children*, imaginative, fanciful origin stories for the distinctive traits various animals have. How did the camel get its hump? The hump allows the camel to work several days without eating. And as you can imagine perhaps long ago the camel refused to work and so was given the hump by a djinn as punishment. The adaptationist explanations offered by evolutionary psychologists are 'Just So Stories for Adults'—perhaps a little less fanciful, perhaps a little more plausible, but still just so stories. Evolutionary psychologists might be inclined to establish their scientific credentials by off-hand references to evolutionary theories and talking about adaptive problems, but on the whole their hypotheses are evidentially weak when judged against the criteria above. This is scientifically indefensible.

Richardson (2007) is but the latest formulation of the complaint. It might not be the last. This line of thought has a long pedigree, going back to Lewontin and Gould. Lewontin declares 'we know essentially nothing about the evolution of our cognitive capabilities and there is a strong possibility that we will never know much about it' (1990: 229). Unsurprisingly, Gould concurs:

> how can we possibly know in detail what small bands of hunter–gatherers did in Africa two million years ago? ... how can we possibly obtain the key information that would be required to show the validity of adaptive tales about an EEA: relations of kinship, social structures and sizes of groups, different activities of males and females, the roles of religion, symbolizing, storytelling, and a hundred other central aspects of human life that cannot be traced in fossils? We do not even know the original environment of our ancestors—did ancestral humans stay in one region or move about (2000: 120)?

Kitcher and Vickers also focus on evolutionary psychology research lacking evidential particulars, claiming that the historical information they put forward is rudimentary, that the underlying genetic details are absent, and conclude 'we ought to dismiss these suggestions as vague speculation' (2003: 337).

So it appears that this indeterminate or rickety method produces thin 'how possibly' explanations rather than deep 'how actually' explanations, insufficiently determinate and insufficiently detailed to be compelling as complete explanations.

All of this throws the method's explanatory value into serious question. But the situation actually gets worse: underdetermination of theory by evidence means that whatever explanation is put forward to account for some phenomenon, an alternative explanation equally as fitting the phenomenon can also be postulated. Indeed, even though evolutionary psychologists conduct experiments to see whether people have psychological adaptations, these results are still subject to underdetermination. Furthermore, one can always challenge the interpretation of experimental results or even the reliability of the experiment. Buller (2005) has especially leveraged such manoeuvres against evolutionary psychology. However, it's important to realise these considerations are not unique to evolutionary psychology—they are ubiquitous to any scientific enterprise. Any principle governing theory selection in light of underdetermination is a principle that can be applied to this debate. I shall make further remarks on this in Chapter 5.

As you can expect, these kinds of considerations have led to a near consensus in philosophy that the adaptationist method in psychology is explanatorily weak. But what about its purported heuristic value?

2.7.5 The Mystery of Discovery

Many philosophers have entirely or almost entirely ignored evolutionary psychology's purported heuristic dimension. And it's not difficult

to see why. We have seen that fitness thinking represents insufficient constraint on adaptationist methodology. How can evolutionary psychology possibly be heuristic if fitness considerations between adaptive problem and adaptive solution can yield multiple trajectories? If I consider adaptive problem *P1* and fitness thinking generates a long list of possible adaptive solutions *S1, S2, ... Sn*, how can adaptationist methodology be of predictive value? How can adaptationist methodology discover anything new?

Indeed, while it seems clear what an adaptationist explanation looks like and precisely why reasoning between adaptive problem and adaptive solution can't possibly produce the assortment of evidence needed for a complete adaptationist explanation, it's somewhat unclear in the literature in virtue of what could a methodology be considered heuristic and whether evolutionary psychology methodology can be considered as such. This way of seeing evolutionary psychology is undeveloped in the philosophy literature. But we will develop it further in the next chapter.

For now it is sufficient to note that, as we saw in the previous chapter, evolutionary psychologists claim to have made important discoveries in psychology—the key example being the cheater detection mechanism. What are we to make of such claims?

Unlike many others, Schulz (2011) has taken a closer look at some of the purported trailblazing examples of evolutionary psychology as a heuristic. And upon closer examination, he has found something apparently destabilising to evolutionary psychology's heuristic credentials: these trailblazing exams are actually explanatory, not heuristic. If this is indeed the case, and if indeed this is representative, this further illustrates the method's limited usefulness, perhaps even its bankruptcy. Note, unlike Buller (2005), Schulz (2011) is not concerned with the finer details of confirmation, whether such and such experiment actually supports a given hypothesis—just whether the hypothesis is genuinely novel in the first place or whether it is instead actually (attempting) explanation or accommodation of what was already known.

Schulz examined the cheater detection hypothesis. Recall from the previous chapter that the cheater detection hypothesis is often submitted as being the leading example of a successful hypothesis generated by reasoning from the past to the present. Cosmides and Tooby (1992) hypothesised that selection would favour the evolution of a mechanism dedicated to detecting, and avoiding future cooperative effort with, cheaters—a cheater detection mechanism, a psychological adaptation for representing and computing social exchanges. They found evidence for this hypothesis in the Wason selection task.

Sounds impressive. Here we have a case of forward engineering: beginning with a selectionist scenario, postulating a hitherto unknown psychological mechanism, and then canvassing evidence to vouch for it. Or do we? Although this is how the hypothesis is typically described as being generated, the description is problematic. Wason selection task anomalies were discovered and known before the cheater detection mechanism was hypothesised: the original Wason selection task was published in 1966 and the Griggs and Cox version was published in 1982. Of course, the fact that Wason selection task anomalies were known before the cheater detection hypothesis was formulated doesn't in itself prevent the Wason selection task anomalies being scored as a novel prediction of the cheater detection hypothesis—a known fact can be considered a novel prediction of a hypothesis as long as it wasn't used in the construction of the hypothesis. But there lies the problem: it seems that the cheater detection hypothesis was constructed specifically to account for Watson selection task anomalies. To see this, merely observe that Cosmides's 1985 PhD thesis, where the cheater detection hypothesis was first proposed, is entitled 'Deduction or Darwinian Algorithms? An *Explanation* of the "Elusive" Content Effect on the Wason Selection Task' (my emphasis).

Schulz is surely right when he says that this is a clear case of evolutionary theory being applied in an explanatory way and not in a heuristic way.

> To see this, note that the key social psychological effect difference to be accounted for had *already been known* when Cosmides & Tooby put their evolutionary hypotheses forward: the difference in the success rates in evaluating the two kinds of conditionals was the *starting point* of their evolutionary investigation—and not an end state (2011: 222; original emphasis).

The obvious response here is to point out how much research has been sparked by the efforts of Cosmides and Tooby. Before Cosmides and Tooby, the Wason selection task was an obscure anomaly, unanchored, being claimed by no theory. Enter Cosmides and Tooby: suddenly researchers take notice of inferential reasoning over conditional rules, both proponents and opponents of evolutionary psychology, and an avalanche of experiments testing competing explanations arises. The Wason selection task was back on the map.

Schulz recognises this response. Nevertheless, he stresses that even though Cosmides and Tooby gave an explanation where none before existed, and even if it led to further tests using the Wason selection

task, and even further experiments to rule out competing explanations, this alone doesn't elevate the accommodation into a novel prediction. 'Because of this,' claims Schulz, 'it seems clear that this case does not support a heuristic interpretation of evolutionary psychology—it quite simply does not exemplify any heuristic application of evolutionary theory at all' (*ibid*.: 224).

Having argued that Cosmides and Tooby's cheater detection hypothesis is a product of accommodation, Schulz briefly looks at another well-known example of evolutionary psychology work: Buss's work on 'Sexual Strategies Theory'. However, Schulz judges that this, too, must also be seen as trying to accommodate existing knowledge of the differences in the way men and women choose mates. 'This comes out clearly from the fact that Buss *begins* his research by empirically substantiating the widespread supposition that males tend to want different things from the things that females want (at least in some cases), and then uses Trivers's theory of minimal parental investment to *account* for these differences' (*ibid*.: 226; original emphasis).

Who can argue with that? Disputing the claim that sex differences in attitudes to sex was an idea doing the rounds long before evolutionary psychology entered the scene would be a brave move to make. Schulz further claims that similar judgements can be made about much of Symons's, Daly and Wilson's, and Pinker's work and 'that of many other researchers in this area (for more on this work, see e.g. Barkow et al., 1992)' (*ibid*.: 266). Barkow *et al*. (1992), of course, refers to the seminal *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*, the research programme's first handbook.

Indeed, recall Daly and Wilson (1980), who proposed that we have a psychological adaptation for discriminative parental solicitude. Is it really a discovery that parents generally tend to care less for stepchildren than they do for their genetic children? Even if studies verifying this were originally thin on the ground, and Martin Daly and Margo Wilson performed new studies, wasn't the *Cinderella* Effect already common knowledge? Wasn't this a phenomenon already known, even if not fully established by research, rather than discovered?

The implication is clear; and forgoing discussing other examples, Schulz reaches his conclusion—the contention that most evolutionary psychology practice is accommodation: 'Moreover, it is easy to see that this conclusion generalises to many other evolutionary psychological research projects' (*ibid*.: 226). Hence, while '*merely possible*, *fictional* evolutionary psychology' is heuristic, *'currently practiced*, *actual* evolutionary psychology' is not (*ibid*.: 218, original emphasis).