The London School of Economics and Political Science

Evolutionary psychology: theoretical and methodological foundations

Andrew Marc Antony Goldfinch

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For Mum and Dad

Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

I declare that my thesis consists of 62,841 words.

Abstract

Of all the research programmes in the evolutionary behavioural sciences, evolutionary psychology is unique in the scale and intensity of criticism it faces, from both philosophers and social scientists, forming a powerful impression that, no matter its purported benefits, evolutionary psychology is a discredited research programme, an outdated research programme, something one can legitimately dismiss.

This thesis contends that those who dismiss evolutionary psychology wholesale fail to entitle themselves to that dismissal.

I begin by championing a streamlined evolutionary psychology, one that navigates away from unnecessary controversy, one that better reflects the actual practice of evolutionary psychology on the ground, and one that doesn't overshadow what's valuable about the programme.

After correcting several common misconceptions about evolutionary psychology, I arrive at the heart of what adaptationist hypothesizing can do for psychology: discovering new design features of extant psychological traits and discovering hitherto unknown psychological traits. I go through the logic of adaptationist reasoning in psychology. *Inter alia*, I argue that, although evolutionary psychology hypotheses might start off as 'simple', they can progressively become more complex, progressively mirroring the adaptations they're targeting. Existing philosophy of science treatments of evolutionary psychology have given prominence to sceptical arguments, which means the positive presentation of evolutionary psychology has come rather short – something I seek to redress by demonstrating its potential for novel predictions across a wide spectrum of phenomena.

It's reasonable to demand greater evidence for evolutionary psychology explanations but it's wrong to demand that evolutionary psychology alone satisfy such demands – these demands are properly allocated to the evolutionary and behavioural sciences collectively. With its legitimate and reasonable role in the evolutionary behavioural sciences correctly identified, evolutionary psychology merits serious consideration – contrary to the prevailing pessimism concerning its credibility.

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Contents

Preface

This thesis focuses on evolutionary psychology's heuristic value. What follows are some general remarks on what heuristics are and how we can judge whether a research programme or effort has heuristic value.

The word 'heuristic' simply means to find or to discover. Heuristics are recipes, strategies, or methods for generating novel hypotheses in a given domain. They work by reducing research space, focusing research effort along constrained, plausible, but risky trajectories. By fleshing out these defining properties or ingredients - hypothesis building, predictive novelty, constraint, and risk - we can establish a benchmark for adjudicating whether a research programme has heuristic value.

First, heuristics guide and organise research – specifically, they guide and govern hypothesis generation. The conceptual framework of a heuristic research programme will specify the objective of the research effort, what its targets are, such as detecting and finding certain patterns or phenomena within a given domain. The methodological framework will be crafted according to the peculiarities of the objective – that is, it will present a recipe or set of strategies for achieving the research goal.

Such recipes or strategies, the heuristics, will specify how to initiate a relevant research trajectory – what constitutes a legitimate starting point, what things to consider, and what things not to consider. A series of methodological steps or stages will be presented, which, when followed, generate a research hypothesis or solution. The steps can take a positive form (consult *X* information) or a negative form (be wary of *Y* information). Some steps will be obligatory – certain questions must be asked; certain information must be consulted and taken into account. Other steps will have a degree of optionality, being more like useful hints than requirements (such as, if *X* and *Y* are the case, consider *Z*). Optional steps usually come into play when refining hypotheses. The end goal – the testable hypothesis – must satisfy some specified criteria.

A research programme's heuristics will often be presented in theoretical manifestos and introductions to edited volumes, often by the leading practitioners of the programme, the trailblazers, and often with one or more flagship examples of successful hypotheses delivered by such means. Naturally, therefore, these are the first port of call for those wishing to understand a research programme's heuristic resources. But here caution must be exercised: one mustn't fall into the mistake of thinking that these are necessarily

complete or comprehensive accounts. They might be, and perhaps often are, only partial codifications. Some heuristics might be implicit and only occasionally articulated. Furthermore, the stages of hypothesis generation might be more sophisticated, more dynamic, than is gleaned from reading standard, introductory accounts. The philosopher who wishes to adjudicate on the heuristic value of a given programme should therefore be wary of consulting such accounts in isolation and then make play with perceived shortcomings - rather, she should examine such accounts in tandem with best practice, and submit a more comprehensive methodological account if existing accounts are incomplete.

Second, heuristics help researchers generate novel predictions. Heuristics are associated with the exploration of hitherto unexplored research paths. They allow researchers to see a domain or subdomain in novel ways, to investigate possible patterns that those without the heuristics are blind to, and to articulate these unique insights to the point of testability. So one of the hallmarks of heuristics is that they generate genuinely *novel* hypotheses: they generate novel predictions about known phenomena or novel predictions about hitherto unknown phenomena. The more novel these predictions are, the more surprising and counterintuitive, the better.

If one seeks to establish a research programme's heuristic credentials, one must establish that in principle the strategies and methods can generate novel predictions, and that in practice it has indeed generated genuinely novel predictions. Likewise, if one seeks to deny that a research programme has heuristic value, one option is to scrutinise and challenge novelty claims. One could challenge whether the methods really have yielded novel predictions. Perhaps behind the fanfare and fireworks, a research effort is simply accommodating what we already know within its conceptual framework.

Third, heuristics drastically cut down (often quite vast) research space, reducing the search space for a given research objective to something plausible and manageable. Suppose our research objective is to discover new facts about extant psychological traits. The search space, the space of all possible hypotheses, will be vast - too vast to execute an exhaustive trial and error search across all possible trajectories and avenues. What we need, therefore, is something to constrain this search space, identifying plausible or promising trajectories, leaving implausible and less promising trajectories unexplored. Heuristics determine what to look for – what constitutes a legitimate starting point, what methodological steps must be taken, what criteria a proposed hypothesis or solution must satisfy – and so perform this function, restricting precious research effort to a relatively

small portion of the possible research space. So to establish whether a research programme has heuristic value, we need to establish that its strategies and methods adequately constrain research space.

Note that we should not read the constraint requirement as a requirement that heuristic strategies and methods produce only one hypothesis for a given target. It's possible, but by no means necessarily typical, for two researchers following the same set of procedures to generate two different hypotheses for a given target. Heuristics consist of steps and procedures and solution criteria and, while they constrain hypotheses, slight differences of interpretation might occasionally generate a handful of hypothesis for a given target instead of just one. The point, however, is that heuristics allow only a limited number of hypotheses to be generated for a given target, whether one or occasionally a small number – a small number at most, suitable for experimentation, not a paralysing, runaway list of hypotheses.

Furthermore, while heuristics significantly reduce the size of the research space, researchers might nevertheless need to rely on a degree of trial and error within the constrained space. For example, strategies and methods might suggest or recommend several options for refining and developing a successful hypothesis, which might implicate a limited amount of tinkering and trial and error. Such permutations will not be too great, being within a suitable range to investigate.

In contrast, strategies and methods that can be used to generate a runaway list of hypotheses for a given target, that generate activity seemingly indistinguishable from blind trial and error, do not have heuristic value. So in the example of seeking to discover new facts about a given psychological trait, strategies and methods that allow an avalanche of competing hypotheses to be generated about the trait cannot be said to be heuristic.

Fourth, heuristics are risky: they constrain research to plausible and promising trajectories without guaranteeing such exploration will be empirically successful.

> The search space is Vast, so the method of search must be "heuristic" - the branching tree of all possible moves has to be ruthlessly pruned by semi-intelligent, myopic demons, leading to a risky, chance-ridden exploration of a tiny subportion of the whole space. (Dennett 1995: 209)

Heuristics will give us a range of research bets to make – reasonable bets, worthwhile bets, but bets all the same, trajectories involving risks of failure, of being blind alleys. To borrow a classic Popperian phase, heuristics 'stick their necks out'.

So heuristic strategies are expected to occasionally lead to predictive failure. Nevertheless, they are not expected to systematically fail. Some predictive bets won't pay off - but others will. The risk, therefore, should pay off, in the form of a harvest of new knowledge. Occasional failures can be excused – but a systematic failure to produce successful novel predictions, to produce new experimental facts, can't be excused. A set of strategies and methods that only, or primarily, lead to failure cannot be called heuristic.

This is why heuristics can win a research programme scientific legitimacy: by charting risky, unexplored but plausible trajectories, by making successful novel predictions, research efforts that employ heuristics make genuine and permanent editions to knowledge, establishing new facts that would otherwise be missed in the sea of possible hypotheses.

Introduction

My thesis is concerned with the theoretical and methodological foundations of evolutionary psychology. By 'evolutionary psychology' I'm not referring to the broad field of applying evolutionary theorising to psychology. For that, I use the term 'evolutionary behavioural sciences'. Rather, I'm referring specifically to the ideas and methods championed by the group of psychologists who explicitly refer to themselves as evolutionary psychologists. This programme is characterised as using adaptationist reasoning to generate testable hypotheses in psychology. When speaking of 'research programme', I'm not attempting to evoke or recruit Lakatos. By 'research programme' I simply mean just that - a programme of research, a set of ideas and methods. One can alternatively call this a 'research tradition', 'research enterprise', or 'research style'. Nothing hinges on that. Furthermore, I do not imply that individuals pursue this programme, tradition, or style exclusively. Some do. Some do not.

The heartbeat of this thesis, my overriding concern, is that what's valuable about this programme has been overlooked by the strong sceptics, underappreciated by the more moderate sceptics, and overshadowed by needless theoretical positioning by its prominent champions.

Adaptationist thinking in psychology should be taken more seriously, not discarded because some have constructed a cardboard version of it or adopted theoretical positions well in excess of what is required to enact the programme. I want to identify, champion, and vindicate a streamlined evolutionary psychology, one that navigates away from unnecessary controversy, one that better reflects the actual practice of evolutionary psychology on the ground, and one that doesn't overshadow what's valuable about the programme. It's both a conservative and a radical move to make. I think it's even an exciting move to make, for it seeks to focus attention on what's really worth fighting for in evolutionary psychology. I believe it's the sort of argument concerning evolutionary psychology that needs to be made, and the way I present it should make it clear that it's an attempt to avoid the critical and theoretical excesses on both sides of the debate.

My broad strategy is as follows. Begin by identifying the tenets commonly associated with evolutionary psychology and then sift through them, see which are required of the programme, and which are not. Those tenets that are required for evolutionary psychology's daily practice I shall adopt and vindicate. After some conceptual clarifications,

I'll reach the heart of the programme, and then seek to vindicate it from deeply entrenched criticisms in the literature. In more detail, my thesis progresses as follows.

In the first chapter, I begin by underscoring the fundamental reasonableness of evolutionary psychology. The daily practice of evolutionary psychology is to focus on adaptive problems, hypothesize adaptive solutions, and then subject these hypotheses to testing. Sounds reasonable enough. Why, then, the controversy? I outline the tenets commonly associated with evolutionary psychology: empirical adaptationism; a strong form of methodological adaptationism; usually a strong form of massive modularity; inferring strong massive modularity from empirical adaptationism; a unification agenda for the behavioural sciences; and public policy interventions. What strikes me is how few of these tenets, some of which are contentious, have anything to do with evolutionary psychology as practiced. If evolutionary psychology is about discovering psychological adaptations, why package all these tenets together? It's time to prize apart the tenets associated with evolutionary psychology. Thus I advocate a streamlined evolutionary psychology.

The sceptical literature is often disfigured by misconceptions about basic evolutionary psychology concepts. These mistakes resemble the classic whac-a-mole game: no matter how many times a mistake is hit over the head and dismissed, it's only a matter of time before it reappears. In chapter 2 I deal with some of these mistakes, mistakes sufficiently common to be worth correcting, and mistakes touching on theoretical issues that have methodological consequences. Evolutionary psychology is often characterised as (i) being conceptually outdated, (ii) proposing that we're the Flintstones in modern attire, struggling to cope with this post-industrial world; (iii) proposing that psychological adaptations are rigid, (iv) consenting to, or requiring, a discredited picture of development, and (v) proposing nice-and-tidy, encapsulated psychological adaptations. The reality, as we shall see, is rather different.

We then arrive at the heart of what adaptationist hypothesizing can do for psychology. The underlying logic of evolutionary psychology discovery is this: *if* psychological trait *T* is an adaptation for *X*, then psychological trait *T* should have configuration *C*, and so we should find phenomenon *P*. That is, if *T* is an adaptation for *X*, we should see signatures of this in the adaptation's design, in the kinds of inputs it receives, in the kinds of output it produces, and so on, and this can locate behavioural patterns we've never observed before. This kind of reasoning is incredibly useful and has led to important new discoveries. I go through the

logic of adaptationist reasoning in psychology in some detail. I argue that, although evolutionary psychology hypotheses might start off as 'simple', they can progressively become more complex, progressively mirroring the adaptations they're targeting. I also deal with three common objections: the fine grain problem, the no constraints objection, and the no stable problems objection.

So evolutionary psychology can generate novel predictions. A question naturally arises: How common is this in practice? Schulz (2011) argues that heuristic applications of evolutionary psychology are actually quite rare. In chapter 4, I challenge this argument. First, for the sake of argument, I grant the characterisation. Even if evolutionary psychology is primarily accommodation, not prediction, this is what one expects of a young programme. Second, I challenge the characterisation, and expose just how flimsy it is. There are hundreds of examples of evolutionary psychology work, yet sceptics only cite a handful of cases – a handful of *old* cases. The heuristic applications of evolutionary psychology are far greater than the sceptics give credit for.

Evolutionary psychology tends to focus on a relatively narrow range of topics. Sometimes this gives the impression that evolutionary psychology's usefulness is limited to those topics. In chapter 5, I demonstrate that this is a mistake. First, I give an example of evolutionary psychology generating a novel prediction about non-social behaviour. Then I give an example of a surprisingly hypothesized by-product of a single adaptation, and a hypothesized by-product of multiple adaptations. I then try my hand at applying adaptationist reasoning to an issue it has yet to be applied to. Climate change is perceived to be a threat, perhaps even an existential one, yet we're not alarmed by it. Surely this is odd: when faced with a threat, we're alarmed into action. This got me thinking about whether our psychology is poorly adapted to deal with the kind of threat climate change represents. I suggest, in light of adaptationist reasoning, that our alarm processes are poorly designed to react to climate change. The tragedy of climate change might not be that it's happening too quickly but that it's not happening quickly enough. On the strength of all these considerations, it should become clear that the scope of evolutionary psychology is potentially very wide.

In chapter 6, I deal with perhaps the most pervasive, the most deeply engrained argument against evolutionary psychology, one with the longest pedigree. I deal with its most recent formulation: Richardson (2007). The argument goes like this: when framed as explanations, evolutionary psychology research is remarkably sketchy in historical details. Such

explanations are incomplete. And they're likely to remain incomplete. From this, Richardson and others dismiss evolutionary psychology as speculation, something safely to be ignored.

I grant that many evidential particulars are missing from evolutionary psychology explanations, but the conclusion Richardson and others draw from this is far in excess of what the considerations tabled actually warrant. His claims will not win for him the conclusion he seeks. Failure to establish all evidential particulars, either at the present moment or in the final verdict, does not in any way undermine the utility and value of adaptationist hypothesizing. The convictions of evolutionary psychologists need not, therefore, be shaken.

At this point, we could just dismiss the challenge, and leave it at that. But I think we should do better than that. In rising to the challenge of better completing adaptationist explanations we better locate evolutionary psychology's role in the evolutionary and behavioural sciences. Establishing the multiple lines of evidence needed to fully vindicate adaptationist explanations requires multidisciplinary activity. In virtue of their successful novel predictions, evolutionary psychologists can help stimulate multidisciplinary activity. So while it's reasonable for sceptics to demand greater evidence for evolutionary psychology explanations, they're wrong in demanding evolutionary psychology alone satisfy such demands – these demands are properly allocated to the evolutionary and behavioural sciences collectively. Furthermore, I report on a project that will help facilitate such multidisciplinary activity by, for the first time, codifying hundreds of purported psychological adaptations and evaluating their evidential strength. I end the chapter by showing how surprisingly detailed reconstructions of past conditions can be made from relatively sparse data. On the strength of this, I offer a moderate optimism about the prospect of the evolutionary and behavioural sciences collectively establishing more complete adaptationist explanations.

If evolutionary psychology is to realise its potential of stimulating multidisciplinary activity towards establishing the multiple lines of evidence needed to fully vindicate adaptationist explanations, the evolutionary psychology debate needs depolarising. In chapter 7, I attempt to do this. I contend that the strong sceptics' basic mistake is to overlook the heuristics of the programme and to instead focus on the current evidential status of psychological adaptation claims. Given that the function of evolutionary psychology is heuristic, and given that vindicating adaptation claims requires multidisciplinary activity

over a long period of time, this is regrettable. Prominent evolutionary psychologists' mistake is to obscure or overshadow the heuristics by associating it with tenets irrelevant to its practice. Given that evolutionary psychology needs multiple disciplines to engage with its findings, needs multiple disciplines to take seriously what the programme has to offer the evolutionary and behavioural sciences, this too is regrettable.

A more moderate scepticism also needs to be addressed. Because this strand of scepticism 'requires' that evolutionary psychology 'change its daily practice' (Bolhuis *et. al.* 2011), it's actually quite subversive. I strongly contest the notion that evolutionary psychologists should do things other than generating testable hypotheses. Evolutionary psychologists have a unique role to play in the evolutionary behavioural sciences. They have a specialised job to do. If they didn't perform this role, the evolutionary behavioural sciences would be impoverished.

I conclude by advocating that strong sceptics, prominent champions of the programme, and milder sceptics respectively focus on, refocus on, and value the heuristics of the programme. Since evolutionary psychology is in the business of generating and testing hypotheses about psychological mechanisms, then it is not only eminently reasonable but it should be welcomed by all. If the various parties endorsed this picture, this streamlined evolutionary psychology, the polarisation can end, and the fruits of genuine multidisciplinary research on purported psychological adaptations can begin to grow.

Philosophy has a special role to play in fostering greater understanding of arguably the most active application of evolutionary theorising in the behavioural sciences. Unfortunately, many philosophers have got the basic picture of evolutionary psychology wrong, which leads to their extreme dismissal of the programme. Instead, I offer an alternative picture, a picture that better reflects daily practice, a picture I think many can endorse, as well as one that conveys the vibrancy and possibilities of this type of reasoning – and its legitimate and important place in the evolutionary and behavioural sciences.

Chapter 1

What is evolutionary psychology?

Evolutionary psychology is reasonable. Evolutionary psychology is scandalous. Why the polarisation? In §1.0, I briefly outline the polarisation. In §1.1, I identify the theoretical tenets commonly associated with evolutionary psychology. In §1.2, I contend that many of the tenets commonly associated with, or purported to be part of, the research programme are not strictly part of it. In §1.3, I streamline evolutionary psychology's tenets. In §1.4, I contend this streamlining better matches evolutionary psychology's daily practice. In §1.5, I contend this streamlining offers multiple benefits. I conclude by adopting this streamlined evolutionary psychology as the basis for the thesis.

1.0 Evolutionary psychology's underlying impulse

We have evolved by natural selection. Even if not everything about an organism is an adaptation for survival and reproduction, it seems inescapable that organisms are well adapted, at least to the environment in which they evolved. Most consent to that proposition. But there is disagreement, often quite animated disagreement, as to what implications this has for psychology.

Our ancestors faced a multitude of recurrent survival and reproduction problems. In order to survive and reproduce, one must locate and secure resources; avoid pathogens; avoid predators; find a mate; counter threats from mate poachers; form and maintain coalitions and relationships; establish and protect one's status in a societal hierarchy; and so on. These problems are known as 'adaptive problems', and solutions to these problems promote reproductive success.

One proposal, perhaps the dominant view, is that selection favoured one or a few general-purpose psychological mechanisms, general learning and decision-making mechanisms sufficient to generate multiple behavioural solutions to multiple adaptive problems. This, for example, is the view championed by Buller (2005). If this is the case, applying evolutionary perspectives to psychology might have limited exploratory mileage.

An alternative proposal is that some or many of the problems were solved by dedicated psychological adaptations. Just as we inherit physiological specialisations, so too we might inherit psychological specialisations. If this is the case, this opens up the possibility that evolutionary theory can open up new lines of research in psychology. This is the possibility evolutionary psychologists pursue.

Evolutionary psychology is a hypothesis-driven empirical science. The daily practice of evolutionary psychology is to focus on adaptive problems, hypothesize dedicated adaptive solutions, and then subject these hypotheses to testing. Hence, the heartbeat of evolutionary psychology is heuristic, one of discovery. All the way back in 1987 Cosmides and Tooby were writing that evolutionary theory can be used 'as a heuristic guide for the discovery of innate psychological mechanisms' (1987: 279), and later wrote that this is the 'essence' of the programme (Ermer, Cosmides, and Tooby 2007: 154). Symons proposed that adaptationist thinking in psychology 'can help guide research', 'inspire new questions', and 'call attention to aspects of human psychology that are normally too mundane or uniform to notice' (Symons 1989: 143). Barkow says 'Evolved mechanisms are useful

heuristic devices' (2006: 21). Buss frequently speaks of 'the heuristic value of evolutionary hypotheses' (Buss 2005a: 252) and that these heuristics are 'powerful' (Buss 2005b: 1). Daly and Wilson concur, also speaking of the 'heuristic assistance of Darwinian insights' (1996: 22) and that these heuristics are 'strong' (2005a: 408). On the strength of this methodological recommendation, many hundreds of psychology research papers have proposed and tested adaptationist hypotheses.

Quite reasonable, isn't it? If selection pressures have favoured physiological adaptations, then they might also favour psychological adaptations, and that this perspective might well be heuristically useful. In a way, pursuit of this line of thought is 'inevitable' (de Waal 2001). Of course, whether this kind of thinking actually is heuristically useful needs to be established, and we'll seek to do this in chapter 3.

Yet despite its apparent reasonableness, evolutionary psychology has been subject to a firestorm of criticism and controversy, often from philosophers of science. Dawkins observes that evolutionary psychology is 'subject to a level of implacable hostility, which seems far out of proportion to anything sober reason or even common politeness might sanction' (2005: 975).

Articles and books carry the titles 'Pop Sociobiology Reborn' (Kitcher and Vickers 2003), 'Perverse Engineering' (Haufe 2008), 'Getting Darwin Wrong: Why Evolutionary Psychology Won't Work' (Wallace 2010), and 'The Seven Sins of Evolutionary Psychology' (Panksepp and Panksepp 2000). If these titles are insufficient to convey the flavour of the sceptical viewpoint, one might cite the closing words of Richardson (2007: 183): evolutionary psychology is to be dismissed as 'idle Darwinizing'. Buller (2005: 481) dismisses evolutionary Psychology as being 'wrong in almost every detail'. And (R.) Hamilton (2008: 105) is on particularly dramatic form: 'evolutionary psychology is empirically unwarranted and conceptually incoherent to such an extent that it is a matter of professional sociological concern why it has come to achieve such a degree of popularity.' Sceptics paint a grim portrait of a research programme characterised by intellectual thinness, methodological inertia, and out-datedness. If one were writing a script for a bad science or a pseudoscience, all the elements would be in place.

Yet this is not the portrait that I recognise – nor one that those sympathetic to the programme and those who practice it recognise. There is, it seems, a mismatch between the underlying reasonableness of the programme on the one hand and the sceptics'

judgement of the programme on the other. Something's gone wrong – perhaps with communication, perhaps with formulation, perhaps with execution. Perhaps with all three.

1.1 How the research programme is theoretically packaged

To begin to make sense of how such a disconnect could obtain and crystallise in the literature, let's identify how the underling impulse of evolutionary psychology, its heartbeat, is cashed out into a programme of research – in other words, what its main theoretical and methodological tenets are commonly purported to be. I interpret the leading proponents of evolutionary psychology – Leda Cosmides, John Tooby, David Buss, and others – as presenting evolutionary psychology as a package of views:

(T1) Modern evolutionary synthesis, with the focus on psychological adaptations

(T2) Empirical adaptationism

(T3) Inference from empirical adaptationism to massive modularity

(T4) Methodological adaptationism

(T5) Metatheory for psychology

(T6) Public policy agenda

The distinction between empirical and methodological adaptationism is due to Godfrey-Smith (2001). Godfrey-Smith discusses these distinctions with respect to evolutionary biology but they're equally applicable to evolutionary psychology. Prominent evolutionary psychologists have no trouble subscribing to both empirical and methodological adaptationism and there's plenty of evidence they do.

We'll go through each of these tenets in order.

(T1) Modern evolutionary synthesis, with the focus on psychological adaptations

The modern evolutionary synthesis, the consensus framework of modern evolutionary biology, is well-established and well-known. Instead of reheating established details of the modern evolutionary synthesis, I'll identify the concepts most relevant for evolutionary psychology.

(i) Evolution by natural selection

Biological evolution is a change in the characteristics of a population over time. More precisely, a standard definition is that evolution occurs precisely when there is a change in the gene frequencies found in a population (Sober 2000). Populations evolve, not individuals – individuals survive and reproduce. Several processes drive evolutionary change: natural selection (heritable variation in fitness), genetic drift (gene frequencies can change due to chance alone), migration (gene flow), and mutation. In the classical view, within a population, selection and drift decrease genetic variation, while mutation and migration increase genetic variation (Culver 2009).

Evolutionary psychologists focus on natural selection. Evolution by natural selection is the process of preserving and increasing the frequency of fitness-enhancing characteristics or traits in a population. For selection to occur in a population, three conditions must obtain. First, variation: individuals in a population must differ with respect to the relevant trait in question. Without variation, all individuals will have the same trait value, and will thereby be indistinguishable with respect to that trait. Second, heritability: the variation found in the population must be heritable to some degree. Non-heritable traits, and the advantages they provide, cannot be passed on from one generation to the next. Third, differential fitness: individuals must have a probability of reproduction that is a function of the trait value in question. Although this classic formulation presents selection as acting on individual organisms, one can view selection as acting on genes, groups, or species.

(ii) Adaptations and by-products

Natural selection results in the evolution of adaptations, where selection increases the frequency of beneficial alleles in a population to the point of fixation. An adaptation can be defined as an inherited and reliably developing trait that was selected for because it helped solve an adaptive problem - a recurring problem of survival or reproduction (Buss 2008; see also Stearns 1986 and Tooby and Cosmides 1992). More precisely,

> 'Characteristic *c* is an adaptation for doing task *t* in a population if and only if members of the population now have *c* because, ancestrally, there was selection for having *c* and *c* conferred a fitness advantage because it performed task *t*.' (Sober 2000: 85)

This way of defining an adaptation - adaptation as an historical concept – represents a 'reasonable consensus on what it is to be an adaptation' (Godfrey-Smith 1998: 191). This is

also how the concept is commonly defined in evolutionary psychology. Accordingly, this thesis adopts that definition. For a discussion of other ways of defining adaptation, see Shanahan (2004).

Adaptations are to be distinguished from their by-products. A by-product is a trait that evolved not because it was selected for, but because it was connected to another trait that was selected for. By-products do not function to solve adaptive problems: the sound of the heart pumping blood and the white colour of bones are non-functional by-products of selection.

(iii) Adaptations advantageous on balance

To be selected for, a trait need not be advantageous under every scenario. Rather, all that is required is that the trait is advantageous overall. For example, a child's enlarged skull displays signs of being an adaptation, since a larger brain is correlated with greater cognitive power. But this makes birth complicated. And dangerous: there's a risk of childbirth death. Yet the enlarged skull is advantageous on balance, and so could have been selected for.

(iv) Adaptations and constraints

Natural selection optimizes under constraints. 'Adaptationist models do predict optimality, but this optimality is always *constrained* optimality.' (Sansom 2003: 497; emphasis original) Genetic and developmental constraints can prevent certain traits and designs being realizable. Co-ordination constraints with extant mechanisms can prevent the evolution of certain traits and designs. Prohibitive costs can also rule out the evolution of certain traits and designs. Furthermore, 'Local optima can prevent the evolution of better adaptive solutions that might, in principle, exist in potential design space.' (Buss *et al.* 1998: 538) In other words, the required fitness chain between one solution and a better solution might not obtain. Buss *et al.* (1998) invite us to think of selection as building adaptations through a relentless mountain-climbing process. On top of a neighbouring mountain a better design might be found 'but selection cannot reach it if it has to go through a deep fitness valley to get there' (*ibid.*: 538).

(v) Adaptation/adaptive distinction

We need to distinguish between a trait being an adaptation and a trait being adaptive.

> 'An *adaptation* is a character favoured by natural selection for its effectiveness in a particular role; that is, it has an evolutionary history of selection. To be labelled as *adaptive*, a character has to function currently to increase reproductive success.' (Laland and Brown 2002: 132; emphasis original)

Hence, 'To say that a trait is an adaptation is to make a claim about the cause of its presence; to say that it is adaptive is to comment on its consequences for survival and reproduction.' (Sober 1993: 211) It follows that a trait can be an adaptation without being adaptive, and a trait can be adaptive without being an adaptation.

The adaptation/adaptive distinction highlights the possibility of a mismatch between adaptations and novel environments. Evolutionary psychologists stress that our contemporary environment differs from our ancestral environment. When an organism's environment changes speedily and significantly, behaviours that were adaptive in the previous environment can become dysfunctional or deleterious in the new environment, undermining reproductive success. Some evolutionary psychologists call such behaviour 'maladaptive' (e.g. Tooby and Cosmides 2005). Sterelny (1995) adopts and illustrates the distinction between adaptive and maladaptive behaviours with the memorable example of hedgehogs responding to danger by rolling into balls: an effective response to dangers from its natural predators, but a poor response to the danger of cars. More on this in the next chapter.

(vi) From physiological adaptations to psychological adaptations: the symmetry principle

It's common to think of the outcomes of selection as being exclusively physiological. Many physiological traits - hearts, livers, lungs, and so on - are widely seen as having been shaped by natural selection for the survival and reproduction of their possessors. But why shouldn't the same be true of psychological traits? As Dawkins notes, the claim that psychology is on the same footing as the body where selection is concerned is 'exceedingly modest' (2005: 978).

A guiding principle of evolutionary psychology, what I call its symmetry principle, is that there is no reason to suppose that only physiological traits can be selected for. As with

physiology, so too with psychology: 'Just as a shared set of digestive mechanisms both enable and constrain the diverse diets of human populations, so do a comparable set of behavioral mechanisms enable and constrain our social-cultural behavior.' (Barkow 2006: 21-22)

Physiologists have divided the body up into different physical organs, based largely on the purported functions of organ. As with physiology, so too with psychology: We can do this with psychology too, with psychological 'organs'. As Cosmides and Tooby (1997a) put it,

> 'Our body is divided into organs, like the heart and the liver, for exactly this reason. Pumping blood through the body and detoxifying poisons are two very different problems. Consequently, your body has a different machine for solving each of them. The design of the heart is specialized for pumping blood; the design of the liver is specialized for detoxifying poisons. Your liver can't function as a pump, and your heart isn't any good at detoxifying poisons. For the same reason, our minds consist of a large number of circuits that are functionally specialized.'

Hence, we can hypothesize that a particular psychological trait exists in the form that it does because it solved a specific problem of survival or reproduction recurrent in ancestral populations. For example, we can hypothesize that fear of snakes is an adaptation, taking the form that it does because it solved a specific and recurring problem in ancestral populations, namely the threat of being poisoned by snake bites (Öhman and Mineka 2001). Since those who possessed this trait would enjoy a fitness advantage over those who didn't possess the trait, the trait would be selected for and become prevalent in ancestral populations.

Since our physiological adaptations are species-typical, appearing in normal developing members of the human species, our psychological adaptations are also held to be species-typical, though of course they can be sex- and age-specific (Buss 2008). And since physiological adaptations tend to be problem-specific (hearts, lungs, and livers, for example, function to solve specific problems), psychological adaptations are also held to be problem-specific - also known as 'domain-specific' (Goetz, Shackelford, and Platek 2009).

(vii) Psychological adaptations as functionally specialised mechanisms

Psychological adaptations, also known as 'evolved psychological mechanisms', are understood as 'complex structures that are functionally organised for processing

information' (Tooby and Cosmides 1992: 33). This reflects evolutionary psychologists' information processing view of psychology.

Buss (1995) provides what I judged to be the clearest definition of what a psychological adaptation is. According to Buss (1995: 5-6), a psychological adaptation is a set of processes that:

> '1. Exists in the form it does because it (or other mechanisms that reliably produce it) solved a specific problem of individual survival or reproduction recurrently over human evolutionary history.
>
> 2. Takes only certain classes of information or input, where input (a) can be either external or internal, (b) can be actively extracted from the environment or passively received from the environment, and (c) specifies to the organism the particular adaptive problem it is facing.
>
> 3. Transforms that information into output through a procedure (e.g., decision rule) in which output (a) regulates physiological activity, provides information to other psychological mechanisms, or produces manifest action and (b) solves a particular adaptive problem.'

So psychological adaptations solved specific problems in ancestral environments, are triggered by only a narrow range of information, and are characterized by a particular set of procedures or decision rules, and produce behavioural output that solved the adaptive problem in ancestral times. In other words, they are 'functionally specialised' or 'modular'. More on this in the next chapter.

Postulating psychological adaptations as information processing mechanisms should not be taken to mean that the outputs of these mechanisms are bereft of emotion. Quite the contrary: evolutionary psychologists stress that such output is often experienced as emotional states (Cosmides and Tooby 2000). Indeed, emotional states are clearly useful in helping solve adaptive problems: to prioritise certain behaviours; to shift attention to potential threats; to be attracted to potential mates; and so on.

As Griffiths (2007: 407) points out, when evolutionary psychologists 'present experimental evidence of domain specificity in cognition, it is generally evidence suggesting that information about one class of stimuli is processed differently from information about another class of stimuli - that is, evidence of the use of different proprietary algorithms in

the two domains'. So, to cite an example that we will look at in chapter 4, Cosmides and Tooby interpret various Wason selection tasks as demonstrating that certain ways of describing a task can activate a domain-specific mechanism for detecting cheaters.

(viii) Unconscious

We're often unconscious of these information processing mechanisms. For example, when people pursue short-term sexual encounters, they're not consciously calculating levels of heritable fitness and the extent to which this might translate into fitness advantages for potential offspring. Rather, they experience sexual attraction and decide to act. The significance of this point is often underappreciated, a point we'll discuss in the final chapter.

(ix) Hypotheses and explanations

Finally, we need to distinguish between psychological adaptation hypotheses and psychological adaptation explanations. A psychological adaptation hypothesis can be defined as a conditional statement: if a psychological trait is an adaptation then certain things should follow – there should be a certain state of affairs in the world. Postulating a psychological adaptation hypothesis need not commit us to the stronger claim that the psychological trait in question is an adaptation. In contrast, a psychological adaptation explanation does make this stronger claim: a psychological adaptation explanation is a statement that a psychological trait is an adaptation, together with a set of statements giving information or details about the relevant ancestral populations and ancestral environments (such as the trait variation present in ancestral populations and information on the demographic structures of ancestral populations).

In chapter 3, we'll examine how evolutionary psychologists generate adaptationist hypotheses. Evolutionary psychologists often make the further step, formulating their hypothesis-driven research as explanations. Whether these explanations are complete explanations, and if not, what implications this has, will be discussed in chapter 6.

(T2) Empirical adaptationism

Godfrey-Smith (2001: 336) defines empirical adaptationism as follows:

> 'Empirical Adaptationism: Natural selection is a powerful and ubiquitous force, and there are few constraints on the biological variation that fuels it. To a large degree, it is possible to predict and explain the outcome of

> evolutionary processes by attending only to the role played by selection. No other evolutionary factor has this degree of causal importance.'

This is the usual understanding, probably the default understanding, of the term 'adaptationism'.

Prominent evolutionary psychologists frequently consent to empirical adaptationism. For example, Tooby and Cosmides (1992: 52) claim,

> 'To the extent that a feature has a significant effect on reproduction, selection will act on it. For this reason, important and consequential aspects of organism architectures are shaped by selection. By the same token, those modifications that are so minor that their consequences are negligible on reproduction are invisible to selection and, therefore, are not organized by it. Thus, chance properties drift through the standard designs of species in a random way, unable to account for complex organized design and, correspondingly, are usually peripheralized into those aspects that do not make a significant impact on the functional operation of the system'

It's useful to distinguish between the power of selection and the ubiquity of selection. Sober characterizes (empirical) adaptationism in terms of various hypotheses. Given a population *X* and a trait *T*, Sober (2000: 124) distinguishes between the following hypotheses:

> '(U) Natural selection played some role in the evolution of *T* in the lineage leading to *X*.
>
> (I) Natural selection was an important cause of the evolution of *T* in the lineage leading to *X*.
>
> (O) Natural selection was the only important cause of the evolution of *T* in the lineage leading to *X*.'

These hypotheses are ranked according to their strength: (O) is the strongest hypothesis and implies (I) and (U); (I) is weaker than (O) and implies (U), and (U) is the weakest. By generalising (O), Sober (2000: 124) arrives at his formulation of (empirical) adaptationism as follows:

> '[Empirical] Adaptationism: Most phenotypic traits in most populations can be explained by a model in which selection is described and nonselective processes are ignored.'

This definition of (empirical) adaptationism holds that selection is the most important cause of most traits in most populations. Empirical adaptationism can weakened by claiming, for example that selection is the most important cause of *some* traits in most populations – or it can be made stronger by claiming that it's the most important cause of *all* traits in most populations.

These different versions of empirical adaptationism can be tested, in the long-run, by proposing and testing various selectionist hypotheses. Empirical adaptationism's confirmation increases as more adaptationist hypotheses are confirmed, and it weakens as adaptationist hypotheses fail. Empirical adaptationism is vindicated 'if in the majority of cases a better fit to the data is achieved by a selection-based model than is achieved by any other model of comparable complexity' (Godfrey-Smith 2001: 344-345)

(T3) Inference from empirical adaptationism to massive modularity

We can distinguish between two senses of massive modularity: a moderate form, which claims that the number of psychological adaptations is significantly greater than has been traditionally believed, and the stronger form, which claims that there are no domain-general mechanisms ('domain-general' means the mechanism is not dedicated to a particular domain or problem). Note, the moderate form of massive modularity is not claiming that there are domain-general mechanisms - it's simply not ruling them out.

Cosmides and Tooby attempt to infer the stronger form of massive modularity from empirical adaptationism - that if natural selection is true then the only outcome is a massively modular mind in the strong sense. A number of arguments have been exchanged in the literature, both in favour of this inference and against it.

The consensus seems to be that 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 - even if we assume empirical adaptationism. 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' (Lewens

2007:153). Although I disagree with the characterisation of specialised psychological adaptations as being 'rigid' – they are anything but, as we'll see in the next chapter – the general point is taken.

(T4) Methodological adaptationism

Another type of adaptationism Godfrey-Smith (2001: 337) recognises is methodological adaptationism.

> 'Methodological Adaptationism: The best way for scientists to approach biological systems is to look for features of adaptation and good design. Adaptation is a good "organizing concept" for evolutionary research.'

Methodological adaptationism is a recommendation for how to go about investigating nature – specifically, to propose, pursue and test adaptationist hypotheses.

Notice Godfrey-Smith's definition slides together two distinguishable versions - the 'best way' of doing research and a 'good' way of doing research. Contextualised into psychology, they are:

> Strong methodological psychological adaptationism: the best way to approach psychological traits is to look for features of adaptation and good design. Adaptation is the best organising concept for psychological research.
>
> Moderate methodological psychological adaptationism: a useful or good way to approach psychological traits is to look for features of adaptation and good design.

Note, the moderate form is not trivial. It is not at all a given – it needs to be justified. And as we'll see later, even the moderate form has been challenged.

The main exponents of evolutionary psychology typically advocate the stronger form of methodological adaptationism. For example, Buss (1995: 6) holds that 'a central premise of evolutionary psychology is that the main nonarbitrary way to identify, describe, and understand psychological mechanisms is to articulate their functions - the specific adaptive problems they were designed to solve'. The main non-arbitrary way of individuating parts of the body is by reference to their evolved function – the heart is distinct from the liver, because the heart functions to pump blood, whereas the liver functions to detoxify.

Likewise, Buss claims, evolutionary psychology provides the main non-arbitrary means of individuating psychological processes, by reference to their evolved function. Cosmides and Tooby (1997a: 14) also make this point, writing that 'knowledge of adaptive function is necessary for carving nature at the joints'.

(T5) Metatheory for psychology

Prominent evolutionary psychologists frequently advocate that evolutionary psychology should (i) reform psychology and (ii) unify psychology.

(i) Scepticism about existing non-evolutionary work in psychology

Buss (1995) characterises psychology as being in conceptual disarray, with each division anxiously working on its own mini-theories, isolated from, and untouched by, empirical findings. Daly and Wilson (1999: 510) claim that non-evolutionary theories in psychology 'have risen and fallen more like a succession of fashions than like the building blocks of a cumulative science' and that 'mainstream social psychology has gone in circles, such that work in the 1990s is in no clear sense an advance over that in the 1950s'. Prominent evolutionary psychologists hold that non-evolutionary work in psychology needs to be reformed: at minimum, it needs to be made consistent with evolutionary psychology.

(ii) Unifying metatheory

But evolutionary psychology can do more than just reform psychology - or so it's claimed. As we just saw, proponents hold that evolutionary psychology provides the main non-arbitrary way of carving up the mind, providing the true psychology taxonomy. This perhaps fuels their claim that evolutionary psychology could be a metatheory for psychology – and that it probably will be. 'EP will emerge as *the* metatheory for psychological science', claim Goetz, Shackelford, and Platek (2009: 15, emphasis original).

Duntley and Buss (2008) are especially dramatic on this point. In the subsection 'What is Evolutionary Psychology?', they criticize a target article's summary of evolutionary psychology as failing to mention evolutionary psychology as a metatheory of psychology. Don't miss the final sentence:

> 'We believe that the Ploeger et al. article misses one of the most important accomplishments of evolutionary psychology—conceptual integration (Tooby & Cosmides, 1992). Evolutionary psychology provides a unifying metatheory for the currently disparate and disconnected

> branches of psychology (Buss, 1995). Evolutionary psychology unites the field of psychology with all the other life sciences, including biology, economics, political science, history, political science [sic], legal scholarship, and medicine; it unites humans with all other species, revealing our place in the grand scheme of the natural world.' (Duntley and Buss 2008: 31)

Claiming that evolutionary psychology can unify not only psychology but the behavioural sciences in their entirety isn't an isolated burst of enthusiasm: 'Evolutionary psychology is an attempt to unify the psychological, social, and behavioral sciences theoretically and empirically within a single, mutually consistent, seamless scientific framework.' (Tooby and Cosmides 2008: 114)

(T6) Public policy agenda

Prominent evolutionary psychologists also encourage evolutionary psychologists to consider, articulate, and advocate public policy implications, if any, of their research. In the context of law, Cosmides and Tooby (2006: 182) frame the claim as follows: 'Which programs reliably develop in the human mind, and how do they process information? Evolutionary psychology seeks to answer this question. Accurate answers, when they are eventually arrived at, will have implications for lawmaking.'

In 1996, the policy think-tank *Demos* dedicated a quarterly issue to work connecting evolutionary psychology to public policy. Thornhill and Palmer (2000) dedicated several chapters looking at possible implications of their research on rape policy. In 2004, Charles Crawford and Catherine Salmon edited a volume on evolutionary psychology and public policy, collected papers on evolutionary psychology's purported applications to policy issues such as women in the work place, marital support, child support, cultivating morality (Crawford and Salmon 2004). More recently, Swami and Salem (2011) claim that evolutionary psychologists working on attractiveness have not paid much attention to practical applications of their research. Swami and Salem argue this is a missed opportunity: evolutionary psychologists 'have a role to play in challenging the notion of ideal beauty and showing how and when beauty ideals can be challenged and changed' (*ibid*.: 163).

Before we move on, for the sake of completeness, it should also be pointed out that Godfrey-Smith (2001: 336) also recognises a third type of adaptationism - explanatory adaptationism – and defines it as follows:

> 'Explanatory Adaptationism: The apparent design of organisms, and the relations of adaptedness between organisms and their environments, are the big questions, the amazing facts in biology. Explaining these phenomena is the core intellectual mission of evolutionary theory. Natural selection is the key to solving these problems -- selection is the big answer. Because it answers the biggest questions, selection has unique explanatory importance among evolutionary factors.'

In the context of psychology, the claim appears to be (1) the apparent complex design of some psychological traits is the most important fact in psychology, (2) only selection explains complex design, therefore (3) adaptationist explanations in psychology are uniquely important.

Prominent evolutionary psychologists certainly subscribe to (2). For example, Buss (1995: 2) holds that 'evolution by natural selection is the only known causal process capable of producing complex physiological and psychological mechanisms'. Nevertheless, it's difficult to ascertain whether prominent evolutionary psychologists subscribe to either (1) or (3). Perhaps they do, but I haven't seen the claim explicitly made in the literature. Not much hinges on this anyway (explanatory adaptationism is logically independent of empirical adaptationism and methodological adaptationism). Explanatory adaptationism is a value judgement. It's a claim that some explanations are more privileged, more important, than others. I agree with Godfrey-Smith that it's difficult to assess this claim, for it is hard to see what makes some set of facts or explanations more interesting, or more important, than another. An evolutionary psychologist could argue for massive modularity, could claim that treating certain traits as adaptations is the best way to organise psychology research, could claim that psychology needs to be reformed in order to be consistent with evolutionary psychology, could claim that evolutionary psychology provides the framework for integrating diverse (but consistent) viewpoints in psychology, but then decline to privilege, to hold as more important, selection explanations over non-selection explanations.

1.2 The need to streamline evolutionary psychology's purported tenets

Evolutionary psychology as a research programme should not be identified with theoretical tenets surplus to requirement. It should not be identified with theoretical tenets that have no bearing on its daily practice. The question animating the remainder of the chapter is this: Does a commitment to the search for psychological adaptations require so many theoretical tenets?

I have a number of worries concerning such heavy presentations of the research programme. Of course, some might object to the inclusion of one or two theoretical tenets above. Perhaps public policy pursuits are a secondary goal, not a major tenet. Perhaps arguing for massive modularity *a priori* is a niche pursuit. Perhaps. Nevertheless, these positions have been seriously and frequently advocated by prominent evolutionary psychologists, and, as we'll see in the final chapter, such claims are associated with evolutionary psychology, at least in the sceptic's mind.

My concern does not depend on the particular formulation I tabled above. One can formulate evolutionary psychology's purported tenets in alternative ways (for example, Cosmides tends to stress that the brain functions as a computer, while Bolhuis *et al.* (2011) focuses on evolutionary psychology's purported gradualism, the idea that evolutionary change is slow and gradual), and add further theoretical tenets, or less theoretical tenets. And that's precisely my point: what tenets are properly part of the research programme, the activity of proposing and testing adaptationist hypotheses in psychology, and what tenets have been associated with the programme, but which are not strictly part of it in virtue of being independent of the programme's stated goal of proposing and testing adaptationist hypotheses, is rarely addressed.

My contention is that evolutionary psychology is often excessively packaged. In introductory paragraphs and articles, theoretical manifestos, and introductions to edited volumes, evolutionary psychology is often presented as something much more than a research programme in the evolutionary behavioural sciences going about its daily business. It's presented as something of a game-changer. Something revolutionary. Something with paradigmatic trappings. Silverman and Choi (2005: 177) speak of 'the prevailing paradigm of evolutionary psychology'. Malamuth, Huppin, and Paul (2005: 394) speak of 'applying the EP paradigm'. We are assured that evolutionary psychology is 'becoming the unifying paradigm upon which the entire field of psychology can be based'

(Fitzgerald and Whitaker 2010: 284). 'Evolutionary psychology represents a true scientific revolution, a profound paradigm shift in the field of psychology.' (Buss 2005c: xxiv; see also Buss 1995)

Of course, such proponents are using the term 'paradigm' loosely. Just as I'm not trying to draw Lakatos into our discussion by referring to evolutionary psychology as a 'research programme', so too I'm sure that the proponents cited above are not aiming to recruit Kuhn by calling evolutionary psychology a 'paradigm'. Nevertheless, it's clear that in using the term, they're trying to suggest that evolutionary psychology can be much more than operationalizing a set of ideas heuristically, but that one can take those same ideas much further, combining them with other tenets and agendas, presenting this larger package of views as a 'scientific revolution' in psychology and the social sciences – as an all-encompassing view of science and, indeed, the world.

The more ambitious and exclusive one wants to make a research programme, the more exposed one becomes, the greater the danger of ending up arguing for highly controversial theoretical tenets, tenets far removed from the daily activities of the programme. The risk is that one can alienate potential allies in other research programmes and fields, perhaps making them unresponsive or unsympathetic to one's work, making them sceptical of one's work – even hostile.

If evolutionary psychology is packaged excessively, then there is a real risk that its underlying reasonableness and its heuristic possibilities will be overshadowed. Naturally, when approaching a research programme, one will first consult the foundational textbooks and foundational edited volumes. If these present and argue for tenets in excess of the practice of the programme, one can get the wrong picture, the wrong end of the stick. And then such a person might be inclined to dismiss the programme wholesale – lock, stock, and barrel. Indeed, their distaste might overstep the bounds of ostensible objections.

As shall become clear in the last two chapters, I suspect this is what has happened, and this sheds some light on why the debate has become so polarised and heated. For now we can just acknowledge that excessive packaging risks serious misunderstanding, whether the risk is realised or not.

One might think this is a risk worth taking. One might think not taking this risk is tantamount to endorsing an oily togetherness, ecumenicalism for the sake of ecumenicalism. But as I shall indicate later, again in the last two chapters, this is a risk not

worth taking, since to reach its full heuristic potential, evolutionary psychology needs to canvass a wider constituency.

For now, my goal is simple: to identify only those theoretical tenets fit for purpose. Or, to put it another way, to distinguish between core tenets and accidental tenets.

1.3 Streamlining evolutionary psychology's purported tenets

What theoretical tenets must we postulate in order to account for and justify evolutionary psychology's daily practice? Put another way, how many of the theoretical tenets associated with evolutionary psychology in the previous subsection can we strip away? Quite a few. In order to propose and test adaptationist hypotheses in psychology, you don't need to adopt a belief in massive modularity or argue for massive modularity *a priori*. You don't need to consent to empirical adaptationism. You don't need to investigate public policy implications of your research. You don't need to believe that non-evolutionary work in psychology is rubbish or that adaptationism is the best way to investigate psychological traits or that psychology needs to be unified. To pursue and test adaptationist hypotheses in psychology one requires only two theoretical tenets.

> (EP1) Modern Evolutionary Synthesis, with the focus on psychological adaptations.
>
> (EP2) A moderate form of methodological adaptationism.

It should be immediately obvious that to formulate and test adaptationist hypotheses in psychology, one does not need to adopt any view on the quality of non-evolutionary work in psychology, nor to adopt the view that psychology needs to be unified, much less that evolutionary psychology will be the metatheory for such unification. Ditto public policy pursuits. Ditto for arguing for massive modularity *a priori*. Hence, I take it as incontrovertible that we can drop (T6), (T5), and (T3).

Furthermore, (T4), methodological adaptationism, only needs to be adopted in its moderate form: namely, the claim that it's useful to hypothesize (functionally complex) psychological traits as adaptations. We only need to establish that this is a fruitful way of doing psychology. We shall do this in chapter 3. We do not need to establish that this is the only way of doing psychology or the best way of doing psychology.

Less obvious is my claim that (T2), empirical adaptationism, can be dropped. Empirical adaptationism and methodological adaptationism often travel together. But this is merely an accident of this world: as Godfrey-Smith (2001) observed, these two types of adaptationism are logically independent of one another.

It's easier to see that one can consent to empirical adaptationism but not methodological adaptationism. One can believe in the power and ubiquity of selection and yet believe it's not useful to treat traits as adaptations, at least not in the manner advocated by evolutionary psychologists. One might hold that the particular methods of evolutionary psychology are useless, or unreliable, or unscientific, or illegitimate. Perhaps even dangerous. Moderate methodological adaptationism is not a given. It can be contested. And it has been.

More difficult to digest is declining to consent to empirical adaptationism but consenting to methodological adaptationism. If one is agnostic about empirical adaptationism or rejects empirical adaptationism, why believe that investigating adaptationist hypotheses can be useful? If we live in a world where selection is limited, how can treating functionally complex traits as adaptations be useful? I suggest four scenarios where one can decline to endorse empirical adaptationism yet consent to methodological adaptationism.

First, if one is agnostic about empirical adaptationism, then one could hold that methodological adaptationism is a useful way of finding out the truth of empirical adaptationism. If there are psychological adaptations, pursuing and testing adaptationist hypotheses is a useful way of finding this out. Second, even if one rejects empirical adaptationism, one could still acknowledge that it can lead to new discoveries, and can stimulate new lines of research, even if ultimately these discoveries are explained by non-selectionist factors. Third, even if one rejects empirical adaptationism, investigating adaptationist hypotheses first might be a good starting point. Godfrey-Smith (2001: 342) cites an observation by Kim Sterelny that 'methodological adaptationism might be particularly useful if non-selective factors like developmental and genetic constraints are elusive and hard to discover'. When adopting optimality as a kind of null hypothesis, deviation 'provides evidence that other factors are at work, and perhaps the nature of the deviation will give clues about where to look next.' (Godfrey-Smith 2001: 342) Fourth, an admittedly unlikely scenario but nevertheless a possible scenario, one could reject empirical adaptationism but hold that methodological adaptationism is a useful way of

establishing the falsity of empirical adaptationism. The continual failure of adaptationist hypotheses in the long-run would discredit empirical adaptationism.

1.4 Evolutionary psychology on the ground

We shouldn't confuse the daily practice of the programme with the ambitions of those at the summit of the programme. I cannot look into the hearts of individual evolutionary psychologists, but I see little evidence that rank-and-file practitioners are beholden to evolutionary psychology as a paradigm. At the very least, many of the theoretical tenets in section 1.1 are completely absent in the research papers of evolutionary psychologists. Such research expresses not an all-encompassing view of science and the world, but a heuristic programme, a programme of discovery.

Furthermore, there's evidence that practitioners behave more as members of one research programme among several in the evolutionary behavioural sciences than as members of an all-encompassing paradigm.

Here, I draw upon recent research by Machery and Cohen (2012). Machery and Cohen (2012) are unique in the philosophy literature on evolutionary psychology in that they used quantitative citation analysis to judge four commonly held hypotheses about the evolutionary behavioural sciences. One of the hypotheses Machery and Cohen consider is whether the evolutionary behavioural sciences divide into several distinct competing paradigms. Machery and Cohen (2012: 184) distinguish between 'research traditions' and 'paradigms' as follows:

> 'First, research traditions within a given scientific field (e.g. psychology of vision) share various commitments— they might agree on what the explananda are, they might share some methodological principles, or they might concur on what the characteristics of successful explanations are—while paradigms have little in common. Second, two hypotheses formulated within two distinct research traditions might both be correct, while the hypotheses formulated within two distinct paradigms tend to be incompatible.'

If the hypothesis that evolutionary psychology and human behavioural ecology are competing paradigms with little in common is true, then evolutionary psychology and

human behavioural ecology papers should display noticeably different citation patterns. Machery and Cohen performed a quantitative citation analysis on articles published in *Evolution & Human Behavior* in the period January 2000 to December 2002. Although, as one would expect, the citation patterns were not identical, nevertheless there were striking similarities: 'evolutionary psychologists and human behavioral scientists appeal to evolutionary biology and non-evolutionary behavioral sciences in the same proportion—that is, they are influenced by these two fields to the same extent' (2012: 216). Because these similarities are greater than one would expect of competing paradigms, Machery and Cohen concluded that evolutionary psychology and human behavioural ecology form two distinct research traditions, not two competing paradigms.

Other evidence also suggests that evolutionary psychology is practised as one of multiple research programmes or traditions within the evolutionary behavioural sciences, rather than practiced as a rival paradigm for doing evolutionary behavioural science. For example, researchers from different traditions and programmes in the evolutionary behavioural sciences attend common meetings and conferences, such as the meetings of the Human Behavior and Evolution Society (Machery and Barrett 2006; Brown *et al.* 2011; Machery and Cohen 2012). Furthermore, recent edited volumes of evolutionary psychology work include contributions from behavioural ecologists (e.g., Barkow 2006 and Swami 2011).

So what we've done is distinguish between evolutionary psychology as a research programme, and evolutionary psychology as a pseudo-paradigm. If one is uncomfortable with those labels, alternative labels are available. For example, we can apply Godfrey-Smith's distinction between science and philosophy of nature. Whereas science is a set of ideas and tools for investigating the world, a philosophy of nature is 'taking science as developed by scientists, and working out what its real message is, especially for larger questions about our place in nature' (Godfrey-Smith 2009: 3). Prominent evolutionary psychologists clearly go beyond the science and make frequent philosophy of nature pronouncements. Recall the Duntley and Buss (2008) quote in section 1.1, where Duntley and Buss not only claim that evolutionary psychology unites the social sciences, but it also 'unites humans with all other species, revealing our place in the grand scheme of the natural world' (2008: 31). Yet, to adopt the science of evolutionary psychology, to use its ideas and methods to investigate the world, we don't need to adopt a philosophy of nature – and if we do, we're free to adopt an alternative philosophy of nature to that subscribed by prominent evolutionary psychologists.

Further alternative labels are available: one can distinguish between a bottom-up evolutionary psychology and a top-down evolutionary psychology, between a streamlined evolutionary psychology and an inflated evolutionary psychology, between a modest evolutionary psychology and a vaulting ambition evolutionary psychology ('vaulting ambition' a phrase from Kitcher 1985). Pick whichever label you prefer.

1.5 The benefits of streamlining

Streamlining the presentation of evolutionary psychology's tenets to better reflect this reality offers multiple rewards.

First, it finally shuts the lid on a range of objections that keep springing up like a jack-in-the box, objections that fail to target, let alone undermine, the core of the programme, being based on periphery tenets, or overly dramatic interpretations of those tenets. For example, evolutionary psychology is frequently derided as being hyper-adaptationist (Gould 2000), as believing that every nook and cranny of the mind is caught within the orbit of selection. Nonsense. Investigating a range of psychological traits to see if any have been selected for does not mean one believes every psychological trait is an adaptation. A streamlined evolutionary psychology is not committed *a priori* to a viewpoint on the number of psychological adaptations that exist. It's entirely an empirical matter. It might be fewer than prominent evolutionary psychologists believe, or as many as they believe, or even more – from just a handful of functionally specialised psychological adaptations, to less than a hundred, to 'hundreds or thousands' (Tooby and Cosmides 1995: xii), to even tens of thousands. Or, indeed, none.

Second, a bottom-up presentation reduces the space for misunderstandings to arise, for misunderstandings to be crystallised and recycled, and so lessen the need for corrections, and continual corrections, to be made. Those who present evolutionary psychology top-down tend to make a number of global claims. They claim that our 'modern skulls house a stone age mind', and make claims about 'human nature' and 'the psychic unity of humankind' (Cosmides and Tooby 1997a). Such global claims are easily misunderstood, and have been commonly misunderstood to mean we are behaviourally inflexible, behaviourally invariable, and behaviourally poorly adapted to today's environment (more on this in the next chapter). However, when we present evolutionary psychology as a bottom-up enterprise, we're less inclined to make global pronouncements, less inclined to

make philosophy of nature statements, and so less likely to be misunderstood. A bottom-up evolutionary psychology simply proposes and tests a range of adaptationist hypotheses. Such research can also be presented as explanations. But the evolutionary psychologist need not go beyond this. If she wishes to make claims about human nature and the mind generally, she can do so, in a separate capacity, wearing a different hat to the one she wears proposing and testing individual adaptationist hypotheses.

Third, a bottom-up presentation of evolutionary psychology focuses exclusively on daily practice, not drawing attention away from daily practice, and so better serves the programme. With the clutter stripped away, the basic picture and the possibilities the programme should be clearer and easier to grasp.

Fourth, streamlining removes many barriers and obstacles to entry. It becomes much clearer that pursuing adaptationist hypotheses in psychology need not commit us to particular philosophies of nature. The use of the tool is consistent with a variety of views, whether one emphasises selection or whether one emphasises drift, whether one believes in free will or not, whether one believes the sciences should be unified or not.

Fifth, streamlining the presentation of evolutionary psychology reduces friction with other programmes or traditions and accordingly allows us to better grasp their underlying compatibility. Much of the diversity in the evolutionary behavioural sciences can be accounted for as differences in focus and differences in emphasis. As Machery and Cohen (2009: 184) note, 'hypotheses developed in each tradition are often compatible in that they focus on different phenomena or on different aspects of the same phenomena.' For example, while human behavioural ecology focuses on the reproductive consequences of behaviour, evolutionary psychology focuses on the mechanisms that generate behaviour. Whereas a strongly packaged evolutionary psychology can, and has, expressed scepticism about 'counting babies', stressing that the advent of reliable contraception in modern societies problematizes such approaches, a streamlined evolutionary psychology doesn't comment on other research programmes.

Our different temperaments, sensitivities, schooling, skills, and disciplinary pedigrees will naturally incline us towards one tradition or programme over others, but just because we elect to work within one tradition or programme does not mean we must believe that all researchers should work in the same programme. In absence of a unifying framework, skirmishes and flashpoints between traditions are possible, and indeed have taken place,

but we need not overplay this. We need not declare for one programme against another. We can be greedy – we can try to eat the entire cake. Indeed, in order to satisfy our hunger for complete explanations, we'll likely need to eat the entire cake.

The literature is increasingly recognising the underlying compatibility and possibilities for convergence. Recognising the diversity of approaches in the evolutionary behavioural sciences, Laland and Brown (2002: 296) observe,

> 'Irrespective of the methodological differences among the practitioners, there is little that is conflicting or incompatible about these findings. In fact, each investigation reinforces the others, collectively building up a panoramic view of the topic at hand that spans genetic to sociocultural levels of analysis and transects distant continents. Here is an advertisement for pluralism in evolutionary perspective. There is no reason for researchers to restrict themselves to a single research technique when, by and large, the different methodologies are highly complementary.'

Indeed, integrated studies, studies that combine the conceptual and methodological resources from two or more traditions, are becoming an important feature of the contemporary literature (Sear, Lawson, and Dickins 2007).

Two final clarifications. First, I am not claiming that the expunged or divorced tenets are not worth discussing. They absolutely are. But they are independent of evolutionary psychology practice. Second, I am not claiming that the divorced tenets are false. I have no quarrel with empirical adaptationism. And though I see no pressing need for unification of psychology, I have no quarrel with those who advocate unification. If individual evolutionary psychologists want to advocate these positions, fine – but doing so should be sharply distinguished from the research programme, not casually or blindly mixed in with the research programme. Proposing and testing hypotheses and pursuing, say, a reforming agenda in the social sciences are independent projects. If this is not clearly made, if they're packaged together, and packaged with other contentious tenets, all under one banner, this threatens to overshadow the research programme. Keep different agendas separate.

An analogy will be instructive, one that has some personal resonance of late. The first draft of an essay or thesis will contain a multitude of points. Often, some of these will obscure the really valuable points. The essay or thesis, therefore, requires editing; it requires

streamlining, so that its proper focus is brought fully to light. Sometimes this will require the removal of interesting material, but such must be done. And so it is with evolutionary psychology's purported tenets. The trimming is overdue.

1.6 Conclusion

We've covered a lot of ground in this chapter, made a number of twists and turns, so it's time to pause - to reflect on, and conclude, the discussion so far.

The thought, motive, and practice of evolutionary psychology is that ancestral populations faced adaptive problems, that some or many of these problems were solved by specialised psychological adaptations, and that by focusing on adaptive problems and solutions we can make new discoveries in psychology. This is a programme primarily of discovery, and represents one of many possible programmes in the evolutionary behavioural sciences.

However, evolutionary psychologists at the summit of the programme, those whose views are widely disseminated and known to people outside the programme, frequently adopt and advocate tenets well beyond what's minimally needed. In itself this need not be a problem. The trouble arises when science is mixed with philosophy of nature without clear distinction, when the ideas and methods for investigating possible psychological adaptations are mixed with more ambitious, contestable tenets, when the research programme is presented with pseudo-paradigmatic trappings.

This carries substantial risks. The more unnecessary posturing, the more unnecessary controversy, the more the heart of the programme is obscured. If the root and branches are not clearly distinguished, then a sceptic might be liable to dismiss the programme root and branch, rather than just pruning back certain branches.

Streamlining evolutionary psychology's tenets, therefore, has much to offer. The effect is to focus more on daily practice, rather than be drawn away from daily practice. Potential objections reduce. Potential misunderstandings greatly reduce. Evolutionary psychology becomes open to a wider audience. And it becomes recognised for what it is - a member of a wider constituency.

The task now before us is to establish and vindicate this streamlined evolutionary psychology. We begin by clarifying some theoretical issues, issues having methodological

implications. We then move to the heart of the programme – the generation and testing of adaptationist hypotheses in psychology.

Chapter 2

Theoretical clarifications

The sceptical literature frequently mischaracterises evolutionary psychology, and some misconceptions are sufficiently common, and sufficiently important, to be worth correcting. In §2.1, I briefly look at the levels of selection debate and what impact, if any, this should have on evolutionary psychology. In §2.2, I identify the concept of the EEA, distinguish between the possibility of environmental mismatch and the ubiquity of environmental mismatch, and discuss whether the possibility of post-Pleistocene selection destabilises evolutionary psychology. In §2.3, I establish why psychological adaptations are often contextually flexible, and in §2.4 I set out why developmental robustness is consistent with developmental complexity and interactionism. Finally, in §2.5, I clarify what evolutionary psychologists mean when they employ the term 'module'.

2.0 Misconceptions

Sometimes a quotation really does say it all.

> 'We would agree with those who think that the extension of adaptationist thinking into the sociobiology of human behaviour is dubious. As we have discussed, there is great flexibility in behavioural development, humans live successfully in many different social structures, and our capacity for cultural learning is large.' (Gluckman and Hanson 2008: 198)

This passage appears near the end of the book *Mismatch*. The central contention of the book, written by two medical scientists, is that our bodies are mismatched with today's environment, and that we are witnessing the impact of this mismatch in the flare-up of diabetes, heart disease and obesity. It's a wonderful book. I imagine that evolutionary psychologists would consent to, or at least be sympathetic with, many of the authors' ideas.

The passage above codifies a number of well entrenched misconceptions. No evolutionary psychologist would deny that there is great flexibility in behavioural development, that humans live successfully in many different social structures, and that our capacity for cultural learning is large. At first I thought perhaps the authors are referring to sociobiology, but a footnote appended to the passage refers to an article sceptical of evolutionary psychology. In common with many, the authors dismiss evolutionary psychology as being crude. But it's only their understanding of evolutionary psychology that is crude. That professionals in fields adjacent to evolutionary psychology could misconstrue evolutionary psychology is depressingly unsurprising: the literature on evolutionary psychology is riddled with misconceptions. Many who have written on evolutionary psychology demonstrate a troubling failure to understand what it is about, to understand its basic picture. Misconceptions inevitably flow from getting the basic picture wrong. As we progress through the thesis, we'll seek to establish the right way of looking at evolutionary psychology. We'll start by clarifying some important theoretical issues.

2.1 Levels of selection

The levels of selection question – the question of whether natural selection operates on individuals, genes, groups, or some other unit – has been, and remains, a lively topic of debate in the literature. Various positions on this question have been championed in the literature. Individual selection was championed by Darwin (1859); kin selection, a term coined by Maynard Smith (1964), was established by Hamilton (1964) and Williams (1966), strengthened by Price (1970), and popularised by Dawkins (1976); classic group selection was proposed by Wynne-Edwards (1962), though this thinking can also be found in Darwin's works; and neo-group selection was established by Sober and Wilson (1999).

The level of selection question has typically been discussed with reference to the problem of altruism. Biological altruism is defined as behaviour that benefits someone else at a cost to oneself (where costs and benefits are calculated in terms of reproductive fitness). Biological altruism is widespread in nature. Classic examples include ant workers foregoing personal reproduction to help the queen reproduce; lionesses nursing cubs that are not their own; ground squirrels warning each other of the approach of enemies (Trivers 1985); when attacked by wolves, musk oxen 'wagontrail', forming a circle while the females and young shelter in the circle's interior (Sober 2000); and bees disembowel themselves when they sting intruders to the nest.

Darwin recognised that altruism poses a problem for the theory of evolution by natural selection. He was troubled by the phenomenon of sterile workers in insect colonies, which devote their lives to helping a queen reproduce at the expense of having offspring themselves. If evolution by natural selection is understood to favour individuals who increase their own reproductive success relative to other members of the same species, then it's something of a puzzle how altruistic traits, traits that aid the reproductive success of other members of the same species at the reproductive cost of the altruistic individual concerned, could have been preserved by natural selection. Indeed, on this basis, it would seem that altruistic traits would be selected against.

Darwin tentatively accounted for this problem by suggesting that selection can also act at the level of the group. Even though self-sacrifice reduces the reproductive fitness of the individual concerned nevertheless it can be beneficial for the reproductive fitness of the group of which the individual is a member. Darwin hypothesized that tribes which contain members that sacrifice themselves for the good of the group will survive and reproduce at

a greater rate than tribes that didn't contain such members. Competition between altruistic and non-altruistic groups could, therefore, have led to the selection and distribution of altruistic traits.

In the decades immediately after Darwin, understanding natural selection as operating at the level of the group became the orthodoxy. Altruism didn't command notable scientific interest as it was not seen as an anomaly. Those who believe natural selection operates at the group level argue that natural selection favours traits that benefit the group as a whole. Evolution thus works for the 'good of the group'. Hence altruism was only to be expected.

Group selection reached its classic exposition in Wynne-Edwards (1962). Wynne-Edwards proposed that many species have mechanisms ensuring that groups do not exhaust their resource base. These mechanisms evolved through group selection. Populations without such mechanism are liable to erode their resource base and thus risk extinction. Infanticide in langur monkeys was an example of a trait claimed to have evolved for the good of the group. Dominant male langur monkeys who take over harems of females frequently kill all infants under six months of age, infants still suckling. This has been claimed to be population control for the good of the group, since it dramatically limits and regulates population growth, limiting the risk of overpopulation.

In the 1960s, however, the 'good of the group' consensus in evolutionary biology was destabilised and displaced. Maynard Smith (1964) and Williams (1966) argued that group selection is, at best, a weak evolutionary force – group selection will only generate significant effects under very special conditions, conditions that rarely obtain outside experimental settings. Classic group selection is vulnerable to the free rider theorem, also known as subversion from within, which demonstrates that altruistic groups are highly vulnerable to subversion from within. Suppose organisms within a group are in competition with one another. Now suppose there are two types of organism within the group: those who act for the good of the group, and those who do not. Selfish organisms receive the benefits that obtain from altruistic behaviours but bear none of the costs. Selfishness is fitter, and so will eventually drive altruism in the group to extinction. Even if the group has only altruistic organisms, a single selfish mutant is sufficient to drive altruism in the group to extinction.

If classic group selection is unlikely to account for how altruism evolved, what could? Kin selection and reciprocal altruism were formulated and championed instead.

Kin selection is selection due to interactions between kin, the process whereby traits are selected for because of their beneficial effects on relatives' fitness. In a nutshell, altruism can evolve if the cost to the altruist is offset by sufficient benefit to sufficiently closely related recipients (Hamilton 1964). This idea can be expressed in a simple equation. In its basic form, Hamilton's rule is $Br>C$, where B represents the benefit to a gene for altruism in the recipient; r is the degree of genetic relatedness; and C is the cost to an identical copy of the gene in the altruist.

West, Griffin, and Gardner (2007) helpfully distinguish between the narrow use of kin selection and the broader use of kin selection. 'The narrower use of kin selection works upon interactions between individuals who are genetically related due to common ancestry – i.e. indirect benefits due to limited dispersal or kin discrimination.' (*ibid*.: 417). Relatedness, of course, can occur without recent common ancestry. A greenbeard gene can identify, and assist, copies of itself in other individuals. This opens up a broader definition of kin selection: 'The broader use of kin selection works upon interactions between individuals who share the gene of interest, regardless of whether this is due to coancestry or some other mechanism – i.e. also includes greenbeard effects.' (*ibid*.: 417). Hence, the difference between the narrow and broad uses of kin selection is 'whether kinship and relatedness are defined on the basis of average genetic similarity over most of the genome (narrow definition), or at the particular locus of the behaviour being examined (broad definition)' (*ibid*.: 417).

Organisms should behave in ways that maximize their average lifetime inclusive fitness, a measure taking into account not merely the number of direct offspring (personal fitness) but also the offspring of relatives. More precisely, 'An organism's inclusive fitness is defined as its personal fitness, plus the sum of its weighted effects on the fitness of every other organism in the population, the weights determined by the coefficient of relationship r.' (Okasha 2008a)

What about the evolution of altruism towards non-kin? This was to be explained by Trivers's (1971) theory of reciprocal altruism. Trivers' basic idea was straightforward: it may pay an organism to help another, if there is an expectation of the favour being returned in the future ('If you scratch my back, I'll scratch your back'). More precisely, an organism behaving altruistically towards a non-related member of the same species will increase its reproductive success if there is a probability of an altruistic act being returned to it by the recipient organism sometime in the future. The reproductive cost to the organism of

behaving altruistically is compensated by the probability of the return in reproductive benefit, thereby allowing the behaviour to evolve by natural selection. For reciprocal altruism to work, it's necessary that individuals interact with each more than once, and have the ability to recognize other individuals with whom they have interacted in the past.

During the 1970s and 1980s, the concept of group adaptation was thrown 'into the outer darkness' (Sober 2000: 107). Kin selection and reciprocal altruism became the new orthodoxy in evolutionary biology, and the pioneering evolutionary psychology practitioners in the 1980s and early 1990s inherited this orthodoxy. However, during the mid to late 1990s, group selection was reformulated, into what is now known as new or neo-group selection. Sober and Wilson (1999) demonstrated how group selection can explain the evolution of altruism, that there are circumstances relating to population structure where altruism can evolve by group selection – which, crucially, demonstrates how subversion from within is not insurmountable for group selection. Sober and Wilson argued that the fact that in mixed groups the fitness of altruists is lower than the fitness of selfish individuals doesn't mean that overall the number of altruists cannot go up. Put another way, at the individual level, the selfish trait will be favoured. However this does not mean that, at the group level, selfish groups will be favoured.

For group selection to work, several conditions need to obtain. One condition is that groups must differ in the number of altruistic members they contain. Another condition is that groups must not be stable, but must continually form and reform (and at a sufficiently fast rate). Classic group selection postulated well-defined groups. In contrast, new or neo-group selection has arbitrarily defined groups. From this perspective, despite being selected against under individual selection, altruism can indeed evolve because altruists tend to associate with one another. We can compare groups of altruists against other groups, and observe that the altruistic groups are the fittest, and that therefore altruism can spread. If groups break apart to form new groups frequently enough, then selfishness will not have the opportunity to spread. Altruism will prevail.

Although group selection remains contentious in more orthodox circles, it's generally agreed that Sober and Wilson's arguments have reinvigorated, and renewed interest in, group selection (Borrello 2005; Gangestad and Simpson 2007; Okasha 2008b). However, despite this reformulation, reassessment, and recognition of group selection, evolutionary psychologists still tend to utilise kin selection and other classic evolutionary theories, not neo-group selection. This has been criticized. Lloyd and Feldman (2002) and Laland and

Brown (2002) note that evolutionary theory has 'evolved', that evolutionary theorists and geneticists now use multi-level selection models, and they accordingly criticize evolutionary psychologists for becoming detached from such recent developments in evolutionary thinking. Gintis (2009: 244) claims that 'Evolutionary psychology... has incorporated the kin selection/reciprocal altruism perspective into a broadside critique of the role of culture in society'. Gintis then dismisses evolutionary psychology because of its adherence to the kin selection/reciprocal altruism framework.

It's a mistake to suppose that the levels of selection debate problematizes evolutionary psychology. First, one could pursue evolutionary psychology along neo-group selection lines without difficulty. Second, kin selection and neo-group selection are actually different ways of looking at the same thing, and there are good reasons to prefer kin selection over neo-group selection.

First, suppose one argues that neo-group selection is 'correct', that kin selection is 'incorrect', and that evolutionary psychologists should adopt neo-group selection. Even if evolutionary psychologists were moved by such an argument, which seems unlikely for reasons to be articulated in a moment, they could quite happily adopt neo-group selection without revising or overhauling evolutionary psychology's theoretical and methodological foundations. Whether selection occurs at one level or another level or multiple levels, adaptations will always be located at the individual level (Davies, Krebs, and West 2012). Whether one adopts kin selection or neo-group selection, adaptationist questions and hypotheses can still be formulated about psychological traits within individuals. In principle, evolutionary psychologists could investigate, and try to find evidence for, group selection on the design of purported adaptations, since traits that evolved by group selection should have features designed to function at that level. Indeed, Kurzban and Aktipis (2007: 230) invite us to consider the possibility of detecting 'the footprints' of group selection in the mind: 'it is possible that certain mechanisms that generated behaviors leading to greater assortment were selected because they increase the strength of group-level selection. In other words, traits that caused cooperative individuals to group together probably increased the likelihood of selection at the group level.'

Second, it's not actually the case that we need to declare kin selection correct and neo-group selection incorrect or vice versa. Recently, a consensus has emerged that kin selection and neo-group selection are formally or mathematically equivalent (Lehmann *et*

al. 2007; Okasha 2008a, 2008b; Kohn 2008; Marshall 2011). They are 'intertranslatable' (Wilson and Wilson 2007). According to West, Griffin, and Gardner (2008: 378),

> 'kin selection and (new) group selection are mathematically equivalent ways of looking at the same thing. We cannot emphasize strongly enough that it is not the case that one is correct and the other wrong, nor that group selection predicts things that cannot also be predicted with kin selection theory.'

So what we have here is a case of underdetermination: both frameworks, mathematically equivalent, can explain the same range of phenomena. Claiming that organisms should behave in ways that maximize their average lifetime inclusive fitness is consistent with claiming there are multiple levels of selection.

Even if both frameworks are formally equivalent, evolutionary psychologists can have good reasons for adopting kin selection over neo-group selection. For example, one could argue that kin selection is more parsimonious than neo-group selection. Indeed, one could argue that kin selection is the more useful framework (perhaps precisely because it's more parsimonious). West, Griffin, and Gardner (2008: 381-381) make the important point that the two frameworks being formally equivalent doesn't mean they're equally useful:

> 'At one level, kin selection and group selection are just different ways of doing the maths or conceptualizing the evolutionary process. However, from a practical point of view, it could not be clearer that the kin selection approach is the more broadly applicable tool that we can use to understand the natural world. This is because kin selection methodologies are usually easier to use, allow the construction of models that can be better linked to specific biological examples, lend themselves to empirical testing and allow the construction of a general conceptual overview.'

So according to this characterisation, which admittedly champions of neo-group selection might seek to resist, evolutionary biologists concerned with modelling animal behaviour find kin selection models far more useful than group level models. Indeed, West, Griffin, and Gardner (2007: 424) claim that important discoveries made using kin selection would not have been as easily made using neo-group selection:

> 'in some of the most successful areas of social evolution, such as split sex ratios in social insects or extensions of Hamilton's (1967) basic local mate competition theory, predictions arise elegantly from kin-selection models, whereas the corresponding group selection models would be either unfeasible or so complex that they have not been developed'

2.2 Environment of evolutionary adaptedness

In considering the adaptive problems faced by our ancestors, evolutionary psychologists consider our environment of evolutionary adaptedness (EEA). The EEA refers those aspects of our ancestral environment that were relevant to the evolution of our adaptations (Hagen 2005). More precisely,

> '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.' (Tooby and Cosmides 1990: 386-7)

So strictly speaking, the EEA is neither a time nor a place. In practice, a few evolutionary psychologists occasionally refer to our environment of evolutionary adaptedness as the Pleistocene, a period of time approximately 1.8 million to 10,000 years ago (e.g. Hagen 2004; Tooby and Cosmides 2005). As Tooby and Cosmides (1990: 388) explain,

> 'for most ordinary analytic purposes, the EEA for a species (i.e., for its collection of adaptations) can be taken to refer to the statistically weighted composite of environmental properties of the most recent segment of a species' evolution that encompasses the period during which its modern collection of adaptations assumed their present form. We have used the word "Pleistocene" in this sense to refer to the human EEA, because its time depth was appropriate for virtually all adaptations of anatomically modern humans'

The thought here is that our ancestors evolved during the Pleistocene into the modern human; thus, psychological characteristics that are distinctly human should have evolved

and solidified during this period, hence the references to the Pleistocene occasionally found in some parts of the literature.

The EEA concept is part-and-parcel of the concept of adaptation. One cannot describe an adaptation without describing an ancestral environment to which the adaptation is adapted. Hence, each adaptation has its own EEA, a statistical aggregate of selection pressures responsible for the emergence the adaptation (Buss *et al.* 1998). For example, the EEA for the human eye will differ from the EEA for the human language faculty (Tooby and Cosmides 1990).

Selection pressures need to be sufficiently stable to give rise to specific adaptive problems. Crucially, the physical environment need not remain constant. Sometimes, one reads objections to evolutionary psychology that boil down to 'the EEA had variable weather'. But that misses the point. The adaptive problem of finding a mate, for example, was a consistent, recurring selection pressure, regardless of environmental variables (and remains so).

The EEA concept encourages researchers to recognise that a selected trait is adaptive to its EEA, not necessarily to today's environment. Post-Pleistocene agriculture- and industry-based societies are very different from Pleistocene hunter-gatherer societies. There has been rapid technological and cultural change. Certain features of our environment are evolutionary novel. Think modern contraceptive interventions to limit fertility. Think speedy international travel. Think eating habits. This mismatch means a selected trait might not have current utility. Indeed, it might be deleterious, either in the technical sense of reducing genetic fitness, or in the everyday sense of reducing health and well-being.

There are clear and striking examples of mismatches between adaptations and today's environment. Perhaps the best known example is our strong preference for fatty and sugary foods. Despite knowing the health risks associated with eating too many fatty and sugary foods, many continue to do so and find it difficult to stop. It is a commonplace to observe that such foods are 'addictive'.

Why do we possess such a strong desire for fatty and sugary foods? The well-known adaptationist explanation runs as follows. As hunter-gatherers, our ancestors' food supplies were likely to have been volatile – certainly a far cry from today's all-too-predictable three meals a day and multiple snacks. Food shortages were likely to have been regular occurrences. In such a situation, a strong preference for fats and sugars would have been

adaptive, since consuming high energy fats and sugars would have supplied the energy needed to survive such restricted energy periods. Indeed, given the relative scarcity of fats and sugars in ancestral environments, gorging on such high energy foods when they became available would have been critical. Hence, our preference for fatty and sugary foods can be understood as an adaptation to recurrent periods of calorie/energy restriction.

These fat and sugar preferences remain with us today. But instead of finding ourselves in a world where fats and sugars are relatively scarce, we now find ourselves in a world where foods rich in fats and sugars are plentiful. Indeed, our evolved preferences explain why such foods are so widespread – such foods 'sell well precisely because they correspond to, and exploit, evolved desires for these substances' (Buss 2008: 65). However, the strong preferences for fat and sugar that served our ancestors so well are shockingly dysfunctional in today's world, leading to obesity, and a cascade of health problems such heart disease and diabetes.

Symons (2005) proposes another possible mismatch. A striking feature of human courtship is the fear that an approach will be rejected. The thought of rejection hurts. The memory of rejection hurts. We take this for granted. But such a strong fear, which scuppers and sabotages many potential approaches, appears 'astonishingly dysfunctional' in today's world (*ibid*: 257). Cities are populated by people we don't know, and, if we meet, unlikely to meet again by chance. In such a world, 'The potential benefits of propositioning an attractive member of the other sex, which include everything from a sexual fling to a lifetime mateship, would appear to vastly outweigh the potential costs, which seem to consist mainly of a small amount of wasted time.' (*ibid*: 257) Even if one doesn't pursue a (socially taboo or potentially even illegal) numbers game, approaching as many potential mates as possible, and instead limits approaches to those perceived to be receptive to being approached, the fear of rejection often remains.

Just as our strong and dysfunctional preference for fatty and sugary foods provides important clues about ancestral environments, so too our strong and dysfunctional fear of rejection might provide important clues about ancestral societal conditions. Symons points out that fear of mate rejection might possibly reflect mate rejection having heavier costs in the past than it does today. As hunter-gatherers, our ancestors lived in relatively small groups. In such a situation, the risk of one's sexual and romantic rejection becoming common knowledge might be significantly higher than it is today. If that were so, and if

rejection in the past carried with it substantial costs such as a diminishing one's perceived mate value in the eyes of others, then mate rejection anxiety could be adaptive. 'On a modern university campus, with thousands of students and enormous scope for anonymity, Bob's anxiety at the prospect of hitting on Bobbi is, perhaps, "irrational" in the sense that he has little to fear but fear itself; but the underlying motivational system may have been shaped by selection to function in an environment in which rejection had substantial costs.' (*ibid*: 257)

Crucially, one must distinguish the possibility of mismatch from the ubiquity of mismatch. One can endorse the possibility that psychological adaptations can be mismatched with today's environment without endorsing the claim that the majority of psychological adaptations are mismatched with today's environment. Symons (2005: 226) makes the crucial point that the EEAs of many of our adaptations are likely to be continuous with the present:

> 'The EEAs of the vast majority of human adaptations still exist today and usually are too obvious to merit explicit mention. For example, a neurophysiologist describing the function of a certain component of the human visual system probably will simply assume that his or her colleagues know: (1) a great deal about the nature of electromagnetic radiation and (2) that the (natural) light falling on human retinas today is essentially identical to the light that fell on our ancestors' retinas during the evolution of our visual system.'

Hence why humans so successfully live and flourish in a variety of social environments. Most of our adaptations, physiological and psychological, work splendidly precisely because their EEAs are continuous with the present. Hence also why cases of maladaptivity are so striking - they're striking precisely for their relative rarity, a Darwinian 'man bites dog'.

Yet this distinction is rarely made. People confuse possibility with ubiquity. Hence, both popularisers and detractors of evolutionary psychology tend to think evolutionary psychology as holding that humans are cavemen bumbling about in a strange and weird world. For example, *Psychology Today*, perhaps the most influential psychology website on the internet, features website subsections called 'Ape Girl', 'Caveman Logic', and 'Caveman Politics', subsections written by professional psychologists on evolutionary psychology topics. The blurb for 'Caveman Logic' is to examine how 'our *primitive* minds are

mismatched to the modern world' (emphasis mine). In a popularist book, *Supernormal Stimuli*, we read:

> 'Human instincts were designed for hunting and gathering on the savannahs of Africa 10,000 years ago. Our present world is incompatible with these instincts because of radical increases in population densities, technological inventions, and pollution.' (Barrett 2010: 3)

Little surprise, then, that sceptics end up absorbing this misconception and then seek to challenge it. Buller (2005: 112) reassures us:

> 'There is no reason to think that contemporary humans are, like Fred and Wilma Flintstone, just Pleistocene hunter-gatherers struggling to survive and reproduce in evolutionarily novel suburban habitats.'

Dupré also finds it 'quite surprising' that we are 'systematically maladapted' and that there is 'no good reason' to accept the idea (2012: 246). Agreed. But name me a serious evolutionary psychologist who would disagree.

Another issue related to the EEA is the possibility of post-Pleistocene selection for psychological adaptations. Referring to the 10,000 years since the end of the Pleistocene period, Barkow, Cosmides and Tooby claim that 'it is unlikely that new complex designs … could evolve in so few generations' (1992: 5). Our psychological adaptations are complex traits, and the construction of complex adaptations perhaps requires tens or even hundreds of thousands of years of cumulative selection. Our species spent over 99% of its evolutionary history as hunter-gatherers. Agriculture, which changed everything, has only been around for ten thousand years; industrialisation for only for two hundred years. The few thousand years since the advent of agriculture is a tiny stretch in evolutionary time. 'Therefore, it is improbable that our species evolved complex adaptations even to agriculture, let alone to postindustrial society' (*ibid.* 1992: 5).

Some have challenged this claim. Sociobiology, the forerunner of evolutionary psychology, did not make this assumption. In fact, Wilson (1975: 569) warned precisely against it:

> 'There is no reason to believe that during this final sprint there has been a cessation in the evolution of either mental capacity or the predilection toward special social behaviors. The theory of population genetics and experiments on other organisms show that substantial changes can

> occur in the span of less than 100 generations, which for man reaches back only to the time of the Roman empire [...] it would be false to assume that modern civilizations have been built entirely on capital accumulated during the long haul of the Pleistocene.'

Indeed, in recent years evidence has emerged that there has been post-Pleistocene selection. For example, Mekel-Bobrov *et al*. (2005) found that a variant of the gene ASPM, which is a regulator of brain size, arose just 5,800 years ago and is now carried by about a quarter of the world's population. According to the researchers, these findings 'suggest that the human brain is still undergoing rapid adaptive evolution' (2005: 1702). Another example is provided by Lewens (2007), who notes that the increase in the use of dairy products resulting from the domestication of cattle has resulted in a significant increase in lactose tolerance. Lewens (2007: 154) draws a lesson from this:

> 'Of course, the invention of dairy farming is only one of the many changes wrought on our environment since the Pleistocene. Many of us now live in cities, we no longer hunt as a matter of necessity, medical technology has improved, and there is no reason to rule out considerable modification of our cognitive adaptations in response to these altered environments, too.'

Laland (2007) draws an even stronger conclusion. Laland argues that, back in the early 1990s, it was tenable for Tooby and Cosmides to hold that selection is typically slow, and hence it was only reasonable to suppose that little selection has occurred since the end of the Pleistocene. However, we now know that rates of genetic evolution can be much faster. On the strength of these developments, Laland claims that 'the theoretical framework underpinning narrow evolutionary psychology is untenable' (Laland 2007: 9).

I believe too much has been made of this issue. Perhaps it's possible for post-Pleistocene psychological adaptations to have arisen, but to place so heavy an emphasis on one relatively small category of possible psychological adaptations, a category of adaptation that so far is empty of actual examples, is to get its true significance out of perspective. To bring out this thought, let's set out a number of responses.

First, evolutionary psychology is not committed to the claim that there has been no post-Pleistocene evolution. That would be an unnecessarily strong claim to make. Rather,

evolutionary psychologists seem to make a much more modest claim: that there hasn't been sufficient time for significant changes or additions to our psychological adaptations.

Kurzban (2011) emphasizes that there is a crucial difference between rapid genetic evolution and the evolution of complex adaptations. Evolutionary psychologists are concerned with the later, not the former. Furthermore, evidence of recent genetic evolution does not constitute evidence for the possibility, let alone the actuality, of post-Pleistocene complex psychological adaptations. Indeed, the reasoning that 10,000 years since the advent of agriculture is insufficient time to select for new complex psychological adaptations remains untouched by evidence of recent, rapid genetic change. An evolution psychologist can happily acknowledge rapid genetic evolution while comfortably denying the existence of post-Pleistocene complex adaptations.

Second, unsurprisingly given what we've just said, no evidence of significant changes to our psychological architecture since the end of the Pleistocene has been forthcoming. The evidence cited relates to simple physiological traits, not complex psychological adaptations. Lewens claimed there is no reason to rule out considerable modification of our cognitive adaptations in response to altered environments but he then declines to identify any possible candidates.

Third, let's assume that a few thousand years is sufficient time for selection to engineer new complex psychological adaptations. However, this alone is insufficient to generate new psychological adaptations. Consider the type of diet consumed in English-speaking countries. If average Anglo-Saxon diets continue to be nutritionally impoverished, continue to be laced with fructose, if obesity levels continue to escalate in America and the UK, this might lead to a selective advantage for some kind of fructose intolerance. In lands of obesity, a mild form of fructose intolerance might offer a selective advantage. But if Anglo-Saxon diets change, if they became more Mediterranean-like, more balanced, wholesome, and nutritious, then the pressure for fructose intolerance would evaporate.

The point is that for the new environments we have faced in the last ten thousand years to create new adaptive problems, these environments need to be stable enough to generate stable adaptive problems. But arguably there's been very little stability in societal arrangements in the last few decades, let alone centuries, let alone thousands of years. Mediaeval England is a different world to Victorian England. The England of my childhood is not the England of my twenties, and it will not be the England of my forties, or sixties, nor,

if I reach it, my eighties and beyond. There is massive flux, massive re-orderings and configurations and developments of society, of economics, of technology, of social norms, culture, and ideas. In short, there has perhaps been too much instability in social structures since the Pleistocene to create novel adaptive problems requiring novel solutions. Perhaps societies will eventually settle into recurring novel patterns, perhaps not. But there's little sign yet of ever-accelerating cultural change slowing down.

Fourth, let's be really generous, and grant both the possibility of post-Pleistocene selection for psychological adaptations and the likelihood or actuality of post-Pleistocene psychological adaptations. Does this discredit evolutionary psychology? Is this game-over for evolutionary psychology? Absolutely not. Such considerations would constitute a new research programme, one dedicated to hunting for post-Pleistocene psychological adaptations, one that can cheerily enough run alongside evolutionary psychology. Even if we have new psychological adaptations, this wouldn't undermine the claim that we have, or might have, psychological adaptations dedicated to solving recurring adaptive problems in the ancestral past, problems with ancient pedigrees.

So no matter which way one looks at it, the issue of recent selection doesn't problematize evolutionary psychology. Certainly, it doesn't make evolutionary psychology's framework 'untenable'.

2.3 Behavioural flexibility

Although evolutionary psychologists seek to understand human behaviour, they do not focus on selection as acting directly on specific behaviours. Rather, they focus on selection acting on information-processing psychological mechanisms. Of course, in practice, evolutionary psychologists often speak of behaviours as being adaptations – such as jealousy as an adaptation. Nevertheless, this is merely shorthand, sparing readers more cumbersome phrases like 'the mechanism underlying or generating the behaviour is an adaptation'.

Focusing on selection and psychological mechanisms, instead of selection and behaviour, is what crucially distinguishes evolutionary psychology from sociobiology: 'this new field focused on psychology - on characterizing the adaptations comprising the psychological architecture - whereas sociobiology had not. Sociobiology had focused mostly on

selectionist theories, with no consideration of the computational level and little interest in mapping psychological mechanisms.' (Tooby and Cosmides 2005: 16, n. 3)

If one applies adaptationist theorising straight to the level of manifest behaviour, one risks becoming blind to behavioural variation. Indeed, it might tempt one to average out variation. Referring to previous evolutionary research that applied direct to the level of manifest behaviour, Cosmides and Tooby (1987: 279) note that 'often the researcher would take the observed variation, average it, and typify the species or group by that average'. Okasha (2007) is possibly a recent example of this. Okasha attempted to provide an evolutionary explanation for the pervasiveness of risk-aversion in decision making. Schulz (2008), however, rightly points out that the focus on risk-*aversion* alone fails to do justice to the diversity and complexity of risk attitudes: 'The real issue concerning human attitudes towards risk is not that people are risk-averse, but that they are risk-averse and risk-loving. More specifically, it is the fact that many people are both risk-loving and risk-averse at the same time. Explaining this non-monotonicity in people's attitudes towards risk is the real heart of the problem – making sense only of their risk-aversion merely covers half of the ground to be traversed.' (*ibid*.: 160). Absolutely. In some conditions, I might be risk friendly, and another person risk averse. In alternative conditions, I might be the one risk friendly, and the other person risk averse. An evolutionary account should account for this diversity, not average it out.

Early sceptics of applying evolutionary perspectives to behaviour highlighted and emphasized the extraordinary complexity and diversity of human behaviour, and then contrasted this with what they judged to be naive, simplistic adaptationist accounts. Even sympathisers of evolutionary accounts of behaviour shared this concern. For example, surveying the contemporary sociobiological work of his day, Dawkins (1976: 191) said they 'are plausible as far as they go, but I find that they do not begin to square up to the formidable challenge of explaining culture, cultural evolution, and the immense differences between human cultures around the world'. Hence, Dawkins instead opted for the cultural evolution route for explaining social behaviour, in particular formulating his own theory of memetics.

Focusing our attention on selection acting on complex psychological traits, instead of manifest behaviour itself, enables the possibility of properly accounting for some of the undeniable complexity and diversity of human behaviour. Again, we can appeal to the symmetry principle. Just as various physiological adaptations function across a very wide

variety of environmental settings – the immune system responds to a variety of pathogens, we can digest a variety of foods – so too should our psychological adaptations function cross a wide variety of ecological and cultural settings (Barkow 2006).

And this is precisely what happens with non-humans. Krebs (2005: 752) notes that members of species ranging from crickets and crayfish to chimpanzees have been found to adopt conditional strategies such as, 'If your opponent seems more powerful than you, defer to him or her; if your opponent seems less powerful than you, intimidate him or her.' Daly and Wilson (2001: 4) also inform us that,

> 'Experimental studies of nonhuman animal foraging decisions have established the ecological validity of such a risk preference model. Rather than simply maximizing the expected (mean) return in some desired commodity, such as food, animals should be -- and demonstrably are -- sensitive to variance as well (Real & Caraco, 1986). For example, seed-eating birds are generally risk averse, preferring a low variance foraging situation over one with a similar expected yield but greater variability, but they become risk-seeking, that is switch to a preference for the high variance option, when their body weight or blood sugar is so low as to promise overnight starvation and death unless food can be found at a higher than average rate (Caraco, Martindale & Whittam 1980).'

In the EEA, it's unlikely that singular, nonflexible solutions to particular adaptive problems would have been successful for all individuals. For example, suppose we hypothesize that, in the EEA, it would have been reproductively advantageous for males to have a preference for casual sexual encounters. It seems relatively clear that various factors could moderate this strategy's success. For example, unattractive males might have experienced significantly less success in securing short-term mates than did attractive males. Thus, in the EEA, an unattractive male might have been reproductively better off pursuing long-term mates, and attractive males reproductively better off pursuing short-term mates. In this case, therefore, selection might have designed mating adaptations to feature both strategies, to be contingently triggered.

So instead of being blind, inflexible instincts, psychological adaptations can be thought of as, and are likely to be, conditional strategies. They can be conceptualized as being

facultative responses, as generating a variety of behaviours, depending on what environmental cues are being received. Diverse inputs, diverse outputs. So, for example, it's not the case that 'we're risk-averse' - which, after all, is empirically false, as attitudes to risk vary greatly between individuals and within an individual's lifetime. Rather, it's under conditions *X1... Xn,* risk averse behaviour will be generated; under alternative conditions, risk friendly behaviour will more likely obtain.

Buss and Greiling (1999) ask us to consider a man who is married to a woman whose value in the dating market is higher than his own. Would the misalignment in mating value influence the calibration of the man's jealous sensitivities? Adaptationist considerations suggest it would: 'a mechanism for adjusting one's threshold for jealousy would have resulted from thousands of selective events in the evolutionary past in which a mate value discrepancy, on average, was associated with a greater likelihood of a partner's infidelity or defection.' (Buss and Greiling 1999: 220) Hence, his relationship with his wife might lower his threshold for jealousy than it would otherwise have been if his perceived market value were equal to, or higher, to hers.

So although evolutionary psychologists postulate that psychological adaptations are species-typical ('universal') at the level of design, these species-typical psychological adaptations can generate a rich diversity of behaviours within populations and between populations – the kind of behavioural diversity we observe around us. 'For this reason, much of the study of behavioral variation can be recast as the study of the underlying (and usually) universal psychological adaptations that generate variation in response to circumstantial input.' (Cosmides and Tooby 1995: 17) Indeed, Tooby and Cosmides (1992) coined the term 'evoked culture' to refer to the fact that diverse inputs evoke different behavioural repertoires, thereby forging different elements of culture.

Note that we don't need to attempt to account for all behavioural diversity in terms of different cues triggering different conditional outputs. For example, genetic variation within populations and between populations could lead to biases in the calibration, and therefore the behavioural output, of our species-typical psychological adaptations. Since evolutionary psychology research focuses on the design of species-typical psychological mechanisms, and not genetic variation within and between populations, its hypotheses (unsurprisingly) tend not to include such considerations. But as shall become clear in the next and subsequent chapters, we shouldn't see evolutionary psychology hypotheses as

one-shot events, and hypotheses can be progressively modified over time to incorporate findings from cognate and adjacent fields such as behavioural genetics.

2.4 Developmental complexity, robustness, and calibration

Adaptations are inherited and reliably developing traits. Developmental outcomes are the result of processes in which there is an ongoing linkage between genes and influences from outside the genome and organism.

Developmental systems theory, pioneered by Susan Oyama, Paul Griffiths and Russell Gray**,** is a conceptualisation of development that seeks to capture and do justice to the sophisticated and complex interrelations between resources that give rise to traits. Developmental systems theory 'is not a theory in the sense of a specific model that produces predictions to be tested against rival models. Instead, it is a general theoretical perspective on development, heredity and evolution' (Oyama, Griffiths, and Gray 2001: 1-2).

Sterelny states that most developmental system theorists, who are a 'loose and evolving alliance', would subscribe to the following claims: '(1) genes are just one element of inherited developmental resources; (2) they are but one critical element in the developmental matrix responsible for ontogeny; and (3) organisms in part construct their world, as well as adapt to it' (2007: 180).

The first claim relates to dethroning genes in inheritance by adopting an extended view of inheritance. Intergenerational similarity is sustained by a 'matrix of interacting resources', including not just genes but also things like symbiotic microorganisms and culturally transmitted information (*ibid.*: 178). The second claim is concerned with dethroning genes in development. More precisely, in developmental systems theory genes are not accorded a privileged causal role in the explanation of the development of an organism's phenotype. Instead, a 'causal democracy' is recognised: 'Not only is most standard interactionism shot through with asymmetries, but the notions of causal *symmetry*, or *parity*, which do have a democratic ring, inform the very concept of a developmental system' (Oyama 2001: 183; emphasis original) As Sterelny (2007) notes, while every developmental perspective is 'moderately interactionist' in that both genetic and environmental contributions to an organism's phenotype are stressed, developmental systems theory is 'radically interactionist' in that it challenges dichotomous accounts of development that partition

causal factors into opposing sides of a boxing ring, genes in the red corner, environment in the blue corner. The third claim is known as 'niche construction', the process in which organisms alter their environments. Again, the idea here is to emphasise the active role of something that is usually thought of as being passive.

Evolutionary psychologists can agree with much of the basic picture, the basic holism, underlying developmental systems theory – and often they do. When discussing development from an adaptationist perspective, Tooby and Cosmides (1992: 83-84) write,

> 'By changing either the genes or the environment any outcome can be changed, so the interaction of the two is always part of every complete explanation of any human phenomenon. As with all interactions, the product simply cannot be sensibly analyzed into genetically determined and environmentally determined components or degrees of influence... "Biology" cannot be segregated off into some traits and not others.'

Indeed, Tooby and Cosmides (1992: 84) even speak of 'developmentally relevant environments', which sound strikingly like 'developmental systems': 'It is this developmentally relevant environment - the environment as interacted with by the organism - that, in a meaningful sense, can be said to be the product of evolution, evolving in tandem with the organism's organized response to it'. Hence, 'The critical question is not... whether every human male in every culture engages in jealous behaviors... instead, the most illuminating question is whether every human male comes endowed with developmental programs that are designed to assemble (either conditionally or regardless of normal environmental variation) evolutionarily designed sexual jealousy mechanisms' (*ibid*.: 45).

This honouring of causal holism is not restricted to Tooby and Cosmides. Buss (1995: 5) writes,

> 'The key issues of this debate have been obscured by false dichotomies that must be jettisoned before we can think clearly about the issues – false dichotomies such as "nature versus nurture," "genetic versus environmental," "cultural versus biological," and "innate versus learned." These dichotomies imply the existence of two separate classes of causes, the relative importance of which can be evaluated quantitatively. Evolutionary psychology rejects these false dichotomies.'

Symons (1992: 140) concurs: ‘every part of every organism emerges only via interactions among genes, gene products, and myriad environmental phenomena’. And Bjorklund and Bering (2002: 6) stress that ‘individual differences in educability should not be viewed as ‘‘genetic’’ or ‘‘environmental’’ in nature, but rather as the result of the transaction between multiple levels of organization.’

So evolutionary psychology is an arena where the old antagonisms no longer pertain. If we claim that *X* is an adaptation for *Y*, we should be as committed to saying it’s socially constructed, a product of post-industrial society, as we would be to saying it has a genetic basis.

Yet there is a common belief that developmental considerations discredit evolutionary psychology. One source of this belief is the misconception of evolutionary psychology focusing on genes. The following paragraph could appear without much challenge in almost any sceptical paper and book:

> ‘it's increasingly apparent that development plays a crucial role in explaining human nature. The old "evolutionary psychology" picture was that a small set of genes was directly responsible for some particular pattern of adult behavior—a "module". In fact, there is more and more evidence that genes are just the first step in complex developmental sequences, cascades of interactions between organism and environment, and that those developmental processes shape the adult brain. Even small changes in developmental timing can lead to big changes in who we become.’ (Gopnik forthcoming)

I'll eat my shoes if a single evolutionary psychologist ever endorsed the ‘old picture’ or rejects the (presumably) ‘new’ picture. Note the invocation of the word ‘old’, the implication that evolutionary psychology is outdated. We’ll return to this theme in the final chapter.

Other sceptics voice a similar thought: Lickliter and Honeycutt (2003: 820) claim that ‘the preconceptions of evolutionary psychology... center around the assumption that basic aspects of an organism, including its morphology, physiology, and psychology, are best understood as the products of its genes’. Nonsense: we’ve already seen evolutionary psychology is committed to environments playing a complex and indispensible role in every step of the causal chain (see also Dickins and Dickins 2008). Lickliter and Honeycutt (2003:

821) further claim that evolutionary psychology views the environment as 'secondary to the role of genetic factors'. Again, this is not the case: 'evolutionary psychologists do not partition genes and environment into primary and secondary roles' (Buss and Reeve 2003: 851).

When speaking of evolutionary psychologists postulating specialised psychological adaptations or modules, Heyes (2012: 2094) claims that 'Experience was assumed to play a limited role in the development of these modules.' Shea (2012: 2234) 'rejects an Evolutionary Psychology that is committed to innate domain-specific psychological mechanisms: gene-based adaptations that are unlearnt...'. But again, this simply cannot be the case. Experience is essential during critical developmental stages, pre-natal and post-natal. For example, there appears to be a critical period for language development: successful language acquisition must occur during the period from early childhood to puberty; language learning after this critical period will be increasingly difficult and less successful (Lenneberg 1967). Pinker (1994) illustrates this critical period with the example of 'Chelsea'. Chelsea was born deaf, but was misdiagnosed as retarded. In her early 30s she was correctly diagnosed as deaf and she received hearing aids and intensive language instruction. However, despite now hearing, her subsequent language acquisition was severely compromised: she produces strings of words, not proper sentences.

Another misconception is to hold that the complex interactions between genes and environment throughout development problematizes the notion of developmentally robust adaptations – that if we take seriously the multiple causal factors in the development of an organism, then it's unlikely developmentally robust psychological adaptations would arise. But this leads to an absurdity: if sophisticated interactionism destabilises robust adaptations, we wouldn't have physiological adaptations!

Physiological and psychological adaptations are reliably developmental outcomes because of, *inter alia*, reliably recurring environmental features. Carruthers stresses that 'natural selection can rely on the presence of reliably recurring features of the environment (such as the presence of face-shaped stimuli) when selecting for particular developmental programs' (2006: 162-3). Barrett (2007a: 188) articulates the point perfectly:

> 'Reliably developing aspects of the phenotype often have the appearance of "innateness" in some respects. They are produced whenever the developing individual's environment sufficiently matches

> the ancestral one along relevant dimensions. For example, developmental schedules of some skills, such as the ability to distinguish between animates and inanimates, are relatively invariant across very different cultures and environments... However, this does not tell us what factors in the environment might contribute causally to development of the competence. Innateness in the folk sense of lack of environmental input is not mandated.'

This goes back to a point I made earlier - that the EEA of adaptations can often match today's environment. Despite the diversity found in Japanese, Brazilian, Nigerian, German societies and so on, they still share strikingly similar ecological, developmental, and societal features. These invariant features enable a person to usually develop ten fingers, a pair of eyes, a pair of legs and so on regardless of where he or she is born. Likewise for psychological adaptations: a woman born in Nigeria will tend to develop the same set of psychological adaptations as a woman in born in Brazil. Whether she is born in Nigeria or Brazil, she will still receive parental investment; she will be raised in a language community; she will encounter animate and inanimate objects; she will learn social norms; she will need to navigate a complexity of social challenges; she will still read human faces; she will engage in courtship and mating; and so on. Many of the reliably developing psychological adaptations that arise from the complex interactions between genes and environment will seem 'innate' in a folk biological sense, in the sense of not requiring environmental inputs, but this is only an appearance, one resonating with our folk intuitions, and shouldn't be construed as a scientific claim.

So it's a misconception to think that the complexity of the interactions between genes and environments discredits, or is in tension with, the notion of developmentally robust psychological adaptations. Indeed, Tooby and Cosmides' repeated use of the term 'developmentally relevant environment' should unnerve those who insist on problematizing psychological adaptations with developmental complexity. As Dennett (2011: 483) nicely puts it, 'the oft-implied claim is that one should be an evo-devo theorist *instead of* an adaptationist/selectionist/agentialist. Why not be both?' (emphasis original).

It's true that evolutionary psychologists usually focus on functional questions about adaptations, and focus less on the ontogeny of adaptations (but without denying the importance of development). But does this mean developmental perspectives are irrelevant to evolutionary psychology hypothesis generation? Absolutely not. Thinking

about developmental processes can greatly strengthen evolutionary psychology hypotheses. Although adaptations are held to be developmentally robust, their calibration can be developmentally sensitive. In animals, early life experiences modulate behaviour. More precisely, animals often adjust their life histories in response to early environmental conditions, in ways that enhance reproductive fitness, at least in the environment of evolutionary adaptation. For example, Frederick (2012) cites Cameron, Fish, and Meaney (2004), which reported high stress early environments lead female rat offspring to mature faster, become more sexually receptive, and reproduce more quickly than they would otherwise. Likewise with humans: differences in the early physical (including prenatal) and social environments can play a critical role in the calibration, in the sensitivities, of particular psychological adaptations.

Belsky, Steinberg, and Draper (1991) investigated the impact of early environmental cues on the development of mating strategies. They hypothesized that an early environment characterised by relatively short and unstable relationships and scarce and unpredictable resources should predict differences in individual propensities to engage in short-term mating strategies versus long-term mating strategies.

> 'Individuals whose experiences in and around their families of origin lead them to perceive others as untrustworthy, relationships as opportunistic and self-serving, and resources as scarce and/or unpredictable will develop behavior patterns that function to reduce the age of biological maturation... accelerate sexual activity, and orient them toward short-term, as opposed to long-term, pair bonds... Individuals, in contrast, whose experiences lead them to perceive others as trustworthy, relationships as enduring and mutually rewarding, and resources as more or less constantly available from the same key persons will behave in ways that inhibit (relative to the first type) age of maturation, will defer sexual activity, and will be motivated to establish-and be skilled in maintaining-enduring pair bonds, all of which will serve to enhance investment in child rearing.' (*ibid*.: 650)

These early environmental cues influence the development of mating strategies in ways that would be adaptive to the type of environment signalled by those cues: 'In essence, we argue that early experiences and the psychological and biological functioning they induce

lead individuals to engage in either a "quantity" or a "quality" pattern of mating and rearing.' (*ibid*.: 650)

2.5 Psychological adaptations as modules

There is some confusion about the concept of modularity – especially among sceptics. This confusion arises from the fact that the term 'module' has developed a range of meanings and is often used without making clear which meaning is intended. Like many concepts, the concept of modularity means different things to different people or, as Ermer, Cosmides and Tooby put it, 'different research communities' (2007: 153).

Fodor (1983) characterizes modularity in terms of nine properties. In order of Fodor's original presentation, modules are information processing systems that are (1) domain-specific (they process a restricted range of inputs); (2) mandatory (they operate in an automatic way once the domain-specific stimuli is present); (3) inaccessible (higher levels of cognitive processing have no access to the internal processes of a module); (4) fast (they generate outputs quickly); (5) informationally encapsulated (modules can't draw on information held outside of it); (6) shallow (they have relatively simple outputs); (7) localized (they are realized in a fixed, dedicated neural architecture); (8) subject to patterned breakdowns; and (9) their ontogeny have a characteristic pace and sequence.

For Fodor, the definitive feature of a module is the fifth property, namely informational encapsulation. Fodor's definitional criteria of modularity stems from one kind of brain system, namely perceptual systems. Perceptual systems are tasked with the function of making speedy interpretations of stimuli. Fodor reasoned that in order to achieve this function, the input systems need to operate automatically and not be influenced by other systems. This kind of system is essentially 'hardwired pipelines bringing information to central systems' (Barrett 2007b: 162). If one is operating under such a narrow definition of modularity, then it 'becomes true virtually by definition' that most of the mind is not modular (*ibid.*: 163).

Crucially, however, few brain systems are like this. For one, brain systems are thickly interconnected (*ibid.*: 163). Indeed, Fodor recognised this, arguing that there are only a handful of modules. Carruthers (2006) notes that 'some of the items in Fodor's list will need to get struck out as soon as we move to endorse any sort of central-systems modularity, let alone entertain the idea of *massive* modularity' (2006: 5, emphasis original).

In particular, in order to have modularity in central-process cognition, informational encapsulation, the most important feature in Fodor's understanding of modularity, needs to be jettisoned.

But if informational encapsulation is not the key to understanding modularity, what is? For evolutionary psychologists, 'functional specificity' is the fundamental feature of modularity (Barrett 2007b). Crucially, they argue that brain systems can be functionally specialized without being informationally isolated and encapsulated (*ibid.*: 163).

Barrett emphasises that 'the core of specialization in biological systems is the fit between *form* and *function'* (*ibid.*: 164, emphasis original). Different functions will require different design criteria. Whereas perceptual systems need to exclude information in order to execute their function, systems responsible for mate choice need to integrate information from a variety of sources to achieve their function (*ibid.*). Both systems are modular in the sense of being functionally specialised but they radically differ in their design criteria. It can therefore be a 'major mistake' to apply 'design criteria from one kind of system to another' (*ibid.*: 167). Accordingly, since Fodor's concept of a module relates to the design criteria of an uncommon kind of functionally specialised processing structure, it 'is neither useful nor important for evolutionary psychologists.' (Ermer, Cosmides, and Tooby 2007: 153)

Despite these statements, some opponents of evolutionary psychology claim that one of the main characteristics of a module as understood by evolutionary psychologists is informational encapsulation (see, for example, Buller 2005: 128). They then proceed to point out how this property is implausible for many proposed psychological adaptations. The dialectic usually runs along the lines of 'the brain is plastic, but modules are rigid'. As Buller and Hardcastle (2000: 311) put it:

> 'Brain plasticity belies the idea of encapsulated modularity, for our information processing streams are not really separate streams at all. There is much informational overlap between what are normally thought of as distinct processing areas. In other words, whatever modules one might want to identify in the brain are not as distinct, or informationally encapsulated, as evolutionary psychologists typically imply.'

But, as we have seen, evolutionary psychologists don't adopt this conception of modularity, hence the argument is irrelevant.

Barrett (2006: 201) observes:

> 'evolutionary psychologists have stressed repeatedly that the core of their notion of modularity is functional specialization (Barrett, 2005b; Pinker, 1997; Tooby & Cosmides, 1992). However, others have read modularity claims as implying much more: for example, automaticity (DeSteno et al., 2002), encapsulation (Fodor, 2000), localization (Uttal, 2001), lack of plasticity (Buller & Hardcastle, 2000; Elman et al., 1996; Karmiloff-Smith, 1992), and innateness verging on preformationism'

So, some - though by no means all - of the controversy surrounding the concept of modularity in evolutionary psychology is rooted in semantic confusion. It might be fruitful, therefore, for evolutionary psychologists to simply drop the label 'module' while retaining the concept of specialised information processing. Indeed, one might be inclined to think that Cosmides and Tooby have caused the semantic confusion by failing to be more explicit with how their understanding of modularity differed from Fodor's.

However, Cosmides and Tooby blame Fodor for the semantic confusion: 'Fodor uses some terms differently from the way we do, leading to some considerable confusion in the literature' (Tooby, Cosmides, and Barrett 2005: 309, n. 4). They claim that their understanding of modularity has a longer pedigree than Fodor's. They point out that the concept of module first arose in artificial intelligence to simply refer to the concept of 'a mechanism or program that is organized to perform a particular function' and charge Fodor with clouding this simple meaning 'in favor of an eccentric set of criteria' and thereby sowing 'a great deal of confusion' over the term. 'For evolutionary psychologists, the original sense of module – a program organized to perform a particular function – is the correct one' (Ermer, Cosmides, and Tooby 2007: 153).

Carruthers (2008) adds that most philosophers suffer from what he calls 'Fodor-Fixation', labouring under a fable about modularity that runs as follows: Fodor engineered a notion of modularity fit for service in cognitive science, with the implication that only peripheral systems are modular; accordingly, theorists who wish to advance the thesis of massive modularity must weaken the concept of modularity in a way that preserves the central idea that modules are encapsulated processing systems. This fable, claims Carruthers, obscures philosophers' understanding of the conception of modularity operational in evolutionary psychology.

However, other misunderstandings over modularity in evolutionary psychology might also be due to what Carruthers calls the 'highly misleading' language evolutionary psychologists themselves are sometimes prone to use (2006: 21). Carruthers stresses the importance of not contemplating modules as streamlined pipes. Using the figurative language of 'Swiss army knives' and 'adaptive toolboxes', evolutionary psychologists perhaps unwittingly fuel this misapprehension.

Modules are 'distinct functionally specialized cognitive systems, that is all' (Carruthers 2006: 21). Each module will have a 'distinct neural realization' but the neural realization might 'be dispersed across a number of different brain regions' (*ibid.*: 62). One of the virtues of a module is 'the flexibility of its demand for real estate' (Pinker 1997: 30). Modules 'are biological systems, and like most such systems they are likely to be built by co-opting and connecting in novel ways resources that were antecedently available in the service of other functions' (Carruthers 2006: 21) This 'recruiting and cobbling together in quite inelegant ways resources that existed antecedently' leads to ''kludgy' architectures that look decidedly awkward in design terms' (Carruthers 2006.: 62). They look 'more like roadkill, sprawling messily over the bulges and crevasses of the brain' (Pinker 1997: 30). Hence, modules are not 'elegantly engineered atomic entities with simple and streamlined internal structures' (Carruthers 2006.: 62).

Carruthers (2008: 294) provides a succinct summary of the concept of modularity in evolutionary psychology:

> 'What emerges from these arguments is a notion of 'modularity' according to which modules are function-specific processing systems which exist and operate independently of most others, and which have complex, but limited, input and output connections with others. Each of these systems will have a distinct neural realization, and will be frugal in its use of information, while having internal operations that are inaccessible to others.'

All modules will have the properties just cited. Furthermore, 'almost all' will be domain specific and 'most' will be 'innate'. 'Some' modules will even be encapsulated (*ibid.*: 295).

2.6 Conclusion

Five common areas of misunderstanding have been corrected. First, it's a mistake to suppose that the levels of selection debate problematizes evolutionary psychology. One could pursue evolutionary psychology along neo-group selection lines without difficulty. Furthermore, kin selection and neo-group selection are actually different ways of looking at the same thing, and there are good reasons to prefer kin selection over neo-group selection.

The second issue we looked at was the EEA. The EEA is part-and-parcel of the concept of adaptation. It should not be controversial. It only becomes controversial when the possibility of mismatch confused with the ubiquity of mismatch. This conflation has been allowed to waste a lot of time and has bred a distorted picture of evolutionary psychology. Furthermore, evidence of recent selection for simple physiological traits does not destabilise the argument that there's been insufficient time for selection for complex psychological traits; not a single recently evolved complex psychological trait has been proposed; even if selection could select for complex psychological traits in a short period of time, the instability of recent social environments problematizes their selection; even if new complex traits have recently been selected for, this doesn't undermine the search for complex traits with longer pedigrees.

Third, behavioural flexibility. If psychological adaptations are species-typical, how do evolutionary psychology hypotheses account for variability? Psychological adaptations are not blind instincts. They generate variable behaviour as a result of their being selected to respond adaptively to variable ancestral conditions. Indeed, as we'll see in the next chapter, evolutionary psychologists do not merely pay lip service to variability of behaviour — much of their research focuses on the specifics of how people respond contingently to environmental conditions.

Fourth, developmental complexity. Evolutionary psychology is an arena where the old antagonisms of genes versus environments genuinely no longer pertain. It is quite possible to endorse causal holism cheerfully enough while also postulating developmentally robust adaptations. Furthermore, early experience can developmentally calibrate psychological adaptations by, for example, calibrating the thresholds for evoking certain responses. Thus we have another dimension of variability: you might be developmentally calibrated one way, and I might be developmentally calibrated another way.

Finally, when some prominent evolutionary psychologists use the term 'module' – I say prominent evolutionary psychologists, for the term 'module' is not frequently employed by practitioners on the ground, nor does it need to be employed – they simply mean functional specificity, not Fodor's use of the term to mean informational encapsulation.

Chapter 3

Hypothesis generation and testing

We've mentioned before that the heart of evolutionary psychology is the generation and testing of adaptationist hypotheses in psychology. It's time to discuss this claim in more detail. In §3.0, I identify two types of hypothesis evolutionary psychologists generate. In §3.1, I identify the two broad research strategies they use to generate those hypotheses and highlight the centrality of engineering thinking in evolutionary psychology. In §3.2, I show how these two research strategies work in tandem. In §3.3, I examine a number of specific resources that guide evolutionary psychologists in their pursuit of hypotheses and novel predictions. In §3.4, I identify the common experiments evolutionary psychologists use to confirm or disconfirm their novel predictions. In §3.5, I argue that, although evolutionary psychology hypotheses might start off as 'simple', they can progressively become more complex, progressively mirroring the adaptations they're targeting. In §3.6, I identify and dismiss three common objections to evolutionary psychology methodology.

3.0 Novel predictions

The primary function and value of evolutionary psychology, its raison d'être, is:

H1 Hypothesizing unknown design features of extant psychological mechanisms, leading to novel predictions of psychological phenomena.

H2 Hypothesizing unknown psychological mechanisms, leading to novel predictions of psychological phenomena.

As with all concepts, the concept of novelty admits a degree of ambiguity, and different versions can be cashed out. Musgrave (1974) distinguished three versions of novel predictions: temporal (a phenomenon is novel for a hypothesis if it was unknown at the time of the hypothesis's formulation), heuristic (a phenomenon is novel for a hypothesis if the hypothesis was not constructed specifically to accommodate the phenomenon), and theoretical (a phenomenon is novel for a hypothesis if it's not predicted by any existing rival hypothesis). We need not debate the merits of these different versions; the empirical work probably falls across the three distinctions.

An example of H1 is Curtis, Aunger, and Rabie (2004), which hypothesized that disgust is an adaption for avoiding pathogens. If the function of disgust is to act as a pathogen-avoidance system, a number of novel predictions follow. First, disgust should be felt more strongly when an individual encounters a disease relevant stimulus than with a similar stimulus with no disease connotations. Second, it should operate similarly cross-culturally. Third, a sex difference is predicted: since women protect not only themselves but also offspring, disgust should be more pronounced in women. Fourth, disgust should be less pronounced as an individual's reproductive potential declines. Fifth, since strangers carry a higher risk of carrying novel pathogens, disgust should be more strongly evoked by contact with strangers than close relatives. Curtis, Aunger, and Rabie tested these predictions using data provided by almost 40,000 participants in an international website survey employing visual stimuli, where subjects were asked to grade the disgustingness of seven pairs of disease irrelevant and disease relevant pictures. Cross-culturally, the disease relevant stimuli were found to be significantly more disgusting than their disease irrelevant counterparts. Curtis, Aunger, and Rabie concluded that the results are consistent with disgust being an adaptation for disease avoidance.

An example of H2 is the previously unknown cue-detection of the ovulatory cycle. In contrast to many other mammals, including our closest cousin, the chimpanzee, human ovulation is concealed. Chimpanzees' ovulation is advertised by a sizable swelling of the genitals. In contrast, humans do not advertise ovulation like this. Evolutionary psychologists hypothesize (1) the possibility of subtle cues indicating stages of the ovulatory cycle, and (2) the possibility that natural selection selected for adaptations dedicated to detecting those cues. Recent research is progressively vindicating both (1) and (2).

With regards to (1), emerging research is identifying discernible cues of fertility in women's movements, scents, and voices. For example, women's voice attractiveness varies across the ovulatory cycle (Pipitone and Gallup 2008). With regards to (2), evidence is suggesting that women's male partners adaptively shift their behaviour in response to cues of approaching ovulation. Evidence has already shown that men subconsciously judge where a woman is in her ovulatory cycle. For example, lap dancers make significantly more money in tips when they're ovulating compared to when they're menstruating (Miller, Tybur, and Jordan 2007).

This is a unique prediction. To some, it might sound a little farfetched, somewhat difficult to believe. It's easy to be dismissive of such ideas and findings. This is an important theme, one shedding light on the importance of evolutionary psychology in the evolutionary and behavioural sciences, and shedding light on a key source of scepticism of the programme. I'll return to this theme in the final chapter.

3.1 An engineering perspective

The underlying logic of evolutionary psychology discovery is this: *if* psychological trait *T* is a psychological adaptation for *X*, then psychological trait *T* should have configuration *C*, and so we should find phenomenon *P*. That is, if *T* is an adaptation for *X*, we should see signatures of this in the adaptation's design, in the kinds of inputs it receives, in the kinds of output it produces, and so on, and this can locate behavioural patterns we've never observed before. Campbell (2006: 91) nicely cashes out this logic concretely and succinctly as follows:

> 'An adaptation (such as fever) designed to produce a particular beneficial outcome (parasite destruction) should occur when the body detects pathogens and not at other times... If we have conserved a herding

> response to danger, we should prefer to be with others when we face threatening uncertainty. If guilt is an emotion signaling unpaid obligation, we can make clear predictions about the likelihood of altruism when it is aroused.'

Recall in chapter 1 we cited Buss's (1995) definition of a psychological adaptation (what he terms 'evolved psychological mechanisms'): a psychological adaptation is a set of processes that: (1) Exists, in the form it does, because it solved a specific and recurrent adaptive problem; (2) takes only certain classes of information and converts that information into output through a procedure (such as a decision rule) in which output adjusts physiological activity, provides information to other psychological mechanisms, or produces overt action.

We can say that (1) pertains to the function of a psychological adaptation and (2) pertains to the form of a psychological adaptation. In generating hypotheses, evolutionary psychologists heuristically reason between function and form of proposed psychological adaptations. Or, to put it another way, they heuristically reason between ultimate questions about the selected trait under investigation (why the trait exists, what it was selected for) and proximate questions about the selected trait (how it works: what information it processes, what outputs it produces).

Two research strategies are available. The first is known as 'reverse engineering', where we reason from form back to function, from the present to the past. Here we begin with an extant psychological trait and then consider what kind of adaptive problem it's well-designed for solving. The second strategy is known as 'forward engineering', where we reason from function towards form, from the past to the present. Here we begin by postulating an adaptive problem and then reason what kind of psychological trait would constitute a well-designed solution to it. Notice that this second strategy, of reflecting on adaptive problems and scenarios, holds out the possibility of discovering new mechanisms. This possibility afforded by forward engineering achieves its sharpest expression in a passage in Ermer, Cosmides, and Tooby (2007: 154):

> 'Evolutionarily rigorous theories of adaptive function specify what problems our cognitive mechanisms were designed by evolution to solve, thereby supplying critical information about what their design features are likely to be – information that can guide researchers to

> discover previously unknown mechanisms in the mind. That is the essence of the adaptationist program.'

No matter whether we use reverse engineering or forward engineering, we need to consider a number of selectionist and engineering questions. What kind of selection pressures obtained in past ancestral environments? What kind of recurrent adaptive problems and opportunities did they create? When considering what would serve as a well-designed solution to such a problem, what kind of information, what kind of cues, would potentially have been available in past ancestral environments for solving such a problem? Are these cues reliable? What kind of computations would a well-designed solution to the adaptive problem make? What would it do with the inputs? More precisely, what feelings, physiological activities, thoughts and actions would it produce? What kind of developmental and contextual calibrations would be required, if any, to adequately solve the adaptive problem?

So adopting an engineering perspective, asking what a well-designed solution to a given adaptive problem would look like, thinking about the fit between adaptive problem and adaptive solution, is the cornerstone of adaptationist hypothesizing in psychology. Cosmides and Tooby (1997a: 15) write:

> 'engineers figure out what problems they want to solve, and then design machines that are capable of solving these problems in an efficient manner. Evolutionary biologists figure out what adaptive problems a given species encountered during its evolutionary history, and then ask themselves, 'What would a machine capable of solving these problems well under ancestral conditions look like?''

Potentially there is much to be gained by hypothesizing a close fit between adaptive problems and adaptive psychological solutions. It's a commonplace that often the most difficult thing to see is right in front of your nose. Hidden in plain view. We need theories and hypotheses to guide us into identifying patterns. Evolutionary psychology can guide researchers into predicting new behavioural patterns, new demographic patterns, new facts – facts that do not readily lend themselves to discovery by non-adaptationist perspectives. The more novel these predictions are, the more surprising and counter-intuitive, the better.

Of course, many psychological traits are not adaptations. Evolutionary psychologists don't take just any random psychological trait and start hypothesizing with it. The heuristics constrain what to look for. Evolutionary psychologists look out for putative or candidate adaptations, where there is evidence, or possibility of gaining evidence, of either (1) special design, (2) universality, (3), ease, speed, and reliability of development or learning, or (4) maladaptiveness. Evidence of any of these indicates that a trait *could* be an adaptation, not that it *must* be an adaptation, and thus warrants further adaptationist analysis.

Special or adaptive design is recognized when a trait exhibits complexity, selectivity, efficiency, and specificity in its outcomes (Williams 1966). What is sometimes called 'Dawkins' gambit' holds that complexity of design is a clear and unambiguous sign of selection: complex traits or behaviour patterns cannot evolve by chance or drift but must have been selected for and shaped by natural selection. However, as Richardson (2007) points out, some biologists challenge this strong inference. We need not get drawn into this debate. Although the gambit is present in prominent evolutionary psychology literature, such a strong inference is not strictly required: we can simply state that special design of a trait signals the *possibility* that selection has acted on the trait.

Adaptations are species-typical (universal) at the level of design. Hence, if a trait is found universally - across cultures, spatially and temporally - this is also suggestive that the trait could be an adaptation. Note, however, that a trait being cross-cultural does not entail it will be expressed the same across cultures; different environmental and social conditions can supply different inputs that result in the trait generating different behavioural outputs. The point is that the variability of expression based on local environmental (ecological and social) and developmental conditions should be predictable in virtue of species-typical design.

Traits that develop especially quickly, traits that are learned especially easily, traits that develop especially reliably can also be considered as candidate adaptations. Examples include the capacity to develop language at a young age, the ability from a young age to recognize other individuals by their faces and inferring the feelings and intensions of others from facial expressions, and a biased propensity to fear snakes.

Furthermore, if a trait is deleterious in today's environment, this could indicate that it's a well-designed adaptive solution to a past adaptive problem but simply maladaptive in today's environment. As we saw in chapter 2, certain traits are strikingly maladaptive -

famously, our preference for fatty and sugary foods. I suggest a further possible example in chapter 5.

3.2 The two research strategies in tandem

So far, we've been representing the methodology of evolutionary psychology as comprising two distinct research strategies: reasoning from form to function, and reasoning from function to the present. This is how Cosmides and Tooby present the methodology (e.g. Barkow *et al*. 1992), how other evolutionary psychologists represent the methodology (e.g. Pinker 1997), and how commentators, sympathetic and sceptical, represent it (e.g. Laland 2011a; Richardson 2007).

I contend that evolutionary psychology's methodology is more sophisticated than the impression given by this presentation. This standard presentation misses out, or rather masks, something crucial. While these research strategies can be distinguished and used independently of one other, they can be used together, a point frequently missed in the literature, especially in the sceptical literature (e.g. Buller 2005; Richardson 2007), which tends to focus on indeterminacy problems arising from reasoning exclusively from form to function, or reasoning exclusively from function to form, but fails to consider the possibility that these strategies can be used in tandem.

Indeed, I contend the following: adaptationist analysis in psychology should utilise both strategies when formulating hypotheses. Furthermore, I suspect they often do, though I do not need to establish this.

One can start at either end of the methodological chain between function and form, between past and present, but the point is to move up and down. For example, suppose we begin our adaptationist analysis with an extant psychological mechanism. We reverse engineer it – we conjecture that it solves an adaptive problem. But we don't end the analysis there. Once we've conjectured a selectionist scenario, we make use of the predictive approach, of moving up the methodological claim, making novel predictions about the design features of the extant psychological mechanism. Machery (forthcoming) articulates this as evolutionary psychology having a 'bootstrap strategy', using knowledge of extant psychological traits to hypothesize past selective pressures, and then hypothesizing from these selective pressures to produce novel predictions about extant or new psychological traits

Evolutionary psychology's proposal of sex differences in sensitivities to different forms of infidelity is a simple but illustrative example of the kind of point I'm making. A useful definition of jealousy is an emotional state 'aroused by a perceived threat to a valued relationship or position and motivates behaviour aimed at countering the threat' (Daly, Wilson, and Weghorst 1982: 12).

Since this psychological trait bears hallmarks of being an adaptation (cross-cultural, functional specificity, and so on), evolutionary psychologists can reverse engineer it. Reverse engineering might yield the following reasoning: (i) The possibility of partner infidelity was a recurrent adaptive problem. (ii) In the EEA, jealously would motivate behaviour that would have discouraged, limited, or prevented infidelity. (iii) Those with the trait of jealousy would have enjoyed a distinct fitness advantage over those who didn't. It would be adaptive. Hence, the trait could have been favoured by natural selection, and so preserved, refined, and spread.

This leads to understanding jealousy as an adaptation functioning to solve the adaptive problem of counteracting threats to a valued relationship. More precisely, Buss conjectured that, 'Jealousy (1) is an emotion designed to alert an individual to threats to a valued relationship, (2) is activated by the presence of interested and more desirable intrasexual rivals, and (3) functions, in part, as a motivational mechanism with behavioral output designed to deter "the dual specters of infidelity and abandonment"' (Buss and Haselton 2005: 506).

So we've moved down the methodological chain, reasoning from form to function. But crucially we can move up the methodological chain again, reasoning anew from function back again to form. Hypothesizing about selection pressures in the EEA led Buss to a further hypothesis: that there should be sex differences in jealousy. He predicted that:

> 'Men and women differ psychologically in the weighting given to sexual and emotional cues that trigger jealousy, such that (i) *men more than women* become upset at signals of sexual infidelity... and (ii) *women more than men* become upset at signals of a partner's emotional infidelity' (*ibid*.: 506, emphasis original)

Men and women faced different adaptive problems, and hence different selective pressures, with respect to infidelity. A man's fitness risks being severely compromised if his partner engages in cuckoldry. If the cuckoldry leads to offspring, and the man remains in

the dark about the cuckoldry, the man would be spending his precious resources on another man's child. In contrast, in the ancestral past a woman's fitness risks being severely compromised if her partner diverts resources to another woman. Her man could have multiple sexual liaisons with women, but this fact alone wouldn't threaten her fitness; only if he became emotionally involved with another woman would her fitness be at stake. Hence, sexual fidelity is a greater threat to men than it is to women, while emotional infidelity is a greater threat to women than it is to men. Hence, sexual infidelity should upset men more than women and emotional infidelity should upset women more than men.

More precisely, evolutionary psychologists have postulated at least thirteen sex-differentiated design features (Buss and Haselton 2005). These include the following novel predictions. Relative to men, women demonstrate greater memory recall of indications of emotional infidelity. In contrast, relative to women, men demonstrate greater memory recall of indications of sexual infidelity. Furthermore, men find it harder to forgive a sexual infidelity than they do to forgive an emotional infidelity and are more likely to end a relationship following the discovery of infidelity if the infidelity is a sexual infidelity rather than an emotional infidelity.

Then the final step: the various novel predictions were tested. In a number of studies, predicted sex differences in jealousy have been experimentally supported (e.g. Buss *et al.* 1992; Buss *et al.* 1999; Shackelford, Buss, and Bennett 2002; Becker *et al.* 2004) and continue to be supported (e.g. Miller and Maner 2008; Schützwohl 2008).

So having begun reasoning from the present to the past, from form to function, we need not stop there, we need not only reverse engineer what the psychological trait could have been selected for – we can also reason from the conjectured past to the present, to predicting novel design features of the already known trait.

This has two consequences. First, even accommodation of some given trait can lead to novel predictions. If psychological trait *T* functions to solve adaptive problem *X*, then we should find certain basic design features in trait *T* for solving problem *X*. Second, a principle emerges: for established psychological traits, traits we already know about, accommodation precedes prediction. We'll be returning to this principle in the next chapter, when it'll be brought to bear on a recent challenge to evolutionary psychology.

Furthermore, in moving up and down the methodological chain, we might also generate novel predictions about the design features of other extant psychological mechanisms that are required for the mechanism under analysis to solve the adaptive problem - or even predict new mechanisms entirely.

3.3 Specific heuristics

We should not confuse something being useful with something being easy. Schulz (2011: 217) claims that evolutionary psychologists 'think that important discoveries about how our minds work can be especially *easily* made once we consider the issue from an evolutionary biological point of view' (emphasis mine). This is not quite right. Indeed, I've never come across such a claim in the literature

Hypothesizing to the point of predictability and testability, rigorously applying engineering thinking to establish function-form match, requires a number of resources. The following specific resources guide evolutionary psychologists in their pursuit of adaptationist thinking.

3.3.1 Historical knowledge

A number of resources are available for reconstructing past ancestral conditions: the archaeological record, hunter-gatherer studies, evolutionary anthropology research, and other evolutionary sciences. There's a lot of information about the past available that enables one to think strategically about possible and probable adaptive problems. Cosmides and Tooby (1997b) note:

> 'Those who actually work across disciplines on the inferential reconstruction of the past realize that we know with certainty thousands of important things about our ancestors – many of which can be useful in guiding psychological (or e.g., medical) research: Our ancestors nursed, had two sexes, hunted, gathered, chose mates, used tools, had color vision, bled when wounded, were predated upon, were subject to viral infections, were incapacitated from injuries, had deleterious recessives and so were subject to inbreeding depression if they mated with siblings, fought with each other, lived in a biotic environment with felids, snakes, and plant toxins, etc. It is a

> certainty that our ancestors lived in a world in which the principles of kinematic geometry governed the motions of objects (a set of facts that allowed Roger Shepard to develop his theories about the evolutionary foundations of psychophysics that, in part, won him the National Medal of Science). It is equally a certainty that hominids had eyes, looked at what interested them, and absorbed information about what they were looking at, making eye-gaze direction informative to on-lookers.'

It's easy to snigger at some items on this list (surely it's obvious that there were two sexes and that gravity was around before Newton?), but this, I believe, misses the fundamental point: many of the things we take for granted in post-industrial society, like finding food, finding a mate, and so on, were major adaptive problems, indeed they remain adaptive problems, and they are known with a certitude to have been adaptive problems – problems likely, the evolutionary psychologist is betting, requiring sophisticated solutions. It's a testament to the success of our adaptations that such a list might sound banal today.

3.3.2 Middle-level theories

Middle-level evolutionary theories, theories of specific selection pressures, theories such as parental investment theory, reciprocal altruism, and sexual strategies theory, provide a further guide and inspiration for hypothesis generation. As Machery (forthcoming) notes, because they are broad, applying to the evolution of numerous taxa, middle-level theories say little about what specific traits might have been selected for by the selective pressures they specify.

Hence, middle-level theories alone do not offer straight deductions into hypotheses. But as Nancy Cartwright has pointed out over the years, even in physics, applications of a scientific theory are often not simple acts of deduction. Applying a theory to real-world situations takes acts of creativity; Nobel prizes are not awarded for just pressing a button on a conceptual vending machine, as if theory just shoots out applications (models) with a little deductive effort.

Although middle-level theories will not give us straight deductions, they can, nevertheless, help guide and build hypotheses. On the strength of kin selection, which favours altruism between genetic relatives, Daly and Wilson (1980) proposed that we have a psychological adaptation for discriminative parental solicitude. Parents will tend to experience parental

feeling for their natural offspring and will be moved to provide care for them. Unfortunately, stepchildren will be less likely to trigger this adaptation. Hence, the infamous 'Cinderella effect'.

The so-called 'good genes' view of sexual selection has helped generate a number of hypotheses and novel predictions. This view suggests selection has favoured mate preferences for healthy individuals, due to inclusive fitness benefits associated with mating with such individuals – if so, attractiveness judgements should reflect health judgements (Gangestad 2000; Jones *et al.* 2001).

Low fluctuating asymmetry is a possible visual marker of underlying health. Indeed, it might reflect an individual's heritable ability to maintain good health in the face of developmental shocks. During development, genetic perturbations, such as deleterious recessives and inbreeding, and environmental perturbations, such as reduced nutrition, disease infection, parasitic infection, toxicity exposure, can give rise to fluctuating asymmetries, deviations from symmetry in bilateral traits that are, on average, symmetrical at the population level (Swami and Salem 2011). Individuals who are better able to maintain developmental stability in the face of such genetic and environmental perturbations tend to have low levels of fluctuating asymmetry. Low levels of fluctuating asymmetry are correlated with health (for example, Waynforth 1998, found lower fluctuating asymmetry successfully predicted lower morbidity in a population sample in Belize), while higher levels of fluctuating asymmetry are associated with certain disabilities. Accordingly, selection might have favoured mate preferences for lower levels of fluctuating asymmetry.

This line of reasoning has led to a harvest of hypotheses and novel predictions on the relationships between symmetry and mating preferences. For example, high symmetry male faces are judged to be more attractive than low symmetry male faces (Perrett *et al.* 1999). Men with high symmetry experience greater mating success than less symmetrical men, begin sex at an earlier age, and gain quicker sexual access to new partners (Gangestad and Thornhill 1997).

Furthermore, as evolutionary psychologists have established, women's sexual preferences change across the ovulatory cycle. For example, during the fertile phase of their ovulation cycle, women particularly prefer more masculine male faces (Penton-Voak *et al.* 1999; Penton-Voak and Perrett 2000) and deeper male voices (Puts 2005). On the strength of ovulatory cycle research and symmetry research, Gangestad, Thornhill, and Garver-Apgar

(2005) hypothesized that women possess an adaptation to be attracted to symmetrical men when near ovulation. From this adaptationist hypothesis, a novel prediction follows: the benefits of extra-pair mating with symmetrical men outweigh its costs only for women with high fluctuating asymmetry partners. Hence, the ovulatory cycle shift in women's extra-pair desires and flirtation should be strongest for women with high fluctuating asymmetry partners.

What if one is interested in investigating a topic where no obvious middle-level theory is applicable, or where a middle-level theory is only distantly related or connected to the topic at hand? Can one still produce hypotheses, and testable hypotheses at that? Yes, but doing so requires a greater degree of conjecture than usual, as well as possibly submitting a greater range of hypotheses to testing.

A simple and illustrative example of this is Buss and Shackelford (1997). Buss and Shackelford examined mate retention tactics in marriage from an evolutionary perspective. One aspect they considered was mate retention behaviour in marriages where there is uneven physical attractiveness. Buss and Shackelford acknowledged that evolutionary considerations are such that, in this area, one could yield opposite hypotheses concerning the effect of perceived unequal attractiveness on mate retention behaviour.

One evolutionary psychology hypothesis that can be formulated is that women in marriages where the man is perceived to be more attractive than the woman will devote more effort to mate retention than women who are in marriages where the man is perceived to be as equal or less attractive than the woman. Here, the cost of losing a more attractive partner, and the increased probability of losing a more attractive partner, is weighed heavily when constructing the pressures that would result in adaptive behaviour regulating mate retention behaviour.

The alternative evolutionary psychology hypothesis that can be formulated is that women in marriages where the man is perceived to be more attractive than the woman will devote less effort to mate retention than women who are in marriages where the man is perceived to be as equal or less attractive than the woman. Here, a different set of considerations is being weighed: men are capable of fathering children in different relationships simultaneously; if a woman engages in mate retention behaviour with a highly prized male who desires to be in more than one relationship then there is a risk that the woman could lose that highly prized man entirely; consequently, there is a choice between having the

partial attention and resources of a highly valued male versus the full attention and resources of a less valued man. Interestingly, Buss and Shackelford, after outlining various evolutionary hypotheses and before reporting on research methodology and results, mention the following:

> 'One of the goals of the current study was to pit these competing evolutionary hypotheses against each other, and, given the relative novelty of evolutionary psychological analyses, it is perhaps worth commenting briefly about this metatheoretical issue. Much of science involves testing predictions derived from competing theories, and, as in the rest of science, the empirical tests are the final arbiters. Sometimes, the fact that alternative evolutionary accounts can be generated is derided, with accusations of telling "just-so stories." [...] The key point is that testing competing evolutionary hypotheses, as in the present case with competing hypotheses about women's mate retention efforts, is merely part of the standard paradigm science.' (Buss and Shackelford 1997: 348)

So such research is still legitimate, and any surviving hypothesis from a range of rival hypotheses tested might prove to be a first step towards a more rigorous understanding of the topic in question.

3.3.3 Cues

Cues - tactile cues, olfactory cues, audio cues, vocal cues, static visual cues, dynamic visual cues, and so on - can have a surprising amount of reproductively relevant information. For example, dance reveals symmetry, especially in young men (Brown *et al.* 2005). There are vocal cues of ovulation in human females (Bryant and Haselton 2009). And during the fertile phase of their ovulation cycle, women particularly preferred the scent of men who were low in fluctuating asymmetry, which, as discussed previously, is a marker of heritable fitness (Gangestad and Thornhill 1998; Thornhill and Gangestad 1999).

Some psychological adaptations can receive surprising information and cues, surprising in the sense that we'd ordinarily not consider the cue type to be relevant to the adaptive problem at hand. Thinking systematically, therefore, about cues can lead to fresh discoveries. For example, research on disgust usually focuses on visual and olfactory detection cues. Oum, Lieberman, and Aylward (2011) investigated whether tactile cues

might also play a role in pathogen detection. In their study, participants briefly touched and then rated stimuli. Results show that participants rated stimuli resembling biological consistencies as more disgusting than stimuli resembling inanimate consistencies, suggesting that tactile cues provide information for disgust-related processes.

3.3.4 Developmental calibration heuristics

Belsky, Steinberg, and Draper (1991), which we briefly looked at in chapter 2, postulated the hypothesis that 'a principle evolutionary function of early experience' is to harvest understanding of the available environment, of the trustworthiness of others and so on. Again, this follows the logic of adaptationist hypothesizing in psychology: if *X* functions to do *Y*, then it should have certain design features. In this case, if the function of early experience is to harvest environmental information, this should lead to alternative developmental calibrations. Since long-term mating strategies will tend to be less effective in unstable environments versus stable environments, early cues that signal that the environment is unstable should lead to a propensity for short-term mating strategies, and vice versa for the signalling of a stable environment.

Indeed, there's a growing literature investigating whether early cues signalling a stressful or suboptimal environment might lead an individual to developing a strategy favouring short-term gains and early reproduction. For example, Nettle, Coall and Dickins (2010) report that among British women being born small for gestational age increased the likelihood of early reproduction. And Frederick (2012) found that birth weight predicts scores on the Attention-Deficit/Hyperactivity Disorder (ADHD) Self-report Scale, as well as attitudes towards casual sex, in college men.

3.3.5 Contextual calibration heuristics

Good design often demands good flexibility. As we saw in chapter 2, adaptations have decision rules that evaluate environmental inputs and produce different responses accordingly. Hence, evolutionary psychology can make context-sensitive predictions about the operation of psychological mechanisms.

For example, concerns about levels of attractiveness in potential mates should be more advantageous for those living in environments subject to high pathogen levels than for those living in environments subject to lower pathogen levels. On the strength of this, Gangestad and Buss (1992) investigated the relative importance of mate attractiveness in twenty-nine cultures. As predicted, they found that people living in high level pathogen

environments valued attractiveness more than did those living in lower level pathogen environments.

More recently, Buss and Duntley (2011) examined the context-dependence of intimate partner violence. Buss and Duntley hypothesize that intimate partner violence is often directed towards solving one or more of nine adaptive problems: mate poaching, sexual infidelity, mate pregnancy by an intrasexual rival, resource infidelity, resource scarcity, mate value discrepancies, stepchildren, relationship termination, and mate reacquisition. Buss and Duntley suggest that violence will be selectively deployed in ways highly contingent on personal, relationship, social, economic, and cultural conditions. 'The key point is that an evolutionary lens has heuristic value for predicting the circumstances in which intimate partner violence is likely, and even the particular forms it is likely to take.' (*ibid.* 415)

3.4 Testing predictions

It's worth briefly looking at the range of experiments evolutionary psychologists can deploy in order to confirm or disconfirm predictions. Evolutionary psychologists are, unsurprisingly, psychologists, and hence they use, devise, and execute standard psychological experiments to confirm or disconfirm their novel predictions.

A standard method of confirming or disconfirming people's predicted responses and preferences is to survey them, whether by questioning them or giving them questionnaires. Such questionnaires can be crafted to investigate short-term mating preferences using various variables. For example, in order to test a prediction that men are more inclined to novelty than females, Symons and Ellis (1989: 133) crafted the following question:

> 'If you had the opportunity to copulate with an anonymous member of the opposite sex who was as physically attractive as your spouse but no more so and as competent a lover as your spouse but no more so, and there was no risk of discovery, disease or pregnancy and no chance of forming a durable liaison, and the copulation was a substitute for an act of marital intercourse, not an addition, would you do it?' (Symons and Ellis 1989: 133, as quoted in Cartwright 2008: 250)

Questionnaires can also be crafted to investigate long-term mating preferences. For example, people can be asked to rank long-term mate characteristics such as 'good financial prospects', 'intelligence', height', identify minimum characteristics required of a long-term mate, or identify ideal characteristics in a long-term mate.

Creating and administrating questionnaires has obvious strengths: the tests are simple and relatively cheap, making them expedient to use cross-culturally. However, questionnaires have an obvious weakness. It's a commonplace that people's stated preferences don't necessarily match their actual preferences. Someone might state a preference for such and such characteristics or set of characteristics, but in fact a different set of characteristics or even entirely opposite characteristics trigger attraction. This mismatch between stated and actual preferences can be due either to lack of personal understanding, often due to personal identity narratives rationalizing away certain preferences, or due to an expectation to conform to certain social norms or stereotypes. This is not to say that self-reports have no utility, but that they should be used with care. And, where possible, other types of test should be utilised to test predictions.

In attraction research, another way to test preferences is to investigate personal dating adverts, in which people describe both their own characteristics and the characteristics they seek in a potential mate (Pawlowski and Dunbar 1999a, 1999b; Pawlowski and Koziel 2002; de Sousa Campos, Otta, and de Oliveira Siqueira 2002). One advantage of investigating personal dating adverts for information on mating preferences is that such resources are unlikely to have an expectation bias skewing stated prefaces: these dating adverts are often anonymous. Furthermore, the adverts represent an investment of effort, and thus reflect actual attempts to find actual partners. Nevertheless, personal dating adverts still have drawbacks. The constituency of those who use dating adverts – those who submit and respond to such adverts – might be unrepresentative of the population at large. Furthermore, stated preferences in dating adverts can again diverge from actual preferences, not because of expectation bias, but due to personal identity narratives rationalizing away certain preferences

An alternative to investigating self-reported preferences is to investigate actual short- and long-term mating, relationship, and activity patterns. This can be done by mining statistical data and archives. For example, if males prefer females who are younger than them and females prefer males who are older than them, this should be mirrored in marriage statistics. Fast-moving technological developments might elevate digital archival data as an

increasingly importance source for testing evolutionary psychological hypotheses. For the first time in history, vast amounts of social data exist in searchable devices. Such endless seas of data are ripe for hypotheses making predictive waves over them. Indeed, Miller (2012) advocates engaging with smartphones seriously as research tools. Miller believes that smartphones have the potential to not only collect vast amounts of data relevant to evolutionary and non-evolutionary psychology from large and diverse constituencies around the world but to also make such data relatively easily available to researchers. 'If participants download the right "psych apps," smartphones can record where they are, what they are doing, and what they can see and hear and can run interactive surveys, tests, and experiments through touch screens and wireless connections to nearby screens, headsets, biosensors, and other peripherals.' (*ibid*.: 221) Miller provides numerous examples where such technological developments can provide new opportunities for testing novel predictions. For example, Haselton and Gangestad (2006) predict husbands will increase mate guarding activity when their wives are most fertile. Call logs could give firm data on whether husbands are indeed performing increased mate guarding activity when their wives are most fertile - for example, by texting and calling their wives more than they would otherwise. Another example is that GPS data could reveal whether peak-fertility women go out more often to bars and clubs, in line with evolutionary psychology predicting that females at peak fertility increase mate searching (Haselton and Gangestad 2006).

A further method of confirming or disconfirming people's predicted responses and preferences is to use physiological measures. When asked to imagine either sexual or emotional infidelity on the part of a mate, Buss *et al.* (1992) found that men showed elevated heart rate and electrodermal activity (EDA) to imagined sexual infidelity than to imagined emotional infidelity. The reverse pattern obtained for women. Instruments exist to measure facial muscle activity, allowing the collection of a large body of reliable empirical data on facial expressions (Schmidt and Cohn 2001). Furthermore, on the strength of prior research, which established a positive relationship between men's testosterone levels and infidelity (in particular, that men with relatively high testosterone levels report sustained interest in sex beyond their current committed relationship, a greater number of sex partners, and a higher number of extra-marital affairs), O'Connor, Re, and Feinberg (2011) predicted that women would attribute high infidelity risk to masculinised men's voices. Testing this prediction was possible in virtue of the PSOLA

method, a standard technique of voice manipulation that selectively manipulates frequencies and harmonics.

3.5 From simplicity to complexity

Moving down and up, or up and down, the methodological chain, between ultimate and proximate questions, is not a once-and-for-all event. I see it as a continuing pattern of activity. One can move up and down the methodological chain multiple times, sometimes in small ways, sometimes in large ways.

Moving up and down the methodological chain allows the instigation and integration of new research. A hypothesis can lead to a cascade of further research, some trying to corroborate it, others trying to undermine it, and the hypothesis can be repeatedly revised until the data hardens – or it can be abandoned. For example, one can make a claim that some physical feature will be attractive across cultures. Cross-cultural research would then be trigged, perhaps offering corroboration from some cultures, perhaps also notable exceptions. The exceptions might well signal that the proposed adaptation has an important developmental calibration. For example, perhaps a society that fails to fit expectations has lower or higher levels of parental investment than the other cultures. Hence, it might well be that the calibration of the adaptation is sensitive to parental investment. Conjecturing why selection would have favoured such a design feature can then give us a better picture of the EEA of the adaptation, which in turn could lead to further novel predictions about other design features of the trait (and, possibly, other traits).

Notice the implication of this: when first formulated, evolutionary psychology hypotheses are not the last word on a given trait, but the first step in uncovering its design, features of the trait that non-adaptationist perspectives are blind to. Hence, crucially, although evolutionary psychology hypotheses might start off as ‘simple’, they can progressively become more complex, progressively mirroring the adaptations they’re seeking to model.

Those with a naive falsificationist temperament might start choking at this, lambasting that such hypotheses can always be adjusted to make them fit ever increasing waves of inconsistent data. Since such falsificationists are an endangered species, I had better reassure them. As we saw in chapter 2, adaptations are exquisitely designed, capable of generating great variability in output. Hence, it is only to be expected that initial

evolutionary psychology hypotheses, based on limited data samples, will probably be capable of further modification and refinement. This is not some shoddy move to save the hypothesis from falsification. Even refined hypotheses should generate further novel predictions – and if a hypothesis is just continually being reworked to save it from an avalanche of awkward data, continually failing in its novel predictions, then it'll be abandoned. This, I submit, is no different to any other hypotheses-driven empirical science.

3.6 Some common objections

A number of objections have been raised against evolutionary psychology methodology. We'll be considering several as the thesis progresses, but for now, we'll tackle three common ones.

3.6.1 The fine grain problem

Evolutionary psychologists hypothesize domain- or problem-specific adaptations. But how do we individuate adaptive problems? How do we characterise them? The problem of how to correctly individuate domains that specialised adaptations are purported to operate on is what Sterelny and Griffiths (1999) call the 'grain problem'. How fine or coarse 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 trivial. 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.

The first response is to highlight that this problem is ubiquitous to functional hypotheses, not unique to evolutionary functional hypotheses. As Machery (forthcoming) notes, psychologists, who are in the business of investigating psychological traits, often

characterise these traits functionally, and hence their efforts are equally subject to the problem of individuation. Nothing new here.

The second response is to stress that these possibilities can be tested. Is predator threat a single adaptive problem, or one of a series of adaptive problems, with different predator threats creating different adaptive problems? Well we can put these different possibilities to the test. These different options should lead to different design features. For example, a psychological adaptation dealing with predator threats in general should have different design features to a psychological adaptation just dealing with a subset of predator threats. Hence, instead of being seen as an embarrassment, the issue of individuation, which often arises when refining a successful hypothesis, can be seen, and I believe should be seen, as a heuristic strength.

3.6.2 No constraints objection

Sometimes it's possible to hypothesize alternative adaptive solutions for a given adaptive problem. This shouldn't be seen as surprising. There is a degree of contingency present in the evolution of traits, and alternative evolutionary trajectories are possible. This also shouldn't be seen as problematic. If alternative adaptationist hypotheses for a given target are possible this should be articulated to the point of generating rival, mutually exclusive predictions across one or more sets of observable measurements. Laying out rival predictions, specifying what would count as evidence for or against these rival hypotheses, finding the data, and then evaluating the hypotheses – that's what a hypothesis-driven empirical science should do.

Evolutionary psychologists themselves are not blind to this. For example, Alcock and Crawford (2008: 37) claim that evolutionary psychologists recognise that sometimes several adaptive solutions can be hypothesized for a given trait and that they test them accordingly:

> 'These articles often have considered several different tentative hypotheses on the phenomenon, a reflection of the fact that adaptationist researchers can often think of multiple explanations for this or that trait. When there is more than one hypothesis to consider, the need for testing in order to reject incorrect ideas is obvious.'

A concern might now arise. Evolutionary psychology is fertile. The trouble, one might venture, is that it's too fertile. Perhaps its strategies and methods can generate of a large

number and range of hypotheses for any given adaptive problem. There needs to be a reasonable limit on the number of hypothesized adaptive solutions for a given adaptive problem. If this is not so, if the permutations are too great, then focusing on adaptive problems and solutions will not successfully reduce and constrain research space. Evolutionary psychology's heuristic value will be wholly compromised.

In the sceptical literature, this concern is often 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' (Richardson 2007: 65). Gray, Heaney, and Fairhall (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.

The concern is understandable but the position that sceptics reach is untenable. In practice, evolutionary psychology's heuristics allow, at most, only a limited range of adaptive solutions to be generated for a given adaptive problem – certainly a range suitable for experimentation. Think of the adaptive problem of infidelity. According to evolutionary psychology's heuristics, a well-designed solution to this problem is jealousy. Perhaps a clever individual could use the heuristics to generate an alternative proposal, but certainly not a runaway list of possibilities. Indeed, if the heuristics allowed for many proposals to be generated for a given adaptive problem, we would see this in the evolutionary psychology literature. Likewise, regarding the finer details of how any proposed psychological adaptation is calibrated, several design possibilities might present themselves, but again the range is well within the acceptable parameters of experimentation.

Indeed, crucially, since function and form should be well-matched, adopting an engineering perspective allows one to quickly identify and discount a large number of possibilities. For example, Symons (2008) asks us to consider a situation where someone proposes that the human female orgasm is an adaptation designed to promote conception by enhancing sperm retention, and that the human female organism is designed to achieve this goal only when the organism occurs close to the time of the male partner's ejaculation. Adopting an engineering perspective on this purported adaptation, a psychological adaptationist might ask

> 'why a series of coordinated muscle contractions in a female body that mimic the muscle contractions of ejaculation in a male body would miraculously turn out to be well designed to achieve an entirely different goal. That is, shouldn't we expect the design of a female device whose function is to promote conception by retaining sperm to differ in important ways from the design of a male device whose function is to propel semen from the body, for the same reasons that we expect a device designed to pump blood to differ from one designed to digest food?' (*ibid.:* 9)

Furthermore, adds Symons, selection would favour males who observe the following behavioural rule: 'thrust until your partner orgasms, then immediately ejaculate'. Since the form and the function are not well matched, and indeed the prediction doesn't obtain, the heuristics rule out this proposal as a serious contender.

Sceptics freely talk about 'unconstrained speculation' but fail to cite any specific areas where evolutionary psychology hypothesis generation is running amok, nor do they make the rather obvious and deeply subversive move of using its heuristics to generate a runaway list of hypothesized adaptive solutions for some given adaptive problem. That would be a decisive blow against evolutionary psychology as a heuristic. That such a move is not forthcoming anywhere in the literature is, I believe, very revealing.

Although there will be some bets that fail to pay off, and some blind alleys, this is unavoidable. Heuristics reduce the size of the research space, but researchers nevertheless still need to rely on a measure of trial and error within the constrained space. The initial practice of evolutionary psychology spreads far and wide, accommodating as much psychological phenomena as possible, a theme I return to in the next chapter. Some of the initial outreaches will be fruitless, others more promising. The initially promising outreaches provide us with some fixed points that can anchor adaptationist hypotheses. Further research questions can be raised and answered, testable questions about calibration, contextual sensitivities, and the level of the grain, leading to an unrelenting examination of the design details that constitute psychological adaptations**.**

3.6.3 No stable problems objection

Sterelny (1995: 372) claims that 'There are no stable problems to which natural selection can grind out a solution.' This argument also appears in Sterelny and Griffiths (1999). The

thought is that evolutionary arm races destabilise adaptive problems, and this problematizes evolutionary psychology.

> '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.' (Sterelny 1995: 372)

Sterelny has made an incorrect inference. The consequence of evolutionary arm races is not that adaptations cannot evolve; rather, it means we should expect them to be richly calibrated, as well as to expect the coevolution of antagonistic adaptations. The more two evolutionary opponents, such as males and females, can second-guess each other, the more complex some of their psychological adaptations become. Just as it seems inescapable that organisms are well adapted to particular environments, so too it seems inescapable that certain adaptations are reinforced and made more complex by repeated bouts of competition with antagonistic coevolving rival mechanisms.

Furthermore, Sterelny is wrong to claim that evolutionary psychology cannot reflect this interactive character of evolution. It can and does. As before, what Sterelny sees as a problem for the programme is actually a heuristic strength. For example, human females have concealed ovulation. Thanks to evolutionary psychology, we now suspect, and have some evidence, that men can unconsciously detect ovulation cycles. During fertile periods, women unconsciously increase mate search activities; and men unconsciously counter-act by increasing mate guarding activities. It's a dialectical dance, a Darwinian dance, one performed daily below the thresholds of consciousness.

Duntley and Shackelford (2012) propose that an antagonistic, coevolutionary arms race has produced adaptations to strategically exploit others and defences to avoid the costs of victimization. They hypothesize that adaptations to damage status co-evolved with victim defences against status damage; adaptations for theft and cheating co-evolved with victim defences against theft and cheating; and adaptations for violence co-evolved with victim defences against violence.

Indeed, one can postulate feedback within dyadic antagonistic coevolution. For example, Duntley and Shackelford (2008) hypothesize that adaptations that produce criminal behaviour create selection pressure for the evolution of counter adaptations in victims, which in turn create novel selection pressures for the evolution of counter–counter adaptations in criminals. Similarly, Trivers (2011) hypothesizes that because selection has led to deception detection, there has been selection for self-deception so as to better limit and hide deception cues from others. In other words, because people have adaptations to detect deception, we self-deceive ourselves in order to better deceive others.

Furthermore, we can propose not just dyadic antagonistic coevolution but also triadic antagonistic coevolution. Duntley and Shackelford (2008) note that when three individuals have conflicting interests in the same adaptive problem domain, a refinement in one individual's adaptation can simultaneously create new selection pressure on the other two individuals. The counter-adaptations that evolve in each of the other two individuals as a result can then create further selection pressures.

3.7 Conclusion

We've identified examples of adaptationist thinking driving psychological research, research that delivers observational findings beyond what we already know, beyond the reach of the prevailing orthodoxy. Reasoning between ultimate and proximate questions, between function and form, asking what adaptive problems ancestral populations faced, considering what would constitute good design solutions to those problems, enables researchers to (i) discover new design features of extant psychological traits, and (ii) discover hitherto unknown psychological traits. This is heart of evolutionary psychology. This is what adaptationist hypothesizing can do for psychology.

The two broad heuristic strategies, which can work in tandem, together with specific heuristics – historical knowledge, mid-level evolutionary theories, possible cue inputs, and possible developmental and contextual calibrations – have enabled an impressive array of hypotheses and predictions to be formulated. Initial evolutionary psychology hypotheses aim, or should aim, not for the last evolutionary word on a given phenomenon, but the first. They are in constant adjustment – both with the research programme's own findings and findings from adjacent research programmes and disciplines, a theme I develop further in chapter 6. If this is done, this should generate sophisticated hypotheses, as well as

generate progressive increments to our understanding of psychological and social phenomena. And this is, of course, the normal course of science.

Since evolutionary psychology operates primarily in the context of discovery, questions about indeterminacy of proposed adaptationist hypotheses are also rendered relatively unproblematic, as these alternatives can be tested too. If raising the issue of indeterminacy is to mean anything, it is to vigorously test a range of adaptationist hypotheses, not to abandon the research programme wholesale. 'Baby' and 'bath water', anyone? The possible permutations of adaptive problems, solutions, and calibrations are not too numerous to investigate; they are well within the range suitable for testing and experimentation.

Chapter 4

Predictions or accommodations?

Evolutionary psychology's theoretical and methodological framework allows the generation of novel predictions. But is evolutionary psychology practice characterised by novel predictions or by accommodations? In §4.0 I outline a recent challenge to evolutionary psychology, one purporting to establish that, despite its heuristic potential, evolutionary psychology is in practice primarily a programme of accommodation. In §4.1, for the sake of argument, I grant the characterisation, but argue accommodation often characterises the early activities of research programmes. In §4.2, I question the characterisation: evolutionary psychology practice is significantly more heuristic than the challenge recognises. Nevertheless, in §4.3 I decline to generalise from this. Establishing that evolutionary psychology is capable of generating novel predictions and has done so in practice is one issue; establishing that this is systematic practice is another issue. Substantiating claims about the latter issue requires systematic, empirical evidence, which we currently lack.

4.0 Schulz's challenge

Unlike most critics of evolutionary psychology, Schulz (2011) is alert to the heuristic potential of evolutionary psychology. Nevertheless, although he acknowledges it's possible for evolutionary psychology to be heuristic, and he cites Csibra and Gergely (2009) as an example of one such instance, he contends that this in fact is very rare, and so we shouldn't overemphasize its importance.

> 'this point must not be overemphasised – in fact, far from being a common occurrence, heuristic applications of evolutionary theory in psychology are actually quite a rarity. While such occurrences do exist, as yet, they are still in a minority: *most* cases of evolutionary psychological research – and, in fact, virtually all of the work of the Santa Barbara ('EP') School of evolutionary psychologists – employ evolutionary theory only to *explain* a known set of phenomena, not to lead us to *discover* these phenomena.' (Schulz 2011: 232; emphasis original)

This is a big empirical claim to make. Yet in his paper, Schulz considers only a few examples. So how can such a case be made? Schulz's strategy is straightforward: first, debunk the flagship example of past-to-present novel prediction as actually being a case of present-to-past accommodation; second, point out that a lot of well-known evolutionary psychology hypotheses are also accommodations of social phenomena we already know; third generalise this into a general claim about evolutionary psychology practice.

The celebrated example Schulz has in mind is the cheater detection hypothesis. The cheater detection hypothesis is often submitted as being the leading, textbook example of a successful hypothesis generated by heuristically reasoning from the past to the present. Cosmides and Tooby (1992) reasoned that the evolution of co-operation required the ability to detect cheaters, those who violate social contracts. More precisely, they hypothesized selection would favour the evolution of a mechanism dedicated to detecting, and avoiding future co-operative effort with, cheaters – a cheater detection mechanism, a psychological adaptation for representing and computing social exchanges. Since the purported adaptation functions to reason effectively about social contracts, the mechanism would be activated only by content cues signally social exchange.

In order to vindicate this hypothesis, Cosmides and Tooby appealed to the Wason selection task (Wason 1966), an experiment designed to investigate reasoning about conditionals. In the task, subjects are presented with a conditional rule, either in a descriptive form ('If something has property *P*, then it has property *Q*') or a deontic form ('If something has property *P*, then it should have property *Q*').

An example of the descriptive form of the Wason selection task is the card-problem experiment. A rule is presented: 'if the letter 'D' is on one side of a card, the number '3' is on the other'. Four double-sided cards are placed on a table. For each card, a letter appears on one side, a number on the other. Subjects can only see one side of each card: 'D', 'F', '3' and '7'. Subjects are invited to identify which of the four cards must be turned over in order to establish whether the rule is true or false. Subjects should identify *P* and *not-Q* cards as falsifying conditional statements of the form 'if *P*, then *Q*'. Hence, only the *P* card ('D') and the *not-Q* card ('7') need to be turned over. Typically, performance on this version of the Wason selection task is poor: on average, only around 10% of subjects correctly identify that the *P* and *not-Q* cards are the only cards that need to be turned over.

However, subjects perform significantly better in deontic forms of the Wason selection task. In Griggs and Cox (1982), for example, subjects were asked to imagine checking a bar to establish whether the following conditional rule was being obeyed: 'if a person is drinking beer, he or she must be over 20 years old'. Four cards conveyed information about people at the bar: 'beer', 'coke', '25 years old', '16 years old'. Typically, performance on this version of the Wason selection task is very good: on average, around 75% of subjects correctly choose the *P* card ('beer') and the *not-Q* card ('16 years old').

Cosmides and Tooby argue that the pronounced difference in performance between the card-problem experiment and the bar-problem experiment is due to the latter experiment having social exchange cues that trigger the cheater detection mechanism to reason effectively over the conditional rule ('if you accept the benefit of drinking beer, you must satisfy the age requirement'), whereas the former experiment has no such cues, and so fails to trigger the effective reasoning.

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 hypothesized: 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, observe that Cosmides's 1985 PhD thesis, where the cheater detection hypothesis was first proposed, is titled 'Deduction or Darwinian algorithms? An *explanation* of the "elusive" content effect on the Wason selection task' (emphasis mine).

Schulz (2011: 222) is surely right when he says that this,

> 'quite clearly shows that evolutionary theory is here applied mostly in an *explanatory*, 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.' (emphasis original)

The obvious response here is to point out how much research has been sparked by Cosmides and Tooby's efforts. Before Cosmides and Tooby, the Wason selection task was an obscure anomaly, 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; emphasis original).

Fair enough. 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. The implication is clear; and forgoing discussing these examples, Schulz leaps to the end game of his paper – 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) So, yes, while '*merely possible*, *fictional* evolutionary psychology' is heuristic, *'currently practiced*, *actual* evolutionary psychology' is not (*ibid.*: 218, emphasis original).

4.1. Dismissing the challenge

First of all, let us suppose Schulz's characterisation is right. Suppose that despite its potential for rolling out novel predictions, evolutionary psychology in practice primarily churns out accommodations. Plausible, intriguing adaptationist rationales for a wide spectrum of social phenomena - but still accommodations. Would this discredit evolutionary psychology's heuristic value?

4.1.1 Why is evolutionary psychology (supposedly) primarily accommodating phenomena?

Schulz doesn't explain why evolutionary psychology in practice mainly accommodates social phenomena rather than generating novel predictions about social phenomena. At first this oversight is somewhat puzzlingly – the question naturally arises after the challenge has been set. Yet a moment's attention reveals that pursuing this question quickly undermines the very challenge.

Practised evolutionary psychology is primarily accommodation either because its methodological framework is too weak or defective to furnish novel predictions or because of contingent, sociological reasons, such as lack of interest or skill or resources on the part of the researcher.

Under Schulz's own characterisation, the first option is untenable. Schulz concedes that theoretical evolutionary psychology can be heuristic. Furthermore, he even acknowledges practiced evolutionary psychology is heuristic in a minority of cases. Clearly, therefore, the problem is sociological. We just so happen to live in a world where the heuristics of the research programme are not being practiced as extensively as they could. Sociological failings of research practitioners should not necessarily speak against the theoretical and methodological foundations of the research programme. The heuristic value of the research programme should not be held hostage to the contingent skills, resources, and interest of individual practitioners.

4.1.2 The normal arc of research programmes

Furthermore, evolutionary psychology follows the usual arc, the usual career curve, of scientific endeavour. Exploring what facts a research programme could conceivably cover is what we might expect a new research programme to do; it seems perfectly reasonable to see how far adaptationist theorising can stretch. Indeed, we can make the point stronger. Recall a point made in the previous chapter: often, in order to generate new predictions about some extant phenomenon we need to first accommodate that phenomenon.

Evolutionary psychologists are not – and should not be - afraid to spread their canvas wide, to explore where adaptationist analysis can succeed. For example, some evolutionary psychologists have hypothesized that rape is a male adaptation (Thornhill and Thornhill 1992; Thornhill and Palmer 2000). Leaving aside the sensitive nature of the topic, and the controversy surrounding purported policy implications (something which has clearly

antagonised sceptics, as we'll see in chapter 7), one could argue that existing evidence for this hypothesis is weak and that purported adaptive benefits of rape can be won by other traits and strategies (Dupré 2001; Kitcher and Vickers 2003). True enough, but this misses something crucial – namely, the hypothesis is exploratory. Hagen (2004) points out that there is currently insufficient evidence to decide whether rape is a male adaptation or not – at this stage, the more appropriate question is whether rape can be plausibly conceived as a male adaptation. Hagen believes the answer is clearly yes, and that this should motivate efforts to conduct further research, to seek out and establish further lines of evidence.

We shouldn't lose sight of the value of accommodation. When evolutionary psychology 'only' accommodates a phenomenon this alone often allows us to see the phenomenon in entirely new light. Evolutionary psychology accommodations can be impressive as they can make the ordinary extraordinary. Things we take for granted, as being obvious, as being given, actually have a history, a natural history. Many find these explanations worthwhile, especially given our justified commitment to evolutionary theory, even if such research has yet to be articulated to the point of testability.

Furthermore, accommodation can be radical. Many of us are too young to know a time when the very notion of evolutionary accommodations of behavioural and psychological traits surprised and shocked people. Many of us weren't around when the early sociobiologist pioneers were subject to assault and battery (Segerstråle 2001). These days, people have become acclimatized and receptive to the notion (though, as the rape adaptation hypothesis shows, there is still plenty of scope for shock and strong emotions). Nevertheless, there is considerable social pressure not to notice the obvious. To some, it's obvious that stepchildren incur elevated rates of abuse and homicide; to others, perhaps ideologically conditioned in a certain way, this is not at all obvious. Hence, perhaps, why the so-called Cinderella effect is dismissed by Schulz (2011) as being obvious but dismissed by Buller (2005) as being flatly mistaken, as not a genuine phenomenon.

A research programme's heuristic power is revealed over the medium- and long-term, not immediately in its infancy. Hence, we should foster a degree of tolerance towards young programmes, those chiefly accommodating phenomena into their explanatory framework. We must allow evolutionary psychology to first conceive, then nurture and develop hypotheses. Establishing what phenomena can plausibly and promisingly be accommodated within evolutionary psychology's conceptual and methodological framework can take some time. However, as accommodation efforts wind down,

prediction efforts energize, as per the heuristic framework I outlined previously. Even if Schulz is right, even if evolutionary psychology is currently primarily the practice of accommodation, I venture that increasing waves of evolutionary psychology practice will focus more and more on generating novel predictions and devising elaborate tests for them. Liddle and Shackelford (2009: 291) put the point nicely: 'Theoretical articles are immensely useful in providing a framework for future empirical research on a particular topic. To dismiss an entire field because some hypotheses have not yet been tested is premature at best and disingenuous at worst.'

So there is no 'instant rationality', to borrow a memorable phrase from Lakatos, in the sense of an immediate judgement of a research effort. To properly appraise a research effort, we must adopt a longer-term view. After all, a research programme that has hitherto been primarily in the business of accommodation might soon become stunningly predictive in stunningly novel ways.

4.2 Unsafe generalisation

So far I have granted Schulz's generalisation without challenge. In fact, this generalisation needs to be challenged, and it is time to do so. Suppose we acknowledge that some well-known evolutionary psychology hypotheses, such as the cheater detection hypothesis, are indeed accommodations of extant phenomena. However, as I argued in the previous chapter, once accommodated, the framework of evolution psychology allows for novel predictions to be generated about the accommodated phenomenon. If this hasn't happened in some cases yet that doesn't mean it can't happen in those cases. Furthermore, despite cases of accommodation, there are clear examples of work that delivers observational findings beyond what we already know, beyond the reach of the prevailing orthodoxy. In the previous chapter I identified some of these: disgust as pathogen avoidance; cue-detection of the ovulatory cycle; sex differences in jealousy; and female preference for symmetrical males.

Indeed, Schulz reluctantly supplies the latter work as a further example of heuristic evolutionary psychology – though one has to sail through the footnotes to find this. After citing Csibra and Gergely (2009) as being, in his judgement, one of the few instances of evolutionary psychology being heuristic, Schulz appends a footnote, saying:

> ‘Andrews et al. (2002, p. 538) and Buss et al. (1998, p. 545) claim that Thornhill & Gangstead’s work on female preferences for symmetric men (see e.g. Gangstead & Thornhill, 1997) provides another example of a heuristic form of evolutionary psychology. Whether they are right in this is not something I shall discuss here (for some critical remarks concerning this, see e.g. Fuentes, 2002); what matters for present purposes is just that most instances of evolutionary psychological research are not heuristic in structure, and that finding exceptions to this requires hard work.’ (2011: 227, n. 11)

Three comments. First, Schulz identifies another example of heuristic evolutionary psychology practice. However, he exiles it to the footnotes. Surely this is more than a footnote. Surely it merits more than a moment’s attention. Second, his paper is not concerned with evidence issues, only whether evolutionary psychology has led to novel predictions in practice – and yet he cites a paper questioning whether the female preference for symmetrical males hypothesis is right. Why mention this? And if he mentions this, why not also mention other studies that corroborate the hypothesis? Third, Schulz tells us not to be too concerned about this case – what matters is that ‘most instances of evolutionary psychological research are not heuristic’ and that he’s found it ‘hard work’ finding exceptions. This, of course, sounds dangerously close to begging the question.

Sceptics never cease to question evolutionary psychology’s output, suspecting it as being plausible but evidentially lacking accommodations, whereas I suspect its real history, especially its recent record, is full of surprising novel predictions gradually leading to bounties of new data.

G. K. Chesterton once remarked that if a job is worth doing, it’s worth doing badly. When adaptationist hypotheses were first pursued in psychology, perhaps some of them were creaky, perhaps some of them were of questionable value. We need not name any names.

But evolutionary psychology is no longer in its infancy – though it’s not quite entering middle age yet either. Reading evolutionary psychology research published in a variety of journals, as opposed to just reading second- and third-hand accounts of evolutionary psychology, what soon becomes apparent is that many papers table novel predictions and seek evidence for them, usually by constructing and conducting their own experiments. Of

course, not every evolutionary psychology research paper does this, but those papers that don't hypothesize novel predictions still carry out important research, such as testing existing evolutionary psychology hypotheses in different contexts; testing existing evolutionary psychology hypotheses across cultures (e.g. Sznycer *et al.* 2012 examined cross-cultural similarities and differences in proneness to shame); finding fresh evidence for existing evolutionary psychology hypotheses (e.g. Ketelaar *et al.* 2012 provided new evidence for the existing evolutionary psychology hypothesis that smiles are associated with lower social status); and testing for, and ruling out, alternative explanations for successful novel predictions – exactly what one would expect of a hypothesis-driven empirical science. Indeed, it has been some time since I've read an evolutionary psychology paper that doesn't articulate its hypotheses to the point of testability or doesn't test existing hypotheses in different contexts or cultures. And rather tellingly, the cases Schulz identifies as accommodation date from evolutionary psychology's early days (citing Cosmides and Tooby's, Buss's, and Pinker's work from the early 1990s, and Symons's and Daly and Wilson's work from the 1980s), while the case he cites as demonstrating the heuristic potential of the research programme - Csibra and Gergely - is more recent, dating from 2009 (and the case he demotes to a footnote, Gangstead and Thornhill, is from 1997). Schulz cited the 1992 handbook *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*, but for a more up-to-date presentation of the programme, it would have been better to consult and cite the 2005 *The Handbook of Evolutionary Psychology*. In that handbook, Buss notes, 'A decade ago, a handbook of this scope would have been impossible. The empirical corpus of research testing evolutionary psychological hypotheses was too slim. Now the body of work has mushroomed' (2005: xxiii). He further adds that, 'Hundreds of psychological and behavioral phenomena have been documented empirically, findings that would never have been discovered without the guiding framework of evolutionary psychology. Evolutionary psychology has proved its worth many times over in its theoretical and empirical harvest.' (*ibid*.: xxiii) Yet Schulz focuses on a tiny sampling of early examples of evolutionary psychology work, work done in the programme's infancy, when accommodation is only to be expected.

The temptation now will be to use the same strategy against the sceptics – make a list of successful novel predictions and then generalise to saying this is typical practice. We'd be spoilt for choice. Penton-Voak *et al.* (1999) and Penton-Voak and Perrett (2000) successfully predicted that women's preferences for masculine faces should be more pronounced during the fertile phase of the ovulation cycle than during non-fertile phases of

the cycle. Haselton *et al.* (2007) successfully predicted changes in women's style of dress associated with ovulation. Gangestad *et al.* (2007) successfully predicted that higher fertility increases women's attraction to behavioural dominance. Lieberman, Pillsworth and Haselton (2011) predicted that women near peak fertility in their cycle should reduce contact with male kin to minimize incest risks. Analyzing itemized mobile phone bills from a number of women, they found that women at peak fertility talked less with their fathers, both in terms of number of calls and duration of class, but more with their mothers.

And that's just the tip of the iceberg of evolutionary psychology work on attraction and ovulation. We could cite many more examples of novel predictions from other areas of research. For example, Sell, Tooby, and Cosmides (2009) advance a new hypothesis concerning the function and design of anger, which they call 'the recalibrational theory of anger'. According to Sell, Tooby, and Cosmides, anger functions to orchestrate two bargaining tools - inflicting costs and withholding benefits - in interpersonal conflicts of interest, inducing the recipient of the anger to give greater weight to the welfare of the angry individual than they would otherwise, thereby helping resolve the conflict of interest in favour of the angry individual. Sell, Tooby, and Cosmides postulate the existence of 'welfare tradeoff ratios' (WTRs):

> 'In social species, actions undertaken by one individual commonly have impacts on the welfare of others (measured in fitness, or in other currencies). Consequently, neurocognitive programs in social species should have been designed by selection to solve the following computational adaptive problem: For a given choice set involving self and other, how much weight should be placed on the welfare of the other compared with the self? We shall refer to the ratio of these weights as a welfare tradeoff ratio (WRT) between the self *(i)* and individual *(j)*: WTRij.' (2009: 15073)

Hence, as we interact with others, a neurocognitive programme is automatically calculating WTRs. If so, individuals could, in conflicts of interest, unconsciously use anger to influence others into recalculating their WTR. Individuals 'with enhanced abilities to inflict costs' (that is, physically stronger individuals) or those with 'enhanced abilities to confer benefits' (that is, attractive individuals) enjoy better bargaining positions when it comes to conflict of interest situations. Because of this, they will have 'a greater sense of entitlement' and hence more liable to use anger in this way. Accordingly, they are more likely to prevail in

conflicts of interest, seeing conflicts resolved in their favour. In total, Sell, Tooby, and Cosmides tested eleven novel predictions they derived from their recalibrational theory of anger. They report that all eleven predictions were empirically corroborated and note that no other existing theory predicts the pattern of results obtained.

Furthermore, consider the single case Schulz recognises as an actual instance of heuristic evolutionary psychology. Csibra and Gergely (2009) hypothesized that the capacity for natural pedagogy is an adaptation for acquiring generalisable local knowledge. If that is the function of natural pedagogy, then infants should be able to distinguish teaching episodes, where objective information is acquired, from teachers' personal tastes. The novel prediction was confirmed. Schulz is right to highlight this as a case of a heuristic evolutionary psychology in action – but entirely mistaken to think this is a unique or special case. The logic underpinning Csibra and Gergely (2009) – if *X* is an adaptation for *Y*, then *Z* will follow - is exactly the logic at work in hundreds of other research papers, the logic outlined and discussed in the previous chapter.

4.3 Temptation denied

Perhaps, then, it's time to make a generalisation. Perhaps we should say that not only is evolutionary psychology's framework capable of birthing novel research, and has clearly done so in a number of cases, but that in actuality this is the norm. It's a tempting thought. Nevertheless, I shan't yield to this temptation. I shan't aggrandise the jurisdiction of my judgement from a number of cases to all cases. I've been schooled better than that.

What's the state of play in evolutionary psychology practice? Is it chiefly prediction or is it chiefly accommodation? Or a mixture of the two? I don't know. And neither do you. Evolutionary psychology's theoretical and methodological framework is one issue; its typical application another issue. The second issue is an empirical issue requiring systematic empirical evidence. I do know that evolutionary psychology as a research programme aims to discover new facts, that it's theoretical and methodological framework allows novel predictions to be generated and tested, and that this happens in practice, significantly more so than Schulz recognises. But whether this characterizes the majority of practice or only a minority of practice, I cannot ascertain.

Yet you've probably heard from one commentator or another the view that, 'Perhaps too much research in the field [of evolutionary psychology] is a documentation of what is

already known, accompanied by a post hoc evolutionary spin and a snappy press release.' (Laland and Brown 2011a: 137) Perhaps. But how are you to know? This strikes me as complacent, an assertion becoming a 'truth' by virtue of frequent repetition. When characterising a research programme, one must resist the temptation to generalise from a handful of cases, to avoid aggrandising one's judgement of a handful of cases to the activities of the entire programme. And one must resist the temptation to subscribe to supposed truths that no-one bothers to cite data for, which no-one can cite data for, for no-one has the data to do so, for few are even aware of the need for such data.

Carruthers notes that 'There is now an *immense* body of scientific work in the evolutionary psychology tradition, of most of which the philosophical critics are simply ignorant.' (2006: 37, emphasis original) Absolutely. Carruthers didn't survey that vast landscape in his book, since the goal of his work was to clarify and vindicate the concept of modularity and the massive modularity thesis. Nor can I do so here, for a similar reason – my task is to clarify and vindicate the theoretical and methodological foundations of the programme. But a meta-review needs to be done. There's a PhD in sociology in that – but not in philosophy.

4.4 Conclusion

How good is the output of the programme? Is it primarily accommodation, or prediction? This is a question posed by Schulz, who, unlike most sceptics, has sailed near enough to evolutionary psychology to recognise theoretical evolutionary psychology's heuristic value, but moves to deny that the discovery of new facts accurately characterises the practice of the research programme. In other words, to acknowledge that while theoretically evolutionary psychology has heuristic value, its actual practice falls far short of this.

I have argued that even if this is the case, this fits the normal arc of research programmes – accommodation often precedes prediction. But is it the case that the practice of evolutionary psychology is chiefly characterised by accommodation, not prediction? We don't know. Schulz considers only a tiny sample of cases. That is no basis on which to make claims about the voluminous output of evolutionary psychology. Indeed, there is plenty of evidence that evolutionary psychology practice, especially more recent practice, is significantly more predictive than Schulz gives credit for – indications found even in his own paper, isolated in a footnote. Someone sympathetic to evolutionary psychology therefore might be tempted to engage in an alternative bookkeeping, whereby one details many

great empirical triumphs of evolutionary psychology, then generalises this as the typical activity of the programme, with weaker examples sharply ruled off. I have avoided this temptation, as it too is an unsafe generalisation.

I have no quarrel with those who prize examining the sociology of evolutionary psychology, but in the absence of a meta-review we shouldn't make global pronouncements, only local judgements. Nor should early cases of weak practice, real or otherwise, be taken to diminish or discount the theoretical and methodological framework of evolutionary psychology.

Chapter 5

The scope of evolution psychology

Evolutionary psychology tends to focus on a relatively narrow range of topics. Sometimes this gives the impression that evolutionary psychology's usefulness is restricted to those topics. That, I contend, is a mistake. In this chapter, I demonstrate that the scope of evolutionary psychology is potentially wider than commonly thought. In §5.1, I give an example of evolutionary psychology generating a novel prediction about non-social behaviour. In §5.2, I give an example of a hypothesized by-product of a single adaptation, and in §5.3 I discuss a hypothesized by-product of multiple adaptations. In §5.4, I apply evolutionary psychology reasoning to an area it's not been applied to before. The upshot is that there's no reason to suppose that the programme is applicable to just a narrow set of topics.

5.0 Limited range of topics?

A common perception is that evolutionary psychology is limited to a narrow range of topics, such as parenting, mating strategies, face recognition, cheater detection, kin detection, incest avoidance, and cooperation. One might be tempted to think, then, that evolutionary psychology is largely irrelevant, since evolutionary psychology is relevant only to a limited range of social phenomena.

Psychology is by no means all, or even mostly, about mating, parenting, and aggression. Psychology is much more than that. Psychology research covers a spectrum of activities: occupational psychology; organisational psychology; media psychology; persuasion psychology; depth psychology, with all the attendant schools and theories like Freud and Jung; psychology of self; psychology of religion; personality psychology; even transpersonal psychology. The psychological phenomena, the patterns of behaviour, studied within these subdisciplines are far removed from natural selection, and hence are explicated in terms of other casual processes. That's why psychologists were able to carry out a range of research independently of evolutionary considerations - and that's why they can continue to do so. Since the topics of evolutionary psychology - mate choice, attractiveness, co-operation and the like - only cover a limited range of psychological phenomena, evolutionary psychology has limited value. It's largely irrelevant.

Evolutionary psychologists might be happy to concede most of the previous paragraph – much work in psychology has been and can be done without adaptationist theorising (though, as we saw in chapter 1.1, prominent evolutionary psychologists are sceptical of psychology done independently of evolutionary theorising). However, this doesn't mean evolutionary psychology can't be applied to these diverse subdisciplines of psychology.

For example, evolutionary psychology can extend beyond social behaviour. DeBruine (2009) cites research illustrating how evolutionary psychology can generate novel predictions about non-social behaviours. It's been known for some time that humans estimate horizontal distances far more accurately than vertical distances. For example, when standing on top of building looking down, the distance people tend perceive from the top of the building to the ground is overestimated. The distance estimated when standing on top of a five-storey building looking down is equivalent to the actual height of a nine-storey building. However, when orientated horizontally, people perceive the same distance much more accurately.

DeBruine points out that Jackson and Cormack (2007) make a further prediction: vertical distances should be overestimated more from the top than from the bottom. Jackson and Cormack propose what they call 'evolved navigation theory', which suggests perceptual and navigational mechanisms reflect navigational costs over evolution. Unlike ascent, descent involves a significantly increased risk of injury and death. In fitness terms, it's better to overestimate descent. Hence, we should find a descent illusion. The existence of this previously unknown descent illusion was recently confirmed in Jackson and Cormack (2007) and Jackson and Cormack (2008). DeBruine notes that this previously unknown descent illusion 'was predicted only by consideration of the fitness consequences of behaviour. Indeed, no one even thought to test this prediction for over 40 years until an evolutionary perspective was brought to bear on the topic.' (2009: 932)

5.1 By-products of single adaptations

Furthermore, an evolutionary psychology analysis of a psychological phenomenon does not require the phenomenon to be directly underwritten by a dedicated adaptation. Evolutionary psychology can apply to phenomena that emerge as by-products of other psychological adaptations – and surely this could be a large class of phenomena.

In chapter 3, I argued that hypothesizing certain traits as adaptations in the manner methodologically recommended by evolutionary psychology can be useful in two ways:

H1 Hypothesizing unknown design features of extant psychological mechanisms, leading to novel predictions of psychological phenomena.

H2 Hypothesizing unknown psychological mechanisms, leading to novel predictions of psychological phenomena.

In light of the distinction between adaptations and by-products, mentioned in chapter 1.1, evolutionary psychology has a further use:

H3 Hypothesizing traits as by-products of psychological mechanisms, leading to novel predictions of psychological phenomena.

A good example of H3 is evolutionary psychology work on the psychology race, as undertaken by Kurzban, Tooby, and Cosmides (2001) and Cosmides, Tooby, and Kurzban (2003).

Previous research had established that people automatically encode the race of individuals they encounter. Race exists in our minds. The problem with this, observe Kurzban, Tooby, and Cosmides, is that there is no objective patterns in the world that could explain why racial categories are so salient, so obvious to adults.

The puzzle deepens when one realises that in the EEA, there were no adaptive reasons to automatically encode race. It's plausible that selection would have favoured automatic encoding for an individual's sex and age. However, it's unlikely anyone would have encountered other people so distinct as to be perceived as a different 'race'. Ancestral hunter-gatherers had a limited travel range, having residential moves no greater than forty miles. 'If individuals typically would not have encountered members of other races, then there could have been no selection for cognitive adaptations designed to preferentially encode such a dimension, much less encode it in an automatic and mandatory fashion.' (*ibid*. 2011: 15387)

The psychology of race must therefore reflect some other psychology. The adaptationist question then becomes: What was the adaptive problem faced by our ancestors that nevertheless apparently makes race so salient? Kurzban, Tooby, and Cosmides hypothesized that it was the adaptive problem of making coalitions. Hunter-gatherers lived in bands. Between bands and within bands conflict can arise and coalitions and alliances can form. Hence, there was an indispensible pressure and need to track the ever-shifting alliances in the social environment – to be alert to fluctuating coalitional cues.

Hence, there was an adaptive problem for detecting coalitions and alliances. Therefore, Kurzban, Tooby, and Cosmides hypothesized that we have a psychological adaptation for tracking coalitions and alliances. Function-form reasoning led to the hypothesis that the proposed coalition detection mechanism will be sensitive to a variety of cues suggesting group membership:

> 'Computational machinery that is well-designed for detecting coalitions and alliances in the ancestral world should be sensitive to two factors: (1) patterns of coordinated action, cooperation, and competition, and (2) cues that predict – whether purposefully or incidentally – each

> individual's political allegiances' (Cosmides, Tooby, and Kurzban 2003: 177)

In today's world, consisting of racially diverse individuals living in close proximity to one another, race-related physical features may incorrectly be perceived as cues for coalitional alliances, hence resulting in the automatic encoding of race.

If there is such a psychological adaptation, a number of consequences follow. More precisely, Kurzban, Tooby, and Cosmides derived six novel predictions, including the prediction that manipulating coalitional variables should introduce variability in encoding. In other words, people shouldn't encode the race of other people when it is not indicative of group alliance. This was tested by experimental manipulation in which race and alliance were decoupled. The result was dramatic: within just a few minutes the tendency to categorise by race faded.

> 'Despite a lifetime's experience of race as a predictor of social alliance, less than 4 min[utes] of exposure to an alternate social world was enough to deflate the tendency to categorize by race. These results suggest that racism may be a volatile and eradicable construct that persists only so long as it is actively maintained through being linked to parallel systems of social alliance' (Kurzban, Tooby, and Cosmides 2001: 15387)

So, rather unexpectedly, the psychology of race can be understood as a by-product of a psychological adaptation for someone else. This led to a new discovery: the automatic encoding of race can be eliminated by modifying contextual circumstances. Hence, according to Kurzban, Tooby, and Cosmides, the tendency to automatically encode race is not to be thought of as an adaptation. It is a by-product of a psychological adaptation evolved for other purposes - our psychological adaptation for detecting coalitional alliances. It is not inevitable.

5.2 By-products of multiple adaptations

But we can do more. We can also hypothesize phenomena as being by-products not just of one psychological adaptation but multiple psychological adaptations. Evolutionary psychology work on religion is an excellent example of this principle. The consensus view in

evolutionary psychology is that religious belief and behaviour are not adaptive traits but rather by-products of several psychological adaptations.

Religion, a belief in supernatural agents and the practicing of rituals, exhibits certain features that attract the attention of evolutionary psychologists. First, religion is a cross-cultural phenomenon. No matter what century, no matter what type of society we examine, religious activity seems relatively widespread. Atheism is relatively rare, even in the West. Second, religion is robust. Despite religion being irrational or a-rational in the sense of lacking evidential or rational justification, religion persists, even in places where the general intellectual Zeitgeist has moved against it. Religion even persists in states which actively persecute against it. Christianity, for example, persisted and flourished within the Roman Empire despite Rome's severe persecution of it. It also survived Soviet persecutions in the 1920s and 1930s.

Beliefs and practices that are pervasive and persistent, and which lack a rational basis in terms of evidential warrant, might very well be drawing on, or be produced by, evolved psychological processes. As we saw in chapter 3, although a given trait being pervasive and persistent does not mean that it must have an adaptationist underpinning, either directly or indirectly, it does nevertheless represent a good reason for pursuing an evolutionary psychology investigation into the phenomenon. If it does indeed turn out to be the case that religion's vitality is drawing on evolved mechanisms, then an evolutionary perspective brought to bear on religion should offer fresh insights.

Despite exhibiting features that suggest a deep psychological undercurrent, religion also exhibits other characteristics that render it somewhat problematic from an evolutionary perspective. Most strikingly, religious practices appear to have high fitness costs. As Atran (2002: 4) succinctly puts it:

> 'From an evolutionary standpoint, the reasons religion shouldn't exist are patent: religion is materially expensive and unrelentingly counterfactual and even counterintuitive. Religious practice is costly in terms of material sacrifice (at least one's prayer time), emotional expenditure (inciting fears and hopes), and cognitive effort (maintaining both factual and counterintuitive networks of beliefs).'

These costs must have been even more pronounced during our prehistory. The task facing the evolutionary psychologist, then, is to explain how religion could have arisen and persisted despite its costs.

Atran (2002), Boyer (2002), and Barrett (2004) and others account for religion as the by-product of a convergence of several evolved cognitive systems interacting with the world. This evolutionary psychology approach to religion recognises that religion has fitness costs. However, these fitness costs are outweighed by the gains in fitness brought about by the cognitive systems which give rise to religion. Thus although religion is not an adaptation, the cognitive systems that converge to give rise to religion as a by-product are adaptations. This approach holds that given how our brains have evolved, religion is almost inevitable: 'We do not have the cultural concepts we have because they make sense or are useful, but because the way our brains are put together makes it very difficult not to build them.' (Boyer 2002: 187)

What cognitive mechanisms are hypothesized as being responsible for giving rise to religion as a by-product? One of the most important mechanisms, and one that certainly does a lot of explanatory work, is our predisposition to perceive agency. Guthrie (1993) brought attention to a perceptual bias in humans: humans have a tendency to interpret ambiguous stimuli in terms of agency. Thus we occasionally see, for example, faces in configurations of clouds, rain-drops, and even on Martian mountains. We hear a noise or rustle in the bushes and are immediately gripped by the expectation of an unknown agent nearby. Guthrie suggests that this perceptual bias is adaptive. Barrett (2004) called the cognitive mechanism producing this perceptual bias the 'hyperactive agency detection device' (HADD). The hypothesized HADD is understood to detect agency when an object is perceived to be contravening assumptions concerning the movement on inanimate objects (such as moving without another body moving against it). Having this perceptual bias to see agency obviously carries with it great increases in fitness. False positives carry little cost; failing to detect a predator, in contrast, carries a heavy price. Error management theory states that one should expect the evolution of biases that minimize costs of errors in judgement, even if such biases produce more errors. More precisely error management theory predicts that, when decisions are made under uncertainty, and the costs of false negatives are greater than the costs of false positives over evolutionary history, selection will favour a bias towards false positives (Galperin and Haselton 2012). Hence, and as its

name suggests, the HADD device is calibrated in a hyper sensitive way, leading to many false positives.

This hypothesized mechanism seems to enjoy a degree of empirical corroboration. Workman and Reader (2008) cite the rather striking example of a study by Fritz Heider and Mary-Ann Simmel, where participants attributed agency to two triangles and a circle depicted in a film as moving in and out of a box (the larger triangle, apparently, was described by one participant as an aggressor).

Other cognitive systems would also play their part. The systems responsible for us possessing a theory of mind would obviously also be crucial. By the age of three or four, most children possess an ability to infer people's state of minds. The survival value of this is obvious, enabling us to predict the actions of animate objects in the world.

A wide range of unexpected events and ambiguous events trigger HADD and thereby are perceived to be invested with minded agency. Since the causes of many of natural events that trigger HADD - such as violent earthquakes, thunderous storms and the like - are unseen, in a pre-scientific world it is a natural move to ascribe them to supernatural agents. These supernatural agents are counterintuitive, which makes them attention-grabbing and memorable. However, as Boyer (2002) stresses, these beliefs are *minimally* counterintuitive: beliefs that are easy to remember (in virtue of being counterintuitive) and easy to use (thanks to largely agreeing with people's expectations). Although they violate our natural expectations of the world, they are not too 'exotic', as Bulbulia (2004) puts it, or too divorced from the world to escape our interest and attention.

Thus, we arrive at the by-product evolutionary understanding of religion: a hyperactive agency detection mechanism feeds into theory of mind and other mechanisms to enable us to 'see' a world imbued with supernatural agents that are minimally counterintuitive, which is then culturally mediated in terms of polytheism or monotheism together with other supernatural agents (such as angels, which are practically interchangeable with the gods of polytheism).

The by-product approach is perhaps the most promising evolutionary psychology approach to religion. It is certainly a reasonable hypothesis that seeing supernatural agency operating in the world is partly due to this perceptual bias. The human experience of the world is as rich in false positives of supernatural agents as it is in false positives of natural agents. In a way, it would be extraordinary if the two were not connected. HADD therefore

seems to have played a key role in the formation of beliefs about supernatural agency. Unlike the false positives of natural agency, the false positives of supernatural agency can be very costly when it results in regular ritualised behaviour, but not so costly as to outweigh the tremendous fitness increases brought about by the HADD adaptation.

If religious belief and practice is a by-product of a convergence of several cognitive systems, then this leads to some novel predictions. For example, the failure of one of these systems in an individual should reduce the likelihood that the individual in question will forge or maintain religious beliefs. So people with autism, who essentially lack a theory of mind, should be less religious than average. And, indeed, this has recently been supported. Caldwell-Harris *et al.* (2011) was the first systematic study of the religious beliefs of people with high functioning autism and Asperger's disorder. They found that people 'with autistic spectrum disorder were much more likely than those in our neurotypical comparison group to identify as atheist or agnostic' (*ibid*.: 3362) This approach also lends itself to the prediction that the most widespread conceptualisations of supernatural agents will be those that are minimally counterintuitive, supernatural agents that are conceived as having intentions, desires, and agendas, agents that are neither too exotic nor too abstract.

The by-product account does, however, face an anomaly of its own. There are lots of minimally counterintuitive representations – Mickey Mouse, Father Christmas, Bob the Builder – that we are not ontologically committed to. As Bulbulia (2004: 662) observes,

> 'Many classicists maintain extremely detailed understandings of Greek religion, but few end up worshipers of Zeus... We can represent and easily recall fictional characters like Mickey Mouse (or Zeus) yet few adults come to believe Mickey Mouse actual[ly] exists outside the fiction.'

Mickey Mouse is just as counterintuitive, and thus memorable, as Jesus, but no-one believes that Mickey Mouse exists; no-one prays to him, or holds rituals directed towards him. If Mickey Mouse is a minimally counterintuitive representation, then why is Jesus the subject of religious devotion but not the mouse? What is motivating an ontological commitment to one set of minimally counterintuitive representations but not another?

Here I wish to submit a conjecture. Perhaps the factor that demarcates minimally counterintuitive representations that we respond to religiously and minimally counterintuitive representations that we don't is the *perceived* power the representation

has and the *perceived* degree of control one can have over it. If a counterintuitive agent is represented as being powerful and as having a mind that can be known, and we perceive it to be so, then it's natural to respond by performing acts that attempt to manipulate its motivations in ways that can get it to act for one's own advantage. Rituals – whether private ones like prayer or public ones – often attempt to commune with a supernatural agent and to persuade it to be favourable to us, or unfavourable to others. Repenting of sins, sacrificing goats or virgins, performing mysterious rites, performing religiously-motivated altruistic acts etc. are all attempts to manipulate supernatural agents in our favour.

Our background beliefs mean that we believe that Mickey Mouse and Zeus are fictional and so we don't perceive them to have powers or to be subject to our manipulation via rituals. So no rituals to Mickey Mouse, nor to Zeus anymore. But, of course, once there were rituals enacted to favour Zeus's attention, to please him etc. If our background beliefs changed and Zeus was again perceived to exist, and to be active in the world and have a mind that can be negotiated with, then temples and rituals dedicated and directed towards him would rise. Ditto even Mickey Mouse.

Of course, this is only a conjecture, but I believe this factor, the degree to which a postulated counterintuitive agent can be manipulated, demarcates quite nicely counterintuitive agents who we worship and counterintuitive agents who we don't. A counterintuitive agent who is perceived to be fictional commands little ritual attention; likewise, a counterintuitive agent who, although we perceive to exist, nevertheless isn't represented as being active in the world, isn't minimally counterintuitive and so will command little ritualised attention. Nietzsche touches upon this: 'What would be the point of a god who knew nothing of wrath, revenge, envy, scorn, cunning, and violence? Who had perhaps never experienced the delightful *ardeurs* of victory and annihilation? No one would understand such a god: why have him then?' (*The Antichrist*, section 16) This, I believe, accounts for the characterisation of contemporary religious thought and practice in Western Europe. In Western Europe, naturalism and evolutionary theory are not disputed; they are generally endorsed. Religion is no longer the force it once was, no longer an organising principle for many Europeans. It's not that science, and evolutionary theory in particular, has disproved the classic depictions of a personal god, a god who has intentions, desires, and agendas, but the classic depiction sits uneasily with what we now know about the vastness of the universe and the facts of evolution. The classic depiction of

god as super-alpha doesn't seem credible. It rings hollow. And it's not that naturalism has made us all Dawkinite atheists. Many of us find a belief, or rather a feeling, in 'some sort of thing somewhere or other', as Alan Watts perfectly puts it, unshakeable – which is precisely what one would expect, given the by-product account. The kind of god that survives naturalism is a more abstract god, a distant god, not a creator god, not a god intervening to settle tribal disputes or kicking up storms. In other words, the conception of god that survives naturalism and Darwinism, a kind of deist god, is no longer minimally counterintuitive. Hence it doesn't have mass appeal. It doesn't orientate our lives. In contrast, in places where naturalism has yet to make inroads, religion is rigorously ritualised and lived out, and the classic depiction of god remains. And places in the West where classic depictions of an agent god persist – i.e. the Bible Belt – are marked by a pronounced anti-Darwinism. I don't think it's any coincidence that those who retain the traditional conception of god as an agent active in the world tend to reject naturalism, or at the very least reject evolutionary theory. One can't comfortably consent to naturalism while retaining the classic, minimally counterintuitive depiction of god.

Finally, it's worth noting that one could apply a cultural evolutionary analysis to the details of religious beliefs. Religion is robust, not only as a general phenomenon and also in its concrete manifestations. Regarding the latter, if conditions in a society change, even if radically, concrete religious beliefs and practices can be modified and adapted to their new environment. So, for example, elements of pre-Christian paganism survived into the Christian world (Christmas, being essentially a pagan festival baptised in Christian symbolism, perhaps being the most pleasing example). Another example is how various elements of Aztec religion survived into Christianity in Latin America (such as Saint Death, a Christianized version of the Aztec god of death). Therefore, there is potentially space for a cultural evolutionary explanation too. Indeed, Dawkins (2006: 190) recognises this possibility:

> 'Even though conventional Darwinian selection of genes might have favoured psychological predispositions that produce religion as a by-product, it is unlikely to have shaped the details...if we are going to apply some form of selection theory to those details, we should look not to genes but to their cultural equivalents.'

Of course, if one holds that evolutionary psychology should only harbour ambitions to explain the origin and persistence of religious belief and practice, rather than explain its

concrete details, one might not be motivated to supplement the by-product evolutionary account with a cultural evolution account. But the option is available.

5.3 New applications

The cases above suggest that many new and diverse applications of evolutionary psychology are possible. Think of how many more psychological adaptations might be awaiting discovery. Think of what kind of phenomena could be by-products of single adaptations – and how many more could be by-products of multiple adaptations. Using adaptationist hypothesizing, including by-product hypothesizing, evolutionary psychology potentially has a wide scope across a range of phenomena, at the very least a scope far greater than simply mating and cheater detection.

I want to illustrate this further with an example of a topic or issue that *prima facie* is unconnected with evolutionary psychology but which might possibly benefit from an application of the adaptationist perspective. I contend that evolutionary psychology might have a valuable contribution to make to the issue of climate change action. It could open up new lines of thinking on this issue and even predictions. Admittedly, what follows is speculative thinking – but I think it does establish how evolutionary psychology could *possibly* apply to this issue, and in doing so show that evolutionary psychology can potentially have a surprising range of applications.

As we all know, climate change is a major threat, perhaps even an existential one. According to the Stern Report, unless the world meaningfully cuts global greenhouse gas emissions by 2030, there is likely to an increase in global temperatures of more than 2 degrees centigrade, which will likely trigger runaway climate change. The longer meaningful action is delayed, the more difficult it will be to ward off disaster. But there's no alarm at this unfolding set of events. We acknowledge and register that climate change is a threat, but it rarely, if ever, elicits alarm. Surely this is odd: when faced with a threat, we're alarmed into action. That's how we're built. Since climate changed is believed to be the greatest threat we face, we should be alarmed into action. Why is the threat of climate change not triggering fear, alarm, panic, or anxiety the way that other threats do?

This lack of alarm mirrors inaction on combating climate change. At the collective level, the Copenhagen Summit in December 2009 was but the latest round of sterile conferences dishing out weak and vague promises of action, with the labour of even these weak

commitments being pushed to future generations. At the individual level, there is also lacklustre response. A few light bulbs changed, bit of recycling, but little more than this.

The usual focus is on various political failings, e.g. parochialism (states passing the blame and responsibility to other nations – e.g. West and China) and passing the buck (when states do agree to act, they push the heavy labour of these commitments to the next generation). These failings could have a tragedy of the commons effect: individuals know that their actions are ecologically destructive, but continue anyway because – in absence of meaningful international agreements – individual actions amount to very little.

But what strikes me is that if we were alarmed at climate change, action would follow. The inertia would evaporate. Certainly, parochialism and passing the buck are factors for why there's been a lacklustre response to climate change, but they shouldn't obscure attention to the simple fact we're not alarmed at the situation. No alarm, no action. Yet we've been alarmed into action before – namely, ozone layer depletion. Why are we not alarmed by climate change? It's no good simply saying that many people don't believe in climate change – plenty of people do believe in climate change without being alarmed. A deeper explanation is called for.

Let's look at alarm or fear from an adaptationist perspective. When faced with threats, fear is activated, redirecting and recalibrating various systems – perception, the ranking of goals, behavioural decision rules – in order to protect an organism from a perceived threat. Cosmides and Tooby (2000) nicely trace out the steps involved. Suppose you are walking alone at night; you hear a noise behind you. This noise acts as a cue to the possibility of a threat; fear is triggered, seizing and recalibrating various systems: the threshold for signal detection is lowered; there are shifts in perception and attention (greater sensitivity to sounds that could indicate the presence of a threat, becoming aware of sounds normally not picked up on); there is a change in the weighting of goals (safety rockets up the priority list, food and mate-finding become lower priorities, and hence one no longer feels hungry, thirsty, or sexually aroused); memory systems are activated (safe places and the location of allies are recalled), physiology changes take place (adrenalin spike, blood rush, heart rate increase), and behavioural decision rules are activated (which are sensitive to the nature of the perceived threat – to fight, to run, to cry, or to be still).

The crucial point is concerning the kind of cues or stimuli that trigger fear and alarm – this is where evolutionary psychology can yield important insights. As we know, in virtue of its

emphasis on specificity of adaptations, evolutionary psychology suggests that the existence of a general 'threat detection' fear mechanism is unlikely. There were no general threats in the EEA; threats were concrete, particular, in flesh, scales and blood, in firestorms and thunderstorms, and consequently cues that the fear mechanism processes will relate to these threats. So evolutionary psychology can yield the following prediction: the closer a threat is framed in terms of, or approximates, a threat faced by our ancestors in the EEA, the more likely it will be to trigger alarm.

What threats did our ancestors face? One of the major threats, perhaps the chief threat, came from hostile fellow humans, whether members of the same tribe or a different one. Consequently, we can expect cognitive systems to have evolved that are very sensitive to threatening cues, such as facial expressions. Indeed, various studies have demonstrated that angry faces are detected more quickly than non-angry faces (e.g. Hansen and Hansen 1988). Furthermore, there is an interesting sex-difference here, one discovered by evolutionary psychologists – although men and women identify facial expressions of anger more quickly than other emotions, men register expressions of anger on male (but not female) faces faster than women do (Williams and Mattingley 2006). On the basis that violence is mostly male-on-male, and probably has always been so, the adaptationist perspective predicted, and confirmed, this sex-difference. Indeed, this nicely exemplifies how selective forces often shape very specific adaptations because very specific needs were present in past environments.

Another major source of danger came from predators, as well as dangerous animals. Our psychologies should be sensitive to predator threats, even if those threats cease to obtain in modern suburban environments. This, indeed, is the case: it's far easier to elicit and condition a fear of snakes in people than it is to condition a fear of cars etc., which represent a greater threat to people today than snakes do. Finally, our ancestors faced ecological threats and disasters – floods, storms, landslides, wild fires, heat waves, draughts, and famines. Indeed, there was an ice-age around 13,000 years ago.

So the way our alarm processes are calibrated is rather like a smoke detector that's calibrated to detect only a certain type or range of smoke: it might work well most of the time when we're asleep, but should a type or range of smoke that it's not calibrated to emerge, the alarm won't be triggered, and we'd only awake when it's too late.

I speculate that climate change does not present the kind of cues or stimuli that the mechanisms dedicated to responding to threats were evolved to respond to. Most obviously, climate change is not a hostile face or a predator. However, it is an ecological threat, so perhaps we should expect climate change to trigger the alarm system? The problem here is that climate change is taking place at a rate below the threshold needed to trigger alarm. Climate change is happening too slowly to trigger the alarm – this slow-paced but looming threat of ruin is evolutionary novel.

Compare the lack of alarm over climate change with the alarm trigged by the discovery of the rapid disappearance of the ozone layer. In 1983, Joe Farman, using a 25-year-old machine in a research station in Antarctic, discovered that half the ozone layer over Antarctica appeared to have vanished. Together with Brian Gardiner and Jon Shanklin, Farman announced his discovery in a paper in *Nature* in May 1985. Initially, there was disbelief, since a NASA satellite had been measuring ozone levels for several years and didn't detect the depletion. Nevertheless, NASA reviewed its satellite findings. Incredibly, the satellite had detected the massive depletion, but its data quality control algorithms had automatically rejected the data as being wildly improbable. Once the data was re-run without the data quality control algorithms, the massive depletion was verified.

The sudden discovery of fifty percent depletion of the ozone over Antarctica shocked the scientific community. It caused international alarm – and action. The Montreal Protocol, a treaty phasing out the CFCs responsible for the ozone depletion, was drafted only two years after the *Nature* paper, and entered force in 1989. The ozone layer is now slowly repairing. The threat associated with ozone depletion - the threat of lethal ultraviolet solar rays - was dealt with quickly. According to the UN, the ozone agreements 'are the most widely ratified treaties in United Nations history, and have enabled reductions of over 98% of all global production and consumption of controlled ozone-depleting substances.' Not a single UN member state has failed to sign up. No ifs, no buts, no passing the buck, no appeals to tragedy of the commons - just speedy, decisive, uniform action on an unprecedented scale.

The difference between the ozone hole over Antarctica and climate change is that the former was a massive change at a rapid rate, while the latter is taking place slowly. Because of the speed, it triggered alarm in a way climate change so far hasn't and might not do until it's too late. Of course, the ozone layer depletion was also evolutionary novel, but one fast enough to trigger the alarm circuitry.

We sometimes hear that the tragedy of climate change is that it's taking place too quickly. If the adaptationist perspective here is right, the tragedy of climate change is actually that it's taking place too slowly. If it were more sudden, if (for example) the Arctic became ice-free abruptly, rather than gradually over a stretch of time, then this would likely trigger alarm, and orchestrate speedy international action to counteract or mitigate the threat. A sudden ecological change would do more for animating climate change countermeasures than all the summits, education programmes, and government initiatives have ever achieved in twenty years, or are ever likely to achieve alone, without our alarm processes being trigged.

So we acknowledge and register the statistics of climate change at an intellectual level, but it's very difficult to respond to it emotionally – and it's fair to say that it's the emotions that really drive us. This, I think, goes some way to illuminating this puzzlingly situation. As I stressed earlier, admittedly this is a speculative application, a first sketching out of a possibility, but I believe it has a resonance, a germ of truth, and I believe it merits further investigation. Regardless, it shows how evolutionary psychology can offer fresh perspectives on topics *prima facie* removed from it.

5.4 Unknown boundaries

Indeed, evolutionary psychologists can cite increasing volumes of evolutionary psychology work that goes well beyond the bread-and-butter of mate choice, attractiveness, and co-operation. For example, in 2012, the edited volume *Applied Evolutionary Psychology*, Oxford University Press, contained twenty-five papers on applications of evolutionary psychology on a diverse range of topics, including evolutionary psychology work on academic learning (Geary 2012); business and management (Nicholson 2012); marketing and communication (Griskevicius, Ackerman, and Redden 2012); intergroup prejudice (Park 2012); psychology of mass politics (Petersen 2012); and even perfume design (Roberts and Havlicek 2012).

So evolutionary psychology can cogently expand far beyond its traditional topics of mating, parenting, kinship, status, and aggression, which were early topics, safe topics, and move beyond those topics in surprising, sophisticated, and useful ways – not in a crass takeover fashion, not in a way that necessarily displaces other explanations, but in a way that genuinely contributes to understanding. There is, then, a continuum of psychological and

social phenomena where evolutionary psychology plays a more or less useful function – adaptations and by-products will be directly involved in some cases, involved less obviously in other cases; implicated more subtly in other cases; and in the frame but more distant in others (an example being Pinker's *The Better Angels of Our Nature* (2011), where psychological adaptations figure in the narrative, but are largely in the background).

Clearly, though, adaptationist analysis will not cover all psychological phenomena. There will be phenomena where selectionist forces are so far removed that adaptationist perspectives will be absent. Evolutionary psychology is going to have boundaries.

What are the limits of evolutionary psychology? Obviously where attempts to apply evolutionary psychology are not useful. Of course, the failure of evolutionary psychology does not mean no evolutionary perspective can be applied - something that that is beyond the bounds of evolutionary psychology might nevertheless conform to cultural evolutionary perspectives. There is one way, and one way alone, to discover where evolutionary psychology can be usefully applied, and where it cannot: to actively pursue evolutionary psychology hypotheses across a broad spectrum of psychological and social phenomena. This must be relentless. Bold. Imaginative. And above all, it must be risky – it must risk failure. To know its failures is as important as to know its successes.

I think the objection we began this chapter with can be converted into a sociological objection: evolutionary psychology can cover terrain beyond the bread-and-butter topics but it typically doesn't, that this is very much a minority activity within the research programme. Suppose there is too much emphasis on the bread-and-butter topics of mate choice, attractiveness, co-operation and the like. This would hardly be surprising – the normal arc of a research programme is to begin with the easier, more obvious topics before reaching out to more difficult terrain. This parallels the evolution of life itself: the first organisms fit into the simplest niches, and later life evolves to fit in, and engineer, more complex and difficult niches. Certainly there is a lot more research to do on the bread-and-butter topics. But eventually more and more research activity should migrate to bolder, less travelled topics.

5.5 Conclusion

Evolutionary psychology has identified possible psychological adaptations underpinning kin relationships, detection of cheating, language acquisition, theory of mind, folk physics, face

recognition, mate preferences, co-operation, and specific emotions, such as fear, jealousy, and disgust. But does this mean evolutionary psychology is limited to these topics? Absolutely not.

We first looked at an example of evolutionary psychology making a novel prediction about non-social behaviours – the previously unknown descent illusion. We then noted that the distinction between adaptations and their by-products potentially enables us to apply adaptationist thinking to a broad range of phenomena. A striking example of a proposed by-product of a single psychological adaptation is psychology of race. Kurzban, Tooby, and Cosmides (2001) reasoned that it is unlikely ancestral populations encountered different races – the psychology of race might well be a by-product of the psychology of coalitions.

We also looked at an example of a purported by-product of multiple psychological adaptations. Religion is costly, counterfactual, and counterintuitive. How, then, could it have arisen and persisted? According to the by-product approach, although religion is not an adaptation, the cognitive systems that converge to give rise to religion as a by-product are adaptations. This is promising and illuminating.

I then applied evolutionary psychology to an area it's not been applied to before. I suggested, in light of adaptationist reasoning, that our alarm processes are poorly designed to react to climate change. The tragedy of climate change might not be that it's happening too quickly but that it's not happening quickly enough – unlike ozone layer depletion, which occurred at a sufficiently rapid rate to trigger alarm and therefore action.

Given the range of phenomena that psychological adaptations, in various degrees, can influence, and given that psychological adaptations can generate a multitude of surprising by-products, evolutionary psychology appears to have a wide remit in psychology. Of course, evolutionary psychology has boundaries - but it's an empirical matter to probe where those boundaries are.

Chapter 6

Evolutionary psychology in the evolutionary and behavioural sciences

Richardson (2007) purports to establish a conclusion that this thesis has been arguing against – namely, that evolutionary psychology is merely speculation, that it is methodologically inert. In §6.0, I outline his argument, which is but the latest deployment of an old complaint. In §6.1, I demonstrate that his argument is invalid. Nevertheless, I don't wish to shirk from the spirit of his argument, for in rising to the challenge of adaptationist explanation we better locate evolutionary psychology's role in the evolutionary and behavioural sciences. In §6.2, I argue that it's for the evolutionary and behavioural sciences collectively to rise to the challenge of adaptationist explanation. In §6.3, I offer a moderate optimism concerning the prospect of the evolutionary and behavioural sciences collectively rising to the challenge.

6.0 The challenge of adaptationist explanation

Richardson (2007) claims that evolutionary psychology hypotheses are 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 faculties 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 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.

6.0.1 What complete adaptationist explanations require

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), which lists five categories of evidence complete adaptation explanations should provide: (i) selection, (ii) ecological factors, (iii) heritability, (iv) population structure, and (v) 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 1.1, 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, since 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.

6.0.2 Evolutionary psychology explanations as incomplete explanations

Richardson applies Brandon's criteria to Buss's research on the evolution of sex differences in jealousy (2007: 60-64), Pinker's research on the evolution of language (*ibid*.: 124-129), and Cosmides and Tooby's research on their proposed cheater detection mechanism (*ibid*.: 132-136). 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) hypothesized that pregnancy sickness is an adaptation to avoid toxins harmful for the foetus. Sterelny (1995) argued that the pregnancy sickness might well have benefits but that since Profet neglected to balance the purported benefits against possible costs, to perform a proper cost-benefit analysis, it remains to be shown that pregnancy sickness is an adaptation. 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)

6.0.3 Epistemological pessimism

Furthermore, Richardson claims not only are 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.

6.0.4 Richardson's conclusion

Since 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. 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 and I suspect it will 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 obtain the key information that would be required to show the validity of adaptive tales about the EEA?' (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).

Richardson and others have adopted the hardest of positions against evolutionary psychology. But how sustainable is this position?

6.1 Responding to the challenge

The daily practice of evolutionary psychologists is to formulate hypotheses and test them. These can be, and often are, presented as explanations. The lesson from Richardson, and the long train of sceptical thought on which he rides, is well taken: these are not complete explanations, since they lack a great deal of evidential particulars.

Notice, it's not strictly necessary to formulate evolutionary psychology research as explanations – one could limit oneself to proposing a range of testable hypotheses: *if* trait *T* is an adaptation for *X*, trait *T* should have configuration *C*, and so we should find

phenomenon *P*. One need not then make the further claim that trait *T is* an adaptation for *X*. The evidential requirements outlined by Richardson only become active when we present such research as explanations. Nevertheless, Richardson would point out that evolutionary psychologists routinely do make this further step: 'This is the root problem that evolutionary psychologists face: they claim that human psychological traits are adaptations and thus need to *explain* them *as* adaptations.' (2007: 98, emphasis original)

Sceptics frequently lampoon what they see as the naive confidence of evolutionary psychologists when making the move from hypothesis to explanation. Sceptics are beholden to a picture of evolutionary psychologists testing a limited number of hypotheses on a limited number of subjects, and then declaring full-blown claims about human nature and working up great public policy recommendations. Kitcher (2009: 91-92) captures the impression well:

> 'I would be less concerned to hold evolutionary studies of human psychology and behavior to high standards, if we lived in a world in which the findings of such studies were soberly presented as preliminary, in which the difficulties of devising adequate evolutionary models of human psychology and behavior were clearly understood and acknowledged, in which "results" were not trumpeted to the general public as exiting new discoveries, firmly established'

If evolutionary psychologists really do believe that their hypotheses stand as complete, established explanations, and market them as such, then this should indeed be highlighted and challenged. Sceptics shouldn't stand for it.

Happily, they don't need to. The lesson was taken in and digested long ago – if, indeed, the lesson was ever needed. For example, in a review of Jerry Fodor and Massimo Piatelli-Palmarini's *What Darwin Got Wrong*, Barrett (2011: 76) observes that the authors provide,

> 'the kind of cautionary note that seems all the rage amongst philosophers writing about "Darwinism" at the moment: a scolding to those of us who are perceived as running amok in the playground of adaptationist explanation (e.g., Buller, 2005; Dupré, 2001; Richardson, 2007)... I happen to find most of these scoldings a bit tiresome—how often do we have to be reminded that all traits of organisms aren't

> adaptations, or that hypotheses should be evaluated against alternatives?'

Absolutely. Perhaps some evolutionary psychologists need to be more modest in what conclusions they draw from the successful corroboration of their hypotheses. Perhaps some evolutionary psychologists need to be more alert to alternative explanations. But this is true of every discipline. Name me a discipline where this isn't true. A shiny penny for every research programme about which this could not be said. The warning not to rush to judgment or jump to conclusions might sound reasonable enough, but the continuous repetition of this point, the continuous *recycling* of this point, is subversive: this is how legitimate viewpoints can be marginalised and patronised away. How many times do highly educated professionals need to be told that while 'evolutionary analyses may generate clues as to the mechanisms of human cognition, these are best regarded as hypotheses, not established explanations, that need to be tested empirically' (Bolhuis *et al*. 2011: 4)?

A sceptic might point to the avalanche of public news about evolutionary psychology findings, how sensationalist they are given their incompleteness with respect to evidential particulars. A sceptic might hold that evolutionary psychologists really do need to be schooled in how to interpret their findings in a way that acknowledges the limitations of what successful experimental outcomes demonstrate. But in response, we should counter that evolutionary psychologists can't be held responsible for how their research enters the public domain in popular science newspaper articles and then disseminated. Science reporting is rife with sensationalist interpretations of quite limited studies. We'd take a dim view on someone who thought quantum physicists need to be re-schooled in the basics of science just because their work is occasionally sensationalised in the popular media.

I want to leave that point to one side now – I believe it's been sufficiently made. The deeper point I wish to raise is this: the charge that evolutionary psychology explanations – when their research is presented as explanation – are incomplete is well taken but, crucially, we should not take from this that their research is therefore methodologically sterile, inert, that all that they're producing is just-so stories.

Indeed, such an inference is deductively invalid. Even if the evidence required to fully vindicate adaptationist explanations is lacking, this fails to undermine the notion that much is to be gained through adaptationist hypothesizing in psychology. Hence, neither the current paucity of knowledge of the past nor pessimism about prospects of gaining such

knowledge in anyway destabilises the legitimacy and utility of evolutionary psychology. A US university textbook on the biology of behaviour makes the eminently sensible observation that lack of certainty concerning our evolutionary past does not frustrate the utility of the evolutionary perspective in psychology: 'Although the events that led to the evolution of the human species can never be determined with certainty, thinking of the environmental pressures that likely led to the evolution of our brains and behavior often leads to important biopsychological insights.' (Pinel 2008: 3)

None of the considerations Richardson tables have any bearing on whether evolutionary psychology is useful or not, on whether it's merely speculation or not. Indeed, one could argue for further explanatory and evidential requirements, or challenge some of the items on Brandon's list; but since evolutionary psychology primarily operates in the context of discovery, not the context of justification, the point is rather moot.

Indeed, Richardson inadvertently hints at this. Usually, Richardson dismisses evolutionary psychology wholesale, but he occasionally softens his line a little. For example, we read,

> 'I do not generally claim that these evolutionary hypotheses concerning human psychology are false. I do think that some do not warrant serious consideration, for lack of evidence. For all intents and purposes, we can dismiss them. Some may eventually yield to empirical discoveries. If this is so, then so be it.' (Richardson 2007: 38)

Clearly, hypotheses yielding empirical discoveries are not 'idle', are not merely 'speculation', and we shouldn't respond to such discoveries with the attitude 'then so be it'.

So although detailed ancestral population information, knowledge of variation and drift in ancestral populations, is certainly highly desirable, evolutionary psychology can nevertheless successfully progress without it. And crucially, the progression of evolutionary psychology can help orchestrate activity that might bring about more complete explanations in the long-run.

In the previous chapters, I argued that hypothesizing certain traits as adaptations in the manner methodologically recommended by evolutionary psychology can be useful in three ways:

H1 Hypothesizing unknown design features of extant psychological mechanisms, leading to novel predictions of psychological phenomena.

H2 Hypothesizing unknown psychological mechanisms, leading to novel predictions of psychological phenomena.

H3 Hypothesizing traits as by-products of psychological mechanisms, leading to novel predictions of psychological phenomena.

Now I identify and argue for another use of evolutionary psychology:

H4 Stimulating multidisciplinary research activities, leading to more sophisticated hypotheses and more complete ultimate and proximate explanations.

An evolutionary psychology hypothesis postulates an adaptation, or a by-product from a set of adaptations, and a set of observable consequences that should obtain. When successfully corroborated, this produces a new line of evidence. But a new line of evidence is insufficient to establish a complete explanation. It's for the evolutionary and behavioural sciences collectively, not evolutionary psychology alone, to establish the required evidential particulars.

Richardson and other sceptics fail to pick up on this. We must, however - and we shall.

6.2 A wider constituency

In order to properly understand a psychological adaptation, we need not only a complete ultimate explanation, as highlighted by Richardson and others, but also developmental and neurological details – in other words, we need complete evolutionary (ultimate) explanations and complete proximate explanations. Or, to use Tinbergen's (1963) fourfold distinction, we need information on the trait's function, phylogeny, ontogeny, and mechanism. Whether one presses for further evolutionary details, or further proximate details, or both, it should be obvious that establishing such details calls for multiple lines of evidence – from multiple disciplines.

Establishing ultimate and proximate details requires the tools, techniques, methods, and knowledge from the full range of the evolutionary and behavioural sciences. It requires not

only evolutionary psychology but also archaeology, evolutionary anthropology, genetics, behavioural genetics, behavioural ecology, evolutionary biology, developmental biology, developmental psychology, comparative psychology, cognitive psychology, and evolutionary cognitive neuroscience.

Evolutionary psychologists cannot be jacks of all trades. Kitcher (2009: 81-82) claims,

> 'Evolutionary psychologists must *themselves* do work that is as precise in its modeling, as painstaking in its accumulation of data, and as sensitive to alternative hypotheses as that undertaken in a large number of admirable evolutionary studies. Yet even in the most celebrated examples, the work of Leda Cosmides and John Tooby on detection of cheating, for instance, the modeling is pitifully crude in comparison to that found in mainstream evolutionary theorizing, the knowledge of environmental details is rudimentary and the underlying genetics simply absent' (emphasis original)

Coyne (2009: 230) express a similar sentiment:

> 'We should be deeply suspicious of speculations that come unaccompanied by hard evidence. My own view is that conclusions about the evolution of human behavior should be based on research at least as rigorous as that used in studying nonhuman animals. And if you read the animal-behavior journals, you'll see that this requirement sets the bar pretty high, so that many assertions about evolutionary psychology sink without trace.'

Why must evolutionary psychologists themselves do all this work? Yes, evolutionary psychologists should be aware of alternative hypotheses, but why should evolutionary psychologists alone accumulate the full portfolio of data required of a complete adaptation explanation, data that requires a multitude of methods and disciplinary skills? Why should they alone establish the full portfolio of developmental knowledge of how genes and environments build specific adaptations? Are critics seriously expecting evolutionary *psychologists* alone to do all this work?

Evolutionary psychologists cannot be expected to engage in genetic and developmental research. They cannot be expected to sequence genomes, to hunt for molecules, to dig around archaeological sites, to enter caves deep and ancient. They cannot be expected to

perform paleontology, paleobiology, arrange and partake in expeditions to remote hunter-gatherer societies, nor to train part-time for PhDs in neuroscience.

As we saw in chapter 3, evolutionary psychologists tend to make psychological predictions and perform psychological experiments, exactly as is to be expected from their disciplinary pedigree. Exactly what one would expect of *psychologists*. They primarily test predictions using self-reports, archive data, and behavioural studies. That's their speciality. That's what they're trained to do.

Evolutionary psychology's role in the evolutionary and behavioural sciences is discovery. Its methodology is crafted for that very role. But evolutionary psychology's tools alone cannot complete the task of furnishing complete explanations. Evolutionary psychology can *contribute* to the portfolio of evidential details in virtue of its successful novel predictions but its *modus operandi* is in the context of discovery. So evolutionary psychology alone cannot do much of what the sceptics demand of it, but the bigger point is that it can stimulate research by other research programmes into gathering such evidence. It is for a wider constituency - the evolutionary and behavioural sciences collectively - to unearth the evidence required to authenticate and justify evolutionary psychology hypotheses.

Schmitt and Pilcher (2004) argue that to support a psychological adaptation claim, evidence should be presented from eight disciplinary lines of support: psychological evidence (such as surveys and behavioural tests), medical evidence (such as fertility and mental health studies), genetic evidence (behavioural genetics, population genetics, molecular genetics), physiological evidence (such as neuroanatomical structures), phylogenetic evidence (comparative psychology, primatology, physical anthropology), hunter-gatherer evidence (from anthropology, and also human behavioural ecology), cross-cultural evidence (cross-cultural behavioural patterns and ecology-dependent variability), and theoretical evidence (such game theory simulations, computer modelling, and cost-benefit analyses). Of course, one or two of these categories might be questioned. For example, one might argue that contemporary hunter-gatherer studies are of limited relevance, while another might argue that contemporary hunter-gatherer studies offer important evidence of ancestral activity patterns. Regardless, it should be incontrovertible that vindicating an adaptation claim requires resources from a number of evidence categories.

Evolutionary psychologists obviously directly contribute to the first category, the category of psychological evidence. Since the postulation of a psychological adaptation raises

empirical questions and issues in cognate and adjacent fields, there is no reason why evolutionary psychologists can't work in tandem with these disciplinary fields. Indeed, this is already happening, albeit slowly, albeit in a piecemeal fashion.

When postulating a psychological adaptation, perhaps the most immediate question is, how cross-cultural is the trait? What ranges of expression can be found cross-culturally? Is there predictable variability of expression based on local environmental (social and ecological) and developmental conditions? Cross-cultural studies are where notable collaborations have arisen. For example, during the last few years, the International Sexuality Description Project, a collaboration involving over 100 biological scientists, behavioural scientists, and social scientists, has conducted several series of tests to examine whether evolutionary psychology predictions obtain cross-culturally. In one series of tests, Schmitt *et al.* (2003) tested for an evolutionary psychology hypothesis concerning sex differences in the desire for sexual variety, surveying 16,228 people via anonymous self-reports in 52 nations across the 10 major world regions - North America, South America, Western Europe, Eastern Europe, Southern Europe, the Middle East, Africa, Oceania, South Asia, and East Asia. This remains one of the largest cross-cultural investigations undertaken on whether the sexes differ in the desire for sexual variety. The 118 biological scientists, behavioural scientists, and social scientists who co-authored the paper, and who collected and analysed the test results, found that the predicted sex differences obtain. Across cultures, men tend to not only have a greater desire for a variety of sexual partners than women but they also tend to require less time to pass before agreeing to sexual intercourse. Furthermore, across cultures, men tend to more actively seek short-term relationships than women.

Evolutionary psychologists are also collaborating with neuroscientists. Platek and Shackelford (2009) is a collection of several papers in the emerging field of evolutionary cognitive neuroscience, which combines evolutionary psychology with cognitive neuroscience. Takahashi and Okubo (2009) investigated sex differences in the neural correlates of jealousy using functional neuroimaging techniques. They observed sex differences in brain activations in response to sexual and emotional infidelity scenarios, 'supporting the view that men and women have different neurocognitive systems to process a partner's sexual and emotional infidelity' (*ibid.*: 211)

Takahashi and Okubo (2009) noted that, for ethical reasons, it's not possible to investigate brain activity in response to actual instances of infidelity. Instead, as per common practice,

the researchers investigated brain activity in response to hypothetical infidelity scenarios. Indeed, as Kitcher and Vickers (2003) note, certain types of experiments that could make significant progress in evolutionary studies of psychology are forbidden as unethical. We cannot genetically or neurologically manipulate humans, nor can we deprive humans of certain developmental contexts. We are handicapped in some respects when studying humans. But we need not overplay this. First, this is not a problem unique to evolutionary psychology: psychologists of all stripes are often ethically bound to postulate hypothetical scenarios on their human test subjects, rather than present actual ones. Second, studying non-human animals also has its handicaps - one cannot, after all, ask a primate his or her feelings or preferences on various topics of interest. Third, there are also 'natural experiments', so to speak, that can be appealed to and analysed – such as those unfortunate enough to suffer a genetic-based disability, those unfortunate enough to suffer accidents, and twin studies, and studies of feral children.

Evolutionary psychologists can, and sometimes do, think across species. For example, Kramer, King, and Ward (2011) discovered that untrained humans can accurately judge aspects of chimpanzee personality from pictures of static, nonexpressive chimpanzee faces alone. On the strength of this result and other studies, they identified the possibility of a shared signal system between the two species: 'We hypothesize a shared signal system for personality from the face in humans and chimpanzees; that is, on the basis of their shared evolutionary past, chimpanzees and humans share aspects of a system for communicating behavioral biases to conspecifics.' (2011: 183) Kramer and Ward (2012) replicated and extended those findings using a new group of chimpanzees.

It should be no surprise that evolutionary psychologists rarely use phylogenetic tools. Phylogenetic analysis is a skill set. It takes training to be proficient in such a skill, and time to become suitably knowledgeable in it. Perhaps evolutionary psychologists could become more aware of relevant phylogenetic analyses, and perhaps a few might even opt to train to use this tool. But this falls short of requiring evolutionary psychologists themselves to generate phylogenetic information concerning primitive and derived characteristics. Instead, phylogenetic specialists can work in tandem with evolutionary psychologists.

And it should be no surprise that evolutionary psychologists rarely cite genetic research. Kitcher and Vickers (2003: 337) misread this as a tactical ploy: 'the latest Darwinizers have learned from the demise of old-style pop sociobiology: Be cagey about genetic hypothesizing!' Caginess? That's not quite right. Evolutionary psychologists focus on

developmentally reliable adaptations, not on genes. And again, perhaps geneticists and developmental biologists knowledgeable of evolutionary psychology work, or working in tandem with evolutionary psychologists, can make progress in this direction.

Proposing novel hypotheses occurs all the time in other parts of the evolutionary sciences without the slightest hint of controversy. As Dennett (2011: 482) notes,

> 'It is worth noting that evolutionary biologists confidently hypothesize historical events—horizontal gene transfers, for instance, that occurred billions of years ago, give or take a few hundred million years, or speciations and migrations that must have occurred at some point, ill-defined in space, time and causation—without fear of being chastised for indulging in Just So Stories. It is pretty much only hypotheses about human evolution that are held to a higher—conveniently unattainable—standard of evidence, by the critics of sociobiology or evolutionary psychology. Yes, there are egregious cases of hypotheses being defended solely on grounds of their plausibility, given the few facts available, but they shade into entirely reasonable cases—across biology, so far as I can see—with no clear boundaries. Much of the progress in evolutionary biology consists in the confirmation or disconfirmation of bold hypotheses that started out as plausible guesses'

Many hypotheses in the evolutionary sciences are born naked, lacking great swathes of information, and only gradually do they become clothed in detail. They are not dismissed as mere speculation. No-one demands that they immediately enter the world fully formed. They are seen for what they are: the first step in perhaps a new, promising, and potentially long and fruitful, line of research, one that can trigger multiple disciplinary engagements, perhaps leading to modification of the original hypothesis or explanation, perhaps leading to abandonment, perhaps someday to acceptance and consensus.

Laland and Brown (2011b) discuss the co-evolution of dairy farming and lactose tolerance, which we touched upon in chapter 2. Simoons (1969, 1970) originally hypothesized that dairy farming created selection pressures that led to genes facilitating lactose tolerance becoming common in dairy farming societies. What is striking is how interdisciplinary subsequent research in this area was. Laland and Brown note that a variety of researchers – anthropologists, archaeologists, geneticists, and so on – engaged with Simoons'

hypothesis in their various ways. Geneticists identified a gene responsible for lactose tolerance, and statistical geneticists confirmed that it shows strong signs of recent selection. Phylogenetic methods and statistical methods were deployed, both to rule out alternative explanations and to further support the dairy farming hypothesis. Furthermore, on the strength of the hypothesis, novel predictions were made. Laland and Brown point out that Durham (1991) predicted that populations with traditions for consuming fermented milk products but not fresh milk should have intermediate levels of lactose tolerance, since fermented milk products have lower levels of lactose than fresh milk. The novel prediction was subsequently empirically supported.

Forty years of multidisciplinary research has produced an impressive portfolio of evidence vouching for the co-evolution of dairy farming and lactose tolerance hypothesis. There is no reason in principle why successful evolutionary psychology hypotheses, those which produce successful novel predictions, making genuine contributions to knowledge, and which survive refutation attempts, cannot stimulate cognate and adjacent fields to pursue further research in directions evolutionary psychologists alone cannot practically reach, to establish better, stronger portfolios of evidence for postulated psychological adaptations.

Such multidisciplinary co-operation takes time. It also requires an awareness and knowledge of the status of purported psychological adaptations, as well as an ecumenical spirit between the various programmes and disciplines in the evolutionary and behavioural sciences. There are, however, stumbling blocks to cross-disciplinary understanding and ecumenicalism. First, researchers in adjacent disciplines might only have a rudimentary understanding of the state of play in evolutionary psychology, only being aware of a handful of purported psychological adaptations, and will often have little idea as to what dossier of evidence is available for each purported psychological adaptation. Second, the evolutionary psychology debate is highly polarised. While this polarisation might be less pronounced on the ground than it is in the literature, it nevertheless does have a detrimental effect (for example, recall Gluckman and Hanson's (2008) characterisation of evolutionary psychology, which we launched chapter 2 with).

I'll tackle the second problem in the next chapter. Here, I identify and discuss a forthcoming development that has every chance of transforming the first problem. Balachandran and Glass (2012) report on the forthcoming launch of 'PsychTable', a taxonomy aiming to codify the number of psychological adaptations that have been hypothesized in the literature, and to evaluate the strength of evidence for each purported psychological adaptation by

aggregating both the supporting and negative evidence available. The taxonomy cites and utilises Schmitt and Pilcher's (2004) identification of eight categories of evidence needed to vindicate a psychological adaptation claim. Against these eight lines of evidence, the PsychTable can identify both the evidentiary breadth and the evidentiary depth of each purported psychological adaptation.

Evidentiary breadth pertains to the range of evidence an adaptation claim enjoys. According to Balachandran and Glass, citing Schmitt and Pilcher, at minimum, a purported psychological adaptation must have evidence falling into one of the eight evidence categories. A 'moderate' level of evidential breadth is held to be an evidence portfolio spanning two or three of the evidence categories. An 'extensive' level of evidential breadth is when four or five evidence categories can be cited. Finally, one can judge the evidential breadth of a purported psychological adaptation to be 'exemplary' when six of more evidence categories can be cited.

Evidentiary depth pertains to the quality of evidence an adaptation claim enjoys. According to Balachandran and Glass, citing Schmitt and Pilcher, single studies with poor methodological control or unrepresentative sampling represent minimum evidentiary depth. At least two studies with good levels of methodological control and good sampling quality represent a 'moderate' level of evidentiary depth. Several studies with high levels of methodological control and high sampling quality represent 'extensive' evidentiary depth. Finally, multiple studies with the highest levels of control and sampling represent an 'exemplary' level of evidentiary depth in favour of a purported psychological adaptation.

The PsychTable will aggregate the citations using an algorithm to evaluate the strength of support for each purported psychological adaptation, automatically assigning an overall evaluative score, as well as generating a table indicating currently available evidence.

The PsychTable offers a rich harvest of benefits desperately needed in the literature. First, by codifying purported psychological adaptations and evaluating their breadth and depth of evidence, it will become immediately apparent to researchers, both within evolutionary psychology and in adjacent disciplines, where further research is needed. As Balachandran and Glass (2012: 313-314) note,

> 'PsychTable will allow researchers and contributors to aggregate studies and evidence from across the spectrum of evolutionary behavioral sciences to classify the adaptations that have been soundly supported,

> *as well as call attention to those which may be lacking in empirical support*. In this way, the project will help researchers debate and empirically evaluate which psychological phenomena are evolved adaptations and which are not, irrespective of formal academic boundaries.' (emphasis mine)

A purported psychological adaptation might have evidence falling in the psychological category (as is likely to be the case for most of these purported adaptations), and evidence falling in the cross-cultural category, but lacking hunter-gatherer evidence, physiological evidence, as well as only having moderate evidentiary depth in some categories. Further studies might be required within the existing categories of evidence or fresh investigations in the other categories of evidence, often requiring the tools and knowledge of adjacent practitioners.

Second, codifying and evaluating evolutionary psychology research should better motivate evolutionary psychologists into pursuing co-operation with researchers across the behavioural sciences spectrum. For example, reading the PsychTable might reveal that developmental evidence for a purported adaption is entirely absent. An evolutionary psychologist who champions the purported adaption in question might then actively seek out co-operation with those who specialise in child development research, to investigate rigorously whether the psychological trait develops easily, quickly, and reliably.

Third, the PsychTable might better organise research efforts. The evidential standing of a purported psychological adaptation might not be as extensive or as rigorous as generally believed. Indeed, there might be strong evidence against a purported psychological adaptation. Knowing the state-of-play will better determine where precious research effort is best spent, which purported psychological adaptations are the most promising, and which are the least promising. Indeed, 'one of the consequences of aggregating published evidence to support or refute purported EPAs [evolved psychological adaptations] will inevitably be the revelation that some or many mental mechanisms proposed and even accepted in the evolutionary psychology literature will not pass empirical muster' (*ibid*.: 314).

Fourth, the PsychTable should enable the layman to better understand the evolutionary psychology programme. Buller's (2005) underlying motivation was to correct what he took

to be the layman's misunderstanding of the evidential strength of evolutionary psychology. As Buller (2005: 16) says,

> 'Evolutionary Psychologists have been very successful in conveying their ideas to a broader public, and I strive in this book to covey to the same broad public the other side of the story. Everyone should be able to understand the problems with Evolutionary Psychology and to understand why we must move beyond Evolutionary Psychology in order to one day achieve a better evolutionary psychology.'

With the PsychTable, we don't have to rely on contentious representations – either from overly enthusiastic proponents or from overly ungenerous sceptics. Rather than being held hostage to a particular individual's experience, understanding, and representation of the literature, we can freely and easily access a much firmer and objective evaluation of the state-of-play, both for each purported psychological adaptation and for evolutionary psychology as a whole.

I hope the reader will feel a quiet sense of excitement at this. I certainly do. With a working beta version expected to be available by late 2012, this, I believe, could be a game changer. No longer does the literature have to proceed in the all-too-predictable dialectics of 'this is a psychological adaptation', then 'but you don't have evidence from this category', then 'well we do, see such and such study in 2002, why didn't you cite that', then 'well that evidence is weak', then 'actually it's a lot better than that', then 'well what about this...' etc. With the ever escalating amount of work done in evolutionary psychology, an accurate characterisation of its actual output is needed now more than ever (hence why in this thesis I emphasize what the programme is capable of doing, as evidenced in localised cases – until the PsychTable is live, we have insufficient grounds for making judgements about the global practice of evolutionary psychology, its overall practice, a point I made in chapter 4). By classifying and evaluating evidence and making this easily available in a central location, clear pathways for new research will become visible from the restless sea of research. This could significantly help foster the interdisciplinary cooperation needed to vigorously vindicate psychological adaptation claims. It is certainly something to hope for. And it may just happen.

6.3 A moderate epistemological optimism

Yes, the evidence required to completely vindicate adaptation claims is difficult to accumulate, very difficult. Nevertheless, this doesn't mean it's unlikely the appropriate evidence will ever be forthcoming. Nor should it be a reason to be uniquely sceptical of evolutionary psychology furnishing more complete evolutionary (ultimate) explanations – the issue of paucity of historical details applies whenever we seek to model evolutionary processes, and it applies as equally to non-selectionist evolutionary models as it does to selectionist models (if one claims that drift played an important role in the evolution of some inherited trait, to establish a complete explanation, one still requires the evidential particulars of ancestral population size and so on). Furthermore, this is no different to the rest of science. In whatever branch of science that attempts to model and uncover unobservables, reality has to be fought and won. History bears witness to many dismissing embryonic scientific developments as speculative, only to find themselves on the wrong side of history. In an entirely different context, Callender (2012) says,

> 'I think it's a fool's errand to try to prejudge inquiries on the basis of being too speculative or not. History is littered with embarrassing judgments stating that such-and-such is too speculative – e.g., Newtonian gravity, quantum non-locality, relative simultaneity - only to see those ideas later vindicated. I want no part of that tradition.'

The point carries over perfectly into the context of our present discussion.

The frequent strides made by the evolutionary and behavioural sciences, by geneticists, evolutionary anthropologists, archaeologists and others should give those given over to counsels of despair some pause for thought. It seems hardly a month goes by now, perhaps even hardly a week goes by, when another facet of the past is illuminated still further.

One fast-moving area of research is the reconstruction of ancestral population migration patterns and gene flows between ancestral populations, one of the five evidence categories cited by Richardson. William Blake wrote of seeing a world in a grain of sand. In a way, we can now do likewise: we can now see a world in a strand of hair. Rasmussen *et al.* (2011) sequenced the Aboriginal Australian genome from an Aboriginal Australian's strand of hair. Comparing the sequenced Aboriginal Australian genome with the DNA from other populations around the world revealed that the ancestors of modern Aboriginal Australians were the first to separate from other humans in Africa, dispersing into eastern

Asia between 62,000 and 75,000 years ago, a dispersal separate from the one that gave rise to modern Europeans and Asians between 25,000 and 38,000 years ago. And on 30 August 2012, a paper in *Science* announced the successful sequence of the Denisovan genome from a tiny finger bone fragment, enabling, according to the authors of the research paper, 'detailed measurements of Denisovan and Neandertal admixture into present-day human populations, and the generation of a near-complete catalog of genetic changes that swept to high frequency in modern humans since their divergence from Denisovans' (Meyer *et al.* 2012).

Finger fluting research is another fascinating window into the past. Finger fluting is a type of cave art, where marks have been made into clave clay with fingers. Sharpe and Van Gelder (2005, 2006) developed a methodology allowing different individuals to be distinguished on the basis of the flute measurements, even allowing the age, and sometimes even the sex, of the individuals to be determined. Intriguingly, deep inside the Rouffignac Cave, France, such art is found not only on cave walls but also high up on cave ceilings, a few feet higher than can be done by an adult tiptoeing. Analysis of the flutes on the ceiling walls, likely to be at least 13,000 years old, reveals they were done by children, presumably lifted up by an adult or supported on an adult's shoulders.

A third instructive example: Berna *et al.* (2012) conducted microstratigraphic investigations on burned bone and ashed plant remains in the Wonderwerk Cave, South Africa. The molecular analytical technique uncovered 'unambiguous evidence' that the burning of the bones and ash took place in the cave during the early Acheulean occupation in the Lower Pleistocene, approximately a million years ago. This means ancestral populations were using fire 300,000 years earlier than previously thought. The researchers concluded, 'We believe microstratigraphic investigations at Wonderwerk cave and other early hominin sites in Asia and South and East Africa will have a significant impact in providing fundamental evidence for the appearance of use of fire and its role in hominin adaptation and evolution.' (Berna *et. al.* 2012: 1220)

Furthermore, two recent research papers shed further light on the Neanderthals' behavioural activities. First, in a research paper titled 'Neandertal Humeri May Reflect Adaptation to Scraping Tasks, but Not Spear Thrusting', Shaw *et. al.* (2012) examined Neanderthal arm asymmetry. As evidenced by their skeletons, Neanderthals had drastically overdeveloped upper right arms: their upper arm bones are up to 50% stronger on the

right side than on the left. This arm asymmetry is significantly more pronounced than what obtains in modern humans – our upper arm bone strength asymmetry is only around 10%.

The arm asymmetry is indicative that Neanderthals were doing something laborious and repetitive, and that this represented a considerable role in their lives. Evolutionary anthropologists usually explained the pronounced arm asymmetry as the result of the Neanderthals, who were right-handed, using spears when hunting. Hunting clearly takes up a great deal of time and would have clearly been a critical activity.

Shaw *et al.* (2012: 2) proposed that other possibilities are viable and that 'testing would help to assess whether this, or any other functional hypothesis, is the most appropriate explanation for the unique morphological characteristics of Neandertals'. Side-scrapers, a tool use to scrape tissue from the underside of animal hides, are often found with Neanderthal skeletal remains. Scraping animal hides would have been a laborious and repetitive task. Accordingly, the researchers investigated whether Neanderthals' arm asymmetry is better explained as an adaptation for scraping, rather than spear-thrusting.

The researchers used electromyography, a technique for measuring the electrical activity produced by skeletal muscles, to measure muscle activity in the three deltoid (shoulder) muscles during three underhanded spear-thrusting tasks and four scraping tasks. They found that the muscle activity required to perform various spearing tasks does not explain the overdeveloped upper right arm strength, whereas the results were consistent with the muscle demands imposed by scraping activities.

While acknowledging the study is not conclusive, and that further research is needed, the researchers nevertheless stressed 'these results yield important insight into the Neandertal behavioural repertoire that aided survival throughout Pleistocene Eurasia.' (Shaw *et al.* 2012: 4) If it is indeed the case that Neanderthal arm asymmetry is better explained as an adaption to scraping tasks, or at least is a significant behaviour determining the asymmetry, then this is indicative that Neanderthals spent a sizeable amount of time on daily subsistence tasks such as preparing skins, possibly for producing clothes (especially likely, given the cold conditions Neanderthals lived in). We therefore gain a richer understanding of the Neanderthals' daily activities.

Second, Hardy *et al.* (2012) extracted material entrapped in the dental plaque of five Neanderthal skeletons in the Sidrón Cave, Spain. Molecular evidence of the material revealed that, in addition to meats, the Neanderthals at the site also ate a range of cooked

plants, including bitter-tasting medicinal plants. This 'suggests that the Neanderthal occupants of El Sidrón had a sophisticated knowledge of their natural surroundings which included the ability to select and use certain plants' (2012:617).

It's astonishing, truly a marvel of recent years, that technological advances are enabling us to recover migration patterns and gene flow from a strand of hair or fragment of bone, profile individual finger fluters, establish the advent of fire-making with every greater accuracy, and recover ever more of Neanderthals' daily activities and dietary patterns. Mithen (2007: 60) discusses a different set of considerations, but the conclusion he draws is equally applicable here:

> 'In a very real sense, this evidence does allow us to "directly observe what happened during human evolutionary history". Not "observe" in the behavioral sense of witnessing specific actions taking place, but in terms of the material products of that behavior, whether it is debris from manufacture of stone artefacts or paintings on cave walls.'

It's likely that that our knowledge of the past, our knowledge of ancestral populations - population migrations, gene flows between populations, and population structures - will continue to grow and mature. Multidisciplinary co-operation is an important driver of increasing such knowledge. When reconstructing our evolutionary past and the evolution of our psychology, evolutionary anthropologists, evolutionary psychologists, cognitive scientists and others should co-operate with one another. Indeed, some of the most exciting and important progress in the evolutionary sciences has arisen from the co-operation, convergence and synthesis of fields. After all, the modern evolutionary synthesis, the consensus framework of modern evolutionary biology, is the synthesis of genetics with natural selection, a (albeit delayed) dialogue between mathematical geneticists and naturalists, between those who studying hereditary, and those studying populations of organisms. In more recent times, molecular evolution and phylogenetics have converged to form molecular phylogenetics, which seeks to establish the rates and patterns of DNA change and to reconstruct the evolutionary trees of genes and organisms, generating fresh insights on a variety of issues, generating new insights into the past.

So we're creating ever better models of the past, illuminating the past like never before. Think of where we've come from, from the time before Darwin, when prehistory was a mystery, how much progress has been made since *On The Origin of Species* was first

published a little over 150 years ago, and how much more awaits to be recovered as more and more genomes become sequenced, as computational power grows, as ever more complex demographic scenarios can be computed and then matched against sequenced genomes and other data sets, as new techniques and tools are invented allowing innovative inferences, as cross-disciplinary activities are further utilised, as evolutionary anthropologists and cognitive psychologists work together more, as the evolutionary and behavioural sciences slowly, quietly converge at various points – and as evolutionary psychology's own hypotheses become ever more sophisticated, in tandem with empirical discoveries in cognate and adjacent fields, and as its own output grows still further and stronger.

Thus, we shouldn't be ignorant of, or dismiss as hopeless, the ever increasing strides in modelling and uncovering the past made by archaeologists, geneticists, evolutionary anthropologists, and others. Such efforts give us real grounds to be cautiously optimistic about the success of gathering sufficient evidence in the long-run to better complete evolutionary (ultimate) explanations – whether selectionist or non-selectionist. At the very least, such remarkable efforts offer little in the way of support for poorly thought-out counsels of despair.

6.4 Conclusion

Richardson updates a complaint with a long pedigree: a complete adaptation explanation requires specific and detailed information about, *inter alia*, the kind and degree of trait variation present in ancestral populations and environments, the specific ecological factors affecting survival and reproduction, and demographic information, such as population size and gene flow. Examining evolutionary psychology explanations, one often finds such details largely absent. Sceptics then conclude that without such detailed information concerning the kind and degree of variation in ancestral populations, specific ecological factors these ancestral populations faced, and specific information on the demographic structures of ancestral populations, evolutionary psychology hypotheses are idle, inert, merely just-so stories, merely Darwinian fairytales for adults.

Complete adaptation explanations are demanding. And the sceptics are quite right in drawing attention to this. But highlighting this issue will not win for them the conclusion they seek. Crucially, and here the sceptic is invited to pay close attention, we can pursue

adaptationist hypothesizing without having to first establish many of the evidential particulars required of a complete adaptationist explanation. Indeed, we engage in adaptationist analysis precisely as a first step in a journey, a long journey, towards establishing such details.

Evolutionary psychology explanation stands continuously exposed to demands for further evidence and details and this shapes directions of research. So yes, while complete portfolios of evidence do not yet exist, the bigger point is that evolutionary psychology can help stimulate other research programmes and disciplines into helping find such details. The forthcoming launch of the PsychTable, a classification system that seeks to codify purported psychological adaptations in one location and evaluate their evidential breadth and depth, should help facilitate this.

Thus evolutionary psychology's role in the evolutionary and behavioural sciences is that of discovery – it's for the evolutionary and behavioural sciences collectively to recover the spectrum of evidential details. Evolutionary psychology can directly lead to new discoveries, as Richardson reluctantly acknowledges, and it has done so, as I have indicated in previous chapters, and it can potentially indirectly lead to new discoveries via stimulating other research programmes, as I have argued in this chapter. So we can, and must, shift the burden of the argument to the other side. Why not carry out evolutionary psychology? The heuristic reasons I have been identifying and vindicating throughout this thesis are, I believe, irresistible at this point. Nothing the sceptics have raised destabilises or discredits this fundamental point.

Chapter 7

Depolarising the debate

In the previous chapter, we saw how evolutionary psychology has the potential to stimulate multi-disciplinary research activities. One of the stumbling blocks to this possibility being fully realised - the lack of a central database of purported psychological adaptations - will soon be removed. But the other stumbling block, the polarisation of the debate around evolutionary psychology, remains. In this chapter, I seek to address this issue. Depolarising the debate requires two things: it requires sceptics to recognise the rich heuristics of the programme, and it requires prominent evolutionary psychologists not to overshadow those heuristics with theoretical excess. In §7.0, I show how sceptics have made a basic mistake, which has led to a cascade of other mistakes, leading to their (incorrect) wholesale dismissal of the programme. In §7.1, I argue that if we want sceptics to focus on the heuristics of the programme, then those heuristics shouldn't be obscured or overshadowed. Furthermore, we need to also be alert to a more moderate scepticism, one which can be just as subversive as the hard scepticism. In §7.2, I set out this milder scepticism, which claims that evolutionary psychology needs to modernise. In §7.3, I argue that calls to modernise evolutionary psychology are based on a failure to recognise or appreciate its unique role in the behavioural sciences. I conclude by advocating that hard sceptics, prominent champions of the programme, and milder sceptics respectively focus on, refocus on, and value the heuristics of the programme.

7.0 The sceptics' basic mistake

The sceptics' basic mistake, the mistake that cascades a host of other mistakes, is to overlook evolutionary psychology's heuristics, to instead focus too strongly on the evidential incompleteness of its explanations. In other words, they get the wrong end of the stick. Given that evolutionary psychology functions in the context of discovery, this is a glaring oversight. Evolutionary psychology's scientific credibility does not derive from its ability to accommodate known social phenomena within an adaptationist framework, nor in its ability to advance plausible reasons for behaviour we already know about, but in its ability – its rich ability - to generate novel predictions about behaviours yet to be investigated. Here we are to find evolutionary psychology's methodological vindication. Here we are to find evolutionary psychology's value and function in the evolutionary behavioural sciences.

Unbelievably, Richardson writes for over two hundred pages without ever achieving the realisation that evolutionary psychology primarily functions heuristically. Relying on his book alone, one would not realise that evolutionary psychology has generated novel predictions, surprising predictions, and that many have been successfully corroborated. For example, Richardson (2007: 88) characterises Buss's research on jealousy as follows:

> '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. He is content to offer evidence from social psychology. Even if this is good psychological evidence concerning current differences between the sexes, it does not give us more than that.'

We can concede the first sentence. But what Richardson neglects to mention is that Buss didn't merely appeal to existing psychological evidence, but rather he created fresh psychological evidence. He expanded the explanandum of jealousy.

Buller (2005) spends over five hundred pages challenging evolutionary psychology, codifying, it seems, every criticism ever raised against the programme, but he never seriously recognises, let alone engages with, its heuristics. Kitcher and others call evolutionary psychology 'sociobiology reborn'. But to think of evolutionary psychology as sociobiology reborn is to confess to a profound misunderstanding of the programme, to signal that the basic picture of the programme hasn't been grasped. It is to miss that

evolutionary psychology is using ultimate considerations to shed light on proximate design, to instead take it as just another attempt to give evolutionary stories for extant behaviours.

Evolutionary psychology's heuristics should occupy centre stage in any evaluation of evolutionary psychology. But such considerations are absent or overlooked in the sceptical literature. That these sceptics fail to get the basic picture right is somewhat disconcerting. While the strident tones and extreme dismissal of evolutionary psychology found in these works might make for entertaining narratives, they don't make for a meaningful portrayal of the experience of hundreds of practitioners of the programme. On the contrary, this basic mistake fuels a seriously distorted picture of the programme – in works bearing unfortunate titles like 'Evolutionary Psychology as Maladapted Psychology'.

A sceptical literature based on a basic mistake and a big misrepresentation can only generate, and sustain itself by, a dizzying succession of other mistakes. I identify six such mistakes.

First, by overlooking its heuristics, sceptics inevitably develop a cartoon understanding of evolutionary psychology's methodology. As we've seen, evolutionary psychology's methodological framework is vibrant. Hypotheses can be continually tested, refined, modified, or abandoned. Far from being simple, it's a rich framework for generating novel hypotheses. Yet by overlooking the heuristics, sceptics fail to recognise, let alone engage, with this sophisticated methodology. Take Kitcher and Vickers (2003) as an example. Kitcher and Vickers call evolutionary psychology's history 'dismal' and then proceed to describe its methodology. It's worth quoting in full.

> 'Here's a recipe for winning fame and fortune as an architect of the new-and-improved human sciences. First, make a bundle of claims to the effect that certain features are universal among human beings, or among human males, or among human females. Next, couple each claim with a story of how the pertinent features were advantageous for primitive hominids, or males, or females, as they faced whatever challenges you take to have been prevalent in some lightly sketched savannah environment. (Don't worry that your knowledge of past environments is rather thin – Be creative!) Finally, account that each feature in the bundle has been shaped by natural selection, and so corresponds to something very deep in human nature (male human nature, female

> human nature), something that may be overlain with a veneer of culture but that molds our behavior and the forms of our societies. Accompany everything with hymns to the genius of Darwin, broadsides against "blank state" views of the human mind, and vigorous denunciations of the lack of rigor and clarity that has hitherto reigned in the human sciences.' (Kitcher and Vickers 2003: 333)

Note the unhealthy distance between this depiction of the methodology and the depiction of the methodology as advanced in this thesis. See how the methodology is setup to fail. Sceptics have a crude picture of evolutionary psychology methodology. It is therefore unsurprising that they find evolutionary psychology crude.

Second, overlooking the heuristics leads to a stark assessment of the programme, stingy with praise, lavish with condemnation. Evolutionary psychology has had a profound influence on charting efforts in psychology. For example, Cosmides and Tooby's work on the Wason selection task ignited much research into a previously ignored and overlooked area. Alternative explanations were proposed. Alternative explanations were experimentally tested. New experimental knowledge was won. Even if such research stands incomplete, and even if their interpretation of the results, their explanation, turns out to be false, evolutionary psychology has been instrumental in increasing knowledge in this area, as it has done so in multiple other areas - and it is likely to still do yet further. Evolutionary psychologists spend large amounts of time engaging in selectionist and engineering reasoning, formulating hypotheses, carefully teasing out surprising novel predictions, spending time constructing psychological tests, recruiting participants, collecting data, evaluating data, submitting their work to peer review, indicating where further lines of research are needed, further tests that need to be made - for which the sceptics denounce them for peddling in just-so stories, and greet their hard-won gains with the unforgettable line 'Some may eventually yield to empirical discoveries. If this is so, then so be it.' (Richardson 2007: 38)

Third, overlooking the heuristics leads sceptics to not recognising how evolutionary psychology merely follows normal scientific practice of modelling unobservables. Of course, behaviours don't fossilize. We don't dig around a cave, pick up a rock and say 'ah here's jealousy'. We'll never unearth a coalition fossil or a cheater detection fossil. But that's not a license for pessimism about recovering past selection pressures. Often the pessimism about recovering the past boils down to little more than a complaint that we don't have a

time machine, a point also noted by Campbell (2006: 90), who says 'some have argued that our inability to time travel to the environment of evolutionary adaptedness (EEA) means that we can never "prove" which environmental problem the adaptation was designed to solve.'

The point is that we can create models or hypotheses of the unobservable past, hypotheses that produce novel predictions, surprising predictions about observables – and this is merely the normal practice, the bread-and-butter, of science. We can't see many unobservables. But we don't give up on trying to understand what obtains beyond observation. We don't fall into counsels of despair about knowing the origins of the universe or what exists at the subatomic levels. By carefully building possible scenarios, by seeing what fits, we can eventually build credible models. Incidentally, on this criterion, evolutionary psychology is actually in a better position than M-theory, which is commonly noted for not generating novel predictions.

Fourth, because they are blind to its ability to generate novel predictions, sceptics simply don't see on what basis one could accept evolutionary psychology explanations. Even if there's currently only an incomplete portfolio of historical evidence, and even if ultimately that portfolio will remain incomplete, there are still grounds for accepting individual evolutionary psychology explanations over rival explanations. Despite lacking a range of evidential details, we're entitled to favour or accept evolutionary psychology explanations if they outcompete rival explanations by explaining the same range of facts as rival explanations while simultaneously making surprising predictions that the other explanations are blind to.

As a general mode of scientific procedure, we are inclined to accept hypotheses and explanations insofar as they make successful predictions of new data. As Lipton (1991: 135) observed, when referring to Dmitri Mendeleyev's 1869 periodic table of elements that reorganised the sixty elements known at the time and by doing so led to the prediction, subsequently corroborated, of two further elements, 'Sixty accommodations paled next to two predictions'.

In the social sciences, explanations are subject to massive underdetermination – many rival explanations can be spun to accommodate a given phenomenon. And this is not just a theoretical possibility, but an actuality: accommodations in the behavioural and social sciences are ten a penny. Therefore, in the behavioural and social sciences, predictive

success is usually a more compelling basis for evaluating rival explanations. If an evolutionary psychology hypothesis has made striking predictions, surprising predictions, highly counter-intuitive ones, which subsequently enjoy a collision of evidence from a variety of sources, then we're warranted to prefer or accept such an explanation, even if we lack all the evidential particulars. Nothing special here.

Imagine if someone came along and stipulated what the 'complete evidence' for the big bang theory would be. Suppose further that this sceptic claimed we don't satisfy every evidential particular, that we're unlikely to do so, and then concluded from this that the big bang theory is unbridled speculation. We would find that absurd. We would politely point out that we can evoke a range of hypotheses, each of which make alternative predictions about observables, over various measurements and indicators. One of them, the big bang theory, offers the best predictions and the best fit with data. On that basis, we're entitled to favour or accept the big bang explanation over rival explanations. Yes, alternative explanations are logically possible, and further research is always possible – but this is no different to the rest of science. Ultimately, we wouldn't take the sceptic seriously. We would consider her to be adopting a scientifically irresponsible attitude. Yet in the highly polarised arena of evolutionary psychology discussion, because the basic picture hasn't been grasped, arguments parallel to this are put forward that would be laughed out in other contexts.

Of course, evolutionary psychology-led discoveries can be accommodated by non-selectionist explanations. But this is uninteresting: all hypotheses can be contested, at least to some extent, and there's plenty of wiggle room to do so – there are always confounding factors with evidence, always alternative explanations possible. There's usually always enough wiggle room to save an explanation. The important point is to distinguish between programmes that lead to empirical discovery, and programmes that simply accommodate the fresh discoveries of other programmes.

Buller (2005) spends half his book looking at several case studies of evolutionary psychology, pointing out possible alternative explanations throughout, all the while failing to mention that all explanations are subject to underdetermination – indeed, I don't recall him even mentioning the problem of underdetermination once in his text, despite exploiting the problem for half his book (and he does a poor job with his case studies – see Cosmides *et. al.* 2005, Buss and Haselton 2005, Daly and Wilson 2005b, and Delton,

Robertson, and Kenrick 2006, who pinpoint a good deal of factual error in Buller's celebrated text).

And of course, some, many, or all of evolutionary psychology explanations might turn out wrong and eventually discarded. But again, how is that any different from the rest of science? This possibility is not unique to evolutionary psychology – it's ubiquitous to all scientific endeavour. One can never have a guarantee that one's research programme will work, and one never has a full explanation at the beginning. It took Newtonians decades if not centuries to provide a more or less complete explanation of the solar system's behaviour, and eventually this explanation was displaced (the famous case of Mercury's perihelion). That's simply how science progresses.

Fifth, focusing too strongly on evidential issues, and too little on the heuristics, means it's only a hop, skip, and jump from claiming evolutionary psychology research is merely speculation to saying its dangerous speculation. Evolutionary psychology hypotheses are routinely condemned as being just-so stories of the worst sort: of recasting stereotypes into an evolutionary mould. Rather than being a lamp to the past, they're a mirror of our prejudices. Evolutionary psychologists are suspected of infusing various biases – sexist biases, ideological biases – into their work, building into their hypotheses as a distorted image of their own views of society. 'Evolutionary accounts of human behavior often involve constructing the past from the present. For this reason, such accounts are highly susceptible to ideological biases and cultural blinders.' (Caporael and Brewer 1990: 287) And as Dupré puts it, 'Reading these accounts of male-female relations, one is struck by a mixture of the stereotypic, the outrageous, and the banal' (2001: 54).

Unsurprisingly, therefore, the sceptical tradition sometimes slides into unsporting language and naked anger. Daly and Wilson note that sceptics' incivility is a 'disturbing and sometimes perplexing element... They not just sceptical, they are angry, and we are still not entirely sure what they are angry about' (2007: 396). As we've seen, Kitcher and Vickers (2003) call evolutionary psychologists 'pop sociobiologists' and 'reformed pop sociobiologists'. Gray, Heaney, and Fairhall (2003) speak of their 'attack' on evolutionary psychology. Using disparaging terms is not by any means the worse thing one do, but it does illustrate how emotions have adulterated the sceptical literature.

I'm not claiming, and need not claim, that it's impossible for evolutionary psychologists to work various ideological biases into their work, but the assertion seems way overblown.

There's no evidence of this, and some evidence they don't. For example, Tybur, Miller, and Gangestad (2007) surveyed the political attitudes of 168 psychology PhD students in the USA, 137 of whom were self-identified as non-adaptationists, and 31 of whom were self-identified as adaptationists. The survey revealed that adaptationist PhD students in psychology are no more politically conservative than non-adaptationist graduate students, and are less politically conservative than US citizens in general. Furthermore, and more importantly, ideas take on lives of their own, beyond the motivations of their authors. Even if an evolutionary psychologist projected suspect stereotypes and ideology into his or her work, such hypotheses are unlikely to survive testing, scrutiny and the peer-review process.

Sixth, by getting the wrong end of the stick, sceptics of evolutionary psychology become isolated and adrift from developments in evolutionary psychology. The PsychTable we looked at in the previous chapter shows how fast moving evolutionary psychology and the evolutionary behavioural sciences are, and how increasingly disconnected and outdated the sceptical literature is. Indeed, if someone had the time to perform one of those word clouds on typical specimens of the sceptical literature, I'd venture certain word- and phrase-clusters would stand out above all others: 'adaptations aren't optimal', 'not all traits are adaptations', 'we don't know the past', 'speculation', 'just-so story', 'cheater-detection', 'rape adaptation', 'where are the genes', 'what about this possibility', 'what about that possibility', 'other explanations possible'. Especially 'other explanations possible' - as if evolutionary psychology is uniquely subject to underdetermination. This unhealthy focus on just a few cases and the running of underdetermination arguments makes the sceptical literature sound like it's stuck on auto play.

It's time to call time on hard scepticism. It's game over. Despite sceptics' efforts, the ever escalating proliferation of evolutionary psychology work strongly suggests the programme is here to stay. Its heuristic tools are just too useful to yield to counsels of despair.

And it must be game over, for this type of scepticism, despite claiming the mantle of science, despite claiming that it's trying to police scientific standards, displays a profoundly anti-scientific attitude. If we demand that evolutionary psychology explanations immediately satisfy heavy evidential demands, the research programme cannot get off the ground, and we would be robbed of important insights. This scepticism is, therefore, dangerous, threatening to discredit a legitimate and important research activity.

Of course, the sceptic is entitled to remain sceptical of the ultimate empirical prospects of evolutionary psychology hypotheses, as well as the ultimate prospects of establishing complete ultimate explanations for proximate mechanisms. Fair enough. Perhaps she's making a bad bet, and as the evidence comes in she will have to weaken her resolve. Or perhaps she's right. But that is neither here nor there – even if many of evolutionary psychology's explanations are displaced in the long-run, even if we ultimately reject many of them and accept other explanations for discoveries made by selectionist hypotheses, pursuing adaptationist questions in psychology is still a legitimate and valuable project.

7.1 Towards a streamlined evolutionary psychology

If we want sceptics to focus on the heuristics of the programme, then those heuristics shouldn't be obscured or overshadowed. As we saw in chapter 1, prominent evolutionary psychologists frequently adopt positions far in excess of what is required. In itself this is not a problem. The problem arises when such excess tenets are presented, either by accident or by design, as part-and-parcel of the programme. I mentioned this risk before, but it's clear this risk has been realised. The overlooking of the heuristics has run in parallel with the overshadowing of the heuristics.

7.1.1 On the very name 'evolutionary psychology'

In a way, the very name 'evolutionary psychology' is a stumbling block. Naming the programme of focusing on adaptive problems and solutions 'evolutionary psychology' was an unfortunate move. 'Evolutionary psychology' can be taken to cover the full range of the evolutional behavioural sciences. As Daly and Wilson (1999: 509) note,

> 'For present purposes, HEP [human evolutionary psychology] encompasses work by nonpsychologists, including even those who have deliberately differentiated themselves from 'evolutionary psychology' as 'evolutionary anthropologists', 'human sociobiologists' and 'human behavioural ecologists'. These approaches are all 'evolutionary' by virtue of their adaptationist, selectionist conceptual framework, and they are all 'psychological' to the degree that they focus on how people acquire and evaluate information and how they use that information in behavioural decision making.' (1999: 509)

Since the name 'evolutionary psychology' suggests the programme covers much more than it actually does, this has inevitably caused confusion. As Kurzban (2010) notes,

> 'Similarly, many critics continue to think, perhaps misled by the word "evolutionary" in the field's moniker, that evolutionary psychologists' hypotheses are about phylogeny, or evolutionary history (e.g., Leiter and Weisberg, 2010, pp. 38–39). Because the logic of adaptationism is central to the discipline, critics' failures to understand it represents a significant impediment to progress.'

It's much like with the name 'London School of Economics', but in the opposite way. Even when stretched out to its full incorporated title the 'London School of Economics and Political Science', it still can give a misleading impression about what its remit is, suggesting that the School covers only economics and political science. Countless times I have received confused looks from conference attendees when I've mentioned I'm at the London School of Economics doing a PhD in philosophy on evolutionary psychology. Indeed, at one international seminar, a Nobel laureate insisted I must know a lot about economics in virtue of my LSE affiliation, despite my protestations to the contrary.

The literature has struggled with one approach out of many being labelled 'evolutionary psychology', getting itself into semantic gymnastics to capture the distinction between evolutionary psychology as the practice of focusing on adaptive problems and solutions, and evolutionary psychology as the broad field of applying evolutionary thought to behaviour. The most common distinction to make is between 'Evolutionary Psychology' and 'evolutionary psychology', a distinction generally credited to Buller (2005) – the former referring to evolutionary psychology in our sense, the latter to referring to the broad field of applying evolutionary perspectives to behavioural phenomena. However, this distinction can be slippery: authors who adopt it can easily drop the distinction midway through a paper, or slide between the distinctions, leaving the reader uncertain as to the proper target of the criticism. Furthermore, authors can be inconsistent with this distinction between publications (for example, Laland and Brown 2011a use 'evolutionary psychology' for the programme, while Laland and Brown 2011b use 'Evolutionary Psychology' for the programme and 'evolutionary psychology' to mean the broader field).

Distinguishing between 'narrow evolutionary psychology' and 'broad evolutionary psychology' is another common convention – but 'narrow' has a negative overtone (who

wants to be narrow minded?). Naming evolutionary psychology 'the Santa Barbara school' is not ideal either. After all, Donald Symons and Martin Daly and Margo Wilson were utilising this style of reasoning long before Cosmides and Tooby, and one doesn't need Santa Barbara pedigree to deploy such tools. The 'High Church' and 'Low Church' distinction is also sometimes used in the literature, but the obvious religious associations make this inappropriate – and, indeed, it's liable to trigger confused feelings in those of the Anglican persuasion, lapsed or otherwise.

The semantic disaster reaches its climax in Gray, Heaney, and Fairhall (2003). The paper, entitled "Evolutionary Psychology and the challenge of adaptive explanation" (underscore original), informs the reader

> 'Before proceeding any further we should emphasize that the target of our critique here is *not* a broad, comparative evolutionary approach to psychology (evolutionary psychology or "evolutionary psychology in the round". Instead, our attack is confined to the specific program of Evolutionary Psychology associated with the "Santa Barbara church of psychology"' (Gray, Heaney, and Fairhall 2003: 248; underscore original)

Since the term 'evolutionary psychology' has such a multitude of possible references, and potentially covers such a complexity of subjects, the main proponents of the programme should not, in my judgement, have permitted themselves the luxury of using it. Instead of being baptised with that name, the programme of focusing on adaptive problems and solutions would have been better served with a name like 'psychological adaptationism' – or, even more accurately, 'methodological psychological adaptationism', though admittedly that would have been a bit of a mouthful. Perhaps just 'heuristic evolutionary psychology' would have done the job. I believe not a small measure of the confusion and hostility in the literature could have been averted had the programme been more accurately labelled from the start.

But there are already too many labels floating about, and I shan't add to the confusion by using fresh terms. Since 'evolutionary psychology' is wedded too firmly with one approach, I believe the best distinction to make is between 'evolutionary psychology' for the programme and the 'evolutionary behavioural sciences' (or 'human evolutionary behavioural sciences') for the field in general, a convention adopted by, among others, Sear, Lawson, and Dickins (2007) and Machery and Cohen (2012).

7.1.2 Distinguishing research agenda and unification agenda

We saw back in chapter 1.1 that evolutionary psychology is sometimes presented as an attempt to unify psychology within a single framework – indeed, not only an attempt to unify psychology but also an attempt to unify the behavioural sciences in their entirety. Such grand claims made by those at the summit of the programme have clearly caught the eye of sceptical hawks. Richardson notes, 'Evolutionary psychology does have as a core part of its agenda the overthrow of much of contemporary social science and psychology' (Richardson 2007: 36). Kramnick (2011) begins her review of literary Darwinism, the application of evolutionary psychology to literature, by disapprovingly quoting Joseph Carroll, one of its leading practitioners:

> 'Literary Darwinists integrate literary concepts with a modern evolutionary understanding of the evolved and adapted characteristics of human nature. They aim not just at being one more "school" or movement in literary theory. They aim at fundamentally transforming the framework for all literary study. They think that all knowledge about human behavior, including the products of the human imagination, can and should be subsumed within the evolutionary perspective.' (As quoted in Kramnick 2011: 315)

And as we saw earlier, Kitcher and Vickers call evolutionary psychology 'dismal' and proceed to describe its methodology. I shan't quote paragraph again in its entirety, just the beginning and the end:

> 'Here's a recipe for winning fame and fortune as an architect of the new- and improved human sciences. ... [Four sentences caricaturing methodology] ... Accompany everything with hymns to the genius of Darwin, broadsides against "blank state" views of the human mind, and vigorous denunciations of the lack of rigor and clarity that has hitherto reigned in the human sciences.' (Kitcher and Vickers 2003: 333)

Notice how the methodology of the programme and the grand gambit for unification of the behavioural sciences are seamlessly joined together. Kitcher and Vickers begin with the unification gambit, then talk about (their understanding of) the methodology, and then return to the gambit. They're woven together as part of one narrative. If the two were

adequately distinguished by prominent evolutionary psychologists, perhaps the sceptics would be more inclined to do likewise.

7.1.3 Distinguishing research agenda and public policy agenda

Evolutionary psychology work is often held to standards far higher than those applied to adjacent disciplines and other social science disciplines. Kurzban (2010) articulates this nicely:

> 'Indeed, the skepticism faced by evolutionary psychological hypotheses is stunning set against the credulousness with which other ideas are greeted. Baumeister and colleagues (e.g., Baumeister et al., 2007) have been advancing an Eighteenth century, pre-enlightenment notion that there is such a thing as "mental energy;" psychology's own phlogiston (c.f., Van den Berg, 1986). This idea is absurd in the context of the computational theory of mind, but its absurdity does not seem to have slowed the pace of publication. From this, it can be inferred that ideas in psychology, even if they are fundamentally incompatible with known facts, don't arouse such skepticism *as long as the idea don't derive from a systematic analysis of evolved function*.' (emphasis original)

It's a fair point. The social sciences are arguably crammed to the brim with weak and questionable work. Even if one believes that evolutionary psychology work is weak or questionable, why single evolutionary psychology work out for special focus?

In their valedictory remarks, Kitcher and Vickers (2003) identify the objection that much work in the behavioural and social sciences is questionable, and therefore we shouldn't be especially upset by problems with evolutionary psychology work. In response, Kitcher and Vickers justify the higher degree of scepticism levelled against evolutionary psychology work by appealing to efforts by evolutionary psychologists to draw out public policy implications of their work:

> 'It's not incumbent on scientific researchers to offer policy suggestions, but some recent pop sociobiologists – including Thornhill and Palmer – have defended their proposals about human nature by declaring that they can help resolve urgent social issues. Even though we conceded that they have good intentions, that they want to help decease the incidence of rape, it's hard to avoid the judgement that Thornhill and

> Palmer's suggestions, where not banal, will do little good. Given the speculative character of their Darwinizing and the elusiveness of their proposals, even their inability to recognize crucial issues, policies influenced by their text might well make matters worse.' (2003: 351)

Recommending public policy interventions on the basis of incomplete explanations seems to particularly irritate a number of sceptics. Hence, the impression of evolutionary psychology as a political agenda, one to be challenged: 'Evolutionary psychology is not only a new science, it is a vision of morality and social order, a guide to moral behavior and policy agendas.' (Nelkin 2000: 20) Since Thornhill and Palmer sought to draw out policy implications for their work, this perhaps accounts for why the rape adaptation hypothesis has attracted a degree of attention in the sceptical literature far out of proportion than its presence in the evolutionary psychology literature warrants.

Of course, it's clear that evolutionary psychology work could provide insights relevant to public policy. First, on the strength of evolutionary psychology work, given how a particular psychological adaptation or a set of psychological adaptations are understood to operate in our current environment, one could identify existing public policies and interventions that are unlikely to be effective. Second, the reverse-side of the coin: on the strength of evolutionary psychology work, given how a particular psychological adaptation or a set of psychological adaptations are understood to operate in our current environment, one could identify – and engineer - policies and interventions likely to be successful. Since psychological adaptations generate a variety of behaviours, depending on what environmental cues are being received, one could propose environmental setups that provide the right inputs, the right developmental or environmental cues, to secure the desired outputs.

Even if this is so, evolutionary psychologists must clearly distinguish between their research agenda and any public policy agenda made on the basis of such research. Public policy recommendations are not strictly part of the evolutionary psychology research programme. To move from hypothesis generation and explanation to public policy is to move from science to an admixture of science and political science and philosophy. This must be explicitly signalled.

Furthermore, if one wants to wear a policy hat, if one wants to use the strength of evolutionary psychology work to formulate public policy recommendations, then the work

in question had better be well-established. The design details of adaptations matter. To be of use to public policy, to inform policy of what interventions are unlikely to work and what are likely to work, we need to know a lot about the design details of postulated adaptations, we need to know the range and type of cues relevant to the purported psychological adaptation and the contingencies, developmental and contextual, that modulate its output. Only mature, established explanations, therefore, should be submitted for policy consideration.

7.2 A subtle subversion

The tone of sceptics like Buller, Richardson, and Kitcher, the wholesale dismissal of evolutionary psychology, can alienate even those who consider themselves critics of evolutionary psychology. Laland (2007: 7), for example, says,

> 'When I read Pinker, Daly and Tooby I am moved to become a critic of narrow evolutionary psychology, and to the extent that I have contributed to this debate it is in this capacity (Laland and Brown, 2002). However, Buller's tome elicited an urge in me to defend evolutionary psychology from what I saw as unjust denigration. In a grey world, I cannot trust the judgment of those who see only black or white.'

This indicates a grey scepticism, one neither harshly black in condemnation, nor snow-white in praise. This milder, more moderate scepticism, a scepticism championed by Kevin Laland, Gillian Brown, and Johan Bolhuis, recognises evolutionary psychology's achievements, but contends that evolutionary psychology's tenets are no longer tenable.

Unlike some of the hardened sceptics, those who subscribe to this moderate scepticism recognise that evolutionary psychology has made valuable contributions: 'There are undoubtedly some very fine pieces of work that show genuine promise of being able to decipher the evolved structures of the mind. The best of evolutionary psychology is as rigorous and sophisticated as any research carried out in the general area of human behaviour and evolution.' (Laland and Brown 2011a: 137) Nevertheless, evolutionary psychology needs to change, needs to 'modernise'. Laland and Brown have been arguing this for a number of years now. For example, Laland and Brown (2002: 187) claim,

> ‘The fact that few evolutionary psychology studies refer to the findings of modern evolutionary biology reinforces the suspicion that evolutionary psychology has become detached from recent developments in evolutionary thinking, which over the last 30 years have increasingly stressed a wide range of processes’

Bolhuis *et. al.* (2011) identify three tenets associated with evolutionary psychology that they judge to be no longer tenable.

The first tenet they associate with evolutionary psychology is gradualism, the idea that evolutionary change, particularly with respect to complex adaptations, occurs slowly. To challenge this tenet, Bolhuis *et. al.* cite some examples of recent selection. However, recall we dealt with this objection in chapter 2.2. In a nutshell, evidence of recent selection for simple physiological traits does not destabilise the argument that there’s been insufficient time for selection for complex psychological traits; not a single recently evolved complex psychological trait has been proposed; even if selection could select for complex psychological traits in a short period of time, the instability of recent social environments problematizes their selection; and even if new complex traits have recently been selected for, this doesn’t undermine the search for complex traits with longer pedigrees.

The second tenet they associate with evolutionary psychology is universalism, a belief in the species-typicality of psychological adaptations, which they criticize as blinding evolutionary psychologists to behavioural variation:

> ‘EP's emphasis on a universal human nature has hindered its exploitation of new opportunities to examine human diversity utilizing evolutionary biology. Contemporary evolution theory makes predictions about behavioural variation within and between populations in traits commonly studied by evolutionary psychologists. For example, sex differences in mate preferences constitute a large proportion of EP research and are generally assumed to exhibit universal patterns [...]; however, sexual selection theory suggests that a number of factors, such as sex-biased mortality, population density, and variation in mate quality, will affect sex roles [...]. A modern EP would make greater use of the theoretical insights of modern evolutionary biology as a source of testable hypotheses’ (Bolhuis *et al.* 2011: 2)

But as we already saw in chapter 2, good design demands good flexibility, and in chapter 3 we saw how developmental and contextual calibration is an important heuristic. There is no theoretical reason why evolutionary psychologists, working under their existing framework, cannot adopt the insights of modern evolutionary biology to further develop their testable hypotheses. What Bolhuis *et al.* offer, therefore, is a sociological objection.

Bolhuis *et al.* also see evolutionary psychology's subscription to the species-typicality of psychological adaptations as blinding researchers to the possibility of genetic variation biasing the configuration of those adaptations. Bolhuis *et al.* (2011: 3) point out that, 'While variation within populations accounts for the bulk of human genetic variation, around 5%–7% of genetic differences can be attributed to variation between populations ... genetic variation could lead to biases in the human cognitive processing between, as well as within, population.' However, this misreads the situation. It's not that evolutionary psychologists' subscription to species-typicality of psychological adaptations blinds them to the possibility of genetic variation within populations and between populations biasing the calibration of our psychological adaptations. Rather, the issue is a pragmatic one: evolutionary psychologists begin hypotheses relatively simple, at the level of species-typical design, and over time they can make their hypotheses more complex – such as by incorporating relevant research from behavioural genetics. One has to start somewhere. The dialectical movement from simplicity to complexity, which I identified in chapter 3.5, is how hypotheses often progress.

Furthermore, Bolhuis *et al.* blame evolutionary psychology's universalism as leading evolutionary psychologists to running experiments primarily with WEIRDs (Western Educated Industrial Rich and Democratic): 'the notion of universalism has led to the view that undergraduates at Western universities constitute a representative sample of human nature, a view that has been subject to criticism from anthropologists and psychologists' (Bolhuis *et al.* 2011: 2). However, running experiments with WEIRD undergraduates is merely a convenience, a first step, not a substitute for vigorous cross-cultural research. And as we saw in the previous chapter, evolutionary psychologists are at the forefront of cross-cultural research in psychology.

The third tenet they associate with evolutionary psychology is massive modularity. They claim that massive modularity is not supported by the neuroscientific evidence since it represents 'an unassailable case for the existence of domain-general mechanisms' (Bolhuis *et al.* 2011: 3). However, recall from chapter 1.1 that we can distinguish between two

senses of massive modularity: a moderate form, which claims that the number of psychological adaptations is substantially greater than has been traditionally believed, and the stronger form, which claims that there are no general purpose mechanisms. If it is indeed the case that there is good evidence for the existence of domain-general mechanisms, then this only speaks against the strong form of massive modularity, not the moderate form. 'Both domain-specific and domain-general mechanisms are compatible with evolutionary theory, and their relative importance in human information processing will only be revealed through careful experimentation, leading to a greater understanding of how the brain works' (*ibid.* 2011: 3). Agreed. Who would disagree with that?

So none of the three objections they raise discredits evolutionary psychology, either the top-down, pseudo-paradigmatic form championed by its main proponents, or the streamlined form I champion. More troubling, I find, are the conclusions they draw.

After claiming (incorrectly) that the objections they've raised call into question the key tenets of evolutionary psychology, Bolhuis *et al.* (2011: 6) claim that evolutionary psychology should 'reconsider its basic tenets'. 'EP should change its daily practice' (*ibid*.: 6). Evolutionary psychology needs to modernise: 'A modern EP would embrace a broader, more open, and multidisciplinary theoretical framework, drawing on, rather than being isolated from, the full repertoire of knowledge and tools available in adjacent disciplines.' (*ibid*.: 6) In short, they call for a 'new science of the evolution of the mind' (*ibid*.: 3).

This judgement, that evolutionary psychology needs to change, needs to modernise, is ambiguous. Two interpretations are possible. The first interpretation is that evolutionary psychologists need to become more aware, at least to some degree, of what is going on in adjacent evolutionary disciplines, and to look for ways to draw on the tools and insights of these adjacent disciplines to make their own work more sophisticated. The second interpretation is that evolutionary psychologists need to focus less on adaptive problems and adaptive solutions and to instead use more 'modern' ideas, like gene-culture coevolution and niche construction.

The basic thought underlying the first claim is reasonable enough (though one can dispute the characterisation of evolutionary psychologists being isolated from other evolutionary disciplines – and we will, in a moment). Who could disagree with the contention that researchers should know what adjacent disciplines are up to, and to borrow tools and ideas where relevant? The second claim, however, is deeply subversive, and I strongly reject it.

I don't know which interpretation the authors intend. Certainly, it could be read as just a sociological 'know thy neighbour'. But the way the authors position themselves, as claiming that evolutionary psychology is 'required' to change its 'daily practice', that there should be a 'new science' of evolutionary psychology, suggests the second interpretation. The ambiguity alone is subversive. Let's run through each interpretation.

Like I said, it's difficult to disagree with the first possible claim. Disciplines necessarily become specialised. There will always be a division of labour. As I argued in the previous chapter, it's not credible to suppose evolutionary psychologists can do all the work that the hard sceptics expect of them. They cannot be jacks of all trades. Nevertheless, it's certainly possible and indeed beneficial to be aware of what's going on in these other disciplines. There is much one could learn by exposing oneself to alternative methods and ideas.

So far, so good. However, this is true of all disciplines. Name me a discipline where it cannot be said that it would benefit from interdisciplinary awareness and cross-fertilisation of methods? And yet this message is especially pressed against evolutionary psychologists, as if they especially need schooling on this point.

Laland and Brown (2002: 187) claim that it's a 'fact' that 'few evolutionary psychology studies refer to the findings of modern evolutionary biology'. But this 'fact' is nothing of the sort. Machery and Cohen (2012) note that the charge of ignoring much of the biological sciences is sometimes pressed more strongly against evolutionary psychology than other research traditions in the evolutionary behavioural sciences. Machery and Cohen (2012) examined the claim that evolutionary psychologists and other evolutionary behavioural scientists are ignorant of the biological sciences. Using quantitative citation analysis, they found the claim to be false. According to Machery and Cohen, evolutionary behavioural scientists, including evolutionary psychologists, are not ignorant of the biological sciences – the proportion of citations of biological science in evolutionary psychology work and other evolutionary behavioural traditions is proportional to biology's scientific output. 'Furthermore, focusing on evolutionary biology in particular instead of the biological sciences in general, it is also not the case that evolutionary behavioral scientists ignore evolutionary biology.' (*ibid*.: 217)

So the characterisation of evolutionary psychology as an island stranded in time and isolated from many recent currents of evolutionary biology is evidentially unwarranted.

Hence, there is no need for the first possible claim to be especially raised and pressed against evolutionary psychology.

Turning now to the second possible claim, the message here appears to run something like this. Evolutionary psychology's achievements are genuine and permanent additions to knowledge. We can never go back to a state of innocence about the role of selection in shaping our psychology. But like all movements, evolutionary psychology was bound to have a limited life of novelty and vitality. It's become boringly predictable to many people who were once sympathetic to it. There's more to evolutionary theory than adaptationism. Evolutionary theory has evolved.

I can agree with the motivation of the first possible claim, even if it's based on an evidentially unwarranted characterisation. But I profoundly disagree with this stronger claim. Beyond becoming more informed of their peers' work, evolutionary psychologists should not change their daily practice. They should remain doing precisely what they're doing, applying the framework I have been articulating and vindicating in this thesis. I have no objection to other methods and ideas being pursued concurrently in the evolutionary behavioural sciences, in fact three cheers for pluralism, but I strongly reject the contention that evolutionary psychologists themselves need to down their tools in favour of other tools and ideas.

The claim that evolutionary psychology needs to change its daily practice in this stronger sense is deeply subversive. Although the hard scepticism and moderate scepticism have different approaches towards evolutionary psychology – wholesale dismissal versus a qualified judgement as to its success – they nevertheless arrive at the same endpoint. Just as the ceaseless evidential requirements of the hard sceptics threaten to deprive us of valuable discoveries, so too does the admonition to change daily practice.

This strategy for changing the practice of evolutionary psychology reflects a classic marketing strategy. When trying to get customers to switch from a rival product to one's own, instead of alienating customers by saying what they have already is rubbish, a better strategy is to claim the product was appropriate in the past but that the marketed product is the future. This from a marketing book:

> 'Looking back, it's easy to see our mistake. We were alienating the very people we needed to win over. No wonder they didn't want to buy from us. In time, we learned to soften our presentation... We learned to send

> the message: “You’re not dumb. The technology you now have in place was perfectly appropriate for its day. But now the world has changed, and Luminous is ready with the next-generation technology you and your customers need.” As you can imagine, our sales results are a lot better with this approach!’ (Weissman 2008: 9)

The basic mistake of the hard sceptics is overlook the heuristics. The milder sceptics recognise the heuristics, but their basic mistake is not weighing those heuristics as importantly as they deserve to be. Laland and Brown (2011b: 358-9 & 361) write,

> ‘Comparative statistical and phylogenetic analyses could be deployed to investigate the factors that explain variation in human psychological attributes, but Evolutionary Psychologists generally do not deploy these methods; we suspect because they believe psychological mechanisms are universals. Deprived of these methods, Evolutionary Psychologists are left with little that they can productively do with evolutionary theory apart from using it to generate hypotheses... evolutionary biology has far more to offer than hypothesis-generation.’

Unlike the hardened sceptics, Laland and Brown get the basic picture of evolutionary psychology right: the generation of testable hypotheses. Yet Laland and Brown appear not to value this activity as highly as this thesis does. Evolutionary psychologists, according to this view, should be doing other things. They should be doing comparative statistical and phylogenetic analyses, as well as deploying gene-culture co-evolutionary analyses.

This, I believe, underappreciates the specific function evolutionary psychology has in psychology. Hypothesis-generation is precisely its strength. As we’ve seen, although the name ‘evolutionary psychology’ suggests the programme should be doing a host of other activities, its actual role is one of discovery, of finding new design features of extant mechanisms and new mechanisms entirely. Moderate sceptics don’t weigh this heuristic as importantly as it deserves to be weighed, I believe, because they do not sufficiently take into consideration the reality of the unconscious. I contend that the evolutionary and behavioural sciences need a programme dedicated to discovering selected psychological processes because, if they exist, they will be largely unconscious to us. Psychological adaptationist thinking provides a non-accidental way of discovering them – perhaps the only non-accidental way of discovering them. This function is very important. If

evolutionary psychologists did other activities, this unique function, this important function, would be reduced or lost.

7.3 The need for evolutionary psychology

While the idea of the unconscious may seem commonplace, it is not in fact at the heart of many social sciences disciplines, nor is it a common assumption in the daily experience of most people. Consciousness is limited to a narrow sequence of thoughts and events. Few realise the scale of physiological and psychological processes operating below the threshold of consciousness. And even if we recognise and consent to the idea of an unconscious it's difficult to feel this reality in our bones, so to speak.

Many physiological processes happen at once: breathing, heart beating, food digestion, hair growth, body temperature re-calibration. All these physiological processes can occur, and often do occur, without thinking, without being conscious of these processes, though sometimes, of course, we tumble into awareness of breathing or the heart beating. Even the act of seeing, which seems so simple, belies a multitude of complex processes, which ordinary experience alone would fail to inform us about. The same is obviously true of psychological processes. Every moment, many psychological processes are in operation – below the threshold of conscious awareness. Selection, it seems, doesn't care much for conscious awareness. It's rather stingy with it. Consciousness is simply not needed for the tens or hundreds of thousands of concurrent physiological and psychological processes operating in each of us this very moment.

Evolutionary psychologists investigate processes that are largely unconscious to us. Natural selection might have selected for a wide variety of adaptations – but such adaptations do not require conscious awareness of their operation. We might find symmetrical faces attractive, but we need not be consciously aware of this, and certainly need not be aware that this because such cues correlate with health. Given the range of adaptive problems our ancestors likely faced, potentially there is much awaiting discovery. Evolutionary psychology offers a torchlight to seeing psychological adaptations. As Tooby and Cosmides (1992: 67) put it,

> 'The tools of evolutionary functional analysis function as an organ of perception, bringing the blurry world of human psychological and behavioral phenomena into sharp focus and allowing one to discern the

> formerly obscured level of our richly organized species-typical functional architecture.'

Take the male adaptation for cue-detection of ovulation. Who would have even thought of that without adaptationist thinking? Perhaps one could have stumbled on it by accident, as a lucky conjecture, thought, or dream. Perhaps a random guessing game could have done the job. But it seems only an adaptationist perspective could deliver this proposal in a methodological way; non-adaptationist perspectives would simply be blind to the possibility. Or take the impact of olfactory cues on attraction. Before evolutionary psychology came along, despite existing research on olfactory communication in animals, little research had been done on the impact of olfactory cues in human attraction. Non-evolutionary psychologists were simply blind to the possibility of olfactory cues having a role in human attraction, despite the huge market for fragrances and perfumes. Without a programme dedicated to search lighting selected psychological processes, we would remain blind to these largely unconscious psychological adaptations. We would have little hope of uncovering them.

That many of the processes evolutionary psychologists investigate are unconscious also accounts for why their hypotheses are rewarded with the predictable harvest of incredulousness, ridicule and dismissal. When I first read about an adaptation for ovulation detection, I too originally found the idea to be outlandish. How on earth could that be? I can't detect ovulatory cues! I have no clue whether my partner is ovulating or not! That's not what I'm thinking when I meet women! I initially believed that this is the kind of thing associated with evolutionary psychology that needed explaining away. But only when I truly appreciated how unconscious many processes are, how selection can engineer truly marvellous mechanisms for competition, survival, and reproduction, how information-rich cues can be, did I get the 'light bulb' moment when I really understood evolutionary psychology. Suddenly it all made make sense. It was all illuminated. I now take ovulation detection, not as an embarrassing example of the programme, but as a flagship example of the programme. And if I, a person sympathetic to evolutionary psychology, could have initially found such an idea difficult to digest, how much more so for those who hold that evolutionary psychology is 'wrong in every detail'?

Furthermore, if we have a naive or immature understanding of the mind, if we think conscious experience represents the near totality of psychology, rather than the tip of a very deep iceberg, then reading evolutionary psychology research is likely to clash with our

personal narratives about what we do and why. It's very common to carry along some very strong notions of who we are or the kind of person we'd like to be: 'I'm this sort of person. I do this for such and such reasons.' That's not helpful, because personal identity is very nebulous, based on all kinds of information and misinformation. It often gets in the way of a scientific understanding of our behaviour.

To paraphrase Jung, knowledge and experience of the unconscious is a defeat for the ego. Those who pride themselves on their personal identity narratives, of seeing themselves as islands of sovereignty, might be offended to learn of selected processes working independently of their socially constructed identities. Hence, perhaps, the common accusation we hear from the more noisy, the more layman commentators that evolutionary psychology is 'dehumanising'. Perhaps it requires a certain maturity of self-awareness, a certain experience that the ego is a satellite, not a star, to be in a position to be receptive to evolutionary psychology thought.

So to answer the question disobligingly asked by the hard sceptics – 'Why generate adaptationist hypotheses in psychology?' – and to answer the question asked by the moderate sceptics – 'Why only generate adaptationist hypotheses in psychology? Why not do more?' – we can reply that the ubiquity of unconscious processes demands a specialised programme of discovery, that the possibility of discovering unconscious adapted psychological processes is the central idea that animates evolutionary psychology. To suggest, therefore, that evolutionary psychologists should be doing things other than what they're doing is to misunderstand or underappreciate their unique role in the evolutionary and behavioural sciences.

The real issue is not whether evolutionary psychology is outdated, but whether it's exhausted, whether it's just recycling old points. And there are absolutely no indications that it is. It shows no sign of slackening. If anything, it's still early days for evolutionary psychology. I venture the prediction that future evolutionary psychology work will, *inter alia*, increasingly focus on the subtle ways in which early conditions can modulate behavioural tendencies later in life, apply itself to new topics, and focus on possible mechanisms that exist in virtue of evolutionary arm races. I further venture that, once the debate is depolarised, once all sides of the debate have a clearer understanding of the role of evolutionary psychology in the evolutionary and behavioural sciences, genuine multidisciplinary co-operation can thrive, offering rich rewards. That's not to say

evolutionary psychology doesn't have a shelf life. In common with all programmes, one day its style of reasoning will reach diminishing returns. But it is not this day.

There's a huge privilege given to new ideas. There's been talk in recent years of 'new thinking' in the evolutionary behavioural sciences, indeed of extending and reformulating evolutionary theory itself. While such ideas may sound exciting, whether they can be translated into pragmatic programmes of research is an open question. Psychologists, at least the ones attracted to evolutionary psychology, are strikingly pragmatic. They're interested in what works, in making new discoveries. And the heuristics of evolutionary psychology work beautifully for that goal. Perhaps because my own temperament is also pragmatic, I've been able to see virtues in evolutionary psychology that many of my peers, who perhaps have different temperaments, miss. There's also a huge privilege given to formal methods and formal modelling. Economics, for example, has become an overwhelming mathematical subject. We shouldn't devalue informal methods. The evolutionary behavioural sciences can benefit from both formal and informal methods, from mathematical and verbal methods. They complement one another.

Some of those who are interested in 'newer' ideas and different methods seem to presuppose that because they are interested in such things that therefore all researchers should be. This is absurd. As Dennett (2011: 483) says in a different context, 'one should always ask whether instead one can have one's cake and eat it too'.

To decide to be uninterested in adaptationist questions is a matter of personal taste. But to insist that these are pointless questions or outdated questions is to deny an important aspect of human psychology and behaviour that warrants scientific study. Evolutionary psychologists have a job to do, and other researchers have other jobs to do. Over time, and in tandem, we have a fair chance of uncovering the tiniest details that constitute unconscious processes. Asking adaptationist questions, asking selectionist and engineering questions about the fit between adaptive problems and adaptive solutions, is an essential part of that collective effort. It's important to find out whether we have psychological adaptations and how they function. And we won't find out without asking. Evolutionary psychologists must therefore keep asking adaptationist questions, without being dismissed as 'idle Darwinizers' or asked to 'modernise'.

7.4 Conclusion

The sceptics' basic mistake is to overlook the heuristics of the programme and to instead focus on the current evidential status of psychological adaptation claims. Given that the function of evolutionary psychology is heuristic, and given that vindicating adaptation claims requires multidisciplinary activity over a long period of time, this is regrettable.

The moderate sceptics' basic mistake is to undervalue the heuristics, and to thereby submit that evolutionary psychology should be doing things other than generating testable hypotheses. Given that we really do need a dedicated programme of discovery, since most psychological processes are unconscious to us, this is also regrettable.

Prominent evolutionary psychologists' mistake is to obscure or overshadow the heuristics by associating it with tenets irrelevant to its practice. Given that evolutionary psychology needs multiple disciplines to engage with its findings, needs multiple disciplines to take seriously what the programme has to offer the evolutionary and behavioural sciences, this too is regrettable.

Kurzban (2010) says, 'I would argue that perhaps the field's greatest challenge lies less [in] coaxing nature to give up her secrets, and more in communicating the insights from evolutionary psychology to those outside the field.' In common with many evolutionary psychologists, Kurzban (2010) is perplexed as to why many people have so badly mistaken evolutionary psychology. I'm less perplexed. I contend that people are badly mistaking evolutionary psychology because they've got the basic picture of what evolutionary psychology is about wrong, and they've got the basic picture wrong at least in part because of the way the programme has been presented and packaged.

Being clear about the role and limits of evolutionary psychology is essential if we are to make the case that evolutionary psychology has a serious and important role to play in the evolutionary and behavioural sciences. If prominent evolutionary psychologists want evolutionary psychology to truly realise its potential of stimulating multidisciplinary activity towards establishing the multiple lines of evidence needed to fully vindicate adaptationist explanations, then they must clearly and efficiently communicate its research goals to those in adjacent disciplines, in a streamlined way, not in an inflated way.

I end this chapter with a call to action. Hard sceptics: since evolutionary psychology functions heuristically, focus on that. You're quite right in drawing attention to the incompleteness of evidential portfolios, but it's for a wider constituency to collectively fill in

the details over time. Prominent evolutionary psychologists: refocus attention on what's valuable about adaptationist thinking in psychology; disassociate tenets not strictly part of the research programme. Moderate sceptics: the evolutionary behavioural sciences would be poorer if evolutionary psychologists changed their daily practice.

If the various parties endorsed these points, the polarisation can end, and the fruits of genuine multidisciplinary research on purported psychological adaptations, as outlined in the previous chapter, can begin to grow.

Conclusion

Evolutionary psychology is a hypothesis-driven empirical science. I've argued that evolutionary psychology can:

H1 Hypothesize unknown design features of extant psychological mechanisms, leading to novel predictions of psychological phenomena.

H2 Hypothesize unknown psychological mechanisms, leading to novel predictions of psychological phenomena.

H3 Hypothesize traits as by-products of psychological mechanisms, leading to novel predictions of psychological phenomena.

H4 Stimulate multidisciplinary research activities, leading to more sophisticated hypotheses and more complete ultimate and proximate explanations.

Since evolutionary psychology is in the business of generating and testing hypotheses about psychological mechanisms, it is not only eminently reasonable but should also be welcomed by all.

Suppose we observe trait *T*. It's developmentally reliable and robust. It can be observed across cultures. It's a suitable candidate for adaptationist hypothesizing – not that it must be an adaptation, but that it potentially could be and to an extent that merits further investigation. Accordingly, we deploy an adaptationist analysis. We can ask whether trait *T* has a function. If so, how has natural selection designed that mechanism to serve that function? Posing these questions can lead to new insights. We can think in new ways about how the mechanism develops and operates, such as developmental and environmental contexts that calibrate the mechanism, the cues that active the mechanism, the strength and variety of outputs, and so on. And crucially, we can articulate these to the point of testability.

Potentially much understanding is to be won by hypothesizing such traits as adaptations. It's a commonplace that often the most difficult thing to see is right in front of one's nose. We need theories and hypotheses to guide us into identifying patterns. Evolutionary psychology can guide researchers into identifying new behavioural patterns, new

demographic patterns, new facts – facts that do not readily lend themselves to discovery by non-adaptationist perspectives. And potentially ground-breaking, reflecting on adaptive problems and scenarios holds out the promise of discovering entire mechanisms we would unlikely find by other methods.

Initial evolutionary psychology hypotheses aim, or should aim, not for the last evolutionary word on a given phenomenon, but the first. They are in constant adjustment – both with the research programme's own findings and findings from adjacent research programmes and disciplines. If this is done, this should generate sophisticated hypotheses, as well as generate progressive increments to our understanding of psychological and social phenomena. In virtue of its successful novel predictions, evolutionary psychology can help stimulate multidisciplinary activity towards establishing the multiple lines of evidence needed to fully vindicate adaptationist explanations. The forthcoming PsychTable, which will codify and evaluate the range and strength of evidence for purported psychological adaptations, should help foster the interdisciplinary cooperation needed to vigorously vindicate psychological adaptation claims.

Given the range of phenomena that psychological adaptations, in various degrees, can influence, and given that psychological adaptations can generate a multitude of surprising by-products, evolutionary psychology has a wide remit. Finally, as a general mode of scientific procedure, we are inclined to accept hypotheses insofar as they make successful predictions of new data. At the very least, evolutionary psychology is capable of meeting this standard and has done so in multiple cases.

This is the positive case for evolutionary psychology. The streamlined case. What I have been championing and what I think best represents evolutionary psychology practice. There should be little to find objectionable about it.

The moderate sceptic claims evolutionary psychologists should change their daily practice. There's more to evolutionary theory than adaptationism. That's true, but this fails to appreciate the importance of such thinking in the discovery of unconscious processes. The real issue is not whether evolutionary psychology is outdated, but whether it's exhausted, whether it's just recycling old points. And there are absolutely no indications that it is.

The strong sceptic focuses on the evidential status of evolutionary psychology explanations. She sets out the tough standards complete adaptationist explanations should meet, notes the shortfall, and then dismisses evolutionary psychology. But evolutionary psychologists

do not have to meet the heavy evidential requirements identified by Richardson and others in order to legitimately pursue adaptationist theorising in psychology. To insist these heavy requirements be met at the outset is dangerous. And to insist or to imply that evolutionary psychology alone meet these heavy evidential demands is disingenuous. Yes, evolutionary psychology cannot possibly meet the evidential demands – but evolutionary psychology doesn't need to do this alone. And there's no reason to suppose the evolutionary and behavioural sciences cannot do so collectively.

If evolutionary psychology is to cease being dismissed, its champions need to shift attention to its heuristics, its rich possibilities of discovery across the spectrum of social phenomena. These heuristics represent a unique opportunity to discover new design features of extant traits and to even discover new traits, to make genuine and permanent editions to knowledge. That's precious - too precious to be knocked about with jibes about just-so stories and too precious to be overshadowed by theoretical excess.

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