Evolutionary social psychology, natural history & the history of ideas

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Abstract

The aim of this dissertation is to analyse two notions which inform contemporary evolutionary psychology. In Part I Tooby and Cosmides' (1992) Standard Model thesis of the history of twentieth century social science is examined with regard to social psychology. In Part II the practical and theoretical fecundity of the Environment of Evolutionary Adaptedness is examined, again with regard to social psychology.

The analysis of the Standard Model thesis yields the result that it is not reliable as an intellectual history of social psychology. A principal reason for this is the failure of the thesis to acknowledge the instinct debate of the late nineteenth and early twentieth century. Further consideration of the instinct debate leads to the conclusion that evolutionary psychology may be in the process of repeating the history of social psychology rather than making substantive advances.

The analysis of the Environment of Evolutionary Adaptedness concept yields two results. Firstly, in use it fails to accommodate the findings of palaeontology. Secondly, it promotes a view of mental capacity and functioning that is at odds with that of modern humans. Further consideration of the natural history of the human lineage leads to the conclusion that the past was not, in some sense, ontogenetically prior to the present and that it will not furnish social psychology with an adaptation that functions in a predictable manner.

In Part III it is recommended that an evolutionary approach to social psychology should dispense with the concept of adaptation as proposed by evolutionary psychology.

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Evolutionary Social Psychology, Natural History & the History of Ideas

by

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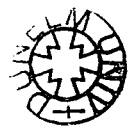

Evolutionary Social Psychology, Natural History & the History of Ideas

Contents

General Introduction

Evolutionary psychology has been advertised as a new approach to those concerns that are encompassed by the rubric "social sciences". In the last ten years volumes of popular science (both theoretical and empirical), the content of numerous journal articles, the re-branding of an established journal as well as new publications, new under- and postgraduate courses and university departments testify to this (Rose & Rose, 2000; Cartwright, 2000).

The idea upon which the edifice of evolutionary science is built is this: the replication of embodied entities in conditions wherein resources are finite is curtailed by the finiteness of resources. It is an idea that has been labelled "universal acid" (Dennett, 1995). In addition to this idea, evolutionary psychology rests on three main propositions. The first is that psychology needs a paradigm if it is to progress just as do other sciences. The second is that mind, behaviour and social interaction are products of evolution and are to be understood within the confines of contemporary evolutionary theory. And the third proposition, in light of the second, is that psychology should enjoy and be bound by conceptual integration with other levels of analysis as are the natural sciences.

The expression of evolutionary psychology that I focus upon is John Tooby and Leda Cosmides' *The Psychological Foundations of Culture* (1992) and, to a lesser extent, the papers that presaged and preceded it, *Evolutionary Psychology and the Generation of Culture, Part I: Theoretical Considerations* (1989), and *The Past Explains the Present: Emotional Adaptations and the Structure of Ancestral Environments* (1990b). Specifically, I examine two of the principal features of evolutionary psychology as Tooby and Cosmides present it, the "Standard Social Science Model" and the "Environment of Evolutionary Adaptedness". Before I go on to outline this dissertation I would first like to say something about how and why it came to be written.

I first began to digest Darwin, Darwinism, neo-Darwinism, evolutionary theory, sexual selection and kin altruism through the resources most available and accessible to me as a second year undergraduate. At that time these happened to be Richard Dawkins' *The Selfish Gene*, David Buss' *The Evolution of Desire*, and Matt Ridley's *The Red Queen*. Jaded by the apparent squabble that seemed to me to pass for social psychology, I was deeply impressed by the prospect of a coherent and respectable theory of sex, violence and cooperation.

Wishing for something more technical on the one hand and psychological on the other, I was led to Karl Sigmund's *Games of Life* and Christopher Badcock's *Evolution and Individual Behaviour*. Still with precision and psychology in mind, alongside the issues Sigmund and Badcock raised, I read again and with sharper appreciation Leda Cosmides' 1989 paper *The logic of social exchange: Has natural selection shaped how humans reason? Studies with the Wason Selection Task.*

The logic of social exchange is a fine paper and justly famous. It cuts through twenty years of work with Peter Wason's Selection Task by offering evidence toward the proposal that elusive problems in psychology may be solved via an appeal to Darwin's notion of an adaptation. As a student of social psychology I was strongly attracted to the finding that the "solution" to the Selection Task was not rational in Wason's original sense but socially rational.

It soon became apparent that the best, and must buy, in the field to which Cosmides' approach claimed allegiance was *The Adapted Mind: Evolutionary Psychology and the Generation of Culture* edited by John Tooby, Jerome Barkow and Leda Cosmides. From that point it was a short step to learning that Tooby and Cosmides' *The Psychological Foundations of Culture* was the set text of evolutionary psychology.

Having read those volumes I have mentioned, some modularity literature and some philosophy of science, much of *The Psychological Foundations of Culture* was not particularly new to me – Tooby and Cosmides accept that "the ideas underlying the Integrated Causal Model [the program detailed in the essay] are not original with us" (Tooby & Cosmides, 1992: 114). Even its stridency seemed of a piece with the wider field. However, I did encounter in the essay what appeared to be terms of art, denotations seemingly unique to evolutionary psychology. The terms in question are the Standard Social Science Model and the Environment of Evolutionary Adaptedness. They bothered me. It did not seem obvious why Tooby and Cosmides needed these concepts given they proclaimed evolutionary psychology to be a straightforward application of neo-Darwinism to human psychology. I was not sure what the two concepts added to their enterprise. Still, keen to get on with some evolutionary psychology I did not let these new terms bother me too much and I planned to start a PhD focussing on female aggression.

My planning included a few months of "grazing" – a general read around wider material on evolution and related topics. This reading came to include Robert Richards' *Darwin and the Emergence of Evolutionary Theories of Mind* (1987) and Robert Foley's *Another Unique Species: Patterns in Human Evolutionary Ecology* (1987a). The former forced me to think again about the Standard Social Science Model and the latter about the Environment of Evolutionary Adaptedness. Put simply, Richards and Foley made me think that two of the most common terms of the approach to social psychology to which I was most drawn were spurious. The possibility that the Standard Model thesis was inaccurate and that the natural history of man was not amenable to being treated as a relatively simple primal state raised again and refreshed the question as to what these concepts added to evolutionary psychology. If spurious, I wondered if these concepts actually detracted from the chances of an evolutionary informed social psychology.

What follows is the result of my inquiry. It breaks down into three main parts. In chapter 1, 'The Adapted Mind', I outline the thesis and detail the components of evolutionary psychology as expressed in *The Psychological Foundations of Culture.* These components are conceptual integration, mind as a suite of adaptations, the Standard Social Science Model and the Environment of Evolutionary Adaptedness.

The paradigm that Tooby and Cosmides ask the social sciences to adopt is called The Integrated Causal Model (ICM). The integration flows from subscription to two views. The first of these is the need for sciences to enjoy conceptual integration. The second is evolutionary theory. The paradigm the ICM is designed to replace is what Tooby & Cosmides call The Standard Social Science Model (SSSM). In the chapters that comprise Part I of this dissertation I detail and distil the SSSM so as to permit an examination of its historical validity. Using the fruits of that labour I then show how, contrary to the claims of Tooby, Cosmides and other evolutionary psychologists, Darwin has been influential in the psychological sciences and social psychology. In showing that the historical account and, subsequently, the attitude toward the past of the discipline is mistaken, I hope to achieve two things. One is to avoid the possibility that an evolutionary approach is built on a myth of revolutionary origins. The other is to show what is different about evolutionary psychology in comparison to the first and most concerted effort to construct an evolutionary approach to mind and behaviour.

In comparison to other approaches to psychology and social behaviour inspired by Darwin, what is most salient about evolutionary psychology is the Environment of Evolutionary Adaptedness (EEA). Resting on an account of the human past, the EEA serves as an adaptation generator. In the chapters that comprise Part II the EEA is scrutinised. I show what role it plays in the general enterprise of evolutionary psychology and criticise its supposed power. I argue that it is questionable on theoretical grounds and show that, in use, it makes assumptions about natural history that are not supported by the evidence. Invocations of the EEA violate the call for disciplines in the social sciences to make themselves conceptually consistent and prominent evolutionary psychologists fail to adhere to the very evidence that is used to show that modern humans have evolved.

In Part III I show how the theory of evolution may be profitably used by social psychologists. In accord with Robin Dunbar's claim that "The significance of Darwinian theory lies not so much in whether it is right or wrong as in its power" (Dunbar, 1988: 161), I try to show what its power is. I take seriously the fact that modern humans are a unique and distinct species and argue that evolutionary theory can provide us with nothing more than a set of broad and flexible constraints on what sort of things may be said of them. Most important, perhaps, I argue that the concept of

adaptation as presented by Tooby and Cosmides should be abandoned by evolutionary social psychology. But let us first look more closely at Tooby and Cosmides work and outline their thesis of the Adapted Mind.

Chapter 1 The Adapted Mind

Summary of chapter 1

John Tooby and Leda Cosmides' adapted mind thesis is introduced and its influence acknowledged. The four central components that comprises evolutionary psychology and Tooby and Cosmides' "integrated causal model" are conceptual integration, the standard social science model, the environment of evolutionary adaptation and psychological adaptations. Each is outlined. The standard social science model and the environment of evolutionary adaptation are isolated for further analysis in later chapters.

1.1 Introduction

The Adapted Mind: Evolutionary Psychology and the Generation of culture (Barkow, Cosmides & Tooby, 1992) may be regarded as "one of the first and most important collections of essays on modern evolutionary psychology" (Badcock, 2000: 17). It is regarded as "first" because it claims to introduce a novel approach. Its importance flows from its ascription as "the seminal publication in th[e] field" (Corballis & Lea, 1999: v).

As a whole *The Adapted Mind* is centred on the supposition that evolved psychological mechanisms generate human behaviour and culture,

> . . . It unites modern evolutionary biology with the cognitive revolution in a way that has the potential to draw together all of the disparate branches of psychology into a single organised system of knowledge. . . [Another goal of the volume] is to clarify how this new field, by focusing on the evolved information-processing mechanisms that comprise the human mind, supplies the necessary connection between evolutionary biology and the complex, irreducible social and cultural phenomena studied by anthropologists, sociologists, economists, and historians (Barkow, Tooby & Cosmides, 1992: 3).

Of the nineteen papers that comprise *The Adapted Mind*, four are wholly theoretical. Of the theoretical papers, Tooby & Cosmides' *The Psychological Foundations of Culture* is the cornerstone and it has been the most influential and frequently cited (see Appendix 1). For example, in Pinker's view ". . . John Tooby and Leda Cosmides . . . forged the synthesis between evolution and psychology" (Pinker, 1997: x). There are four main claims made in this paper. Together they constitute the argument for an evolutionary approach to psychology and they provide the conditions and rationale for the experimental studies in the volume.

In order of presentation, the claims can be summarised as;

- conceptual integration

- the standard social science model
- the environment of evolutionary adaptedness
- minds are adaptations

Conceptual integration as a claim belongs to the philosophy of science. The standard social science model is a thesis in the history of idea. The environment of evolutionary adaptedness is a methodological tool based on a depiction of natural history. And the assertion that minds are adaptations is a particular philosophy of mind derived from evolutionary theory. In pulling these strands together, *The Psychological Foundations of Culture* presents a paradigm. This paradigm aims to subsume social psychology.

As indicated, Standard Model is the concern of Part I of this essay, and the EEA is the concern of Part II. In the reminder of this chapter I will outline the other principles, curtailing the exposition to points that have a direct relationship to the SSSM, the EEA and arguments in Part III.

1.2 Conceptual integration

Conceptual integration,

> . . . refers to the principle that the various disciplines within the behavioural and social sciences should make themselves mutually consistent, and consistent with what is known in the natural sciences as well . . . A conceptually integrated theory is one framed so that it is compatible with data and theory from other relevant fields (Cosmides, Tooby & Barkow, 1992: 4).

According to Cosmides, Tooby & Barkow, "As a result" of the failure of social scientists to adhere to the principal of conceptual integration,

> . . . one finds evolutionary biologists positing cognitive processes that could not possibly solve the adaptive problem under consideration, psychologists proposing psychological mechanisms that could never have evolved, and anthropologists making implicit assumptions about the human mind that are known to be false (ibid: 4).

Barkow, Cosmides & Tooby liken conceptual integration to "vertical integration". Vertical integration amounts to a philosophy of science that is reductionist in the classic sense. Integration is necessitated by causation, and causes flow vertically upwards from lower levels of analysis. Every scientific claim is explicable and consistent with what is known in the science immediately below it in a hierarchy. The hierarchy is determined by the precision of physical detail and prediction. Thus "the laws of physics apply to chemical phenomena, and the principles of physics and chemistry apply to biological phenomena, but not the reverse." (ibid: 13)

Conceptual integration would appear to amount to a slight softening of this position. Sciences as a whole are a family of bodies of knowledge that form a cluster rather than a "epistemological or status hierarchy" (ibid: fn; 13). The criterion of mutual consistency remains but the difference between the hierarchy of vertical integration and the "heterarchical relationships" (ibid: fn; 14) encouraged by conceptual integration comes with the reciprocal influence between sciences that the latter permits. The shift between vertical and conceptual integration is essentially methodological. So, whilst there is no suggestion that culture is responsible for psychological mechanisms in the manner that psychological mechanisms are responsible for culture - the causal chain that *The Adapted Mind* proposes is; natural selection > evolution > adaptations > psychological mechanisms > culture. The demand that culture be consistent with psychological mechanisms is matched by the demand that psychological mechanisms must be consistent with culture. In principle, a cultural "fact" can cast doubt on a psychological theory.

If we are to take it as seriously held, there is an important implication of this view. Evidence produced by one discipline (or "level" in the terminology of vertical integration) enjoys no privileges over evidence produced by another adjacent discipline (or level). Accordingly, then, theories in psychology, which ought to be consistent in their own terms, may be verified by evidence from social psychology, and so on. The importance of this will become clearer when we come to discuss the role of the Environment of Evolutionary Adaptedness in evolutionary psychology. Our understanding of those aspects of the Environment of Evolutionary Adaptedness that were social relies entirely on adjacent disciplines. What those adjacent disciplines have to say about the pre-human past suggests that typical construals of the EEA far exceed the palaeoanthropological and archaeological evidence.

1.3 Minds are adaptations

The term "adapted mind" might imply that the mind is whole, a unitary thing, but this is not what Tooby and Cosmides mean. To grasp what it is Tooby and Cosmides mean by the term "adapted mind" we need first to review the argument that the mind is modular.

In the most general terms, for a system to be modular it must consist of functionally and/or physically separable units. These units/modules are (more or less) specialised. On the one hand, the system - the whole - is nothing more than the sum of its parts (nothing more than its modules). On the other hand, the system achieves a concert beyond that which isolated modules might suggest. There are a number of ideas in psychology that converge toward the modularity view. Let us look as some of them (keeping in mind their vintage which will be of use later on).

From the view point of physiology, the human brain, quite literally, looks modular. Anatomy presents it as a conjunction of cortices, lobes and hemispheres (MacLean, 1990). For example, the role of the ventromedial nucleus may be specified in female-typical sexual behaviour (Le Vay, 1995). Parts of the brain are posited as critical for certain processes and these parts are (more or less) unique in their exhibition. The neurophysiological literature consists of such examples from Broca to Crick. It is a literature of anatomical modularity.

However, Tooby and Cosmides are not physiologists. They stress that evolutionary psychology is concerned with cognitive processes rather than physiology (the demand for mutual compatibility withstanding). It is concerned with what we may call mental modularity as opposed to what we might call brain modularity. Perhaps most forcefully and influentially expressed by Fodor (1983), mental modularity rests on distinctions between types of process. Types of process are not necessarily types of function (although types do have a function) but of instantiation. The argument for computational modularity runs like this. Perception systems are "smart". That they are smart is demonstrated by their discriminations and augmentations, i.e. their ability to sort and enrich information. Discrimination and augmentation are demonstrated by the poverty of stimulus argument which shows that there is more to output than just input, more in a response than there was in the stimulus (Chomsky, 1986). However, in terms of processing and computation, perceptual systems also need to be "dumb" if they are to be fast, automatic, functionally robust, and content and/or domain specific processes - the key benefits of modularity. They need to be "encapsulated" from general cognition (or thinking) if this is to be achieved. Encapsulation divorces processes from one another.

In a modular mind, perception is brought about by a range of domain-specific encapsulated modules (e.g. seeing, hearing) that are inferential in a very limited sense. Raw sensory data is enriched via limited processing and the influence of small domain-specific "proprietary data bases" (Fodor, 1989). Thus, sensory processes are modular because encapsulated, semi-smart because inferential, yet essentially "dumb" in that they cannot be informed by higher order cognition and are relatively inflexible. Modules generate information for the non-modular thinking process. And, importantly, general cognition - a combinatorial belief for example - does not interfere with perceptual modules[1].

Modularity ends where thinking and problem solving begin. Modular "dumb" cognition and general "smart" cognition meet over an unspecified code understood by both types of cognition. Whereas modular cognition is characterised as being fast, automatic and encapsulated, general cognition is characterised as slow, global, flexible and contingent.

Evolutionary psychology embraces cognitive modularity. Advocates of cognitive modularity stress its ecological plausibility and its consistency with evidence from studies of patients with brain damage. These studies also suggest that modular systems can suffer local failures without a subsequent global failure. This point is one of the reasons why modularity suits evolutionary psychology - it amounts to good engineering (Tooby and Cosmides, 1992; Pinker, 1997).

There are other ways in which modularity meshes with evolutionary psychology. The most important of these is with the claim that mind is a suite of adaptations. In principal, modular processes should dovetail with solutions to adaptive problems. If mind was other than modular - were it to be domain general - it would be too clumsy to be effective in natural environments. Modules deliver to generalised thought a relevant and relatively "clean" synopsis of the distal environment - just the sort of speedy, acute, discriminative perception that evolution has demanded.

Aside its philosophical and technical difficulties (e.g. "It is easier to think of examples of a domain [of thought] than to give a definition of one", Hirschfeld and Gelman, 1995), in adopting the modular view of mind, evolutionary psychology imports other problems. The most significant of these for an evolutionary social psychology based on the framework of *The Adapted Mind* is the relative importance of the modular and the general with regard to social perceptions and discriminations. Just as the adaptive value of modular cognition is in its "dumb" speed, the adaptive value of general cognition would appear to be its ponderous intelligence, in its "adaptability". We will return to his issue in Part III.

The radical move that Tooby and Cosmides claim to make is that the suite of module-like systems that comprise the mind are adaptations: that it is a complex system of computational mechanisms. These mechanisms may be readily compared to anatomical features or organs in that they are products of natural selection designed to perform specific functions. We can flush out this position by recourse to an alternative: to hold that mind is an adaptation is to be distinguished from the position which holds that the mind is adaptive.

According to Tooby and Cosmides, to say that minds are adaptations is to say that they have a functional specification in light of certain species-specific problems posed by species-specific environments in the past. Thus, "The human mind consists of a set of evolved information-processing mechanisms . . . these mechanisms . . . are adaptations, produced by natural selection over evolutionary time in ancestral environments" (Tooby & Cosmides, 1992: 24). In principle, mind taken to be an adaptation is no different from, in the widest sense, say, gills, and in the narrower sense, say, opposable thumbs. However, to say that minds are adaptive is to say that they can or might function adequately in light of novel problems posed by novel environments. They can be seen

to be adaptive by virtue of their ability to negotiate novel problems and environments against some criterion of reproductive success (replacement would be a minimum). Evolutionary psychology rests of the first of these characterisations.

The distinction between the adapted mind and the adaptive mind is important. In positing the mind as adapted, Tooby and Cosmides create for evolutionary psychology an obligation: it needs to explain why the adaptation that is mind can generate the adaptive prowess it exhibits. We will be addressing this issue in chapter 7, section 3, 'Adaptations, exaptations, spandrels and by-products'. The hub of my argument there is that, by virtue of the disjunction between pre-history and human history (i.e. the last 10 000 years) at the narrowest, or ancestral and modern environments more generally, much of what we see in contemporary human behaviour is exapted behaviour generated by adapted mechanisms. The difficulty for those who hold to the adapted mind view is that humans are so good at many behaviours to which they cannot possibly be adapted to perform - are such competent exaptors - it is difficult to tell what is an adapted function and what is an exapted function. The more insistent we are on specifying adaptations generated by a dead ancestor, the more exaptations appear.

What unites conceptual integration and modularity of mind are the information processing adaptations that Tooby and Cosmides place at the centre of an evolutionary stance towards psychological, behavioural and social sciences. Psychological mechanisms are the flying buttresses that keep evolution and culture apart whilst knitting them into the same edifice. Conceptual integration and modularity are not new approaches[2]. It would, then, appear to follow that these two proposals are not, in and of themselves, responsible for the influence of Tooby and Cosmides' *The Psychological Foundations of Culture*. Accordingly, we need to look at those elements of Tooby and Cosmides thesis that are novel to explain its impact: the Standard Social Science Model and the Environment of Evolutionary Adaptiveness.

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[1] Fodor offers the Mueller-Lyer illusion as an illustration of the relationship between the modular and non-modular: Although we may know that the two main lines in the diagram are of equal length we are consistently deceived by the information generated by 'dumb' modular processes. Not being able to not be deceived shows that general processing cannot override modular processing in the first instance.

[2] This is readily seen in the work of David Marr. Marr has been an influential advocate of the information processing/cognitive approach. Marr's most telling theoretical contribution to modern cognitive practise is what he called "levels of processing" (1982) - also known as "the classical cascade" (Dennet, 1991). The classical cascade divides the study of cognition into three parts. First there is the actual problem to be solved by the system (for example, edge detection in vision - a direct concern of Marr's). Then there is the algorithm

that will reliably solve the problem. Lastly there is the physical specification of the matter that instatiates the algorithm. Marr argued that cognitive science need only concern itself with the first two levels. Physical instantiation was the preserve of neurology. In order to constrain what "problems" at the top of the cascade might be he suggested that a proposed problem ought to make sense in evolutionary terms. The utility and veracity of algorithms, put simply, are that they work - they either do or do not solve the proposed problem. However, their psychological status depends upon their ability to be instantiated in the nervous system. In Marr's scheme, the cascade equates to conceptual integration, the primacy of the problem to be solved equates to an adaptionist approach, and single purpose algorithms equate to modularity.

Part I: The Standard Social Science Model

Chapter 2 The Standard Social Science Model

Summary of chapter 2

This chapter is an exposition of Tooby and Cosmides' thesis of the Standard Model paradigm. First I show how Tooby and Cosmides set it out as a movement. Then the reasoning that is said to hold it together as a movement and the reasoning that is said to hold it apart from the natural sciences is elucidated. For purposes of analysis in later chapters I show how the thesis can be presented as two complimentary propositions. I call these propositions the 'culture as cause commitment' and the '*tabula rasa* commitment'.

2.1 Introduction

Chapter 1 provided an outline of two aspects of John Tooby and Leda Cosmides' adapted mind program. An additional element of *The Psychological Foundations of Culture* is an intellectual history of social science in the twentieth century. Tooby and Cosmides argue that the social sciences have been dominated by a paradigm-like set of principles and they call this paradigm the "Standard Social Science Model". As Rose and Rose (2000) have indicated, as a component of evolutionary psychology, The Standard Social Science Model is one of the elements that marks it out from sociobiology and human behavioural ecology. Whilst the role of the Standard Social Science Model in the overall scheme of evolutionary psychology is not necessary it does, nonetheless, play an important part. The contrast between the Standard Model and the evolutionary model which Tooby and Cosmides propose legitimises the claim that, as a whole, evolutionary psychology is a new approach.

To show what Tooby and Cosmides mean by the term "The Standard Social Science Model" (hereafter the "SSSM" or "Standard Model") I follow their order of presentation. Beginning with a section titled 'The epoch of the SSSM' I offer quotations from the relevant passage of *The Psychological Foundations of Culture*. These quotations reveal the SSSM in broad form. Following that I present a summary of what they call the "The central logic of the SSSM". Next, in a section titled 'The SSSM and culture' the implications of this logic are explicated. Finally I present a set of criteria that may be derived from their arguments regarding the history of social science. These criteria are presented as a set of commitments in the final section of the chapter. Presented as commitments, the two core elements of the SSSM will be examined in chapter 3, 'General remarks on the validity of the SSSM with regard to Social Psychology'. In chapter 4, 'Instincts', I review and discuss the instinct debate of the late nineteenth and early twentieth century with a view to showing

that many academics and researches of the period interested in psychology and social behaviour could not possibly be regarded as advocates of the SSSM. I argue that, in fact, the debate over instincts refutes Tooby and Cosmides thesis of the Standard Social Science Model.

It should be noted that this chapter is purely expository. No attempt is made to assess the SSSM.

2.2 The epoch of the SSSM

> . . The intellectual worlds we built and grew used to over the past 3,000 years were laid out before much was known about the nature of the living, the mental, and the human. As a result, these intellectual worlds are, in many important respects, inconsistent with [the] new unified scientific view and, hence, in need of fundamental reformulation. . . To many scholarly communities, conceptual unification became an enemy, and the relevance of other fields a menace to their freedom to interpret human reality in any way they choose (Tooby and Cosmides, 1992: 21).
>
> After more than a century, the social sciences are still adrift, [amounting to] a contradictory stew of ungrounded, middle level theories expressed in Babel of incommensurate technical lexicons. This is accompanied by a growing malaise, so that the largest single trend is toward rejecting the scientific enterprise as it applies to humans . . . Instead of the scientific enterprise, what should be jettisoned is what we will call the Standard Social Science Model (SSSM): The consensus view of the nature of social and cultural phenomena that has served for a century as the intellectual frame work for the organisation of psychology and the social sciences and the intellectual justification for their claims of autonomy from the rest of science (ibid: 23).
>
> . . . Because, it is reasoned, a "constant" (the human biological endowment observable in infants) cannot explain a "variable" (inter-group differences in complex adult mental or social organisation) the SSSM concludes that "human nature" (the evolved structure of the human mind) cannot be the cause of the mental organisation of adult humans, their social systems, their culture, historical change, and so on. . . . Whatever "innate" equipment infants are born with has traditionally been interpreted as being highly rudimentary . . . Because adult mental organisation (patterned behaviour, knowledge, socially constructed realities, and so on) is clearly absent from the infant, infants must "acquire" it from some source outside themselves in the course of development. That source is obvious: This mental organisation is manifestly present in the social world . . . the social (or cultural or learned or acquired or environmental) . . . contains everything complexly organised . . . Humans raised without a social or cultural environment would be "mental basket cases" with "few useful instincts, fewer recognisable sentiments, and no intellect" (Geertz, 1973, p45) . . . The cultural and social elements that mould the individual precede the individual and are external to the individual. The mind did not create them, they created the mind. . . [the] action of the social world on the individual is compulsory and automatic - "coercive" to use Durkheim's phrase. . . The individual is

acted upon (the effect or the outcome) and the socio-cultural world is the actor (the cause or prior state that determines the subsequent state) (ibid: 26).

> . . . the question is not so much, What are the forces that act on and influence human culture and human affairs? but rather, What is the generator of complex and significant organisation in human affairs? . . . the advocates of the SSSM are united on what the artificer is not: It is not in "the individual" (ibid: 27) . . .
>
> . . . the SSSM denies that "human nature" - the evolved architecture of the human mind - can play any notable role as a generator of significant organisation in human life (although it is acknowledged to be a necessary condition for it). In doing so, it removes from the concept of human nature all substantive content, and relegates the architecture of the human mind to the delimited role of embodying "the capacity for culture". Human nature is "merely the indeterminate material that the social factor moulds and transforms. [This] contribution consists exclusively in very general attitudes, in vague and consequently plastic predisposition's which, by themselves, if other agents did not intervene, could not take on the definite and complex forms which characterise social phenomena." (Durkheim, 1895; p106) (ibid: 28).

Tooby and Cosmides argue that the "cognitive turn" – and by this we may take them to mean the adoption of what Farr (1996) has called "information theory" after the second world war - offered the SSSM a new, technical cloak and jargon:

> . . . the tabula rasa [has a] fully modern equivalent, a general purpose computer. Such a computer doesn't come pre-equipped with its own programmes, but instead, and this is the essential point, it obtains the programs that tell it what to do from the outside, from "culture". . . The conclusion that human nature is an empty vessel, waiting to be filled by social processes, removed it as a legitimate and worthwhile object of study. . . . Such efforts were (and are) viewed simply as crude attempts to serve ideological ends, to manufacture propaganda, or to define one way of being as better and more natural than others (Tooby & Cosmides, 1992: 29).

Though specific in their portrait of an intellectual tradition, to this point Tooby and Cosmides are general with regard to disciplinary focus. With specific reference to psychology, they claim,

> In the SSSM, the role of psychology is clear. Psychology is the discipline that studies the process of socialisation . . . Thus the central concept in psychology is learning. The prerequisite that a psychological theory must meet to participate in the SSSM is that any evolved component, process, or mechanism must be equipotential, content free, content independent, general purpose, domain-general, and so on. Moreover, their structures must themselves impose no particular substantive content on culture. (ibid: 29)

2.2.1 The central logic of the SSSM

Tooby and Cosmides go on to expose what they call the "central logic" of the SSSM (ibid: 24-32). This logic is presented as a series of ten "steps". These steps flow from,

> . . . one salient causal and temporal sequence: how individuals change over their development from "unformed" infants into complexly competent adult members of their local social group, and how they do so in response to their local human environment (ibid: 25).

The ten steps that Tooby & Cosmides present as "the central logic of the SSSM" break cleanly into two arguments. The first of these makes a claim about the nature of mind, the second about the nature of culture. Although the two arguments are independent in the sense that their validity may be assessed separately, their conjunction amounts to the SSSM. The argument about mind runs thus:

1. The psychic unity of mankind: The claim that human beings are more similar in terms of biological endowment than they are dissimilar. This claim forms the basis of the "moral authority" of the SSSM. It permits an interpretation that patterns of within-group similarity and between-group differences show culture to be the formative mode.
2. Variation begins at birth. This follows from 1. "Nature" in the raw form of the new born is overridden and overwritten by culture. That nature is overridden demonstrates that human biological and genetic endowment is insignificant when it is not depicted as infinitely malleable.
3. The Wild Child. This follows from 2. Individuals, undirected by patterns of organised behaviour and thought, would not spontaneously exhibit organised behaviour or recognisable emotions because they are born without form.
4. Conclusion: *Homini Tabula Rasa.* Human nature is defined by its capacity to be enculturated. Its most universal feature is its flexibility and variability. Its essence is in the variety of its expression.

The argument about culture runs thus;

1. The causal arrow. This follows from *homini tabula rasa.* Given that the mind acquires organisation from a ready organised social world, the cause of organised behaviour and mental content is without and not within the individual.
2. *Omnis cultura ex cultura:* The claim that cultural 'facts' as consequences, or effects, are invariably preceded by cultural antecedents, or causes. Given this relation, culture is (at the very least from a methodological point of view) independent of human nature.

3. Conclusion: Given that culture is the cause of behaviour and mental content, it is cultural that must be studied if an account is to be given of its effects.

According to Tooby and Cosmides, these arguments constitute the logic of the SSSM. On the assumption that they are correct, the consequence is that psychology is necessarily the science of learning and social psychology is similarly best considered as the science of learning how to behave toward and think about others. Rendering the terms psychology and learning as synonymous amounts to the concession that humans are integral to the expression and perpetuation of culture. Culture is instantiated in minds and behaviour by some process or another of learning. The task of psychology is to determine how this is achieved.

2.2.2 The SSSM and culture

The logic of the SSSM leaves the characterisation of culture largely unattended aside its causal role. The next stage of Tooby & Cosmides' depiction of the SSSM goes on to elaborate on its notion of culture. This is necessary because, according to Tooby & Cosmides, the general scheme of the SSSM at once rests on the hegemony of culture as cause and its variable forms as manifest in behaviour as evidence that it is the primary cause. Just as the essence of mind is its variability of expression, the invariant power of culture is demonstrated by the variable expression of its local and contingent effects.

Tooby & Cosmides claim that the logic of the SSSM licences a highly constrained view and account of culture. Their account can be summarised in two parts. The first explains how culture replicates itself. The second explains how culture instantiates itself in minds.

The argument about replication runs thus;

1. Cultures are (more or less) local systems of "group universal behavioural practises, beliefs, ideation systems [and] systems of significant symbols". These systems are bounded but not necessarily mutually exclusive.
2. Culture is maintained horizontally and transmitted vertically by the group across and through time.
3. There is nothing for the child to learn other than culture. Its learning is nothing other than the sum of cultural effects. Individuals are (more or less) passive products of cultural prescriptions.
4. Cultures replicate with near-fidelity by virtue of 1, 2 and 3.

The argument about instantiation runs thus;

1. The absorption of culture by the individual is a prescription of culture as it is expressed at the group level.
2. By virtue of the conclusion to the argument about minds, all identifiable organisation and content in human minds can be identified in the requisite culture.
3. Cultures operate and are fully expressed at the group level. The totality of a culture equates to the totality of group thought and behaviour.
4. All and any biological or genetic endowment in humans subserves culture. The sum of human innateness is the capacity for culture.

2.3 The SSSM as a set of commitments

We are now in a position to summarise the SSSM as a set of commitments. As I have unpacked Tooby and Cosmides' thesis, five core elements emerge. Four of these correspond to the conclusions to the four arguments Tooby and Cosmides present. From the two arguments regarding the central logic of the SSSM we may say that the argument regarding mind summarises as the *tabula rasa* commitment and the argument regarding culture summarises as the culture as cause of mental content commitment. And from the two arguments regarding the manner in which the Standard Model construes culture we may say that the argument regarding replication summarises as the independence of culture from mind commitment, and the argument regarding instantiation summarises as the central place of learning commitment.

Here then is the set of commitments that constitute the SSSM.

a. That mind, aside its capacity to learn, is a *tabula rasa.*

b. That culture is the cause of mental content.

c. That culture is independent of mind.

d. That psychology should be concerned with the study of enculturation.

The term "commitment" is used to represent four of the elements of the SSSM because they are said by Tooby and Cosmides to be just that. They are the fundamental propositions – be they explicitly stated or implicitly assumed - that constitute social science. They are commitments in that they are subscribed to by social scientists.

It is apparent that these four commitments collapse into two. To say that the mind is a *tabula rasa* in its initial condition but exhibits content at a later stage implies that it learns. Similarly, to say that culture provides content implies that it is independent from any given instance of its psychological

manifestation. This is reflected in Tooby and Cosmides account of how the SSSM exhibits itself in the social sciences as practised.

Having presented the thinking that comprises the SSSM, Tooby and Cosmides explain how this thinking has coalesced into the disciples that constitute the social sciences. The *tabula rasa* commitment finds its expression in what we know as psychology and the independence of culture as cause commitment finds its expression in what we know as anthropology. Here is Tooby and Cosmides' account of how this "division of labour" came about in the social sciences.

> One major consequence of the adoption of the Standard Social Science Model has been the assignment of a division of labour among the social sciences. It gave each field its particular mission, stamped each of them with its distinctive character, and thereby prevented them from making much progress beyond the accumulation of particularistic knowledge. Anthropology, as well as sociology and history, study both the important and variable content of human life (the signal) and the more vaguely defined processes and contingent events that generated it (the artificer or author of the signal). Psychology studies the medium on which this socially generated content is inscribed, the process of inscription, and the mechanisms that enable the inscription to take place (ibid: 40-41).

That is how Tooby and Cosmides show how the sub-disciplines of social science have been derived from "central logic" of the SSSM. The two that are to them most salient and important are "Content-Independent Psychology" and "Particularistic, Content Specific Anthropology". They are logically complimentary exercises that follow from "the world built by the Standard Social Science Model".

Accordingly,

> In advance of any data, the Standard Model defined for psychology the general character of the mechanisms that it was supposed to find (general-purpose, content-independent ones), its most important focus (learning), and how it would interpret the data it found (no matter what the outcome, the origin of content was to be located externally . . .) . . . A content-independent (or content-free) psychology symbiotically requires a content-supplying anthropology to provide the agent – culture – that transforms a malleable generalized potential into specifically realized human beings. So anthropology's mission was to study the particular. Consequently, anthropology became the custodian of the key explanatory concept in the paradigm, "culture" (ibid: 41).

The binding element in the thesis of the SSSM is Tooby and Cosmides' claim that the primary commitments are ubiquitous in terms of temporal scope and influence: the whole of the twentieth century and the social scientists that constitute it. Our next task is to see if this depiction is valid for social psychology. In the next chapter we will consider some facts that suggest that it is not. Following that, in Chapter 4, we will look closely at a debate that dominated the period between

1890 and *c.*1930 and see that Tooby and Cosmides thesis is seriously inadequate and even misleading.

Chapter 3 General remarks on the validity of the SSSM with regard to Social Psychology

Summary of chapter 3

Tooby and Cosmides' Standard Model thesis argues that the social sciences are governed by the conviction that culture is an independent force which writes scripts that humans minds absorb. This chapter examines this claim with regard to social psychology in two main parts. First the possibility that social psychology has been, in effect, a branch of sociology is examined. Second the possibility that social psychology has been, in effect, a branch of behaviourism is examined. A preliminary conclusion is that, by virtue of the dominance of a psychological as opposed to a sociological emphasis in social psychology, the discipline cannot reasonably be said to have committed itself to the culture as cause commitment. However, although not dominated by behaviourism, the branch of psychological social psychology that has gone by the name of social cognition would appear to have imported the general-purpose machine view along with the computational metaphor. The general conclusion is that a heavily qualified version of the Standard Model thesis holds for social psychology.

3.1 Introduction

Tooby and Cosmides offer the SSSM as an account of the fundamental philosophical stance of the social sciences over a given period. The propositions that constitute the SSSM are said to have been universally held in an implicit paradigm. As a thesis in the history of ideas, the Standard Social Science Model, since being introduced by Tooby and Cosmides, has been adopted by a number of authors (e.g. Badcock, 1994, 1998, 2000; Corballis & Lea, 2000; Dennett, 1995; Ellis, 1996; Foley, 1995; Gaulin & McBurney, 2001; Holcomb, 1998; Pinker, 1997; Rose, 2000; Wilson, 1997; Wright, 1994) and discussed by evolutionary psychologists "as though it were a technical abbreviation rather than a rhetorical device" (Kohn, 1999: 19). However, Tooby and Cosmides do temper their claim. They add a brief addendum, pointing out that their characterisation of social science is just that,

> Of course, readers should recognise that by so briefly sketching large expanses of intellectual history . . . we are doing serious violence to the specific reality of, and genuine differences among hundreds of carefully developed intellectual systems. We have had to leave out the qualifications and complexities by which positions are softened, pluralism's espoused, critical distinctions lost . . . It is the larger intellectual system we are criticising . . . (Tooby & Cosmides, 1992: 31).

They point to research in physiological psychology, perception, psychophysics, physiological motivation, psycholinguistics, and "much of comparative psychology" and credit it with being "strongly connected to the rest of science" (ibid: 30), and to "many important subcommunities in sociology, anthropology, economics, and other disciplines, which have sloughed off or never adopted the Standard Social Science Model" (ibid: 31). Also Tooby and Cosmides oscillate between

two time spans. They suggest that the SSSM has been dominant for the century prior to the time of their writing in 1992 (ibid: 23, 122), and that its hegemony persisted for the sixty years prior to their time of writing. Further, their final endnote offers a list of references to works that inculcate and develop the ideas underlying the ICM (ibid: 114). The oldest of these works is Tinbergen, 1951, and of the 117 cited 32 are from the nineteen-sixties and seventies (ibid: 124). It must also be noted that Tooby and Cosmides discuss what they call "the reasonableness of the Standard Social Science Model". They point out what is right about it in their view (e.g. the assumption of a universal human nature), why its anti-nativism is so attractive, and how it has generated a systematic bias toward the spirit of the natural sciences if not their methods. Also, others who have been influential in the rise of evolutionary psychology have presented a softened position. Specifically, Martin Daly and Margo Wilson, the authors of *Homicide* (1988), have briefly argued that psychology "never abandoned the adaptionist programme" (Daly & Wilson, 1998: 510). However, they do make an exception of social psychology, suggesting that it has "gone in circles" (ibid).

Nevertheless, Tooby and Cosmides' point is forcefully expressed and clear. The SSSM has been an "overwhelming success" (ibid: 34). It is the success of the Standard Model that legitimises its characterisation as a paradigm. The aim of this chapter is to examine the Standard Social Science Model as it pertains to social psychology. Narrowing the examination to social psychology still leaves us with a claim of the same temporal scope and generality. Although Tooby and Cosmides neither specify social psychology or specific social psychologists as an exemplar of the SSSM, we must take it that the SSSM encompasses social psychology. The list of exceptions listed above clearly excludes social psychology. For social psychology to be an exception would fatally weaken their general case.

Our examination of the veracity of the Standard Model as a history of social psychology rests upon an informative and widely addressed distinction within the discipline. It is widely accepted within social psychology that the discipline has been and is made up of a number of schools and traditions (Augoustinos & Walker, 1995; Cartwright, 1979; Collier, Minton & Reynolds, 1991; Farr, 1978, 1987b, 1991b, 1996; Flugel, 1964; Gilmour & Duck, 1980; Markova, 1982; McGarty & Haslam, 1997; Parker, 1989; Strickland, 1979) and even popular introductory general texts aimed at A level students present social psychology as a cluster of approaches (e.g. Gross, 1995). One common way of framing this diversity has given rise to the terms "sociological social psychology" and "psychological social psychology" (Cartwright, 1979; Collier, Minton & Reynolds, 1991; Farr, 1996; Good, 1993; House, 1977; McGarty & Haslam, 1997; Parker, 1989; Stryker, 1977, 1997). These two

construals of social psychology see the discipline as being, on the one hand, a sociological enterprise and on the other a psychological enterprise.

So far as Tooby and Cosmides' thesis is concerned, this division appears to map quite neatly onto their depiction of the division of labour in the social sciences outlined in Chapter 2, section 2.3 'The SSSM as a set of commitments'. Recall Tooby and Cosmides' claim that the SSSM hands anthropology and sociology the task of studying how content is generated and psychology the task of studying how this content is acquired or learned. This division of the SSSM opens up the prospect that social psychology as a branch of psychology maps onto one main commitment of the SSSM, and as a branch of sociology it maps onto the other. In prospect, if there is a discipline that embodies the Standard Model in the fullest sense social psychology is it.

To tackle this prospect, I will first look at the extent to which social psychology is a branch of or dominated by sociology. We will see that sociological social psychology (hereafter SSP) has played less of a role in the general enterprise of social psychology than has psychological social psychology (hereafter PSP). From this we can conclude that if social psychology fits the Standard Model it must do so in its psychological form. This consideration will lead us onto an examination of, first, behaviourism and social psychology and then cognitivism and social psychology. These discussions will show that social psychology has never been behaviouristic but that it is open to the charge of having adopted a general purpose machine view of mind.

Before going on it needs to be made clear that no attempt is being made to assess the SSSM in terms of its internal validity or conceptual coherence. It also needs to be made clear that no attempt is made to show that the SSSM does not "exist" at all in any one or all of its senses. The object of the exercise is to assess its plausibility as a history of social psychology.

3.2 Psychology, Sociology and Social Psychology

It is common for accounts of the birth and development of social psychology to locate it somewhere between sociology and psychology (Allport, G. 1954/1968; Danziger, 1990; Doise, 1986; Gilmour & Duck, 1980; McGarty & Haslam, 1997; O'Neil, 1982). However, social psychology is not always located between but within one or the other (Parker, 1989; Strickland, 1979). The result is a tension concerning its proper place (Farr, 1996; Good, 2000).

Some historians (e.g. Jones, 1985. Collier, Minton & Reynolds, 1991) date the birth of the 'two social psychologies' as coterminous with the birth of social psychology itself - 1908. In that year sociological social psychology appeared in the form of the sociologist E.A. Ross's *Social Psychology: an outline and source book*, and psychological social psychology appeared in the form

of the psychologist W.D. McDougall's *Introduction to Social Psychology*. As may be expected in an enterprise which carries the tag "psychology", the common term and point of contention between PSP and SSP as expressed by McDougall and Ross is the notion of mind (or of the mental). McDougall construed psychology and mind in terms of the mind-body relation. Broadly speaking, psychological forms of social psychology follow this lead. Ross, on the other hand, construed mind in terms of the man-society relation. Equivalently, sociological forms of social psychology follow this alternative lead[1].

Farr (1996) has argued that the distinction was not entirely apparent at the time that these volumes appeared but that subsequent events have crystallized it. For example, the American Psychology Association and the American Sociology Association established their own social psychology divisions some time later. In 1937 Britt reported that of the seven hundred and twenty nine academics engaged in social psychology only nineteen were members of both Associations (Parker, 1989:35). The distinction, division and tension between PSP and SSP has given rise to a literature that, itself, has become part of social psychology and its history. However, there is not a substantive debate on which of the two potential parents or guardians has, as a matter of fact, been dominant.

The impact of McDougall's volume compared to that of Ross's appears to have presaged later events. One edition of *Social Psychology: an outline and source book* was printed. *Introduction to Social Psychology* ran to twenty-one editions within twenty years. Parker claims that by the time McDougall's students were themselves academic social psychologists, "Ross had been written out of the discipline" (Parker, 1989: 35).

This situation has not very much altered. Defending the inclusion of social psychology as a subdiscipline of psychology, Jones reported in third edition of *The Handbook of Social Psychology* (Lindzey & Aronson, 1985) that of the seventy three general textbooks that were produced between 1947 and 1980 three-quarters were authored by psychological social psychologists, less than one-quarter by sociologically inclined authors and the remainder were attempts at an integrated approach (Jones, 1998: 4). In a similar vein, the editors of *The Messages of Social Psychology* – a volume that might be described as an alternative "handbook" – situate the collection of essays within the PSP tradition (McGarty & Haslam, 1997). In the year before *The Psychological Foundation of Culture* was published, Collier, Minton and Reynolds showed that the number of textbooks produced by psychologists between 1908 and 1989 was three times greater than that produced by sociologists over the same period (Collier, Minton & Reynolds, 1991: 6). And in the same year Backman claimed that there was a ratio of approximately eight psychological social psychologists to one sociological social psychologists (Backman, 1991). More recently Good has suggested that "Whatever the ratio, sociological social psychologists can be seen to constitute a small minority" (Good, 2000: fn392).

Taken as a form of sociology, sociological social psychology would appear to be amenable for categorisation as an example of the SSSM[2]. Tooby and Cosmides quote Emile Durkheim, suggesting that he is germane to its elaboration and exposition[3] and Durkheim is certainly a major figure is sociology. Similarly, Marx is implicitly invoked. The claim that culture is the cause of mental content was, perhaps most forcefully and influentially expressed in his phrase, "It is not the consciousness of men that determines their being, but, on the contrary, their social being determines their consciousness" (1858)[4] [5]. However, the fact that SSP has been a minority form of social psychology from its inception to the present day shows that social psychology cannot be characterised as being dominated by the conviction that culture is solely responsible for mind and mental content.

Still, the predominance of psychological forms of social psychology leaves open the possibility that the history of social psychology may be adequately characterised by that aspect of the Standard Model which is more explicitly psychological. The *tabula rasa* commitment invites us to look at behaviourism and its impact on social psychology.

3.3 Social psychology and behaviourism

In this section we will consider Tooby and Cosmides' claim that social psychology has viewed the mind as a *tabula rasa* (recall that the *tabula rasa* commitment is the assertion that mind, aside its capacity to learn, is devoid of content and would remain so unless enculturated). The *tabula rasa* commitment appears to be the most influential element of the SSSM in the sense that it is most frequently taken as synonymous with it. Thus, terms such as "the neo-behaviourist tradition" (Nicolson, 1999: 5) and the "the *tabula rasa* view" (Crawford, 1998a: 4) are used to denote the SSSM.

We can begin our analysis of the validity of the *tabula rasa* commitment by looking to what is most frequently taken to be its source. Once we have a clear understanding of what the term refers to we will be better placed to pass judgement on it supposed ubiquity in the 20th century. We will then move onto considerations about the motivations of behaviourism and its influence on social psychology. Lastly we will discuss the impact of the general purpose machine view on social psychology.

The genesis of the *tabula rasa* view in the post-Renaissance western canon is John Locke's *Essay Concerning Human Understanding* (1689) (especially Book's I and II). It was Locke's contention that minds are void of specific and explicit beliefs, ideas and knowledge at birth. What they do contain are functions such as combination, induction, deduction, division, multiplication and

abstraction. All beliefs, ideas and knowledge that develop in a given mind are the product of functions operating on sensations[6].

Now, the validity or completeness of any formalised logic that seeks to capture Locke's position, and the validity of it as a representation of mental functions aside, the only school of psychology to explicitly reject this sort of view of the mind in the direction of a literal blank slate, devoid of even elementary functions, is that associated with John Watson. Watson's oft quoted claim that the proper object of psychology should be the manipulation of behaviour with a view to its control and prediction, together with the view that the potential manipulations are endless, is the closest psychology has come to a rejection of Locke's basic position (Watson, 1913).

Aside the fact that the *tabula rasa* view, when considered a product of Locke, never held the mind to be absolutely blank, there are three important things to note about the *tabula rasa* view in relation to the claim that it has, as a principal component of the SSSM, dominated social psychology. The first is that the behaviourist stance is at least as much a philosophy of methodology as it is a theory of human nature. The second is its duration as a dominant view within general psychology. And the third concerns its influence on social psychology. We will address each of these considerations in turn. The general conclusion to be drawn is that the *tabula rasa* view has been influential but for nothing like the period or as widely as proposed by Tooby and Cosmides. A narrower conclusion is that it has not been held in any very literal sense by influential schools and thinkers in social psychology.

3.3.1 Behaviourism as a method

A common misconception of behaviourism is that it denies there are mental states. This would appear to flow from its explicit rejection of the positions that it dubbed introspectionism and structuralism[7]. But neither John Watson nor Frederick Skinner - two main architects and advocates of behaviourism - issued such a denial. What they did deny was the utility of positing mental states.

Watson set out his position clearly in the first paragraph of what has since become commonly referred to as "the behaviourist manifesto" (O'Donnell, 1985; Robinson, 1981; Leahey, 1994; Wozniak, 1997), *Psychology as the Behaviourist views it* (1913).

> Psychology as the behaviourist sees it is a purely objective experimental branch of natural science . . . Introspection forms no essential part of its methods, nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation in terms of consciousness (Watson, 1913: 158).

Watson's dispute was not with those who said that humans, or for that matter animals, have mental states and that such states are conscious. What he did dispute was the claim that these states are amenable to controlled experimental study. He suggested that introspectionists, or "content psychologists" as he called them, themselves tacitly admitted this;

> Psychology, as it is generally thought of, has something esoteric in its methods. If you fail to reproduce my findings, it is not due to some fault in your apparatus or in the control of your stimulus, but its is due to the fact that your introspection is untrained. The attack is made upon the observer and not upon the experimental setting. . . If you can't observe 3-9 states of clearness in attention, your introspection is poor. If, on the other hand, a feeling seems reasonably clear to you, your introspection is again faulty. You are seeing too much (ibid: 163-4).

Watson's point was that the natural sciences take exactly the opposite stance.

> In physics and chemistry the attack is made upon the experimental conditions. . . In these sciences a better technique will give you reproducible results (ibid: 163).

It would appear to be the case that Watson, and behaviourism more widely, has been misunderstood for the following reason. The claim that the mind should be treated as a black box because its contents are not amenable to being treated as objects with (to give Watson's example) extension and duration, is read as a claim that the mind is a black box, devoid of content.

An early misinterpretation of Watson's position is clearly demonstrated in his exchange with McDougall in 1929. McDougall pushed his objection to Watson's refusal to take self-reports of mental states as data to the point whereupon he strongly implied that Watson denied the existence of such states altogether.

The position set out by Watson was echoed by Skinner over sixty years later. Skinner's remarks show that behaviourism was, essentially, a method:

> . . . cognitive [i.e. mentalistic] psychology is an appeal to ignorance. It is putting explanatory entities of one kind or another inside the organism - things associated with thinking, reasoning, intuition. I want to get at the environmental manifestation of the behaviour which is attributed to these inner thought processes. When you do that, you take a step forward because if you explain behaviour in terms of what a person is thinking, you then have to explain that thinking. You have a whole new problem (interview with Skinner, in Cohen, 1977:279).

Again, it is quite clear that "thinking, reasoning, intuition" are not being denied. What Skinner did repudiate is the utility of such concepts. What is also indicated in the methodological approach he adopts is his interpretation of mind - body dualism. As Farr (1995) and Markova (1982) have intimated, behaviourism concentrated on the physical side of the dichotomy. In doing so, it made but

also suppressed the assumption that some casual relation held between the two. This helps us to see one of the aspects of mind - body dualism. The assumption that the mind is causally dependent on the body quite plausibly leads one to a certain sort of characterisation of mind – that the key relationship is with the body and not with culture or society. These implications are characteristic of the *tabula rasa* view of mind as it has informed psychological social psychology's methodology. It is not at all clear that they are consistent with Tooby and Cosmides' depiction of the SSSM.

An unmissable irony emerges from Tooby and Cosmides' accusation that *tabula rasa* view and, by implication, Watson and behaviourism, has so badly misled psychology and social science. "The Integrated Causal Model" that Tooby and Cosmides propose seeks to end the state of affairs whereupon psychology and the study of social behaviour are "cut-off" from proper science. Watson's proposal sought exactly the same end.

3.3.2 The influence of behaviourism on social psychology

The consideration that behaviourism never fully embraced the *tabula rasa* view at the root of the SSSM is, to some extent, rendered redundant by the fact that social psychology never fully embraced behaviourism. One of the reasons for this is because behaviourism did not endure for nearly so long a period as Tooby and Cosmides imply. Let us look at this point before moving onto a consideration of what impact it may have had on social psychology.

Should one concede that behaviourism did incorporate the view that there was no such thing as mental content, it does not follow that this view persisted throughout the twentieth century or for the major part thereof. In attempting to assess the temporal scope of behaviourism as the leading paradigm in general psychology we can be guided by Skinner. The following quotation helps us to mark its start date as a paradigm,

> My colleagues among graduate students at Harvard [in the mid nineteen twenties] were all interested in Titchenerian psychology. . . They were all mentalists of the first order. I wouldn't say that behaviourism won its place at that time at all (interview, in Cohen, 1977: 275-76).

These remarks are consistent with the account given by other historians (e.g. Samelson, 1981; Wozniak, 1997) who also suggest it was at least a decade after Watson's "manifesto" before behaviourism could be called dominant in any sense of the term. Wozniak (1997) goes further and suggests that it was twenty five years before it became mainstream in America.

Allied to the delay that might be expected between the date of Watson's manifesto and its widespread adoption, the clear suggestion is that it would be premature to date the "winning place"

of behaviourism much before 1930. We may then consider this fact with the work and impact of Tolman.

Tolman is frequently cited as the first influential behaviourist to break ranks and posit cognitive structures as key in the explanation of learning and behaviour. His approach is first evidenced in the 1930 paper *Introduction and removal of reward and maze learning in rats* (produced in conjunction with Honzik) and received its mature expression in the 1948 paper *Cognitive maps in rats and men.* If Tolman is not accepted as representing a break with the *tabula rasa* view as it was espoused by behaviourism we may move forward to 1957 and Chomsky's celebrated attack on Skinner's *Verbal Behaviour*. This date is taken to mark the beginning of the cognitive revolution (the movement to which Skinner refers above). The point here is that, should we hold the classical view of the *tabula rasa* aside, and should we ignore the behaviourist utilisation of it and accept Tooby & Cosmides version, we are left with a span of dominance that could be as short as ten years, is probably no more than thirty, and may, at best, have been about fifty years in duration. Again, this is not consistent with the SSSM.

With regard to the influence of behaviourism on social psychology we might assume that it had none on sociological forms of social psychology (see Field, 1974 for an opposing view). But we can also find more general statements to the same end with regard to psychological forms of social psychology:

> The major theoretical advances in recent years have been primarily concerned with cognitive processes within individuals. And although it would not be correct to say that other determinants of behaviour have been completely ignored . . . the fact remains that the central focus of attention has been on cognition . . . There have, of course, been radical behaviourists who reject such statements as utter scientific nonsense, but their protests have not significantly affected mainstream social psychological research and theory (Cartwright, 1979: 89-90).

Fifteen years later Martha Augoustinos and Ian Walker make much the same claim.

> Social psychology has always prided itself on never succumbing to the behaviourist revolution which so debased and derailed the rest of psychology. During the hey-days of behaviourism, social psychologists were researching internal mental constructs such as attitudes, values and stereotypes (Augoustinos & Walker, 1995: 3).

Of course, the second part of this quotation may lead us into the SSSM - the mental constructs talked of may be derived from culture. We can now turn to a consideration of the sort of mind that has or contains mental constructs and that phase of research and thinking in social psychology that comes under the rubric "social cognition".

3.4 Social Cognition

Mainstream social psychology, especially since the second world war, but evident as early as 1924 in Allport's volume *Social Psychology* and the influence of gestalt ideas, has been interested in mental constructs as causal mechanisms. Moreover, these constructs, or mechanisms, are squarely located within the individual. These concerns and the approaches they entail bedded in under the banner term "social cognition" in the nineteen fifties when "cognitive psychology came to be seen as the most legitimate approach to psychology in general" and,

> The cognitive orientation became so pervasive in social psychology after the war that the phrase "cognitive social psychology" is somewhat redundant, in much the same way that the phrase "experimental social psychology" is redundant (Collier, Minton & Reynolds, 1991: 198).

The question we need to address in light of the *tabula rasa* commitment is whether the hypothesised mental constructs responsible for social cognition are the result of exposure to culture.

Theodore Newcomb was amongst the first social psychologists to adopt in a self-conscious manner what we are calling here social cognition. Whereas in psychology this outlook took the form of a search for the algorithms that solved the problems of cognition such as visual perception and language processing, in social psychology it took the form of a search for how such systems communicated with one another. Consider how Newcomb was evaluated by his contemporaries:

> Newcomb's theory is couched in system terms. Each variable - attraction, orientation, perception of the orientation of the other person - is in part a consequent of and in part a determinant of each other variable. . . For example, assume that *A*, who is attracted to *B*, discovers a discrepancy between his attitude and *B*'s attitude toward an object of common relevance, such as another person, *X*. *A* likes person *X*; i.e. has a variety of affective and cognitive components of a positive or favourable nature with respect to *X*. He discovers, however, that *B* dislikes *X* and views many of *X*'s attributes unfavourably. Given the attraction of *A* toward *B*, this discrepancy between *A*'s attitude and his perception of *B*'s attitude would give rise to strain and to a postulated force toward change in the relations between these three system components (Secord & Backman, 1964: 248).

I would argue that there is something about this way of conceptualising mind that is typical of social cognition. There is the notion of some sort of tension and some sort of goal-state. Minds generate and focus upon incongruities and inconsistencies that they then need to eliminate or accommodate. Leon Festinger's theory of cognitive dissonance rests on the same premises. And there is some attempt to formalise these notions such that there is a tractable covariation between them. What also bound researchers such as Newcomb, Kelley, Festinger, Murphy, Schachter and Sherif, the people

who dominated the "cognitive orientation" in social psychology (see Appendix 2), was the rejection of the behaviouristic learning approach. Only with Bandura did what Secord and Backman (1964) call the "reinforcement orientation" approach rise again for a relatively short time in the late sixties and early seventies (Collier, Minton & Reynolds, 1991).

Tooby and Cosmides argue that the switch to a cognitivistic lexicon glossed over the underlying persistence of the *tabula rasa* view. The "general purpose machine" approach was a SSSM inspired idea that imposed itself upon the application of computation to psychological problems. They are right in suggesting there was no attempt by social psychologists to assess the plausibility of their models in computational terms – at least not until AI entered social psychology (e.g. Schank & Abelson, 1977). Furthermore, when we consider the length of the research programme commonly called "social cognition" we see that, in 1992, it had constituted nearly half the life span of social psychology and more than half the total effort courtesy of the growth in numbers of researchers calling themselves social psychologists (Cartwright, 1979).

However, an irony arises from the concession that the social cognition approach fits the Standard Model. Above, in section 3.2, 'Psychology, Sociology and Social Psychology', we touched upon recent complaints that "social cognition research is individualistic because it searches within the person for the causes of behaviour"(Augoustinos & Walker 1995:3) and that this has resulted in an undersocialised view of man;

> . . . research and theory in social cognition is driven by an overwhelming individualistic orientation which forgets that the contents of cognition originate in social life, in human interaction and communication. . . As such, societal, collective and symbolic features of human thought are often ignored and forgotten. Contemporary social cognition research is individualistic because it searches within the person for the causes of behaviour. . . Today the dominant perspective in North American social psychology is known as *social cognition*. Some have argued that the "Social" is a misnomer and that the only thing social about social cognition is that it is about social objects - people, groups, events (Augoustinos & Walker, 1995:3)

With the tenor of Tooby and Cosmides thesis in mind, consider the fact that above passage is a preamble to a discussion of what is known as the "crisis" literature in social psychology (Cartwright, 1979; Jones, 1998; Parker, 1989). Contrary to Tooby and Cosmides' thesis as it applies to social psychology, this literature centered on the complaint that the discipline was not social or sociological enough (Good, 2000). For example, Parker suggests that ". . . social psychology as an academic institution is structured in such a way as to blot out what is most interesting about social interaction

(language, power and history)" (Parker, 1989: 1) as a result of a "thorough-going individualism" and a "mechanistic framing of human action".

The irony is that only subsequent to relatively recent complaints against the prevailing dogmas of positivism and individualism has a mood been engendered such that theories and approaches much more Standard Model-like have been given voice. Perhaps the most obvious of these is social representations theory (Moscovici, 1972. 1984a. 1988). Social representations theory does fit Tooby and Cosmides' thesis given the emphasis on Durkheim. The social representations approach acknowledges and rests on his notion of "collective representations". One cannot deny that the following fits Tooby and Cosmides' depiction.

> The central and exclusive object of social psychology should be the study of all that pertains to *ideology* and to *communication* from the point of view of their structure, their genesis and their function. The proper domain of our discipline is the study of cultural processes which are responsible for the organization of knowledge in a society, for the establishment of inter-individual relationships in the context of social and physical environments, for the formation of social movements (groups, parties, institutions) through which men act and interact, for the codification of inter-individual and intergroup conduct which creates a common social reality with its norms and values, the origin of which is to be sought again in the social context (Moscovici, 1972: 55-6; italics in the original).

Undoubtedly Moscovici's ideas have been influential but they do not enjoy hegemony in social psychology any more than have any other forms of SSP.

3.5 Conclusion

My argument is the first part of this chapter has been aimed toward a dismissal of the claim that social psychology has subscribed to the "culture as cause" commitment. Whilst social psychology at its inception as a science - as opposed to a branch of social philosophy - was informed by sociology and, by extension, Durkheim, the influence did not last through into what is often called the "modern period" (Farr, 1996). The extensive "two social psychologies" literature shows this. Some of this literature has as its goal a corrective to the imbalance toward psychological forms of social psychology (e.g. Stryker, 1997). But it would appear to be the case that the mainstream has not been and is not particularly interested (Jones, 1998). The conclusion is that, even in principle, the "culture as cause " commitment could hold only for the minority form of social psychology called sociological social psychology. This conclusion led us onto an examination of the "*tabula rasa* commitment".

My argument in the second part of this chapter has been aimed towards an examination of social psychology in relation to the claim that it has been dominated by radical behaviourism. This involved some consideration as to what *tabula rasa* meant to behaviourists and we saw that behaviourism did not deny mental states but their utility. These considerations aside, the important point is that social psychology never fully adopted behaviourism and it didn't become another branch of learning theory.

Still, in relation to the Tooby and Cosmides depiction of history, behaviourism has had two distinct legacies. Its "black box" psychology, once repudiated became an anathema. What we might call a "crammed box" approach to psychology in general - the shift commonly called the cognitive revolution – did not retain behaviourism's disdain toward the possibility that humans were comprised, *a priori*, of a set of dispositions. However, the cognitive revolution was built upon a technical understanding of general-purpose computational machines. What is pertinent here is that social psychology has never fully subscribed to the metaphor of man as rat but it does appear to have subscribed to the metaphor of the machine.

The result is that social psychology as a whole has not been driven by Durkheim or Watson but it may, in some sense, have been driven by Turing and von Neumann. The evidence needed to repudiate Tooby and Cosmides' characterisation need not be overwhelming. This follows from the strength of their claim. Accordingly the conclusion of this chapter is that whilst the SSSM does contain grains of truth it is not an adequate history of social psychology. "The Standard Social Science Model" is, in my view, what Dennett might call a "luscious slogan"[8]. To sustain my argument I have elucidated some of the more commonly accepted facts of twentieth century social psychology and it theoretical underpinnings.

The consequence of this conclusion is serious for Tooby and Cosmides in the following sense. A failure to appreciate what social psychology is, how it operates, and what constitutes it in terms of ideas makes it unlikely that the remainder of their argument will fall on receptive ground. "Everything should be made as simple as possible, but not simpler" said Einstein. The price of over simplification is, he suggested, vacuity.

In the final analysis this chapter is an appeal to reason (as the term is used in the very cliché it is embedded in). In the form of the Standard Social Science Model, do Tooby and Cosmides represent, by implication, a social psychology that a social psychologist would subscribe to? The answer, appealing to my own reason, is no. However, I would like to extend the examination of the SSSM. Next we will consider one of the most protracted and keenly fought debates in the history of modern psychology, the instinct debate.

[1] I follow the general scheme of Farr (1996), Markova (1982) and Robinson (1981) here. The argument is that social science at the beginning of the 20th century was guided by the following ideas: Psychology:- Psychology at the start of the 20th century construed mind in term of the mind-body relation. This involved the repudiation of Cartesian dualism. This is not to say that it claimed to have solved the mind-body problem. However, it does say that it assumed the problem to be soluble. Accordingly, it "saw" mind mechanistically and, thus, causally. Psychological social psychology, as a general enterprise, similarly rested on this basic assumption. At bottom, it rested on Decartes' notion of the reflex as a unit of explanation, and it charged itself with the task of expanding its complexity so to account for all behaviour. Sociology:- Sociology at the start of the 20th century construed mind in terms of the man-society relation. This involved an implicit acceptance of Cartesian dualism. This is not to say that sociology claimed to have proved classical dualism. However, it is to say that it accepts it in the sense that mind proper was not to be accounted for via the body and cause as it was construed in notions such as the reflex. Its alternative account of the mind was via society. Accordingly it "saw" mind dialectically, as the product of a synthetic account involving society as the founding affect. Sociological social psychology, as a general enterprise, rested on this basic assumption. It rested on Hegel's notion of a rolling synthesis of man and society, and charged itself with the task of specifying this synthesis at the inter-individual level.

[2] Even this can be readily questioned. We might also bear in mind that whilst sociological forms of social psychology are, very broadly, influenced by a stance toward dualism distinct from that which has shaped general psychology, the dualism of man and society itself has at least two elementary interpretations. One of these is "man in society" and the other is "society in man" (Field, 1974). The dualism of each remains distinct from that between mind and body by virtue of the dialectical stance. Nevertheless, there is an implication of the differing emphases. "Man in society" implies a certain priority of "man" in the relation and "society in man" implies the contrary. (Field [1974: 1] labels the different approaches "symbolic" and "sociologistic"). Good (2000) shows that whilst sociology has always been interested in psychological notions such as emotion, social notions such as culture have not been of interest to psychology. I suggest that, possibly starting with Mead, sociological forms of social psychology have accepted some sort of priority of the term "man" in relation to "society" (Farr, 1996, takes this view of Mead). The justification for this employs two strains of reasoning. Firstly, there is the long established general heuristic of a hierarchy of disciplines. This hierarchy is typically ordered according to levels of specification in much the same way as Tooby and Cosmides talk of the "rules" of conceptual consistency. If we restrict the hierarchy to those enterprises that make some claim to be scientific, at the "bottom" we have physics, and at the "top" we have sociology. Psychology sits between biology and sociology, and social psychology sits between psychology and sociology. As we move up through the sub-levels of specification in psychology we move from physiology to social psychology. As we move down through the sub-levels of sociology we move from the historical to the social psychological. If we accept that sociology is broadly concerned with the relation between man and society, and if we accept that social psychology represents sociology at its most reduced, the suggestion is that sociological social psychology

emphasises the "man" in the relationship between man and society. My point here is that, following Tooby & Cosmides' own analysis of the relations between disciplines, sociological social psychology posits man as primary in the dialectic that constructs society and culture.

[3] Tooby and Cosmides appeared to have eschewed Durkheim's oft quoted assertion that, "There exists a social consciousness of which individual consciousness' are, at least in part, only an emanation. How many ideas or sentiments are there which we obtain completely on our own? Very few . . . we find [them] ready made" (Durkheim, 1885)).

[4] It is Pinker's view that the SSSM is an "off the shelf moral package" (1997:47) adopted by the "New Left or Marxist[s]" (ibid).

[5] The idea of content here goes beyond the Lockean conception. For Locke content was more a physical notion in the sense of psychophysics. By this I mean that he was concerned with discrete sensations. In Russell's (1912) language this equates to "sense data", in Ayer's (1936) "sense datum", and in contemporary phenomology it equates to "qualia" (i.e. Dennett, 1988). Colours are an example of discrete sensations. For Marx content is, if you will, "above" the level of every day sensations. Whereas Locke, and associationism in general, sought to analyse everyday consciousness into components, Marx presented a historical account of the overall source of the stream of consciousness. In essence, he argued that the particular "form of life" (to use Wittgenstein's term, Brearley, 1991) that amounts to one's conduct results in an exhaustive account of content. (There is, curiously, a behaviourist whiff in this view. We can find in Skinner's *Science & Human Behaviour*, in the chapter "Man a Machine", the claim that, "Behaviour is a primary characteristic of living things. We almost identify it with life itself"). When we look to social psychology for evidence of this sort of construal of content I suggest that, by strength of definition, it is not to be found in the dominant form of the discipline, psychological social psychology (Graumann, 1986). This stance is reinforced by many proponents of sociological social psychology who have consistently echoed Ross (1908) in calling for a more historically informed approach (e.g. Sedgwick, 1973).

[6] We can illustrate what this view of the mind amounts too. By functions Locke means operations such as quantifiers in first order (or predicate) logic. Let us take the universal quantifier "$\forall$" as an example. Here is how such an operation typically works;

$\forall x(a(x) \rightarrow b(x))$ - which means that for all x's if x has property a then x has property b.

This statement, allied with a second,

$\forall x(b(x) \rightarrow c(x))$ - which means that for all x's if x has property b then x has property c

leads to the conclusion that,

$\forall x(a(x) \rightarrow c(x))$ which means that for all x's if x has the property a then x has the property c

The quantifier $\forall$ ("for all"), the connective $\rightarrow$ ("if . . then"), and the relation of transitivity are content neutral. They merely govern the relationships between the ascriptive (or propositional) items a, b, c and x, which are here unassigned but could, in principle, stand for anything. This distinction between functions and content is the basis of the tabula rasa view. Precisely what it is that is said to be "blank" or undetermined prior to experience are the ascriptive terms in sentences (or compound propositions).

[7] "[By behaviourists] Words have been coined for the opponent school, words like "introspectionism" or "introspectionalism", but I have never heard anyone apply such a term to himself. Someone once suggested "Tichenersism", which had the advantage of seeming to indicate at least one Tichenerist definitely" (Boring, 1929: 118).

[8] Dennett, 1995:491.

Chapter 4 The Instinct Debate and the SSSM

Summary of chapter 4

This chapter reviews the long and intense effort by psychologists to establish the concept of instinct in psychology and social psychology. Many of the most important figures of the late nineteenth and early twentieth century were involved. Whilst the concept of instinct developed it never lost its grounding in Darwinian theory and most proponents explicitly invoked Darwin to support their case. The examination of the instinct debate prepares the way for the conclusion that the instinct debate refutes Tooby and Cosmides Standard Model thesis. Additionally, whilst the term instinct lost favour in the 1930's, the concept continued to find expression in notions such as "driving adjustments" and Darwin's concept of habit was used by behaviourism to support its emphasis on how, why and with what outcome behaviour develops. One implication of the instinct debate for evolutionary psychology is that its integrity is damaged by its seeming ignorance of its own past.

4.1 Introduction

One cannot read an introductory text in the history of psychology or social science and not encounter some treatment of the concept of instinct. Similarly, few general textbooks of psychology fail to incorporate a discussion of its status. More widely, it can be seen that the concept of instinct has been an issue in psychological and behavioural thought since Aristotle (Beach, 1961; Drever, 1917; Hobhouse, 1901; Richards, 1987; Robinson, 1981). However, with a view to assessing the validity of the Standard Model thesis, the weight of the concept in the history of 20th century social psychology is our concern. In this chapter we will concentrate on authors who worked on the idea in the late nineteenth and the first quarter of the twentieth century.

The first pertinent point to be made with regard to this debate and the SSSM is its extent. The majority of those who today we take as representative of academic psychology at the time made contributions: e.g. Allport (1924), Angell (1906), Baldwin, (1896a,b), Dewey (1887, 1923), Dunlap (1919, 1932), James (1890), Lloyd-Morgan (1894), Mead (1936), Thorndike (1911), Titchener (1914), Tolman (1923, 1924, 1932), Watson (1913, 1919, 1931). Others, and I have in mind here William McDougall who published one of the first two textbooks of social psychology, have become almost synonymous with concept.

Of course, the presentation of a list of work that discusses instinct does not in itself show that the authors were sympathetic to the idea, and, indeed, some were critical from the outset. But, as I hope to show, most favoured some formulation or another. The extent of the instinct debate alone suggests that the SSSM is not a reliable representation of twentieth century psychology. Moreover, instinct

theory was explicitly Darwinian. Most proponents took themselves to be Darwinians and most proposed instincts were for or about social thought and behaviour.

In order to show that the instinct debate refutes the SSSM I will detail some of the many definitions and formulations offered by psychologists over a period of forty years. Courtesy of an explication of Charles Darwin's position, it can be seen how closely allied to his most formulations of instinct were. In a later section we will look at what happened to the concept of instinct once the term itself lost favour. It can be shown that although pushed to the margins the term was never fully abandoned. The final part of the chapter will consider what lessons evolutionary psychology might draw from the failure of the Standard Model as history.

4.2 Evolution and the instinct debate

Darwin devoted Chapter VIII of *The Origin of Species* to a discussion of instinct, and he returned to the topic again in *The Descent of Man* and *The Expression of Emotion in Man and Animals*. His opening remark, "Many instincts are so wonderful that their development will probably appear to the reader a difficulty sufficient to overthrow my whole theory" (Darwin, 1859: 191) indicates his strategy: he took the concept as ready defined and devoted the bulk of his attention to showing how and why instincts may be regarded as products of natural selection. Although he stated "I will not attempt a definition of instinct" he did, in effect, produce a guarded definition by writing,

> An action, which we ourselves require experience to enable us to perform, when performed by an animal, especially by a very young one, without experience, and when performed by many individuals in the same way, without their knowing for what purpose it is performed, is usually said to be instinctive. (Darwin, 1859: 191)

Additionally Darwin clearly equated the concept with the mental by adding, "It would be easy to show that several distinct mental actions are commonly embraced by this term." (ibid: 191).

Presaging the debate that he licensed, Darwin discussed the readiness with which instinct and habit may be compared. He mentioned the apparent lack of thought that each may involve, their expression contra to will, and that they "often remain constant throughout life" (ibid: 192). Whilst he doesn't offer an example, he disambiguated the two terms by reference to the impossibility of certain crucial patterns of activity having been built up through habit. Perhaps the key element in his disambiguation of instincts and habits comes in the pitching of instincts as quintessential instances of natural selection;

> No complex instinct can possibly be produced through natural selection, except by the slow and gradual accumulation of numerous slight, yet profitable, variations (ibid: 193).

In other words, it is the origin of instincts and habits that distinguishes them most. Three further points about instincts are revealed by Darwin in the following statement,

> Again, as in the case of corporeal structure, and conformably to my theory, the instinct of each species is good for itself, but has never, as far as we can judge, been produced for the exclusive good of others (ibid: 193).

The first of these points is the distinction between "corporeal structure", i.e. anatomy, and behaviour, the second is the species-specific nature of instincts, and the third is the focus on individual fitness. Darwin added that instincts are not mutually exclusive of "judgement or reason", that instincts vary in their expression among individuals of a species, i.e. that the strength of expression is distributed in any given population, and that instincts, by virtue of some inherent rigidity of operation, do not invariably function perfectly well in relation to the goals of the organism. We will see how these points influenced others in the course of this chapter.

There are aspects of Darwin's discussion that are highly pertinent to our own: he positioned instincts within the framework of natural selection; he asserted a psychological component to them; and he did not pursue a rigid and formal definition of the term instinct. An important consequence of these aspects of Darwin's case was that future writers were given sanction to consider instincts as basic to a description of purposive behaviour and mental states but were left without a formal classification.

Alfred Wallace too discussed and defined instincts: *viz.*, "the performance by an animal of complex acts, absolutely without instruction or previously acquired knowledge" (Wallace, 2000: 2). He introduced his definition by reference to "The most perfect and most striking examples of what is termed instinct, those in which reason or observation appear to have the least influence, and which seem to imply the possession of faculties farthest removed from our own, [which] are to found amongst insects" (ibid). These comments give us, perhaps, some key to Darwin's view of him as "loose cannon" (Desmond & Moore, 1991). At once he is dogmatic with regard to the plausibility of spontaneous yet complex acts and he entertains the notion of a discontinuity of faculties between species nonetheless worthy of comparison[1].

Let us now move on and look at the view of William James[2]. In doing this we move into psychology proper and on to, arguably, the most influential psychological theorist of his generation. I hope to make it apparent that James considered instincts to be of fundamental importance to the study of mind and behaviour and show how closely allied was his view to that of Darwin.

James differed from Darwin with regard to emotions because he took them to play no causal role in individual behaviour. But James did see mind more widely conceived as both having a causal role and being a product of natural selection by virtue of the efficacy of its role. This approach set up his definition of instinct as,

> . . . the faculty of acting in such a way as to produce certain ends, without foresight of the ends, and without previous education in the performance. . . They [instincts] are functional correlatives of structure. With the presence of a certain organs goes, one may say, almost always a native aptitude for its use (James, 1890: 383).

James, presumably with Darwin's efforts in mind, added "That instincts, thus defined, exist on an enormous scale in the animal kingdom needs no proof" (ibid). Noteworthy here is the insistence on a distinction between anatomy and its "native aptitude". Following Darwin, James saw instinct as a process, as something that an animal, or its component parts, did. In making a distinct between anatomy and physiological process or bodily movement James marked a distinction between adaptations and behaviour.

Having defined instinct James went on to take to task the "common way of talking about these admirably definite tendencies to act by naming abstractly the purpose they serve, such as self-preservation, or defense [*sic*], or care for eggs and young" (ibid). He argued that "this represents the animal as obeying abstractions which not once in a million cases is it possible it can have framed" (ibid: 384. In opposition James suggested that instincts be conceived as "conforming to the general reflex type", "called forth by determinate sensory stimuli in contact with the animals body, or at a distance in his environment" (ibid). He defended the view that "mutual dependence" of organ and environment can be "so intricate and go so far" by appeal to the evidence. "The minuteness of adaptation thus shown in structure knows no bounds. Even so are there no bounds to the minuteness of adaptation in the way of conduct" (ibid: 385).

The apparent synonymy of instinct and reflex exhibited in these comments belies James' concept of reflex. In criticising the attribution of abstract goals to instincts and their possessors, James promoted a compositional account of instincts. He took any given instinctive act to be comprised of distinct responses of indefinite number. In saying that instincts are best classified as being of the "general reflex type" he did not say that a reflex is purely physiological. On the contrary, James followed Schneider and forwarded "idea-impulses" and "imagination impulses" as expressions of instincts. By these terms James meant mental representations of simple states of affairs that bear just that sort of relation as do physical stimuli and response. Thus, in an example that he offered, the "idea" of food to the lion performs the function of initiating an "imagination" of obtaining food.

Having introduced the notion that instincts have a mental component, James argued that they may be modified by their history of success or failure. Accordingly, should a certain imagination-impulse that follows a certain idea-impulse fail to satisfy the conditions of the latter it will subsequently be adjusted. It is from this foundation that James went on to propose that,

> Wherever the mind is elevated enough to discriminate . . . wherever, instead of plumping into action instantly at the first rough intimation of what *sort* of thing is there, the agent waits to see which *one* of its kind it is and what the circumstances of its appearance are; wherever different individuals and different circumstances can impel him in different ways . . . we have a masking of the elementary constitution of the instinctive life (ibid: 392)

We will return to James's definition and discussion of instinct when we look more closely at attempts to reconcile intelligence and instinct in Part III. We need only note for now the similarity of his account of instincts to that offered by Darwin, his emphasis on process and, moreover, mental process as fundamental to instincts in the human case.

Writing in 1894, Conway Lloyd-Morgan defined instincts as;

> Complex groups of co-ordinated acts, which, though they contribute to experience, are, on their first occurrence, not determined by individual experience: which are adaptive and tend to the wellbeing of the individual and the preservation of the race; which are due to the co-operation of external and internal stimuli; which are similarly performed by all other members of the same more or less restricted group of animals; but which are subject to variation, and to subsequent modification under the guidance of individual experience (Lloyd-Morgan, 1894/1903[3]).

He went on to demarcate the fixed and the flexible elements of his definition. Instinct is due, in its entirety, to the effects of selection on the nervous system of the species under consideration and intelligence is the extent to which the nervous system is susceptible to adaptive acquisitions from experience. In Lloyd-Morgan's definition we see the reprise of Darwin's criterion. Instincts are complex, adaptive, species typical - although not invariant - selected for, and are to be considered as, in some sense, continuous with and subject to experiential consequences.

In 1901 L.T. Hobhouse, primarily a sociologist, offered this description of instincts in a volume titled *Mind in Evolution*:

> Instinct, in short, is a product of evolution. . . its territory is not apart, but strictly continuous with other powers of organised beings. . . It presides at a certain phase, and has, in due order, its beginning, its rise, its culmination, and its decline. . . Among the higher animals, but particularly among the most developed insects, there are long trains of intricately adjusted actions . . . independent of any intelligent apprehension of their ultimate end . . . These form the instincts

> proper, and of their genesis we can only repeat what has been said of reflexes and of structure in general. They arise from variations, the original source of which is unknown, but which depend for their permanence on their suitability to the requirements of the species (Hobhouse, 1901: 53).

Again with an eye towards Darwin, and, by virtue of comparison with James and Lloyd-Morgan, the criterion of selection, adaptiveness, species typicality with variation and continuity with intelligence mark Hobhouse's approach.

The next consideration of instincts that we will look at comes from James Angell. Angell's orientation to psychology is somewhat different from those we have encountered so far. A "mentalist" (to use F.B. Skinner's term), Angell's general position is revealed in the title of his book *Psychology: An Introductory Study of the Structure and Function of Human Conscious* (3rd ed., 1906). Angell claimed that,

> They [instincts] represent structurally reformed pathways in the nervous system, and stand functionally for effective inherited coordinations made in response to environmental demands. . . If the activity involves a number of acts, each one of which, considered singly and alone, is relatively useless, but all of which taken together lead up to some adaptive consequence . . . it will be safe to call the action instinctive (Angell, 1906: 288-9).

It is apparent that this is not a formal definition but rather a gesticulation. This follows from the manner in which Angell builds instincts – adaptively consequential activities, from reflexes – single acts. His point is that one can only conclude that an activity is an instinct by the specification of a "definite end dominating a series of acts" where the end can be determined as being adaptive.

> The difference thus pointed out [between reflexes and instincts] is founded theoretically upon the nature of the functions subserved by the two types of action . . . It must be frankly confessed, however, that many cases are discoverable in which all distinctions seem arbitrary and fictitious. Too much stress should not be laid, therefore, upon the matter of ultimate differences. It is rather upon the identity of service to the organism that the emphasis should fall (ibid: 290).

With a view to specifying the psychological aspect of instincts, Angell went onto say that "some reflex acts are essentially unconscious, whereas instincts, in the higher animals at all events, appear always to involve consciousness",

> In the earlier views of instinct we always find it contrasted with reason, as though the two were radically distinct. The keener insight of our own time shows us that although reason represents the individual's contribution to his own fate in terms of his own experience, while instinct represents the contribution of racial experience, the actual operation of the two factors often displays most intimate interrelations. . . the great difficulty in studying instinct in human beings is due to the fact

> that intelligence immediately comes in to transform the native reactions in accordance with the dictates of the individual's personal experience (ibid: 291).

While retaining most of the elements that previous authors deployed in their definitions of instinct, Angell accentuated the notion of function. That is, he wanted to refrain from saying what is and what is not instinctive in behaviour until the results of the behaviour were known. This opened him up to the charge of being a teleologist. It also left him a conceptual tool that had no power to predict behaviour. Our chronology now brings us to probably the most influential instinct theorist in social psychology, William McDougall.

It would appear to be difficult to over play the importance of McDougall, ". . . unquestionably one of the most striking and forceful figures in the psychology of his day" (Hearnshaw, 1964: 186). McDougall's most successful and most widely read book is the *Introduction to Social Psychology* (1st edition, 1908) which ". . . marked not only an epoch in McDougall's own development but an epoch in the history of psychology" (ibid: 188). Of the fifteen chapters that comprise Hearnshaw's *A Short History of British Psychology: 1840-1940* (1964) only three are devoted to a treatment of individuals; Alexander Bain, Francis Galton and William McDougall. Additionally, Robert Farr (1986) supplements this observation with a note of approval that McDougall be treated in this manner.

As Hearnshaw's comments suggest, McDougall's treatment of instincts represented a development of the concept. What was distinctive about his definition was a strong emphasis on the "conative, affective and cognitive" aspects. So, whilst there is no break with what we have already seen in McDougall's claim that,

> The human mind has certain innate or inherited tendencies which are the essential springs or motive powers of all thought and action . . . These primary innate tendencies . . . are probably common to the men of every race and every age (McDougall, 1908/23: 19).

Something new can be discerned in the following,

> We may, then, define an instinct as an inherited or innate psycho-physical disposition which determines its possessor to perceive, and to pay attention to, objects of a certain class, to experience an emotional excitement of a particular quality upon perceiving such an object, and to act in regard to it in a particular manner, or, at least, to experience an impulse to such action (ibid: 29).

What is new is the emphasis on what we might call the phenomenological properties of instincts. The suggestion is that they organise our attention and imbue sensations with felt qualities; instincts make the world compulsive in some sense. Whilst such an emphasis is to be found in Darwin's *The Expression of the Emotions in Man and Animals*, McDougall suggested that an analysis of the

particular qualities of these mental events is necessary in any account of human instincts. What was consonant with previous definitions was the attributes of an instinct as being due to natural selection, their being species typical with variation, functional but fallible, and continuous with physiological and behavioural processes. Due to his importance and to the very modern flavour of both his account of instincts and the objections raised against it, we will return to McDougall in Part III when the old notion of instinct and the contemporary notion of psychological adaptations are compared.

James Drever's *Instinct in Man* (1917) is a book length conceptual analysis of instinct. This may be taken as a sign of the muddied water the concept had come to inhabit. Almost sixty years after Darwin had skirted a formal definition due to the apparent obvious denotation of the term, Drever's volume was devoted to just such a definition.

Drever was anxious to point out the heritage of the instinct debate and devotes much attention to the discussions which had preceded Darwin[4]. Drever's strategy was to clarify and reinforce the distinction between physiological and psychological senses of instinct. Let us first consider his physiological definition:

> As a factor determining the behaviour of living organisms, Instinct, physiologically regarded, is a congenital disposition of the nervous system, consisting in a definite, but within limits modifiable, arrangement and co-ordination of nervous connections, so that a particular stimulus, with or without the presence of a certain co-operating stimuli, will call forth a particular action or series of actions; this predisposition, biologically regarded, is apparently due to the operation of natural selection, and determines a mode of behaviour, which ensures a biologically useful end, without foresight of that end or experience in attaining it (Drever, 1917: 81).

There is nothing new here. We have an emphasis on behaviour, natural selection, heritability, relative stupidity and functionality. However, Drever was much less forthright with regard to a general definition of psychological instinct. Rather, he seemed anxious to discuss actual instances. Still, instincts in their psychological manifestation are described by Drever as,

> . . . an innate impelling force guiding cognition, accompanied by interest or emotion, and at least partly determining action (ibid: 20). . . Psychologically, the only possible interpretation of instinctive behaviour seems to be in terms of specific impulse determining specific act, on presentation in perceptual consciousness of a specific situation (ibid: 107).

And he added to this characterisation the claim

> That the instinct structure is a marvellous adaptation to the conditions in which it must function, and that this adaptation is the result of evolution, working in the main through natural selection, no one would attempt to deny (ibid).

By isolating physiological and psychological senses of the term instinct, Drever created room for his treatment of the "innate impelling force" which he labelled "Instinct-Experience". This concept then became central to his discussion. Drever followed McDougall with his emphasis on the emotional corollaries of instinctive dispositions to act and the role emotions play in the satisfaction of instinctive impulses. In Drever we find an early formulation of the notion of drive reduction and satisfaction as explanatory devices and, importantly, a way of discussing psychological drive that did not need to make direct reference to physiological underpinnings.

Drever also developed Darwin's point concerning the fallibility of very precise responses to very precise stimuli. He proposed that a lack of rigidly mechanical responses can be adaptive under conditions of ecological instability. And, whilst Drever exhibited a tendency (probably in line with psychoanalysis) to counter James' claim that humans have more and not fewer instincts than other animals, he endorsed James' view that intelligence just is the successful manipulation of instinctive impulses.

Whilst there was in Drever's treatment much in common with his predecessors and with Darwin, and while he was clearly a proponent of instincts as evolved properties of humans, he also broke with what had gone before. As we will see in Part III when we will look at some of the reasons why instinct theory was rejected, his emphasis on intra-psychic processes and their analytical independence from behavioural processes generated objections to instinct theory that could not be fairly levelled at those who preceded McDougall. Let us now move on and see what John Watson had to say about instincts. An unthinking acceptance of the Standard Model thesis might promote an expectation seriously at odds with the facts. While I am inclined to suppose by appeal to chronology that James was more influential on what Ronald Fletcher (1957) called the "early doctrine of instincts" (by which he means that which preceded Lorenz and Tinbergen) than was Watson, Watson's view is more central to considerations of the SSSM.

Watson devoted chapter VII of *Psychology from the Standpoint of a Behaviourist* (1919) to instincts. He effectively predefines the term by calling the chapter "Unlearned Behaviour: 'Instinct'". Worthy of remark is the bracketing of the chapter with its predecessor, "Unlearned Behaviour: 'Emotion'"[5]. What is implied by these is affirmed in the text, i.e. "there is no sharp line between emotion and instinct"(Watson, 1919: 262). Here is his fuller treatment;

> We should define instinct as an hereditary pattern reaction . . . It might otherwise be expressed as a combination of explicit congenital responses unfolding serially under appropriate stimulation. . . . In every instinct of a more complicated type, we see that the human animal does the same thing, makes

some sort of adjustment. The fact accomplished (what he does) may or may not be adaptive (ibid: 262).

Having defined the term Watson then set out the conditions for its verification: " . . . look upon every definite act that the infant performs at an early age, and hence without learning, as an instinct", adding, "In advance it may be said that if we look upon all untutored activity of the child as instinctive, we shall have to admit that man has a large repertoire of instincts" (ibid: 263). Watson then added qualifications to his definition. These comprise of a distinction between "the fully-fledged pattern type" and "less spectacular acts". Instances of the former include fighting, running and swimming. Instance of the latter include sneezing, yawning, crying and defecation. He formulated the distinction in more familiar terms by calling the former "instinct plus habit" and the latter "pure instincts".

Following Herbert Spencer and Edward Thorndike, Watson unpacked the phrase, "a combination of explicit congenital responses unfolding serially under appropriate stimulation", in terms of reflexes. However, he noted that this was an abstraction which permits of an analysis of an instinct into its component physiology.

While there is nothing in Watson's definition and discussion of instincts that either breaks with or denies what had become orthodox he was keen to down play the role of intelligence. This is exhibited in the possibility that instinctive behaviours might be "non-adaptive or even anti-adaptive" (ibid:262). His reasoning behind the emphasis on the non- or anti-adaptive flowed from his view that as essentially mechanical acts instincts may be evoked when inappropriate for the conditions and needs facing the organism at any given moment. Still, we see the same essentials of heritability, function, the coupling with emotion, species typicality and fallibility.

What was novel was not Watson's basic approach but his subsequent attitude toward instincts. Having specified what they are via a list he openly derived from Thorndike and James, he went on to propose how they may be altered and extinguished. Watson sought to predict behaviour via recourse to instincts and he sought to control them via recourse to conditioning. We will return to Watson again in a later section of this chapter when we consider his influence on later writers who looked to replace the concept of instinct with that of habit.

It was in the early 1920's that confidence in the concept of instinct amongst psychologists and social scientists began to break down (Collier, Minton & Reynolds, 1991). Papers such as Knight Dunlap's *Are there any instincts?* (1919) and Zing Yang Kuo's *Giving up Instincts in Psychology* (1921) were amongst the first revolts from what was to develop into mainstream behaviourism. This

consideration gives us some context to Charles Cooley's discussion of instincts in his *Human Nature and the Social Order* (1922).

Cooley's introduction to the volume is titled "Hereditary and Instincts". He quotes Darwin's "original sense of the word" (see above) as his preferred definition (Cooley, 1922: 18). He then goes on to argue for a form of development within the human lineage,

> . . . when investigators began to study our behaviour from the evolutionary point of view, they saw that if not instinctive in the strict sense it has yet grown out of instinctive behaviour, was historically continuous with it, and, in short, that there was no sharp line to be drawn . . . (ibid: 18).

It is through seeing the status of instinct in humans vertically – that is, in terms of development through descent – rather than laterally – that is, in comparative terms, that Cooley came to the view that it is best,

> . . . to avoid the word "instinct" as applied to most human behaviour, which has nothing of the fixity of animal instinct, and speak instead of "instinctive emotion", since the emotional side of our activity clearly includes a hereditary element which seems to remain much the same under the most diverse manifestations (ibid: 19-20).

Thus, in his definition of instinct, if not in its role in his general account of social behaviour, Cooley resembled McDougall. He resorted to citations from Darwin's *The Expression of Emotions in Man and Animals* to support this view, and in doing so he rejects a reflex account of instinct in man because instinct does not manifest itself as reflex. Where instinct can be discerned is in reason because "reason is itself an instinctive disposition".

> Reason, in this view, does not supplant instinct, any more than the captain supplants the private soldiers; it is a principle of higher organisation, controlling and transforming instinctive energies . . . a disposition to compare, combine, and organize the activities of the mind (ibid: 23).

It is apparent that there is nothing substantially different here than that expressed by James thirty years prior.

Edward Tolman defended the concept of instincts in 1923 and 1924 before his mature and rather hedged definition in the glossary of the 1932 volume *Purposive Behaviour in Animals and Men*. In full:

> *Instinct.* This term, if it is to be retained at all, is to be used for all those demands and sign-gestalt-readiness which practically all the individuals of a given species, irrespective of "special" environmental training, tend to exhibit. The term in this sense will cover all such varieties or phases

of response as are primarily due to innate endowment plus a biologically provided relatively "normal" or "standard" environment.

> In the case of man, or at any rate in the case of many of the lower species, there seem to be three main types of activity which qualify as instincts as thus defined, viz., (a) first-order and second-order drives (*q.v.*), (b) certain chains of minor appetites (*q.v.*) and minor aversions (*q.v.*), which are to be found in especially developed form in birds and insects; and (c) certain innate discrimination and manipulation dexterities or skills. These latter are to be conceived as of the nature of capacities for successful discriminanda and manipulanda commerces-with (Tolman, 1932: 447).

Let us briefly note what is portentous in this statement. First there is the suggestion that the concept is close to redundant. Then there is the link with the phenomenology of gestalt. Thirdly there is the deconstruction of instinct into the notion of drives, skills and orders thereof.

But our main concern is with that which is conventional and here Tolman's definition exhibits a concern with the unlearned, the functional fit between behaviour and environment, and "endowment" which we may take to mean that which is inherited. He appears to differ from those of his immediate peers we have so far considered in the suggestion that instincts are not subject to modification. On other key aspects such as the selected basis of instincts and the nature of their psychological manifestation he is silent.

As he made clear in the preface to *Purposive Behaviour in Animals and Men*, Tolman made use of a glossary as an aide mémoire and to keep the body of the text as uncluttered as possible. His actual discussion of the term instinct, principally in Chapter XX, 'Instinct, Chain Appetites and Aversions, Skills', reveals more of his stance. First of all Tolman discussed what he called the "convergence" view – that which asserts that inherited endowment and experiential maturation are inseparably linked such that neither concept is of use if deployed in isolation. He argued that "There can be no behaviour purely dependent upon innate endowment and none purely upon past training" (ibid: 304). Any suggestion to the end that behaviour can be dichotomised into the learned and the instinctive is undermined by two considerations;

> . . . First, because the individual, while in the embryonic stage, is provided in practically all species with a relatively standardized environment; and, second, also because of the fact that even after birth, an individual's own internal intra-cellular and inter-cellular environments remain relatively standardized . . . there will be varieties, or phases, of response which may still usefully be called instincts because their maturation depends upon and requires interaction with only such relatively standardized environments (ibid: 304-5).

From these considerations he draws the conclusion that, "The term instinct can thus be retained" (ibid: 305).

To complete our review of definitions of instincts we will look at two more. One of them shows how the concept travelled in a circle from and back to Darwin's basic and most conservative position. The other I take as a fair summary of definitions from James in 1890 to Tolman in 1932.

Dunlap's (1932) definition of an instinct is straightforward. He began and ended with the "logical, and logically unobjectionable" distinction between responses that have and responses that have not been learned.

> . . . a learned response is one which has been formed by modifying a previous response. But the process must start somewhere. If we start with no responses we have nothing to modify. Therefore, there must be, in the animal, a certain initial equipment of unlearned responses (Dunlap, 1932: 38).

I take Dunlap's view to be what Darwin was referring to when he said that instincts are just those actions performed "without experience".

For his attack on instinct theory, Luther Bernard (1924) took as his starting point The New Standard Dictionary's definition of instinct. I am of the view that Bernard's critique worked so well just because the definition he concentrated on was such a reasonable and relatively uncluttered representation of the concept at the height of the debate in the mid-twenties:

> Reactions that are merely reflex or automatic, in the purely physiological meaning of these words, are not properly spoken of as instinctive. Instinct implies at least a low degree of consciousness; but its reactions are not learned or directed by conscious process or reasoning. The following characteristics are, therefore, attributable to every form of instinct: - (1) It is adaptive, or directed toward some end; (2) that end is somehow connected with the welfare of the species or of the individual as a member of the species; (3) the reaction is psycho-physically complex; and (4) it is native or inherited and not learned. No other animal is as full of instincts as man (The New Standard Dictionary, 1922)

4.3 The instinct debate refutes the SSSM

What I hope to have demonstrated in the preceding section of this chapter is the length and something of the breadth of the instinct debate. If we omit Darwin's comments and concentrate only on those who called themselves psychologists, social psychologist or sociologists, our survey covers a period of over forty years. Forty years is forty percent of the "century" Tooby and Cosmides claim that the Standard Model has been "the consensus view" in the social sciences (Tooby & Cosmides, 1992: 23). Moreover, the instinct debate cannot be consigned to one of the exceptional sub-

communities that have resisted the SSSM. And with regard to the SSSM and social psychology, it can be seen that most of the instincts said to comprise human nature were social. Let us look at some comments that reinforce the first of these points before substantiating the second.

Should one harbour doubts as to whether Darwin and Darwinism has ever been of import in the psychological and behavioural sciences consider the following passages. The first author is McDougall on the status of two competing paradigms *c.*1900. The second author is Cooley on the agreed authority of natural history *c.*1920. And the third is Bernard on the limits on the new "dogmatism".

> During the last century most of the workers in the social sciences were of two parties – those on the one hand who with the utilitarians reduced all motives to the search for pleasure and pain, and those on the other hand who, recoiling from this hedonistic doctrine, sought the main spring of conduct in some vaguely conceived intuitive faculty variously named the conscience, the moral faculty, instinct, or sense . . . (McDougall, 1908: 14).

McDougall went onto signal the point of change.

> Darwin, in the "Descent of Man" first enunciated the true doctrine of human motives, and showed how we must proceed, relying chiefly on the comparative and natural history method, if we would arrive at a fuller understanding of them (ibid).

And then he outlined what he took to be the coming set of conditions.

> Happily this conception of psychology is beginning to prevail. The mind is no longer regarded as a mere *tabula rasa* or magic mirror whose function it is passively to receive impressions from the outer world or to throw imperfect reflections of its objects . . . Nor are we any longer content to supplement this Lockian conception of mind with only two principles of intrinsic activity, that of the association and reproduction of ideas, and that of the tendency to seek pleasure and avoid pain. The discovery is being made that the old psychologising was like the playing of "Hamlet" with the Prince of Denmark left out, or like describing steam engines while ignoring the fact of the presence of and fundamental role of the fire or other source of heat. On every hand we hear it said that the static, descriptive, purely analytical psychology must give place to a dynamic, functional, voluntaristic view of mind (ibid: 15-16).

Cooley's comments confirmed McDougall's analysis.

> We have come in recent years to look upon all questions of human life from an evolutionary point of view. It may be worth while to recall something of what that phrase means.
>
> It means, for one thing , that all our life has a history, that nothing happens disconnectedly, that everything we are or do is part of a current coming down from the remote past. Every word we say,

every movement we make, every idea we have, and every feeling, is, in one way or another, an outcome of what our predecessors have said or done or thought or felt in past ages. There is an actual historical continuity from their life to ours, and we are constantly trying to trace this history to see how things come about, in order that we may understand then better and may learn to bring to pass those things we regard as desirable.

It means also that if we go back we find that man and the other animals have a common history, that both sprang remotely from a common ancestry in lower forms of life, and that we cannot have clear ideas of our own life except as we study it on the animal side and see how and in what respects we have risen above the condition of our cousins the horses, dogs and apes. Life, it appears, is all one great whole, a kinship, unified by a common descent and by common principles of existence; and our part in it will not be understood unless we can see, in a general way at least, how it is related to other parts. (Cooley, 1922: 1).

Consider now Bernard's challenge to this "dominance" in the mid-twenties. Notice that it is a call for change in "mental and social science" and not a herald to the end that the desired change had come about.

The uncritical assumption that instinct dominates the formation of habits and determines the character of institutions is due to the dominance of the biological viewpoint in modern mental and social science. The spectacular and transforming discoveries of Darwin and his contemporaries and successors . . . are responsible for this dominance (Bernard, 1924: 8).

However;

Doubtless reflex and tropism are basic to human activity, just as they are to the behaviour of sub-human types. But the greater synaptic flexibility and habit-forming power of man render it possible for him to build his behaviour patterns very extensively upon this foundation, with the result that the instinctive element in action is largely lost in the larger volume of superimposed acquired behaviour content. . . The paucity of respect [among instinct theorists'] for sociology has been equalled only by the smallness of their knowledge of what is has to offer in the way of an environmental analysis. Their approval of psychology has been largely limited to its biological aspects. . . (ibid: 11).

Aside the "appalling ignorance of the facts of sociology and social psychology", Bernard was of the view that devotion to the concept of instinct had led to,

. . . a profound dogmatism and cocksureness about a matter which has no better foundation in theory to support it than a crude analogy. In this analogical assumption the biologists have been guilty of a method of violating the canons or tests of scientific truth for which they have strongly condemned the sociologists (ibid: 12).

The message is clear. Although there is a marked difference on the desirability of the doctrine of instincts, McDougall, Cooley and Bernard all say clearly that the doctrine was widely accepted. More widely accepted still was the acceptance that Darwinism was the soil in which psychology must be rooted.

The second point that pertains to my argument that the instinct debate refutes the SSSM with respect to social psychology concerns the social nature of most of the instincts put forward by theorists. Collier, Minton and Reynolds claim that "*Introduction to Social Psychology* remains a landmark in social psychological thought" indicates this (Collier, Minton and Reynolds, 1991: 19). They label McDougall a "social-instinct theorist" in light of the suite of instincts McDougall proposed and his argument that instincts build and colour the social world humans inhabit.

To demonstrate that the instincts proposed were often about, or orientated toward, social thought and behaviour we can drawn upon the work of Bernard (1924). In his study of how the term instinct was used and applied he produced a list of sorts or groupings of instinct (also see chapter 9). Each group added different numbers of specific types. For example, what Bernard called the "aesthetic instincts" contained fifty-one instances. Of the twenty-three groups, the altruistic, anti-social, ethical, family, gregarious, play, retaliative, self-assertive, self-display, and sex instincts are obviously social, and the imitative, religious, self-abasement and work instincts are arguably social. Furthermore, the groupings that are undeniably social contribute over half to the total number of specific types (825 against 763). Adjust this ratio to account for the arguably social types and the balance is 987 against 601.

A third point that casts further doubt on the validity of Tooby and Cosmides' thesis is the fate of the instinct concept after the term itself fell out of favour in the 1930's. Whilst the term was dropped from mainstream social psychology (see Herrnstein, 1974) the concept went on. It did so in three guises.

4.3.1 The fate of the instinct concept

There are three answers to the question as to what happened to the concept of instinct. One of these is that it was rejected by behaviourism and replaced by the concept of habit. As we have seen (chapter 3, section 3, 'Social psychology and behaviourism') behaviourism was never fully embraced by social psychology. Nonetheless, in the spirit of this and the last chapter, it is worthwhile looking at how the term habit replaced instinct because we can discern Darwin's influence on behaviourism. I will show in what ways proponents of the concept of habit took themselves to be consistent with his thinking. The second answer as to the fate of instinct in social psychology shows

that it never really disappeared. Rather, the concept was broken-up and a less contentious terminology was used to represent the core notions. I hope to show that concepts which were derivatives of instincts lived on in psychology in the form of the ideas of dispositions to behave toward functionally defined ends. And the third answer is that it has returned in the guise of adaptations. In this section we will look at the first two answers. In chapter 8 'Instincts and adaptations' I will present a case for the third.

4.3.1.1 Instincts as habits

It is important to note that behaviourists (broadly conceived) considered themselves to be good Darwinians. Indeed, viewed from a certain angle, behaviourism would seem to be the most Darwinian of the major schools of twentieth century psychology. It flowed from pragmatism, concentrated on a pragmatic view of learning and it brokered no principled distinction between species. John Watson clearly took himself to be true to Darwin. To justify the position that, "The behaviourist . . . recognizes no dividing line between man and brute" (Watson, 1913: 158) and the benefits of homology over analogy he appealed to advances made in biology courtesy of Darwin in a very modern tone;

> The whole Darwinian movement was judged by the bearing it had upon the origin and development of the human race . . . It is strange that this situation should have remained the dominant one in biology for so many years. The moment zoology undertook the experimental study of evolution and decent, the situation immediately changed. Man ceased to be the centre of reference (ibid: 161).

Skinner too thought he was true to Darwin.

> You have precisely the same problems with operant behaviour that Darwin faced with evolution. Natural selection and operant conditioning are very similar. Both move purpose from before to after. This explains origination (Skinner in Cohen, 1977: 280).

Nevertheless, behaviourism took the quest to replace instincts with habits and their formation as sound in evolutionary terms. Darwin issued the licence.

> How unconsciously many habitual actions are performed, indeed not rarely in direct opposition to our conscious will! . . . Habits become easily associated with other habits, with certain periods of time, and states of the body. When once acquired, they often remain constant throughout life. Several other points of resemblance between instincts and habits could also be pointed out. As in repeating a well known song, so in instincts, one action follows another by a sort of rhythm; if a person be interrupted in a song, or in repeating anything by rote, he is generally forced to go back to recover the habitual train of thought (Darwin, 1859: 192).

Darwin developed this line of thought in a comparison of what he took to be an obvious exhibition of a habit (an acquired action pattern) to the behaviour of a caterpillar building a hammock: if interrupted the caterpillar too will return to stage one of the "very complicated" operation. For Darwin the distinction between habit and instinct was not to be discerned in their exhibition - "the resemblance between what was originally a habit and an instinct becomes so close as not to be distinguished"(ibid: 192) - but in their origin. In Darwin's view, part of the difficulty stemmed from the fact that "slight variations of instinct might be profitable to a species" (ibid: 192). It is not difficult to see that a consequence of this is that whilst the possibility of variations is a result of natural selection, the cause of the variation exhibited is due to factors extrinsic to the organism. Darwin viewed habits as a form of functional adjustment bounded by the problems and needs faced by an organism in the course of its lifetime.

He returned to and reinforced this point in *The Expression of the Emotions in Man and Animals;*

> . . . although some instincts have been developed through long-continued and inherited habit, other highly complex ones have been developed through the preservation of variations of pre-existing instincts – that is, through Natural Selection (Darwin, 1872/1998: 47).

One of the earliest and most persistent critics of instinct theory was Knight Dunlap. Seizing on Darwin's conflation, he attempted to pick apart how one might be able to distinguish between instincts and habits;

> At the present time, I can see no way of distinguishing usefully between instinct and habit. All reactions are definite responses to definite stimulus patterns, and the exact character of the response is determined in every case by the inherited constitution of the organism *and* the stimulus pattern. All reactions are instinctive: all are acquired. If we consider instinct, we find it to be the form and method of habit-formation: if we consider habit, we find it to be the way in which instinct exhibits itself. Practically, we use the term instinctive reaction to designate any reaction whose antecedents we do not care, at the time, to inquire into; by *acquired reaction,* on the other hand, we mean those reactions for whose antecedents we intend to give some account. But let us beware of founding a psychology, social, general, or individual, on such a distinction (Dunlap, 1919: 92).

What is especially noteworthy in this passage, aside it being an implication of Darwin's view, is the classification of the issue into theoretical and practical components. Dunlap suggested that any insistence on the pursuit of instinctive designations was, in effect, a resignation of the pursuit of cause.

As the debate moved on through the twenties, amongst other difficulties, advocates of instincts could not shake off the apparently obvious fact that the behavioural repertoire of organisms comes to

be comprised of actions that were not evident in infancy. This left open the possibility that in their mature expression instincts were only explicable in terms of the concrete experiences of their possessors. In other words,

> . . . that instincts or inherited action patterns do not appear originally in definite form, ready for the task of mediating adjustments of the organism to its environment, but that all instincts must be educated by learning (Bernard, 1924: 5).

That Bernard went on to deny that the foregoing is a sound argument to the end that instincts are habits gives us some indication of how tenacious was the concept of instinct. He suggested that to say "all instinct must be educated by learning" "is rather a denial of the existence of instinct, or at least of definite inherited complex action patterns". And furthermore, "Such an admission would establish at once the primacy of environment and habit in the determination of individual character and of social adjustments and institutions" (ibid: 5). These words of Bernard describe with some accuracy the moves that presaged the switch of interest from instincts as inherited adaptations to their development as "serviceable habits".

Dunlap's position in *Habits: Their Making and Unmaking* (1932) returned to and seized upon the difficulty of distinguishing instincts from habits and sought to establish the later as primary. Dunlap's overall case rests on what we might call the criterion problem:

> To maintain a contrast between unlearned reflexes and responses which are in part due to learning, we must in some way identify the unlearned, original reflexes [but] the distinction between the responses which are customarily listed as reflexes and other responses is not absolute, but it is a matter of gradation; and many reflexes are so modifiable that there is extreme difficulty is ascertaining what the original, unlearned reflex was. (Dunlap, 1932: 38).

His next step is to show that one solution to this problem – Watson's recommendation that we observe and itemise the behaviours of the newborn – renders a list of instances of instinct that is too impoverished to explain the bulk of normal adult behaviour. Next Dunlap looked to show that another solution – the classification as instincts of all first-time responses – renders a list that would be being added to right up until death. Dunlap suggested that, by incorporating into the definition of instinct the qualification that it needs to be an unlearned response, one simultaneously denied the concept a criterion of satisfaction. He rested this point on the fact that, at the time of his writing, psychology was not in possession of a clear grasp of what a learned response was.

In Dunlap's final approach habits are located between instincts and learning. In his hands, the concept of habit can accommodate that in the notion of instinct which ties humans to the rest of nature and that in the notion of learning which gives flexibility of response from one generation to

another. What was not dropped was the suggestion that the invocation of instinct amounted to a failure of interest in the causal antecedents of a given behaviour and, therefore, the absence of a scientific account. This I take to be the fundamental stance which behaviourists came to adopt. Habits became synonymous with instinct before being taken as superior in the construction of causal accounts of behaviour.

I am not suggesting that behaviourism is of a piece with Darwinism. What Darwin meant by a "new footing" was not what Watson meant in his paper of 1913, *Psychology as the Behaviourist Views it*. The comparative psychology that appeared to be there was lost in the detail of accumulated and artificial stimulus-response matrices that came to be what behaviourism was in practise. In its disdain for grand theory, faculty psychology and cognition, behaviourism drifted away from evolutionary theory and the attendant respect for the differing natural histories of species. It missed the key point that Breland and Breland were to make long after the instinct debate: organisms are disposed toward certain behavioural patterns over others and are plastic only in relation to these dispositions (Breland & Breland, 1961). Behaviourism also imported a certain form of Lamarckism. The idea that the native environment of an organism could safely be ignored tells us that, along with a species-universal notion of association rules that differ only in their sequential complexity, behaviourism held a concept of behavioural plasticity that defied the speed at which adaptations could appear. The trouble with inferring a plasticity that affords substantial behaviour change within the lifetime of the organism is that, at least implicitly, it implies the past of the species need not be taken too seriously. What I am suggesting is that behaviourism took itself to be consistent with evolutionary theory.

4.3.1.2 "Driving adjustments"

The first sustained and influential shots against instinct theory were fired after the first world war. Amongst the responses was Tolman's *Can instincts be given up in psychology?* (1922). The paper is, essentially, a rhetorical piece which tried to show how impoverished psychology would become should it reject instinct – and with it evolutionary theory - altogether. But at the same time as defending instincts, Tolman also showed what was to be the fate of the concept. Tolman, in an attempt to further refine the definition of the term, suggested that the more modest "driving adjustment" take its place. We have here the beginning of what we might call the break-up of instincts as an explanatory monolith. The following is a list (it is not exhaustive) of terms that represent the break-up. It conveys something of what happened to the concept of instinct in social

psychology during the twenties, thirties and forties. I will pay attention to one of these ideas so to illustrate the point that instincts were not given up in psychology – Kuo's "units of reaction".

"Fundamental desires" – Dunlap, *Elements of Scientific Psychology* (1922).

"Human proponent reflexes" – Allport, *Social Psychology* (1924).

"Native impulse's" – Ellwood, *The Psychology of Human Society*, (1925)

"Primary desires" – Dunlap, *Civilized Life* (1934)

"Motives" – Gurnee, *Elements of Social Psychology* (1936)

"Dependable motives" – Woodworth, *Psychology* (1929) and, later, Klinberg, *Social Psychology* (1940, 1953).

"Drives" - Murphy, Murphy and Newcomb, *Experimental Social Psychology* (1937).

"Viscerogenic and psychogenic needs" – Murray, *Explorations in Personality* (1939)

Kuo's proposed alternative to instincts were "units of reaction", proposed in a volume titled *Psychology without Heredity* (1924). The title reflects his place amongst the least compromising critics of instincts. Indeed, so anxious did he seem to be to dispense with the concept, I am of the view that he rather misrepresents those he attacks: for example, he attributes to McDougall a dogmatism and a rigidity that is not evident in his writing (i.e. Kuo, 1921). As Tolman suggested, then, there is some irony in Kuo's alternative to instinct in understanding "man's native equipment".

For Kuo "units of reaction" were "the elementary acts out of which various coordinated activities of later life are organized". They are reflexive in character and best described in physiological rather than neurological terms. Perhaps most importantly, Kuo rejects any notion that these early behaviours are "teleological" or have biological significance aside the "vegetative actions" such as eating, pain avoidance or excretion.

Units of reaction are shaped by natural selection in the course of their development within the lifetime of an individual. Their results determine their future. This process of adjustment slows but never ceases thus allowing the individual to accommodate novelty throughout the lifespan: "On the whole, the plasticity of habits depends upon the richness of experience of the organism. The more experience or the more variety of stimuli it has, the less fixed and rigid are its organized reactions" (Kuo, 1921: 661).

With regard to predicting the course and final shape that units of reaction might take, Kuo's "chief contention" was that,

> The type of integration of the elementary acts into complex reaction systems largely depends on the nature of the environment. . . If a man is born and raised in a highly civilized community, he may acquire a powerful trend of parental care which he extends to humanity as a whole and even to animals. On the other hand, if he is brought up in a savage tribe where the custom of cannibalism prevails, he may acquire a habit of taking pleasure in killing. At one time the same native equipment may be developed into compassion, while at others it may be developed into cruelty (Kuo, 1921: 661).

The similarity with Watson's "manifesto" is apparent. So too is the similarity with other authors who were keen to dispense with the term instinct but found use for some of its basic properties. Though there is variation in the list of concepts cited above, none entirely dispensed with a biological construal of motivation and the view that motives orientate individuals toward things that need to be achieved even if the final attainment comes through an acquired adjustment of initial and unlearned behaviours.

4.4 Boring's trap

My position is this: if one takes Tooby and Cosmides' lower estimate and views their Standard Model thesis as an historical account of the past sixty years in social psychology, then the instinct debate cannot be levelled against it. However, if one takes their upper estimate of the SSSM as being dominant for 100 years then the instinct debate refutes the thesis of the SSSM. It does so because the duration and extent to which instincts were put forward as explanatory devices in social psychology shows that the Standard Social Science Model is not a reliable representation of the late nineteenth and first quarter of the twentieth century. If we take James as our starting point, and the early thirties as the time of the demise of instinct theory, then the period over which instincts were a live issue covers forty years of Tooby and Cosmides century of the SSSM[6]. Few theoretical standpoints have been so extensively and lengthily dominant.

Furthermore, instinct theory was positively Darwinian in cast – indeed, so much so that the instinct debate would not have taken place in the form that it did had it not been for Darwin and his influence. Whilst there has been a passing acknowledgement of this fact from within evolutionary psychology (e.g. Cosmides & Tooby, 1989), the caveat is added that the theorising was shallow (e.g. Buss, Haselton, Shackelford, Bleske & Wakefield, 1998). The definitions of instinct that configured the debate included the majority of key Darwinian notions, i.e., adaptive, heritable, selected for, species typical, unlearned, variable in expression, subject to modification, having a psychological dimension, and behavioural in manifestation. The predominance of instincts for and about social life shows the concept was of especial interest to social psychologists. This point is reinforced given that

no instinct theorist was more influential than McDougall and no volume more widely read than his *Introduction to Social Psychology*.

Tooby and Cosmides thesis is further undermined when we look at what happened to the instinct concept. On the one hand it was converted into habit. On the other it was downgraded into notions such as "driving adjustments". Both notions can be traced back to Darwin.

We will return to the concept of instinct again in Part III, 'Evolutionary Social Psychology without Adaptations'. To bring this part of the dissertation to a close I want to try and draw a lesson from the failure of Tooby and Cosmides' account of the intellectual history of social psychology. To do so I will draw upon Boring's APA presidential address of 1929, a year or so before the instinct debate of the early twentieth century ceased to be dominant.

Edwin Boring's 1929 APA Presidential Address was titled *The Psychology of Controversy*. In the paper, Boring mined some of the controversies which constituted psychological debate in the nineteenth and early twentieth centuries to support the case that scientific movements have traits comparable to those of the protagonists themselves.

Boring argued that the psychologist as "knower" of "the human mind that is both subject and object of his work", does not, in fact, enjoy any superiority over other scientists. This claim rests upon the argument that ". . . yet not the behaviourist, nor the 'gestaltist', nor the purposivist, nor the late functionalist, nor even the introspectionist himself has yet to succeed in maintaining clear vision with the eye rotated through 180° to see the mind that is at work" (Boring, 1929: 97). Still, Boring was keen to place psychological research within the wider enterprise of scientific inquiry. He did so by showing that psychology was rooted in and driven by controversy, just as were the "exact sciences".

> . . . I have come reluctantly to the conclusion that scientific truth, like juristic truth, must come about by controversy. . . It seems to me that scientific truth must transcend the individual, that the best hope of science lies in its greatest minds being often brilliantly and determinedly wrong, but in opposition, with some third, eclectically minded, middle-of-the-road nonentity seizing the prize while the great fight for it, running off with it, and sticking it in a textbook for sophomores written from no point of view and in defence of nothing whatsoever (ibid: 98).

Whilst taking this view to be "personally abhorrent" Boring claimed it to be the "verdict of the history of science". The claim is supported by a series of examples from psychology, including controversies that involved "ancients of the late nineteenth century" such as Mesmer, Fechner, Wundt and Stump, and contemporaries such as Titchener and Baldwin. Boring was keen to highlight

the language that coloured those debates, pointing out not just their rhetorical bent but also the air of personal antipathy.

On a more promising note, Boring's second objective was to elucidate what he called the "negativism of progress".

> With respect to scientific movements there seems to exist something like Newton's third law of motion: action equals reaction. You cannot move – in the sense of starting a movement – unless you have something to push against. . . Science can actually, by the empirical method . . . lift itself by its own bootstraps, but the result is not what we call a "movement" because motion can be defined only with respect to a frame of reference. A movement must move with respect to something, and progress must move away from something, if the movement is to command observational attention. It is therefore the business of the founders of new schools, the promoters and propagandists, to call persistent attention to what they are not . . . (ibid: 108).

Boring gave a series of examples. One of these was the duel between functionalism and structuralism. In Boring's view, the dependency of apparent progress on negativism is tempered by the observation that some controversies are "often one sided because directed against no particular opposition".

> In those days the opposite of functionalism was structuralism, but nobody – except perhaps some graduate students – ever called himself a "structuralist". Titchener adopted the phrase "structural psychology" and abandoned it long before it went out of use. No, the functionalist had to have something definite to push against, and it was only they who talked about "structuralists" (ibid: 110).

It was Boring's view that observed progress so depends upon negativism that many of the apparent "fights" in psychology are, in fact, against "windmills" (ibid).

One is unable to read *The Psychology of Controversy* in light of the tenor, tone and testaments of Tooby and Cosmides' account of the history of social science and fail think that what held in Boring's time holds now for evolutionary psychology and the Standard Social Science Model. Boring's contention was that the controversies of psychology elucidate the psychology of controversy. This, in turn, tells us something about the nature of scientific enterprise. My reason for discussing Boring's ideas are slightly different. I am of the view that evolutionary psychology has needlessly embroiled itself in an unwinable controversy from which it needs to extricate itself.

First, as I have shown, psychological and behavioural thinkers have for long periods been immersed in the implications of Darwinism. It is plainly and factually incorrect for evolutionary psychology to deny this. And it is disingenuous to down-play it. Evolutionary psychologists who use

the term "Standard Social Science Model" and rhetorical equivalents (see the introductory comments in chapter 3) undermine their own much vaunted rigor. On Boring's account, they invite comparison with the arch windmill fighters of twentieth century psychology, the behaviourists (Boring, 1929: 110).

Second, the Standard Model thesis encourages neglect of an extensive literature of carefully constructed arguments and closely fought debates that, at bottom, agreed on so much (see chapter 4). Now, whilst we may take Boring's point that "Respect for parents may be laudable and yet hinder the free development of youth", I am of the view that evolutionary psychology could profit from an emphasis on those agreements. Let us consider this in a little more detail.

The instinct debate was comprised of two main antagonisms. The first of these was a discussion aimed at settling upon and refining terms. The objective was to develop the core notion of an instinct into an idea that would encompass mental content, mental processes and behaviour. It was acknowledged that not every natural phenomena was an instinct - even some things that were deemed to be adaptive - and a principled means of valid denotation was sought. The second sort of antagonism appears to have crept into the debate at a later stage. This second sort was more overtly confrontational and it marked the end of the instinct debate as a progressive exercise. It consisted of opposing sides, if you will, rather than schools. Dewey and Watson exemplify this difference. Both first embraced instincts, accepting them as a fact of human psychology and behaviour, albeit adopting different stances (Dewey, 1896; Watson, 1916a). Both later came to oppose instincts: I have in mind here Dewey's volume *Human Nature and Conduct* (1921) and Watson's *Behaviorism* (1930). As opponents they sought not to discern to what extent instincts illuminated psychology and behaviour but to present a case to the end that the answer was "negligible". Schools of thought that sought to refine the concept of instinct gave way to sides which sought to undermine one another's basic position. In my view, evolutionary psychology has needlessly engaged itself in this second sort of antagonism. It may be able to extricate itself from this situation by retracting the thesis of the SSSM. Aside being closer to the facts of the history of social science in general and social psychology in particular, by retracting the thesis of the SSSM evolutionary psychology may be able to build upon past thinking. It can address those objections and problems that faced instinct theory rather than imply that any such objections are false by virtue of being motivated by something other than genuine critical inquiry. I am suggesting that evolutionary psychology can escape Boring's trap by using the history of psychology rather than becoming, prematurely, part of it. Using the terms Boring applied to behaviourism, I am suggesting that evolutionary psychology "is past its prime as a movement because movements exist upon protest" and it no longer needs to protest" (ibid: 111).

[1] It is also noteworthy that, just as did Wilson over one hundred years later, he based his argument on the behaviour of insects.

[2] A note on William James (1842-1910): James read Spencer's *First Principles* as an undergraduate. He was in Cambridge when *The Descent* was published in 1871, and his letters to friends at the time suggest that he was familiar with its contents (Richards, 1987). James' first lecture course of his own device was titled "Natural History 2: Physiological Psychology". It was explicitly concerned with the implications of evolution for the philosophy of mind and action. Spencer's *Principles of Psychology* was used as the core text. However, Spencer's volume increasingly served as a source of criticism for evolutionary thinking in psychology. James believed that Spencer's Lamarckism was an assertion to the effect of what we might call environmentalism – that is, James thought Spencer to be advocating a form of what Tooby & Cosmides call the SSSM. He thought that Spencer saw the mind as overly passive in light of the need to be flexible. James argued in "Spencer's Law of Intelligence" that behaviour and its underlying physiology is not in a direct correspondence to the environment (not in some lock and key sense) but that behaviour is a result of a physiological response to problems.

[3] Cited in Drever, J., 1917: 17.

[4] Hearnshaw (1964) suggests that Drever's emphasis on the literature prior to Darwin was, in fact, designed to undermine McDougall's claims of originality.

[5] Every other page in each of these two chapters is headed with "Hereditary Modes of Response: Emotion" and "Hereditary Modes of Response: Emotion" respectively. I have not been able to ascertain if this method of presentation was due to Watson.

[6] Writing in 1955, Beach goes considerably further than the conclusion forwarded here:

> Although there are militant opponents of the instinct doctrine among present-day psychologists, it is undoubtedly correct to say that the concept of instincts as complex, unlearned patterns of behaviour is generally accepted in clinical, social, and experimental psychology (Beach, 1955: 408).

Part II: The Environment of Evolutionary Adaptedness

Chapter 5 The justification and conceptual role of the EEA

Summary of chapter 5

Tooby and Cosmides (1990b, 1992) promote a concept called the Environment of Evolutionary Adaptedness (EEA) and place it at the heart of evolutionary psychology. The EEA refers to the set of conditions that created modern humans, it is their natural history. The role of the EEA as a concept is to furnish forth a specifically human psychology. The EEA marks a distinction between evolutionary psychology and other Darwin-inspired approaches to mind and behaviour, i.e. between the adapted mind approach and the adaptive mind approach. In this chapter we look at the concept of the EEA in use and the assumptions that it makes about natural history.

5.1 Introduction

I have, I hope, cast serious doubt on the historical validity of the Standard Social Science Model as the prevailing orientation in 20th century social psychology. It may be granted that, since the collapse of instinct theory, there has not been a sustained and explicit attempt to address questions in social psychology and the social sciences more widely from an evolutionary point of view. However, from this state of affairs it does not follow that atheoretical models of human behaviour and concomitant models of human nature have dominated social psychology. This part of the dissertation presents arguments to the end that the alternative view of social psychology presented by evolutionary psychology requires a conceptual device that is flawed.

In this chapter an outline of the Environment of Evolutionary Adaptedness (EEA), its usage and its conceptual role will be presented. In the following chapters two classes of criticism of the EEA will be discussed. Of these, one class may be characterised as empirical, the other as conceptual. Empirically, the EEA can be criticised on three specific counts. One, it is contrary to the palaeoarcheological evidence to classify the genus *Homo* as hunter-gatherers. Two, the EEA consists of both a set of physical and social causes. And three, the fossil evidence we have of the EEA does not predict and cannot account for novel behaviours that have emerged since. Conceptually, the EEA can also be criticised on three specific counts: that it is circular, that when not circular it invokes a vicious regression, and that it fails to deal with the problem of meaning in minds that are not, by definition, human. Let us now look further at the term "Environment of Evolutionary Adaptedness."

John Tooby and Leda Cosmides argue that the EEA is critical to our understanding of extant human psychology. The term, which entered the psychological lexicon courtesy of John Bowlby (1969), has become a common part of the terminological apparatus of evolutionary psychology (e.g.

Badcock, 2000; Buss, 1994, 1995; Buss, Haselton, Shackleford, Bleske & Wakefield, 1998; Campbell, 1999; Cartwright, 2000; Crawford, 1998b; Corballis & Lea, 2000; Foley, 1995c, 1996a, 1996b; Gaulin & McBurney, 2001; Irons, 1998; Janice & Krebs, 1998; Kohn, 1999; Leubbert, 1999; Miller & Fishkin, 1997; O'Neil, 1998; Orians & Heerwagen, 1992; Riadh, 1998; Ridley, 1993; Rose, 2000; Rose & Rose, 2000; Segal, 1998; Stevens & Price, 1996; Symons, 1979, 1990, 1992; Smuts, 1990; Thornhill, 1997; Wright, 1994), and it is at the heart of the enterprise. It is argued that the EEA is the environment which the human mind evolved to cope with and function in. We are to use this device as an overlay to the tenets of evolutionary theory if we are to be successful in constructing a principled model of the set of adaptations that is the phenomenon of mind. The supposition is that the EEA is our natural environment in the sense that it is the past and not the present that we evolved to cope with and act in. Accordingly, "the past explains the present" (Tooby & Cosmides, 1990b).

Let us now be a little more precise and look at Tooby and Cosmides' definition of the EEA.

> 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 & Cosmides, 1990b: 386-7).

Whilst other writings of theirs reiterate this definition, neither Tooby and Cosmides, nor those that take their lead, are so consistent as these passages suggest. In practise, the EEA is not reliably treated as "a complex statistical composite" of the past. In practise, and in terms of natural history, the EEA is typically characterised as being co-terminus with the Pleistocene (Barkow, 1992; Caporael & Baron, 1997; Cosmides & Tooby, 1992; Crawford, 1998b; Foley, 1995, 1996; Krebs & Denton, 1997; Liken, 1995; Miller & Fishkin, 1997; Orians & Heerwagen, 1992; Profet, 1991, 1992; Tooby & Cosmides, 1990b, 1992).

"Pleistocene" is the geological and palaeoanthropological term that covers the *c.*2 million year period prior to that which has seen the emergence of modern man, *Homo sapien sapien*. (The latest/current period is referred to as the Holocene). The beginning of the Pleistocene, also known as the early Pleistocene, saw the emergence of the genus *Homo* – the genus that encompasses modern man. We will have reason to return to some of the established geological and palaeoanthropological terminology in due course.

In addition to "Pleistocene" there are a number of other terms which are more or less synonymous and are more or less interchangeable with the EEA. These include "the ancestral environment" (e.g. Crawford, 1998b; Mashman, 1997; Pinker, 1997; Proulx, 1999; Richardson, 1996; Tooby & Cosmides, 1990b; Watson, 1998; Wright, 1994), the "environment of selection" (e.g. Dennett, 1995)

and various uses of the term "heritage" (e.g. Bryne, 1999; Foley, 1995c; Orians & Heerwagen, 1997). It must be noted, however, that these terms are not identical to the EEA. When used, they are typically done so to weaken the direct temporal and phylogenetic consequences of the mapping of the EEA onto the Pleistocene (e.g. Foley, 1996a).

The reason for this comparative temporal isolation is clear. If we want to be species specific, if we want to furnish forth a human psychology, we must look to those aspects of the past that are responsible for what we take to be specifically human. David Buss displays this reasoning in relation to the important case of human sociality;

> It is unlikely that our huge brains - the 900 cubic-centimeter advantage we have over chimpanzees – have evolved to help us pick berries or avoid snakes. These survival problems are all solved by chimps with a much smaller brain.
>
> It is far more likely that humans evolved such large brains as a consequence of the complexities of social living and social competition that includes forming coalitions, executing a rich repertoire of short-term and long-term mating strategies, negotiating the intricacies of complex kin networks and social hierarchies, forming long-term reciprocal alliances, and socializing children for years or decades (Buss, 1997: 399).

The Pleistocene, in practice, is the EEA for all of those features, supposed or extant, that are particular to humans. Accordingly, Tooby and Cosmides, notwithstanding their claim that "The EEA is not a place or a habitat, or even a time period" (Tooby & Cosmides, 1990b: 386), concede that;

> . . . 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 . . . (ibid: 388).

Having specified what period is being referred to by the term the "EEA" – its temporal aspect - we can also specify its spatial aspect. The place of the EEA is Africa. More precisely, it is predominantly eastern and, to a lesser extent, southern African (Wilson & Cann, 1992). This placing of the EEA follows the majority view of palaeoanthropologists that modern humanity is "Out of Africa" (Tattersall, 1997). However, we cannot be as sure of its location as we can its duration for there is some debate on the matter of where hominid development prior to modern man took place (Stringer, 1990). Again, we will have cause to revisit this issue when we discuss the social and the physical EEA in chapter 6.

5.2 What the EEA adds to evolutionary psychology

It is important to consider why we might need a concept in addition to evolutionary theory to develop an evolutionary psychology. The first and most obvious motivation stems from the fact that evolution is a general theory, mathematical in nature, which claims to account for the functional features of any self- or sexually-reproducing organism. It is not specifically about humans. In fact, applied to all the distinct species that have ever existed, the human story would be difficult to find such would be the size of the library. Motivated as such, the EEA is a tacit admission that evolutionary theory in and of itself is not entirely adequate as a generator of hypotheses about psychological adaptations. As we will see, to suppose there is something right about an evolutionary approach to human psychology involves nothing more than the assumption that humans are evolved instances of an evolved species, species as a whole are exhibits of functional design in light of selection pressures and brains are no exception. This is indeed surely true. But we would have no need for a concept over and above evolution if evolution theory was adequate in and of itself. Let us now look at the need for the EEA in more detail. We can do so by first asking what it is that an evolutionary approach adds to our speculations about human psychology.

Robin Dunbar (1988) has suggested that the proper question we should be asking about human psychology and behaviour in light of modern evolutionary theory is not whether the principles apply but to what extent. The (perfectly reasonable) assumption behind Dunbar's question is that since evolutionary theory is correct, it must have something to say about products of the process of natural selection, including modern humans. The answer to Dunbar's question is that evolutionary theory, in and of itself, says of humans only the following:

- They will seek to have sex (i.e. to reproduce, to enter and maintain sex-based relations)
- They will behave differently towards kin than they will toward non-kin (i.e. exhibit nepotism)
- They will seek and, if at all necessary, compete for resources

Furthermore, evolutionary theory says that,

- Historically speaking, sex, kin and resource acquisition are what humans are about
- Functionally speaking, sex, kin and resource acquisition are what humans are for

It is for the reader to decide on the profundity of these statements. I am of the view that any statement which lays claim to certainty is, axiomatically, profound. What it is that these statements tell us for certain is what sorts of ends humans will consistently pursue and why they will do so (on the assumption that the given event can reasonably be linked to sex, social interaction or resource manipulation). However, I am also of the view these statements do not provide the sort of detail that psychological, social and behavioural science seeks, e.g. what class of humans do what sorts of

thing, approximately when, and, in the case of social psychology, to what other classes or sorts of group.

What I would like to do in this section is discuss what that an evolutionary approach adds to questions and answers of the general sort social psychology deals with. This task breaks readily into two: an evolutionary approach to hypotheses (i.e. questions) and an evolutionary approach to conclusions (i.e. answers). The second task will be addressed in a later chapter. Here we will explore the way in which an evolutionary approach adds to or enriches our hypothesis generation. The answer is threefold.

Firstly, it doesn't. Not being conversant with evolutionary theory does not, of necessity, restrict what it is one may postulate about psychology and behaviour. Evolutionary theory does not somehow open up lines of inquiry into human psychology and behaviour that could not, in principle, have been pursued otherwise. As was suggested above, evolutionary theory points us toward sexual behaviours, kin and non-kin relations and resource acquisition. But it is apparent these subjects have long been of interest to social psychology. For example, Tajfel (1970) and his followers were not, and did not need to be, inspired by an evolutionary approach to investigate in- and out-group formation and its persistence to formulate Social Identity Theory.

Second, it promotes hypotheses that serve only to confirm the theory from which they are derived. Because evolutionary theory, in and of itself, says so little (however profound), hypotheses drawn directly from it must be relatively modest. Again, strictly speaking, hypotheses will be of the sort, "Humans will seek sex", "Humans will behave differently toward kin than they will toward non-kin" and "Humans will seek resources". It is not clear that there have ever been substantial bodies of thought in social psychology that would deny these statements. Accordingly, they have the appearance of being obvious. Certainly they are obvious to any that have or do subscribe to evolutionary theory and they appear to be obvious on inspection of the long-standing concerns of social psychology. An evolutionary approach simply adds justification to hypothesis generation in the areas of sex, kin and non-kin relations and resources acquisition. It tells us that natural history sanctions an emphasis on sex, kin relations and resource acquisition. This leads us to the third point that is raised when we ask in what way does an evolutionary approach add to hypothesis generation: it acts as a constraint.

Subscription to the basic tenets of evolutionary theory limits what we might say to be the case of human psychology and behaviour. It cautions against negations of just those hypotheses it promotes. It recommends that we take a deeply skeptical approach to hypotheses to the effect that humans will not seek sex, humans will not discriminate between kin and non-kin and will not seek and, if necessary, compete for resources. Additionally, an evolutionary approach promotes skepticism that

such claims are historically sound and that a basis for them can be discerned in human anatomy and physiology.

In light of these considerations I suggest that we accept the conclusion that, in actuality, evolutionary theory adds nothing to hypothesis generation about the nature of modern humans. At best it acts as a constraint, recommending only that we do not seek to test hypotheses that are, at bottom, negations of its principle tenets. What the preceding considerations do show is why it is we need to invoke something like the EEA. Now we can look further into the question as to what it is that the EEA adds to psychology.

By invoking the EEA, evolutionary psychology is arguing that questions about the nature of modern humans demand that we become natural historians. To say something specific about human phenotypes and morphology one needs to isolate the natural history of the lineage. After all, the story of human evolution, strictly speaking, begins when the story of life on earth begins. We can now see that inclusive fitness has generated *Homo sapien sapien*. But this need not have been the case. Inclusive fitness necessarily applies to all sexual reproducers. The necessity of natural selection is what is meant by "Universal Darwinism" (Badcock, 2000; Dennett, 1995; Plotkin, 1995; Tooby & Cosmides, 1992). The great power of evolutionary theory is that it is not specifically or exclusively about humans. Darwin and Alfred Wallace did not appeal to the human species to demonstrate natural selection and the bulk of their peers did not require them to do so. However, the trouble with the very generality of evolutionary theory for social science is that it is not anthropomorphic.

To make evolutionary theory an account about humans we must reach back into our natural history. Although there have been periods when very many species have suffered extinction (see Leakey, 1986), it is thought that total extinction of life on earth has never occurred. Thus there is a strain of continuity. But there is also some point at which the overall story begins to contain within it a genesis of the human: a point at which something like humanity began to be, in retrospect, possible because nature the tinkerer, at some point, began to tinker with something human-like. There are any number of possible points at which this became the case and there are a variety of views on this (see Foley, 1995c). A conservative view would posit the appearance of the great apes at the far end of this possible spectrum, the divergence of the australopithecine's from *Pan troglodytes* in the middle, and the appearance of hominids (*c.*2 million years ago) at the near end. The EEA concept focuses our attention on those parts of the past which promise to add human detail. And it promises to add empirical detail because, though we may not be able to visit it, statements about it are, in positivist terms (Ayer, 1936) terms, verifiable at least in principle.

The role of the EEA in evolutionary psychology is to:

- "Humanise" the general process of evolution
- Psychologise that which is particularly human
- Isolate key selection pressures in order to posit universal psychological adaptations
- Overcome the analogous nature of comparative psychology and generate a homologous theory of psychology

These considerations show that the EEA concept is vulnerable to criticisms that are not applicable to evolutionary theory. As a device, the EEA can fail - and with it attempts at an evolutionary social psychology that depends upon it. Ultimately, it is conceivable that we can accept that minds are evolved without being able to generate the adaptationist psychology proposed by Tooby and Cosmides. Let us now look at Tooby and Cosmides' argument in more detail. We can begin by considering the following passage;

> Wilson (1975) defined an adaptation as "any structure, physiological process, or behaviour pattern that *makes* an organism more fit to survive and reproduce in comparison with other members of the same species". However, I would replace Wilson's word *makes* with the phrase *made an ancestral* to emphasize that adaptations came into being in an ancestral environment. (Crawford,1998: 278).

Here we are listening to a joint editor of volumes titled *Sociobiology and Psychology: Ideas, Issues and Applications* and *Handbook of Evolutionary Psychology: Ideas, Issues and Applications.* And here we see the proposed difference between sociobiology and evolutionary psychology writ plain. Fitness and functionality for the former is here and now, for the latter it is there and then. The role of the EEA is to determine what adaptations made ancestral *Homo* fit to survive and reproduce. The supposition is that the EEA caused modern humans.

Let us now see how Tooby and Cosmides sanction this approach in evolutionary psychology. In a sub-section of *The Psychological Foundations of Culture* titled *The Evolutionary Contribution to Integrated Explanation* they present *The Peculiar Nature of Biological Functionality.* Its task is to reposition the concept of teleology.

> In certain narrowly limited ways, then, the spontaneous process of evolution parallels the intentional construction of functional machines by human action. But, whereas machines built by human engineers are designed to serve a diverse array of ends, the casual process of natural selection builds organic machines that are "designed" to serve only one very specialized end: the propagation into subsequent generations of the inherited design features that comprise the organic machine itself (Tooby & Cosmides, 1992: 53).

Tooby and Cosmides claim to subscribe to the "standard biological terminology" expressed in the statement "design features are selected for to the extent that they promote their inclusive fitness" (ibid: 53). They call this view "design-propagation". Their next move is to dismiss the idea that designs can be propagated by anything other than natural selection. The (seemingly ubiquitous) "benevolent deity or extraterrestrial being" is dismissed by an accusation that the world is not arranged for "human convenience". *Viz*,

> Wild horses could be born with saddle-shaped backs, luggage racks, and a spontaneous willingness to be ridden: chronic bacterial infections could jolt humans with caffeine every morning 45 seconds before they need to get up (ibid).

And the possibility that pure chance is responsible for good design is also dismissed on the charge of human convenience.

> Similarly, the non-living world could be full of intricate functional arrangements not created by humans, such as mountains that naturally mimic hotels down to the details of closet hangers, electric wiring, and television sets (ibid).

By showing that the world is not designed intelligently or perfectly, Tooby and Cosmides create room for natural selection to be the only viable account of any sort of biological design and organisation[1]. Natural selection is the only known process able to create "peculiar" forms of semi- or sub-functional design. Imperfections in organisms demonstrate that natural selection is not teleological.

The next move makes a return to inclusive fitness. Tooby and Cosmides' own "standard biological terminology" for inclusive fitness, "design-propagation", suggests that to subscribe to inclusive fitness entails subscription to the idea that organisms, including humans, are reproduction maximisers: that first and foremost, humans are about reproduction. The obvious question, then, is, Why not study them that way? Why not treat them as if totally disposed toward the goal of immortality via sex and kinship. Their rejoinder,

> A life history of successfully achieved reproduction (including kin reproduction) requires accomplishing the entire tributary network of preconditions for and facilitation's of reproduction in complex ecological and social environments. Of course, this includes all of the information gathering, inference, and decision-making that these tasks entail. For this reason, humans display a diverse range of adaptations designed to perform a wide and structured variety of subsidiary tasks . . . (ibid: 54).

To this point their argument is not quite finished. In saying that, "Individual organisms are best thought of as adaptation-executors rather than as fitness-maximisers", Tooby and Cosmides are

saying that psychology must break down design-propagation as a whole into the tasks that comprise the whole. Most importantly, we must establish what these tasks were and not what they are or might be for any given extant human or population. Accordingly, the implication of the following is that social psychology must become overtly historical;

> Natural selection cannot directly "see" an individual organism in a specific situation and cause behaviour to be adaptively tailored to the functional requirements imposed by that situation. . . . Selection acting over evolutionary time has constructed the mechanisms we have inherited in the present, and it is this set of mechanisms that regulates our behaviour – not selection directly (ibid: 54).

Tooby and Cosmides conjoin the propositions that minds are adaptations, that adaptations are modular, and that adaptations are our collective inheritance from the past. They then show how evolutionary psychology distinguishes itself from what they call Darwinian Social Science: *viz.*, " humans as fitness maximisers (fitness-teleology) versus adaptation-executors (adaptationism)". Here is the example they use to illustrate their point:

> Fitness teleologists may observe a situation and ask something like, "How is Susan increasing her fitness by salting her eggs?" An adaptionist would ask, instead, "What is the nature of the evolved human salt preference mechanisms – if any – that are generating the observed behaviour and how did the structure of these mechanisms mesh with the physiological requirements for salt and the opportunities to procure salt in the Pleistocene?" (ibid).

Tooby and Cosmides place in the very front row of evolutionary psychology the idea that it has and uses knowledge of the past. Moreover this knowledge is very specific and it is of the very distant past of (to use Foley's phrase) "humans before humanity". The conceptual role of that human past – the Environment of Evolutionary Adaptedness – is to generate hypotheses. And these hypotheses, if confirmed, are then taken to be statements about the component parts of human mental functioning. Our next task is to show how the concept of the EEA is used in practice.

5.3 The EEA in use

In this section we will look at ways in which a prominent evolutionary social psychologist appeals to the past and how the concept of EEA is used in practice. The examples given pertain to what is, arguably, amongst the most important aspects of social psychology – important in that these aspects of behaviour have traditionally been of considerable interest - sex and mating.

Before we look at the EEA in use, I want first to explicate the method by which one may determine whether or not the concept of the EEA is, or has been, invoked. To see if any given hypothesis or any

account of any given psychological or behaviour data invokes the EEA (or a functional equivalent), all that is required is to ask the question: does the hypothesis or account make an assumption(s) about a non-extant organism and/or the environment to which it was adapted and/or needed to adapt to? In practice, the organism in question is typically a hominid and the environment is the group in which that hominid functioned. Where one finds such an assumption one finds an appeal to the EEA.

As an instance, imagine any evolutionary account of language. Because it is widely accepted that chimpanzees do not possess language as it is possessed by modern humans (Brown, 1986), and because australopithecines appear to have had a brain akin to that of chimpanzees (in terms of size and endocasts), the advent of language occurred by virtue of the features of the EEA. As a general rule, then, take it that the EEA is being invoked whenever a behaviour or trait not evident in the known repertoire of extant great apes is supposed and/or whenever a selection pressure that is not known to have exerted itself on the great apes is supposed. (Should one wish to be narrower, we may replace the category "great apes" with African apes.)

To reinforce the point, we can look at it this way: the EEA is not being invoked when some appeal is being made to a general evolutionary principle such as parental investment. Any period in natural history may be invoked to support ideas to the end that humans are, in some sense, governed by parental investment. And any sexually reproducing species will offer a model for comparison. The EEA is invoked when one wishes to say something particular, species-specific and/or build a model of the adaptation(s) that govern parental investment in modern humans.

Having clarified a rule-of-thumb which will tell us when the EEA is being invoked we can now look at some examples. Those I give are taken from the work of David Buss. Buss is probably the foremost and most prolific evolutionary social psychologist researching sex and mate choice. Here I will focus on his contribution to *The Adapted Mind*. This choice enjoys the benefit of being based on one of the most extensive studies conducted into mate preferences. (Buss, 1989a; Buss *et al*, 1990). The work has also been influential and very widely read. First published in *Behavioural and Brain Sciences* in 1989, at the time of writing it has been cited by other authors in science and social science journals 319 times (Web of Science citation search, 24/05/01). The examples I give show how the EEA is invoked, what it is supposed to have consisted of, and what sorts of claim it supports. In the chapter *Mate Preference Mechanisms: Consequences for Partner Choice and Intrasexual Competition* (1992a) Buss assumes that,

- Hominid females lived well beyond menopause and that males with a preference for mating with "older" women would have been subject to negative selection (ibid: 250).

- Hominid males were able to understand and transmit information regarding the "reputation" of females, "i.e. knowledge gleaned from others regarding the age, health, condition, appearance, behaviour, and prior sexual conduct of a female" (Buss, 1992a: 250). Additionally, "Observed cues or reputation suggesting that a female has diverted (or might divert) [reproductive value and potential] to another male should be disfavoured; those suggesting fidelity should be favoured. One cue that appears to be afforded by reputation is chastity – the lack of prior experience in sexual intercourse. Still another consideration is nurturance – which could provide a cue to good mothering skills. . . . To the degree that prior sexual experience by males provides this clue [to promiscuity] females also should value chastity in a potential mate" (ibid: 250-1). The clear implication is that it is known that something like monogamy or at least serial monogamy was the norm in hominid populations.
- Hominid males potential r_{max} value was enhanced by virtue of their "capacity to sire offspring across the lifespan [and] into the fifties, sixties etc." (ibid: 250).
- Hominid females both preferred resource-holding males and were capable of judging whether a potential mate would acquire resources by virtue of "ambition, industriousness and intelligence" (ibid: 251).
- What is frequently referred to as "Machiavellian Intelligence" (Byrne & Whiten, 1988) – tactical deception in chimpanzees and some other primates – had developed in the EEA such that hominid females would "lie about their age, alter their appearance, and conceal prior sexual encounters". Hominid males, on the other hand, would "exaggerate their resource holdings, inflate perceptions of their willingness to commit, and feign love to induce a female to mate with them" (Buss, 1992a: 252). This ability (and, indeed, need) to deceive is allied to the ability in hominids of both sexes to adopt and successfully deploy either long or short term mating strategies according to some (implicitly or explicitly) perceived benefit.
- That "tactics" employed by hominids in the EEA included "deception and dissembling", bragging, the alteration and improvement of appearance, emotional manipulation and derogation of competitors via the manipulation of their reputation (ibid: 251-252).
- Additionally, in keeping with the appellation of a capacity to construct a theory of mind sufficiently complex to deceive a conspecific about one's age or emotional attachment, hominids are invested with the capacity to accurately judge one another's intelligence. Amongst other things, hominid intelligence is said to comprise of "understanding . . . capacity to cope with changing circumstances . . . a host of social skills involved in negotiating complex interpersonal

arrangements, intricate constellations of allies and foes and extended kin networks" (ibid: 255-6).

These assumptions are about the past, and they are presented as facts, or quasi-facts of the natural history of modern humans. As facts about the past Buss uses them to propose further facts about the present. Let us briefly look at two other examples from other evolutionary social psychologists of how the EEA is used in mating studies. The assumptions each makes are, I think, apparent.

> In contrast to environments encountered by some humans today, that involve relatively low levels of parental investment (e.g., father-absent societies), the stable pattern for humans over hundreds of thousands of years is likely to have been one that included maternal and paternal involvement with offspring and emotionally intimate relations between mates. . . . Because Pleistocene mothers were apt to have spent much of their time gathering (to provide the primary food source for the family), fathers - perhaps even more so than today - were apt to have been heavily involved with their offspring for a large percentage of time each day. (Miller & Fishkin, 1997: 199).

> Because of the small size of groups, the nomadism, the lack of alternatives to mothers' milk for nourishing young, the mother and her mate would have been the primary target of succorance requests by the child. This kind of child played a major and active role in sustaining a high level of parental investment. Even though other members could assist the parents, it would be unlikely that a system of substantial surrogate caretaking would develop (Drapter & Harpending, 1988: 358).

Bear in mind that Tooby and Cosmides claim that for each supposed problem there is in the modern human mind an adaptation. Following the adapted mind view, Miller and Fishkin, and Drapter and Harpending are arguing that these adaptations are fixed mechanisms in the human mind. The situations in which these adaptations are embedded constitute the natural environment of humans. The EEA, as depicted, is the inescapable and unalterable condition to which humans are attuned. It has fixed our mental functions just as surely as it has fixed the properties of our hearts. Let us now follow Tooby and Cosmides' advice and see if these assumptions are consistent with those disciplines that concern themselves with the natural history of the human lineage. In the next chapter we will look at the palaeoanthropological and archaeological evidence of human evolution.

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[1] I leave it the reader to think of how very much more convenient a place the world would be should "Wild horses . . . etc."

Chapter 6 Empirical problems in trying to specify the EEA

Summary of chapter 6

This chapter aims to explicate more fully than has hitherto been the case the difficulties of recovering the details of the EEA. Specifically: the evidence which suggests that *Homos* prior to *Homo sapien sapien* were not hunter-gatherers in the same sense as are modern humans and neurological differences are considered; the problem of discerning which of the several hominid species issued *Homo sapien sapien* and a distinction between physical and social forces of selection is discussed; and the tendency to take as natural history the constituent propositions of evolutionary theory is criticised. The general conclusion is that Tooby and Cosmides underestimate the difficulty of ascertaining the natural history that their program demands and evolutionary psychologists overlook the fossil record.

6.1 Introduction

The problem of reconstructing the past has been indicated by a number of authors (e.g. Corballis & Lea, 2000; Gould, 1991, 1997, 2000; Rose, H, 2000; Sober, 1984, 1988), but they have not been fully examined with the Tooby and Cosmides and the EEA in mind. The reason for this appears to be due to ". . . the belief that late twentieth-century people can know the human psychological architecture of our early ancestors with any degree of certainty and accuracy is difficult to take seriously" (Rose, H. 2000: 118). Accordingly, " . . . many social scientists looking at the construction of the family produced by EP [evolutionary psychology] see them as embarrassingly like the Flintstones" (ibid). Still, as Rose points out, there are disciplines that seek to discover and describe the pre-history of our species. It is to the findings of these projects that we turn to too see if the EEA as used is supported by the evidence. In order to support the criticisms that comprise the next three sections we can take licence from Tooby and Cosmides. This is granted by their plea that the psychological and behavioural sciences be integrated. A brief exploration of the aspect of their Integrated Causal Model that Tooby & Cosmides call "conceptual integration" is necessary (Cosmides, Tooby & Barkow, 1992; Tooby & Cosmides, 1992). Recall that conceptual integration differs from vertical integration in that the former insists on no epistemic privileges for one discipline over another.

> Conceptual integration has been such a powerful force in the natural sciences not only because it allows some scientists to winnow out improbable hypotheses or build aesthetically pleasing bridges between disciplines, but because it has been crucial to the discovery of new knowledge . . .
>
> Evidence about cultural variation can help cognitive scientists decide between competing models of universal cognitive processes; evidence about the structure of memory and attention can help cultural anthropologists understand why some myths and ideas spread quickly and others do not; evidence

> from evolutionary biology can help social psychologists generate new hypotheses about the design features of the information processing mechanism that govern social behaviour; evidence about cognitive adaptations can tell evolutionary biologists something about the selection pressures that were present during hominid evolution; evidence from palaeoanthropology and hunter-gatherer studies can tell developmental psychologists what kind of environment our developmental mechanism were designed to operate in; and so on.
>
> . . . by calling for conceptual integration in the behavioural and social sciences we are neither calling for reductionism nor for the conquest and assimilation of one field by another. Theories of selection are not theories of psychology; they are theories about some of the causal forces that produced our psychology. And theories of psychology are not theories of culture; they are theories about some of the causal mechanism that shape cultural forms. In fact, not only do the principles of one field not reduce to those of another, but by tracing the relationships between fields, additional principles often appear (Cosmides, Tooby & Barkow, 1992:11-12).

Now, there are a number of noteworthy implications in this passage. However, it is the general tenor to which I wish to draw attention. Tooby and Cosmides' attitude toward the history of the social sciences notwithstanding, there is a clear suggestion that disciplines enjoy no privileges over one another; they sit in no "epistemological or status hierarchy". Tooby and Cosmides invite us to examine the evidence produced and found by palaeoanthropology and archaeology. Moreover, their case about the utility of the past and its power to explain the present rests upon what it is that the past actually tells us. We can begin by seeing if evolutionary psychology's most common model of humans withstands scrutiny. I have in mind here the supposed synonymy of the EEA and hunter-gatherers.

6.2 Hunter-gatherers as the EEA

Treating the EEA as synonymous with the Pleistocene, or the ancestral environment as synonymous with Pliocene and Pleistocene, is a means of bracketing a set of problems. A common shorthand is used for the set of problems that amounted to the Pleistocene. It is "hunting and gathering". More precisely, it is common for evolutionary psychologists to précis the archaeological and palaeontological records into an EEA that is identical to the suite of problems faced and solved by extant hunter-gatherers (Tooby & Cosmides, 1992; Cosmides & Tooby, 1992; Wilson & Daly, 1992). *Viz.*,

> If our goal is to understand evolved human propensities . . . It seems reasonable to seek out those humans today who experienced a social environment most similar to that of our Pleistocene

> ancestors: Such humans would be most likely to manifest behaviour that provides a window on our human evolutionary heritage. (Miller & Fishkin, 1997: 218).

Turning the EEA into a set of problems more or less equivalent to those faced by extant and documented hunter-gatherers circumvents the profound difficulty of reconstructing the past. Thus, the rejoinder to the claim that the EEA is unknowable is that we do not need to recreate it because it is exhibited in the present (and an extensively recorded recent past). On this account the means of subsistence deployed by earlier forms of *Homo*, through to the appearance of *Homo sapien sapien* *c.*100 000 years ago and up to the advent of agriculture 10-15 000 years ago was a blend of hunting and gathering. Thus Cosmides, Tooby and Barkow state:

> . . . our ancestors spent the last two million years as Pleistocene hunter-gatherers, and, of course, several hundred million years before that as one kind of forager or another. . . . The few thousand years since the scattered appearance of agriculture is only a small stretch in evolutionary terms, less that 1% of the two million years our ancestors spent as Pleistocene hunter-gatherers (Cosmides, Tooby & Barkow, 1992: 5)[1].

Accordingly, that specific form of living, that set of problems, is indelibly stamped on a mind adapted to solve them. Taken as a whole, then, the claim is that the modern mind is, essentially, a hunter-gatherer machine by virtue of the fact that it evolved to guide its possessor through that particular form of subsistence. In addition, the forms of social life that are either conducive to, or are a product of (or some combination of the two), hunting and gathering also create social problems and needs that the mind is adapted to negotiate.

This view is taken from anthropology. The "Man the Hunter" hypothesis appears to have reached its apogee following publication of Lee and DeVore's edited volume of the same name in 1968. Subsequently Glynn Isaac, amongst others, promulgated this view (e.g. Isaac, 1977, 1978). In Foley's (1988) view, Man the Hunter became, in substantial part, an "axiom" of hominid evolution, engendering a homologous model of the taxa. Lewis Binford (1985) uses the term "consensus view".

For evolutionary psychology the difficulty of this position is two-fold. On the one hand, the consensus view of the late sixties and seventies has crumbled because there is no firm evidence to the end that hominids other than *Homo sapien sapien* were hunter-gatherers in the sense that *Homo sapien sapien* are hunter-gatherers. On the other hand, it may be that typing modern humans as hunter-gatherers is unjustified. Explicating the reasons for this doubt necessitates a furnishing of some more information from the palaeoanthropological record.

6.2.1 Hunting and gathering, but not as we know it

Let us assume that conceptual integration, as expressed by Tooby & Cosmides, is sincerely held – and we may in this instance take it to be sincere given that anthropologists are cited in support of the *Homo* the hunter-gatherer view. In this case we would expect Tooby and Cosmides to obey their own dictum and accept that a best or definitive characterisation of "hunter-gatherer" is not to be found in the psychological sciences. This is a reasonable position. The term "hunter-gatherer" rarely appears in dictionaries or encyclopaedias of psychology, it rarely appears in the indices of textbooks and it was not a currency of psychological discourse prior to the advent of evolutionary psychology (just as claims for the originality and newness of evolutionary psychology would suggest). Under these conditions the following argument may be developed.

- Anthropology furnishes the most complete description for the term "hunter-gatherer"
- Evolutionary psychology defers to the furnished term
- Anthropology holds to the view that *Homo*'s prior to *Homo sapien sapien* were not hunter-gatherers in the same sense that *Homo sapien sapien* is taken to be.
- Therefore, what we see as extant hunter-gatherer behaviour and the hunter-gatherer "form of life" does not furnish evolutionary psychology with an EEA.

The key premise in this argument is the third: anthropology holds the view that *Homos* prior to *Homo sapien sapien* were not hunter-gatherers in the same sense that *Homo sapien sapien* is taken to be. Let us examine some of the reasons to doubt that hominids in general and, more particularly, *Homos* prior to *Homo sapien sapien* were or even could have been hunter-gatherers in the modern sense.

- *Homos* prior to *Homo sapien sapien* are speciated according to morphological differences, most notably brain size and organisation as indicated by cranial shape and capacity (Stringer, 1992). These differences are taken to imply behavioural differences not stasis (Mellars, 1990).
- The earliest "archaic" (i.e. African *Homo heidelbergensis*) male forms, whilst more or less modern in biomechanical terms, were more robust than modern forms. Their bones were heavier and they had greater muscle mass (Wood, 1992). These differences imply a different diet to that required by *Homo sapien sapien* (Aiello, 1992). Moreover, the evidence suggests that the sexes were more dimorphic. If anything, this implies that the division of subsistence labour between the sexes was more acute than seen in modern forms (Foley, 1988).

- The evidence suggests that *Homo*'s prior to *Homo sapien sapien* were not central place foragers (Binford, 1985). This suggests that they did not share food in anything like the same way as do *Homo sapien sapien* hunter-gatherers.
- The brain size of pre "archaic" forms (i.e. *Homo erectus*) suggests that females neither needed or had the same amount and sort of fat and protein to support foetal brain development during gestation (Aiello, 1992).
- Although it is accepted that the hominid line as a whole is distinguished by its bipedalism, the pelvic anatomy of all but the immediate precursors to *Homo sapien sapien* suggests that they did not enjoy mobility or speed to match our own (Day, 1985). This has implications for what was hunted, how it was hunted, and why.
- If tool manufacture and use is taken to indicate something about diet – as it customarily is (e.g. Isaacs, 1972) – then the evidence suggests that modern hunters-gatherers do not subsist in a comparable manner even to immediate predecessors. The archaeological record suggests two sorts of marked difference. The tool technology of moderns shows variety across geographical space within regions compared to similarity across the continents of Asia, Africa and Europe for other *Homos*. And the tool technology of moderns shows repeated innovations as opposed to stasis of design over tens and hundreds of thousands of years (Mellars, 1990). Even as presented by a prominent advocate of evolutionary psychology, the sophistication of *Homo sapien sapien* tool manufacture and use has no predecessor (Pinker, 1997:199)
- There appear to have been multiple radiation's out of Africa into Europe and Asia following what is taken to be the first radiation of *erectus c.*1 million years ago (Foley, 1987a). These radiation's are notable in two senses. Firstly, most were doomed (Tattersall, 2000). Secondly, those that enjoyed prolonged success restricted themselves to temperate, tropical and sub-tropical climates in the Old World continents (Foley, 1987a). The implication is that pre-moderns *Homos* were far more habitat specific than *Homo sapien sapien.* Turner (1984) suggests that this tie to certain habitats is in keeping with large carnivorous mammals in general. It is not consistent with the range of environs and climates *Homo sapien sapien* and extant hunter-gatherers are known to successfully occupy.
- There is reason to suppose that a number of *Homo* lineages were extant at the same time (Tattersall, 2000). Further to co-existence (and in the case of *neanderthalis* and *Homo sapien sapien* this is taken to be almost certain, see Trinkaus & Shipman, 1993; Klein, 1996), there is the possibility of competitive replacement. (Competitive replacement simply refers to

competition between *Homo* species over the same resources.) There are two implications of this. The first is that two distinct forms of subsistence were in operation (ibid). And the second is that these differing modes of subsistence were the result of distinct selection pressures.

The various forgoing considerations have led to the collapse of the orthodoxy of "Man the Hunter" (Foley, 1988; Binford, 1985). Robert Foley maintains that whilst it probable that early hominids did hunt, their diet was not heavily dependent on meat. On balance, Foley concludes that it is " . . . untrue to say that early hominids were full hunter-gatherers in the same way as modern hunter-gatherers" (Foley, 1988: 211). He goes on,

> Overall, there is a declining willingness among palaeoanthropologists to accept the existence of modern forms of subsistence behaviour among anatomically pre-modern humans. It seems quite probable that the earlier hominids did eat meat, but that this was not integrated into a central place foraging and food-sharing system as found among modern hunter-gatherers (ibid: 212).
>
> Despite being omnivorous there is no reason to assume that their foraging behaviour was of the same level of organisation as modern hunter-gatherers in terms of planning depth, scheduling, subsistence activity and foraging flexibility. In the absence of clear-cut evidence for central place foraging similar to that of modern hunter-gatherers, inferences about the social and sharing behaviour of early hominids must be tentative only. In other words, if the term hunter gatherer is to mean more than just wild resource omnivory (in which case it would include baboons, chimpanzees and many other animals!), then early hominids were neither human nor hunter-gatherers, (ibid: 215).

In light of these considerations from "an adjacent discipline", and in this case the one to which we must turn to for advice about the EEA (Cosmides & Tooby, 1989: 40), we must conclude that the casual assumption that the EEA or the Pleistocene is in some sense equivalent to hunting and gathering is unwarranted. Accordingly, Foley suggests that we revise the claim that

> Cultural man has been on earth for some 2 000 000 years; for over 99 percent of this period his has lived as a hunter-gatherer . . . Of the estimated 80 billion men who have ever lived out a life-span on earth, over 90 per cent have lived as hunter gatherers" (Lee & De Vore, 1968a: 3. Cited in Foley, 1988: 208).

Should we revise our chronological scheme subsequent to the conclusion that palaeoanthropologists are unwilling to "accept the existence of modern forms of subsistence behaviour among anatomically pre-modern humans", the hunter-gatherer foundation for evolutionary psychology that Tooby and Cosmides recommend is weakened. On the standard and conservative assumption that the genus *Homo* is *c.*2 million years old and that *Homo sapien sapien* is 100 000 years old, hunter-gathering as

we know it to be exhibited by *Homo sapien sapien* has occupied 5% of that period. Should we insist on a very close tie between tools and subsistence the period is shorter still (Mellars, 1990).

There is a cautionary tale here. One of the key reasons why the "Man the Hunter" hypothesis was doubted was due to the variety of hominids that lived during the Pleistocene. That there were several distinct *Homo* species prior to *Homo sapien sapien* encourages and tallies with the idea that they embodied marked differences in subsistence. These differences are the evidence that evolution has taken place. Accordingly, for evolutionary psychology to "air brush" these differences undermines the very evidence it alludes to for scientific support.

6.2.2 Are modern humans hunter-gatherers?

Research conducted in the name of evolutionary psychology appears to absorb the debates within archaeology and palaeoanthropology about hominid subsistence behaviour by adopting a relaxed or loose conception of hunter-gatherers. For example, if we look at the work of Buss (see above) we get the impression that hunter-gatherer means that *Homos* in the EEA lived in groups of undefined size but not in the thousands: men did the hunting and it is suggested that sharing took place but it is not said how often they hunted, or with whom, precisely, they shared: women did most of the gathering and possibly assisted one another with childcare. And so on. A relaxed treatment of the term hunter-gatherer seems to be constructed and constrained by importing only those putative forms of meat eating and acquisition provided for us by social and cultural anthropology. Unfortunately this move invites a difficulty. By weakening the definition of hunter-gatherer such that *Homos* prior to *Homo sapien sapien* may be classed as hunter-gatherers we introduce the possibility that modern humans are not best characterised as hunter-gatherers. The reasoning is as follows.

Most considerations of the cognitive capability and plasticity of *Homo* prior to *Homo sapien sapien* take into consideration their means of subsistence (e.g. Mellars, 1990; Mithen, 1996; O'Donnell, 1999). Combinations and proportions of hunting and/or scavenging and/or gathering and/or foraging are used in support of a given profile and estimate of the cognitive capabilities of a given species. The extent of the cognitive capability proposed of the given *Homo* is constrained by the means of subsistence it is known to have been capable of devising. What are taken to be quasi- or pseudo-scientific accounts aside[2], the bulk of accounts of the Pleistocene in terms of the minds of its occupants appeal to the evidence of material and social technology (Clark, 1997; Foley, 1992; Jowlett, 1992; Mellars, 1990; Potts, 1992; Rensch, 1972). Slight alterations in assumptions of what ratios of hunting, gathering, and foraging amounted to the subsistence sum of a given *Homo* radically alters the pattern of sociality and the extent of cognition that sustained it.

The supposed cognitive capability of a given *Homo* may be enhanced by the insertion of what we might call a reversion clause: a given *Homo* may have been capable of reverting to a previous form of subsistence. In this way, cognitive capacity is cumulative. The reversion clause is not matched with what we might call a prospective clause. This is to say, it is not assumed that the archaeological evidence expresses only a fraction of what the organism was in fact capable of from a cognitive point of view. Thus, a given *Homo* is not taken to be capable of, say, a pastoral existence if there is not evidence that it lived a pastoral existence. Material evidence is taken as the final arbiter. Accordingly,

> Because modern hunter-gatherers are often presumed to provide a reasonable picture of early human foragers, and living apes are closely related to modern humans, studies of these two groups might provide an insight into early hominid life. Such field studies are no doubt significant, but any more specific inferences about the behaviour and ecology of early hominids must rely on material evidence from the geological record. (Potts, 1992: 326).

A consequence of approaching cognitive capacity via the evidence permits one to argue that modern humans are not hunter-gatherers. We need only look toward population numbers together with the archaeological record. Consider the following. Although subject to variation of *c.*±40 000 years, *Homo sapien sapien* is commonly estimated to be 100 000 years old (Ardley, 1961; Corballis, 1991; Foley, 1987c, 1995c; Wood, 1997; Tattersall, 2000). Whilst a hunter-gatherer subsistence accounts for 85-90% of that time for most populations (obviously for all of the period in the case of current hunter-gatherers) it is only true to say with any certainty that modern man can hunt and gather. It seems likely the historical record will show that, 500 000 years hence, hunting and gathering will have formed 15% (or so) of our total history, and 1 million years hence it will have formed less that 8%, and so on (unless an argument can show that we will revert to a hunter-gatherer subsistence). The point to be made is that it may be premature to say that modern man is, in some essential sense, a hunter-gatherer.

In addition, on consideration of the geological and archaeological record for modern humans we find (alongside art, abstract media of exchange, symbol systems) a completely novel form of subsistence to have come about in the form of agriculture. Moreover, estimates of population growth suggest that very many more *Homo sapien sapiens* in terms of brute head-count have existed in and exhibited this form of subsistence than hunting and gathering (Rensch, 1972; Tooby & Cosmides, 1992). The same sort of argument can be proposed for industrialised populations. In other words, a concentration of hunter-gatherers and hunter-gathering as a form of subsistence because they and it exhibit the modern mind functioning in its "natural" setting is of no especial help because it is not at

all clear that modern man is a hunter-gatherer *per se.* It follows that it may be a romantic perversion to talk of any concomitant environment as natural to him.

It needs to be noted that this is not to say that *Homo sapien sapien* cannot subsist as a hunter-gatherer any more than it is to say that earlier *Homos* could not revert to means of subsistence that preceded hunting and gathering. Obviously the modern mind exhibits the facility to adopt a hunter-gatherer strategy, but it also exhibits the facility for agriculture. That facility is not something palaeoanthropologists assume to be a characteristic of *Homo* prior to *Homo sapien sapiens* so far as the archaeological record can be taken to exhibit their behavioural repertoire.

It has been pointed out that declining to declare *Homo sapien sapien* a hunter-gatherer because the future might preclude such a characterisation has some peculiar implications[3]. For example, might we say that modern humans are not really dimorphic? That is, might we be on the way to morphic convergence, thus casting doubt on what current evolutionary thinking has to say about mate choice? The best reply to this is the evidence as it stands: it is clear that modern humans are on some sort of trajectory that takes them away from hunting and gathering; it isn't clear that we are on paths of divergence from gross morphological characteristics of our hominid ancestors.

6.3 The EEA and the minds of its occupants: the plurality of our ancestors

Darwin (1871, also see Foley, 1987a) made the point that the apparent uniqueness of modern man is, in part, due to the extinction of near relatives on the phylogenetic tree. To illustrate this point let us imagine that chimpanzees and bonobos become extinct tomorrow. On the assumption that *Pan troglodytes* are our nearest relatives, our nearest extant relatives would then be gorillas. At the close of tomorrow's world we would appear to be that much more unique in comparison to the natural world available to observation today. However, in terms of natural history in the fullest sense we would be no more or less unique than has ever been the case.

Were it the case that there were no chimpanzees to study, it may be taken that the task of evolutionary psychology would be that much more difficult. This would be for two reasons. Firstly, comparative psychology would be weakened due to the widely held belief that chimpanzees are our nearest comparitor. And secondly (following Dennett, 1995), our knowledge of what is actual, and not just possible, in primate design space would be weakened. These considerations indicate why those species that are taken as transitional between *pan* and modern humans are of such importance and interest. Were we able to study them our knowledge of our own evolution and condition would be enhanced. The EEA is, in effect, a notional study of some or all of these organisms. In looking to pre-modern *Homo*'s – to the actual past (in addition to the mechanisms of evolution that brought

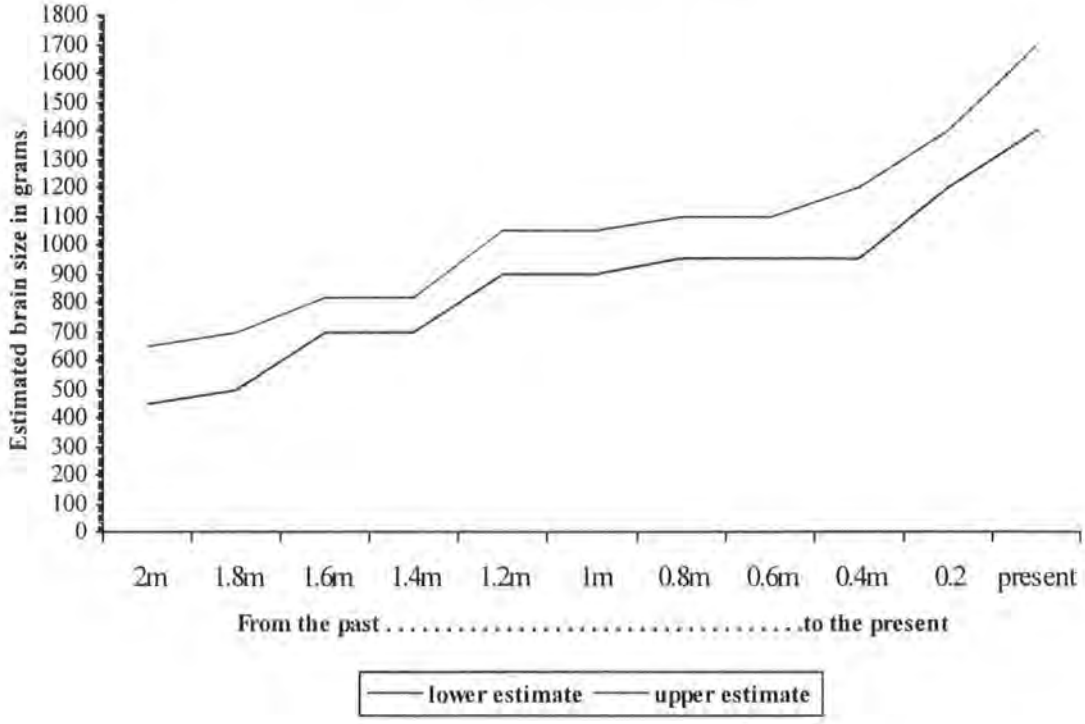
those pasts about) – the concept of the EEA is a necessary step if we wish to be species-specific in our generation of hypotheses. Species-specificity is the prime motivation behind the EEA.

However, for an evolutionary social psychology there are difficulties with this notional study. These difficulties flow from accepted facts about the organisms that constituted the EEA (taken as the Pleistocene) or, more broadly, the ancestral environment of the hominids (taken as the Pliocene and the Pleistocene). The first difficulty is that these organisms were almost certainly not in possession of a neurology like that of modern humans - the size of their brain's alone testify to this. This suggests that these organisms were not psychologically identical to modern forms. Secondly, the EEA produced and sustained a plurality of distinguishable hominids and we do not know the path of the *Homo sapien sapien* lineage through those distinct lineages that have thus far been identified. Let as deal with each of these problems in turn.

6.3.1 Minds not modern

Why might it be common for textbooks and edited volumes of biological anthropology to employ images which invite the comparison of human, hominid and great ape skulls (e.g. Clark & Willermet, 1997; Corballis & Lea, 1999; Foley, 1987; Le Gros Clark, 1954; Relethford, 1997)? It is because the

Figure 1 **Brain growth in hominids**



seat of *Homo* behaviour and uniqueness is taken to be in their minds. As we will see, there is a good deal of debate concerning how many distinct species and minds have come about and become extinct since the divergence of the hominid lineage from a species that is assumed to be something akin to an extant chimpanzee or something that may be considered to be a hybrid of an extant gorilla and a chimpanzee (see Foley, 1987c, 1995c; Ruvolo, 1997; Tattersall, 1995, 1997; Wood, 1992; Wood & Brooks, 1999). Conservative estimates are of the order of ten. More liberal estimates suggest twenty or more (Tattersall, 2000). What is agreed is that these organisms were not in possession of brains that characterise *Homo sapien sapien*. *Figure 1* offers a simplified account of brain growth in hominids over the last two million years.

Summary of *Figure 1* Brain growth in hominids

2.0 - 1.6 million years ago *Homo habilis* had a brain size of 450-750 ml

1.8 – 0.4 million years ago *Homo erectus* had a brain size of 800-1250 ml

0.4 – 0.1 million years ago (archaic) *Homo sapien* had a brain size of 1100-1350 ml

0.12 – 0.03 million years ago *Homo neanderthalensis* had a brain size of 1200-1750ml

0.13 – 0.06 *Homo sapien sapien* had a brain size of 1500-1700 ml

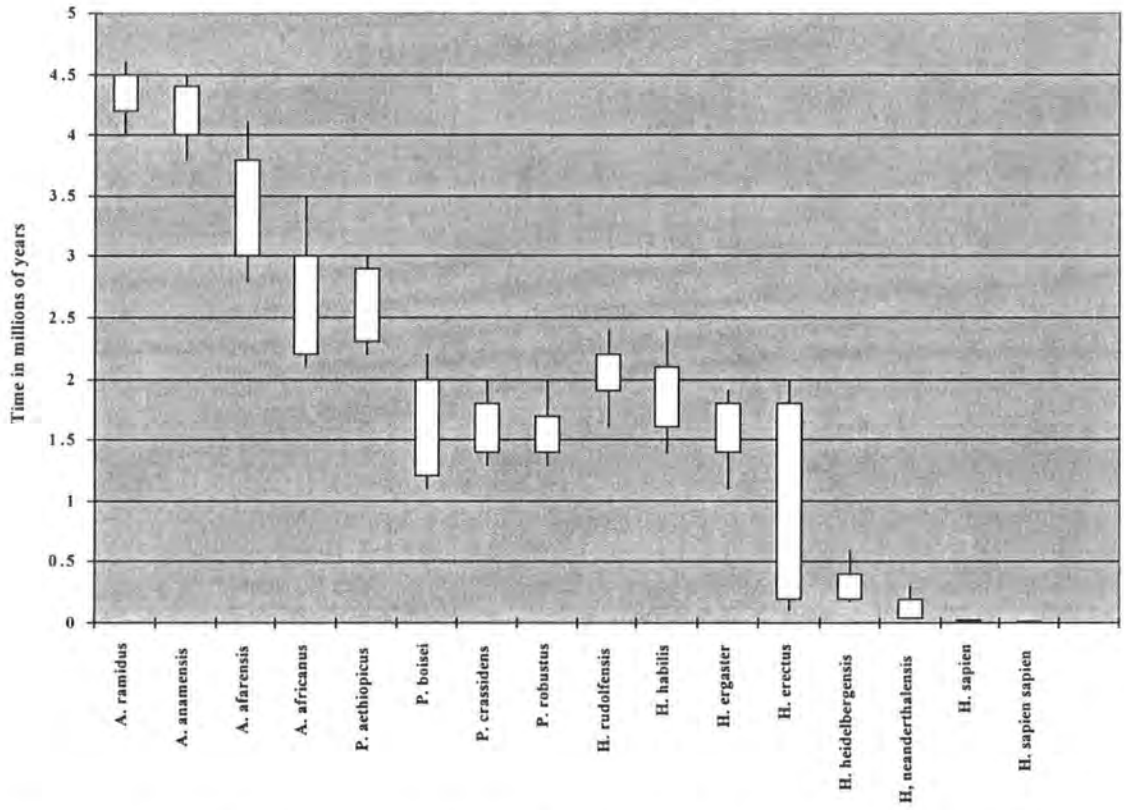
Note: The information in, and summary of, *Figure 1* is taken from Foley, 1995 and Tattersall, 1995. The graph and the written summary are simplified to the extent that they suggest as neat and progressive a development in brain size as the data permits. It could be represented quite differently, showing much sharper distinctions between the brain size of proposed species through time.

For the purposes at hand, the point to be drawn from the graph and its written summary is that we simply do not know what exactly it was that psychologically mediated the EEA. Even on the assumption that we could fully specify the physical characteristics of the EEA and the morphology of the hominids that populated it, we could not, in principle, be sure that what we would see would have the same meaning, sense or reference to us as it did for our predecessors. From any standpoint this is a problem. In Gibsonian (Gibson, 1979) terms, for example, it would not offer us the same set or type of affordances. Nor can we be confident that, although a belief-desire analysis of their behaviour might be plausible, we could stipulate the nature of their beliefs and desires. Similarly, we have no idea of their IQ or short term memory capacity.

This problem can be thought about further using Dan Sperber's distinction between the "proper domain" - the EEA, and the "actual domain" - the extant environment (Sperber, 1994). Sperber classifies the difference between the two as the "domain mismatch". This distinction, although not

deployed by Sperber as it is here, does serious harm to the potential utility of the EEA when we use it to illustrate the mismatch between the brains and minds (rather than just the time and place) that might consider the EEA as content. Nothing in the argument depends on any assumptions about a relationship between brain size and intelligence. In fact, the problem of a mental mismatch would be

Figure 2 . Hominid lineages



considerably reduced if there were a simple linear relationship between cognition, its depth, breadth, type, and brain size. However, although it is not altogether unreasonable to make some assumptions of this type (Beran, Gibson & Rumbaugh, 1999), such a relationship has not been demonstrated (Badcock, 2000; Byrne, 1999; Corballis, 1999; Deacon, 1992), and in abandoning behaviourism academic psychology, at least implicitly, acknowledged this. Whilst we can make some informed guesses about the power, flexibility and dispositions of *Homo habilis*, *erectus* and *sapien* minds, being guesses they are not especially helpful to cognitive psychology or detailed behavioural analysis, and certainly not to phenomenology or hermeneutic forms of psychology. It seems highly unlikely that we will ever have an understanding of the minds of those organisms that occupied and amounted to the EEA that we currently have of chimpanzee minds. I take this as a crippling to our chances of coming to a really precise understanding of the selection pressures behind the growth in

hominid brain size and cognitive capacities. As it turns out, whilst this is a very serious obstacle to the adaptationist program as inculcated in the adapted mind thesis of Tooby and Cosmides, it need not be fatal to the sort of evolutionary social psychology we will look at in chapter 9.

6.3.2 Which lineage?

It is time now to complicate the picture. *Figure 1* (above), in depicting brain growth in the *Homo* lineage, ignores the tree of speciation following the development of the hominid line after divergence from an unspecified African ape *c.*8-5 million years ago. *Figure 2* (taken from Foley, 1995 and Tattersall, 1995) offers a fuller though still a conservative depiction of what existed and when over the past *c.*5 million years.

There are two implications of this diversity for evolutionary psychology. The first we will pursue is the point that the lineages named are considered to be distinct species. The second concerns the fact that, in winnowing the number of hominid species down to one, evolution appears to have judged that the others were not well adapted.

Terms such as *Australopithecus afarensis* and *Australopithecus africanus*, *Homo habilis* and *Homo erectus* refer to species of hominid. Hominids as a class are those species thought to have appeared since the split with the pongoid clade. In assuming that they are species and not varieties, I am following the orthodox view of most recent palaeoanthropological literature. However, the debates concerning precisely which fossil form is and which is not a distinct species from another fossil form is vigorous (e.g. Tattersall, 1995)[4]. These debates indicate some of what is at stake when a given lineage is said to be speciated from another. The extent of hominid speciation, especially that within the Pleistocene, has consequences for evolutionary psychology and any suppositions concerning the proximate mechanisms of those organisms which constituted it. Asking the question "What is a species?" indicates what some of these consequences are.

An orthodox and generally accepted answer to this question is that offered by Mayr; "Species are groups of actually or potentially interbreeding populations, which are reproductively isolated from other such groups". To clarify this definition we can be more precise about the term "reproductively isolated". Following Butlin & Ritchie (1994), it can be consider in two ways: intrinsic isolation and extrinsic isolation, and pre- and post-copulation isolation.

Precopulation

For organisms to be classed as members of the same species they need to,

1. be present in the same locality at the same time

2. be sexually receptive at the same time

3. recognise one another as potential mates

4. stimulate one another as potential mates

5. (in the case of primates) achieve intromission

Postcopulation

It is then necessary that,

6. fertilisation is achieved

7. a healthy zygote develops

8. the offspring achieves puberty

9. the offspring is fertile

10. the offspring obtains mates via the same cycle 1 - 9

11. the cycle 1 - 10 is reiterated

Any interruption or barrier to any of the stages results in a failure to reproduce. Intrinsic barriers usually mean that the two organisms are not con-specifics ("usually" because con-specifics can be reproductively incompatible). Extrinsic barriers are usually the result of geographical and/or temporal contingencies. Intrinsic barriers present a stronger case for classifying two organisms as distinct species. A failure to achieve stage 6, for example, may be taken to mean that there are definite biological or genetic barriers. Extrinsic barriers, on the other hand, often result from a failure to achieve stage 1. Such a barrier is often the first stage in speciation.

As is orthodox, let us now assume that the hominid line descended from one great ape species. To account for subsequent speciation within that lineage we may next assume that the organisms which constituted the lineage subsequently dispersed such that geographical isolation resulted in speciation. To do so avoids having to specify any intrinsic biological factors. However, at a later point in time, when the fossil evidence suggests that certain of these lineages were temporally and spatially co-terminus, one is forced to suppose that intrinsic factors were responsible for their continued speciation. There is a clear and plausible implication of this. It is that temporally and spatially co-terminus hominids were speciated by virtue of their respective psychological make up.

To advance the point let us assume that, in the absence of the extrinsic barriers of time and place, speciation was maintained by virtue of a failure of step 7 (a healthy zygote does not develop), step 8 (offspring do not achieve puberty) or step 9 (offspring are not fertile). If speciation was maintained between, for example, *Homo rudolphensis*, *habilis*, *ergaster* and *erectus* by virtue of a failure of step 7, 8 or 9, then we could maintain that whilst attracted to one another as mates these lineages were sufficiently distinct biologically to continue as distinct species. Whilst a biological distinction sufficient to count as speciation is not a trivial matter, this does not necessarily imply that these organism were psychologically distinct

Consider now step 3 (two organisms do not recognise one another as potential mates) or step 4 (they do not stimulate one another as potential mates). These stages are classed in zoology as mating signals. Mating signals include the message, the messenger, and the recipient. Complex signalling involves a chain of such messages, receptions and responses. It is reasonable to assume that such chains were typical of hominids given their prevalence and comparative length in primates. Continuing with our example, if speciation was maintained between *rudolphensis*, *habilis*, *ergaster* and *erectus* by virtue of a failure of step 3 or 4, the implication is that a psychological difference amounting to a speciation barrier existed between Pleistocene hominids. These psychological barriers could have prevented reproduction even if these lineages were quite capable of achieving stages 6 to 10 (Butlin & Ritchie, 1994).

The fossil evidence promotes the assumption that *Homo* lineages were divided by psychological factors. Whilst it may be an error to read precise claims about intelligence and cognitive capacity into empty skulls, crania from different lineages are themselves sufficiently different to support such a view. The difficulty that this presents for evolutionary psychology comes from the fact that palaeoarcheology does not present a unified view as to which of the later *Homos* is the direct ancestor of *Homo sapien sapien*. This too has implications. There is the danger that we might pick the wrong ancestor and in doing so pick the wrong set of psychological adaptations – a composite picture would neither capture the truth nor permit precision. But there is also the danger that we pick a species that has demonstrated, by virtue of it being extinct, that it was not, ultimately, well adapted. We could specify a suite of adaptations that, together, could not and would not have rendered the species even semi-functional in extant environments[5,6]. The Pleistocene, if you will, killed most of the decedents of our nearest ancestor, the chimpanzee. The natural history of the hominids is not one of seamless adaptationism.

6.4 The physical and the social EEA

The variety of *Homo* lineages promotes a distinction between two construals of the EEA, the physical and the social. This follows from the fact that similar environments produced distinct outcomes: if climate, fauna, flora, as selection pressures were at times and places similar then speciation must have been influenced by other features of the world that *Homo* lineages occupied. The dominant proposal is that the other features were social. This raises the question as to whether the important problems of the past are tractable. Let us look further at the idea that modern brains are a result of social selection pressures.

The natural organ that is the modern human brain has come about with relative speed in evolutionary terms (Corballis, 1999). Moreover, its size and physical organisation distinguishes it markedly from all but the very late *Homos* (Beran, Gibson & Rumbaugh, 1999). This aspect of human anatomy is the single most significant fact of our evolution from the point of view of psychological and behavioural science. In light of this importance, and in consideration of the finely balanced debates about other aspects of our anatomy, it may seem surprising that there is something nearing a consensus on the view that the human brain is a product of the degree and manner of our sociality (Baron-Cohen, 1996; Buss, 1995, 1997, 1998; Byrne, 1999; Corballis, 1991; Dunbar, 1988, 1997; Haslam, 1997; Humphrey 1976, 1983; Jolly, 1966; Plotkin, 1995; Whiten, 1999; Whiten & Byrne, 1988)[7]. The general theme of the argument is that the need to negotiate larger social groupings of "Machiavellian"[8, 9] others is the single most significant selection pressure toward elaboration and enlargement of the neo-cortex. Pinker displays the rudiments of the reasoning thus:

> There's only so much brain power you need to subdue a plant or a rock, the argument goes, but the other guy is about as smart as you are and may use that intelligence against your interests. You had better think about what he is thinking about what you are thinking he is thinking. As far as brainpower goes, there's no end to keeping up with the Joneses (Pinker, 1997: 193).

To see why sociality can have a particularly strong influence we need to keep in mind the fact that there is no evolution – there is no change – if there is no selection pressure (less genetic drift). Static environments render stasis in a species. Additionally, not all selection pressures are equal; that is, the size of their effects are distinct; and not all selection pressures are equally pertinent on the assumption that what one wishes to explain is precise. In the case of sociality as a selection pressure in the *Homo* line, there are grounds to suppose a runaway effect when one complex adaptation is pitted against another and when both are (all things being equal) examples of the most sophisticated "pieces" of Darwinian engineering/computing known. Such forms of competition and the

consequences are referred to in much of the literature as "evolutionary arms races" (e.g. Sigmund, 1993).

If this view is correct, the nature of the arms race could have centred on a number of different problems engendered by increased sociality. Suggestions have included the need to construct adequate theories of others intentions (Baron-Cohen, 1996), the need to communicate (Pinker & Bloom, 1992), and the various benefits that flow from successful tactical deception (Byrne & Whiten, 1987). Either way, encephalisation due to sociality means that the selection pressures that amounted to the EEA were psychological and exerted by con-specifics. The clear implication is that the selection pressures responsible for human brains are social in character. The difficulties of reconstructing the physical past are readily apparent. To build an evolutionary psychology that generates hypotheses via the engine of the EEA - in other words, an adaptationist psychology - we must go beyond that task and specify social selection pressures. We must reconstruct the social past. What is the extent of the task facing those who wish to produce evidence to show that a given social pressure exerted itself over a period sufficient to come to fixation in *Homo*? Consider first the following passage.

> They have set out early, this band of six purposeful individuals, striding across rolling, grassy terrain punctuated here and there by flat-topped acacia trees. The sky hovered between gray and pink as the sun rose close to breaking the line of hills in the east, on the other side of the vast lake. . . Everyone had heard the sabre-toothed cats during the night, repeated choruses of throaty moans, a sure sign of a hunt in progress. Even though the band felt itself relatively safe at its riverside camp a mile from the lake, there was always tension when sabre-toothed cats were near. Only a year ago a child had been attacked when he strayed from the watchful eyes of his mother and her companions. Returning hunters, the same group of men who were setting out this day, arrived just in time to drive the predator away. But the boy had died some days later from the loss of blood and the kind of rampant infection that can be so deadly in the tropics. Not surprisingly, this morning's discussions urged extra care on the women and their offspring, gathering tubers and nuts near the camp, and the men on their hunt. These men too were predators. (Leakey & Lewin, 1992: 3-4).

As the language of the piece suggests, this is an entirely imaginary and fictitious scene. The authors accept this. It is a work of scientific fiction. As a prelude to a volume concerned with biological anthropology and palaeoanthropology, it is also an expression of the goal of those disciplines. In its detail, the passage represents the kind of story that palaeoanthropology seeks to establish as facts about the Pleistocene. It may be read as a sort of wish-list and it betrays what is not yet empirically established. The assumptions made in the passage are the goals of palaeoanthropology. Let us look more closely at what the assumptions are:

- It is known that the hunting party took place in a specific place, at a specific time by specific hominids;
- The relationship toward specific neighbouring organisms is known, encompassing predators, prey and their respective behaviours;
- The home-range is known;
- The nature of intra-sexual competition is known between both males and females, and the nature of inter-sexual competition is known;
- The nature of parental investment is known, together with kin and reciprocal strategies toward that end;
- The development path of children is known, including sex differences;
- The length of puberty for both sexes is known;
- The occurrence and length of menopause is known;
- The size and constitution of the group is known;
- These organisms had an aesthetic sensibility;
- These organisms had language and used it in known ways to known ends.

All of these remain objectives for biological anthropology. Some are closer to being realised than others. For example, time, place, the anatomy of the hominids and the fauna and flora around them is fairly well established. But others are open to a wide range of speculations. Moreover, whilst they remain objectives there is, at present, no agreed means of establishing which hypotheses are best supported (Clark, 1997; Clark & Willermet, 1997; Leakey & Lewin, 1992).

If we are unable to say with any certainty how large hominid groups were and how they were constituted demographically and economically, how are we supposed to say with any degree of specificity what sort of group living problems these organisms faced? The questions repeat themselves for every claim in the list that refers specifically to the social dispositions, proclivities and behaviour of these organisms. As Tooby and Cosmides suggest, if we cannot specify the problem we cannot specify the algorithm that solves it. It is legitimate to insist that evolutionary psychology be in possession of evidence as to what the problems were before it asks us to accept

evidence for solutions. It is, I think, also legitimate to wonder whether the precise formulation of the social problems that characterised the EEA is, in actuality, an empirically tractable exercise.

Unfortunately, the sociality account of encephalisation presents a further difficulty. The rapid growth in hominid brain size does not appear to have abated through the two million years since the emergence of the *Homo* line, and it appears to have accelerated in the past 500 000 years. This creates a problem regarding the point at which one "stops" the evolution of the *Homo* lineage and establishes the stability required to document problems and their resultant adaptations. It creates difficulties in setting the parameters of the EEA. To flesh this point out, consider the following.

As the information presented in section 3, 'The EEA and the minds of its occupants', suggests, the last two million years have featured a degree of change in the *Homo* lineage which is atypical of large mammals in general. This is a fact of the current palaeoarcheological record[10]. For instance, there is no evidence that other primates have undergone anything like a comparable amount of neuroanatomical change in the same period. Indeed, early palaeontology used the stasis of great ape anatomy as the benchmark of hominid change (Falk, 1983). Although there are disputes concerning the gradualism and optimality of evolution, Tooby and Cosmides' version of evolutionary theory used to inform evolutionary psychology is essentially adaptionist (Buss, Haselton, Shackelford, Bleske & Wakefield, 1998; Symonds, 1992; Tooby & Cosmides, 1992). In short, this amounts to the orthodox view of step-wise, steady change devoid of the "hopeful monsters" and grand extinctions said to be implied by punctuated equilibrium, the main alternative (see Gould and Lewontin, 1979; Dennett, 1995). The EEA, then, is that period wherein *Homo sapien sapien* gradually emerged from the genus *Homo* by virtue of gradual changes in the characteristics of the ecological and social environment.

Changes in human genotypes are brought about by genetic recombination via sexual reproduction and genetic mutation. It is widely argued (e.g. Ridley, 1993; Williams, 1975) that sex has succeeded because it guarantees change: genetic recombination sees to it that any given organism will not be identical to any that preceded it (in the case of monozygotic twins, that any given pair will not be identical to those that preceded them). Sex allows a species to escape the vicissitudes of change through mutation alone. Whilst sexual recombination allows for the evolution of the species, we cannot be certain that it has dictated the shape and speed of change in the *Homo* lineage. We also need to look for instabilities (though not chronic instability such that the lineage becomes extinct by virtue of the constraint on the speed of phenotypic adaptation determined by sex and mutation) in the environment. The sociality account of encephalisation suggests that pressures created by this

instability were exerted by con-specifics. So, the difficulty presents itself; how do we specify the EEA in terms of variable and changing forces that brought about *Homo sapien sapien*?

As I hope to have shown, we do not know of what the social EEA consisted even when construed as static. The problem of saying what the social environment was for, say, *Homo erectus*, is acute and central. This problem is compounded given that the environment(s) in question were not static. This makes it difficult to specify what counted in, or was typical of, this changing situation. Because it is the nature and reasons for social change that we need to understand, even if a time slice (including all details of the physical environment, the brains that mediated that environment and the meaning of the organisms had for one another) could be specified it would not generate a reliable account of extant psychology. The point is that an evolutionary social psychology in which the EEA is central is probably not a tractable enterprise.

6.5 The EEA and ARE

By typing the EEA as synonymous with the Pleistocene (or the Plio-Pleistocene) and restricting the organism in question to hominids, I am following the practice of evolutionary psychologists in being quite specific in temporal and spatial terms. As we have seen, this practise appears to follow Tooby and Cosmides who, along with others, repeatedly use the term Pleistocene alongside hunter-gatherer to denote the EEA (e.g. Buss, 1994; Crawford, 1998b; Sherman & Reeve, 1997; Irons, 1998). Despite this practise, Tooby and Cosmides have also stressed that the supposition of a singular unified EEA is an error. An alternative term for the needed "ancestral conditions" proposed by William Irons (1998) is the "Adaptively Relevant Environment" (ARE). The concept of ARE says that, for any given adaptation, there is specific set of problems. Each adaptation has, if you will, its own particular EEA and any given EEA may or may not be co-terminus with any other EEA. This move seems to be designed to offset the problems associated with reconstructing a particular past in all of its aspects. However, it imports at least three problems of its own. To begin with it defies the notion that, whilst modular, the mind acts in concert. Secondly, the ARE appears to weaken the prospect that we might work toward constructing a unified set of ancestral conditions. And third, it invites slippage between ultimate and proximate causes.

To suppose that we can carve the mind up into functional elements is the penultimate goal of evolutionary psychology – the ultimate goal being a computational (or machine) description of those elements. The ARE presupposes this goal to have been achieved. The EEA has the potential benefit of being open minded as to what selection pressures acted to generate modern minds. Invoking

adaptively relevant environments suggests that we already know which selection pressures are pertinent: we simply search natural history to find them.

Whilst it may be reasonable to assume that extant human minds are modular, it is not legitimate to shop around through natural history and piece this modularity together. It is not reasonable to assume that one can look in place *y* at time *x* and specify the problems that led to selection for a given mating strategy in females, for example, and quite separately look in place *w* at time *z* and specify the problems that led to selection for a given mating strategy in males. The mating strategy of females at any given time just is, in large part, the problem encountered by males, and vice versa. Similarly one cannot separate, say, the form of subsistence employed by a given hominid from, say, the precise characteristics of its reciprocal altruism. And so on. Additionally, in spreading selection pressures the ARE probably makes the task of reconstructing the past more and not less difficult.

One benefit of the EEA as confined to the Pleistocene is that it sets a temporal parameter on physical and ecological facts and forces. It gives us at least a notional opportunity to confine all of the past to a critical past. Why do I say "notional"? Well, let us assume that we are able to fully specify a period of time that we could call the EEA. When we recall why it is evolutionary psychology postulates the EEA it is because Tooby and Cosmides have successfully argued that we cannot understand the present less an understanding of that particular past. One can witness this process in operation in evolutionary psychology texts, papers and studies. For example, Wrangham and Wright (1997) advert to the behaviour of male chimpanzees to explain the nature of aggression exhibited by human males. The assumption is made that the behaviour of chimpanzees sets a certain scene for the EEA of hominids. As a strategy, the assumptions satisfies the general demand that "to understand any particular process of evolution, we need to know where it started from, what selective pressures were acting on what kind of gene pool in what kind of ecosystem – physical, biological, social, and cultural" (Lea, 1999: 21). This EEA is then fleshed out from the chimpanzee base line to provide the adaptations that have generated modern humans. The point is that a past preceding the really informative past – the EEA – is required for the construction of that past. Such a demand repeats itself for that particular past: to understand that past we would need to understand a specific past that preceded the one of interest. If "the past explains the present" (Tooby & Cosmides, 1990b) is to be adopted as general rule of evolutionary psychology, it follows that a past prior to the past of interest explains the past of interest. A consequence of this is that we would then seem to have a regression, proposing pasts to explain pasts to explain pasts. The EEA cuts this process off. It specifies a set of problems specific to the *Homo* lineage in light of the assumed condition of the species. The ARE, on the other hand, is a trap door. On the one hand there is the vanishing empirical

tractability of the series of pasts. And on the other, aside the problem of posting some sort of constraint on how many ARE's we might need to be confident that we have enough to specify each individual adaptation, there is a negative relationship between the difficulty of reconstructing pasts beyond the most immediate and their explanatory power[11]. Whilst our understanding of the organisms that preceded *Homo sapien sapien* is very much incomplete, still more so are their predecessors. For example, in the Wrangham and Wright case, where a second EEA is posed, there is the problem that we simply do not know that what we see as an extant chimpanzee existed in its current form before the advent of the australopithecines (Wood & Brookes, 1999)[12].

6.6 Ultimate and proximate causes

The ARE invites us to ignore parameters on time and species. In doing so, I am of the view that it further compounds the difficulties that evolutionary psychology already has in maintaining a distinction which, it claims, differentiates it from sociobiology and Darwinian social science. An important part of the distinction rests on that between ultimate and proximate causes. Let us first discuss the distinction between ultimate and proximate causes. Next, we can look at some examples of slippage between the two. Finally, and it is here that the central point of this section is made, we can examine how and why conflation's of ultimate and proximate causes are not considered legitimate according to the tenets of evolutionary psychology as proposed by Tooby and Cosmides.

The distinction between ultimate and proximate cause is ancient and Dewsbury (1999) has shown that it was discussed by instinct theorists in the late nineteenth and early twentieth century. However, the prevalence in biological thought of the distinction between and the concepts of ultimate and proximate cause is frequently attributed to Mayr (1961, 1974, 1983, 1994)[13].

The ultimate-proximate distinction is quite simple. "Ultimate" refers to causes that precede an organism's life. "Proximate" refers the properties of the organism that are caused by ultimate forces and are themselves causal.

Ultimate causes are the principles of natural selection. They are those principles that are taken to be universally applicable. The first of these is, of course, that self-replicating organisms will be selected for in conditions of finite resources. Indeed, so fundamental is this cause that it is the necessary condition for life of any description;

> What is life? . . . From a Darwinian perspective, it is the reproduction by systems of new and similarly reproducing systems that is the defining property of life. An organism is a self-reproducing machine. . . the logic of Darwinism would apply equally well to self-reproducing robots, self-reproducing plasma vortices in the sun, or to anything else that reproduces with the potential for heritable change (mutation) (Tooby & Cosmides, 1992: 50).

Other, and slightly more modest ultimate causes include the typical effects and constraints that flow from sexual reproduction, sexual dimorphism, the extent to which a species is *r* or K selected, the percentage of an organisms life that is pre-pubescent, the dispersal of the higher investing sex, sex ratios, and inclusive fitness. Proximate mechanisms are the final actual working effects. Proximate mechanisms seen as causes, on the other hand, are those features of an organism that, having been shaped by ultimate causes, actually produce behaviour and the outcomes upon which natural selection acts. Ultimate causes operate independently of a given species. For example, ultimate causes were responsible for generating the set of proximate causes that constituted the brain of say, *Australopithecus afarensis*. That particular configuration of proximate causes no longer exist but the ultimate causes do.

Evolutionary psychology distinguishes itself from sociobiology and Darwinian social science (Buss, 1995; Symons, 1992) by virtue of its emphasis on proximate mechanisms and the effects that they produce. Proximate causes are instantiated in evolved psychological adaptations. The emphasis on proximate causes distinguishes the adapted mind view from the adaptive mind view. It is in the form of the proximate causal role of psychological adaptations that the past is taken to be informative about the present.

6.6.1 Slippage between ultimate and proximate causes

Consider the following passage;

> Selection operated across ancestral hominid populations according to what were, in effect, systems of categorisation, screening cross-cultural variability for any recurrent relationships that were relevant to the solution of adaptive problems. To be thoroughly metaphorical, natural selection scrutinised the structure of human cultural and social environments, searching for regularities that could be used to engineer into our evolved architecture effective techniques for adaptive problem solving. Thus, the issue is: During the Pleistocene, were there any statistical and structural uniformities to human life from culture to culture and habitat to habitat, from any perspective – no matter how subtle or abstract or unobservable – that could have been used by species-typical problem-solving machinery for the adaptive regulation of behaviour and physiology? (Tooby & Cosmides, 1992: 88).

Now consider the proposed list of instances of "species typical" universal proximate causes that natural selection has produced in humans;

> Adults have children; humans have a species-typical body form; humans have characteristic emotions; humans move through a life history cued by observable body changes; humans come in two sexes; they eat food and are motivated to seek it when they lack it; humans are born and eventually die; they are related through sexual reproduction and through chains of descent; they turn their eyes towards objects

> and events that tend to be informative about adaptively consequential issues; they often compete, contend, or fight over limited social or subsistence resources; they express fear and avoidance of dangers; they preferentially associate with mates, children, and other kin; they create and maintain enduring, mutually beneficial individuated relationships with nonrelatives; they speak; they create and participate in coalitions; they desire, plan, deceive, love, gaze, envy, get ill, have sex, play, can be injured, are satiated; and on and on. (ibid: 89).

From this list it is concluded that,

> . . . human architectures are "pre-equipped" (that is, reliably develop) specialised mechanisms that "know" many things about humans, social relations, emotions and facial expressions, the meaning of situations to others, the underlying organisation of contingent social actions such as threats and exchanges, language, motivation and so on (ibid: 89).

In other words, Tooby and Cosmides want to say that the list of universals tells what proximate adaptations constitute human minds. The remainder of the task of evolutionary psychology is specify exactly how it is that these adaptation-mechanisms work. However, their list, with the possible exception of "plan" and "envy", is comprised of ultimate causes. It is nothing more than a statement of "the logic of Darwinism" that "would apply equally well to self-reproducing robots, self-reproducing plasma vortices in the sun, or to anything else that reproduces with the potential for heritable change (mutation)". It says nothing specifically about humans. Whilst it is true that "adults have children" it is also equivalent only to the claim that organisms reproduce. Similarly, we may ask how informative it is to say that humans have a species-typical body form? Or that humans come in two sexes? What would it be like if it were not the case that "humans eventually die"? And who is arguing with the claim that humans "eat food and are motivated to seek it when they lack it"? Or, that "they are related through sexual reproduction and through chains of descent"?

6.6.2 The illegitimacy of conflation

Again, recall the need for the EEA; it generates the suite of adaptations that were formed prior to their extant exhibition in modern humans. Accordingly, accepting the focus on proximate psychological mechanisms as the key features of modern humans entails the acceptance of such mechanisms as the key features of the organism that formed the EEA. Such an account needs to show not only that a suite of ultimate causes exerted pressure on ancestral hominid populations, it also needs to show precisely how these ultimate causes exerted pressure and what proximate adaptations arose as a consequence.

Let us make this point as clear as possible. On the one hand, the unifying feature and power of the very broadest of ultimate causes (i.e. natural selection of resource-consuming reproducers in conditions of finite recourses), through means of hertitablity causes (i.e. the operation of inclusive fitness on distributed units such as genes), and onto condition-specific causes (e.g. those that operate on sexually dimorphic species) is the fact that they are not species specific. Ultimate causes are not said to, nor do they, necessarily generate the same proximate outcome in all species. Although we must not violate the constraint of ultimate causes in the construction of evolutionary accounts of mind and behaviour, in and of themselves they do not generate for us a specifically human psychology. This is one of the precise and distinct points made by evolutionary psychologists in contention with sociobiology and it is why Tooby and Cosmides have highlighted the EEA. On the other hand, the unifying feature of proximate mechanisms as causes is that they are species-specific in their manifest operation. This is immediately apparent in that they need to be orchestrated toward achieving reproductive success via con-specifics. If a given spcies suite of proximate mechanisms was not causally efficacious toward the end of reproduction in some prior place/time then, by definition, that form of organisation will not be expressed in the present.

Conflation between ultimate and proximate cause comes when an example of the former is stipulated in place of the latter. The leap from saying that "humans have sex" to the assertion that humans have a sex module(s) is an example. Amongst other things, this sort of conflation misses the point that the proximate mechanisms of other con-specifics probably provided the source of the selection pressures behind the particulars of human psychology. The reiteration of ultimate causes in the stead of proximate mechanisms reveals impatience with the details of natural history. We can test for slippage between ultimate and proximate causes by asking the question; would the proposed cause operate on any sexual organism? I suggest that the ARE is an invitation to post ultimate causes as proximate mechanisms just as Tooby and Cosmides already have.

6.7 Conclusion

It is not necessary to engage in a lengthy discussion of philosophies of science before saying that the bulk of experiments and quantitative research have as their cornerstone the null hypothesis. Subscription to the null hypothesis involves an obligation. The obligation is on the data (of whatever type) to demonstrate itself as improbable courtesy of the assumption that it is the null hypothesis that has to be displaced. The null is never disproved. It is rejected in favour of the alternative, the original hypothesis[14]. Evolutionary psychologists also subscribe to this method. Buss for example, argues that evolutionary psychology adheres to "normal paradigm science" when it develops "hypotheses about

the psychological mechanisms that have evolved in humans to solve particular adaptive problems under ancestral conditions" (Buss, 1995: 4) that are then accepted or rejected according to the result of the chosen test.

Now, given that evolution by natural selection in the guise of inclusive fitness theory is not itself being tested because it is deemed to be beyond reproach by evolutionary psychologists (Buss, 1995; Tooby & Cosmides, 1990b, 1992; Symonds, 1987, 1992), we may ask what it is that is being tested. The answer is the EEA. *Prima facie*, hypotheses in evolutionary psychology test claims about psychological mechanisms. In doing so they also test assumptions about species-specific selection pressures that operated in the past. A key question raised in this chapter is whether one may make a principled choice between two alternative assumptions of this type given the manner in which evolutionary psychology treats evidence which pertains to the EEA.

Recall that we can see if any given hypothesis or any account of any given psychological or behavioural data invokes the EEA (or a functional equivalent), by asking if the hypothesis or account makes an assumption(s) about a non-extant organism and/or the environment to which it was adapted and/or the need to adapt to it?[15] I have argued that where one finds any such assumption one finds an appeal to the EEA.

It may then be asked as to what sort of assumptions about the past underlay ostensibly competing hypotheses. What evidence is there in the fossil and archaeological records to support the assumptions? Is the evidence equal to the explanatory power assigned to it? It is not inevitable that hypotheses based on the EEA must go beyond the very evidence used to justify the activity of constructing such hypotheses. It is possible to allow the evidence to constrain hypotheses. And it is, of course, possible for two differing hypotheses to be derived from the same evidence. However, I hope to have shown that evolutionary psychologists tend to have little regard for the archaeological and fossil records. Assumptions that go far beyond the available evidence are routinely made. The past is applied to in order to legitimise hypotheses and arguments but disregarded in its detail.

Probably the prime and most pervasive example of this disregard for the fossil evidence is the assumption that all *Homo* lineages were hunter-gatherers. The available evidence on hominid subsistence behaviour is taken by prominent researchers in the field of palaeoanthropology to mean that modern hunter-gatherers exemplify only themselves and that pre-modern forms of *Homo* did not behave as *Homo sapien sapien* has. The orthodox stance in biological anthropology is that they were cognitively incapable of doing so. From these consideration I have drawn the conclusion that hunter-gatherers are not especially illuminating about the past because they did not exist in the past.

The assumption that pre-modern forms of *Homo* did not posses the behavioural repertoire of modern hunter-gatherers is buttressed by the size of their brains. Evolutionary psychology routinely appeals to species that were not in possession of our neurology. It is, of course, legitimate to do this but only if one is open about the comparative nature of the work. To invest early forms of *Homo* with the behaviours that they are often invested with is illegitimate. Additionally, the various pre-modern *Homo* forms were themselves neurologically distinct from one another and there is no firm agreement as to which of these modern humans are the descendants. Failure to accommodate these issues in the depiction of the EEA sets evolutionary psychology adrift from its goal of building an accurate and detailed theory of human nature courtesy of conceptual integration with adjoining disciplines.

Aside the self-inflicted failure to be constrained by the evidence, evolutionary psychology faces the prospect of never being in possession of evidence that will show what intra- and inter-psychological selection pressures existed in the past. This is a real problem posed by evolution itself. Appeal to the theory and ultimate causes does not dissolve the emphasis on proximate mechanisms as selection pressures.

Frustration with the incompleteness of the fossil record in relation to the aim of specifying proximate psychological mechanisms may be offset by a fuller concentration on what is known about the natural history of modern humans. What there are in the way of facts about the past can tell us something about selection pressures in the Pleistocene. But the frustration is not helped by appeal to statistical composites exemplified by the ARE approach. The ARE ensures that we lose the specificity wanted by a human psychology. It does this by further muddling the distinction between ultimate and proximate causes. The ARE also involves a spurious method of building the suite of adaptations presumed to comprise the human mind because adaptations have to co-exist in an organism at every step in their development.

Still, the problems detailed in this chapter are empirical in nature. It would be as premature to say that evidence to come will not go some considerable way toward solving them as it is to assuming that they are already solved. The next chapter looks at a different class of problems. These are conceptual in nature.

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[1] It is not clear what the "several hundred million years" refers to. The orthodox estimate for the age of mammals is 60 million years and primates 35 million years.

[2] I have in mind Foley's attitude toward Elaine Morgan's Aquatic Ape hypothesis

[3] I owe this point to A. C. Campbell.

[4] Biologists adopt a variety of techniques to establish genealogical relations between species. Three of the main techniques are as follows. Cladistics: This approach is strictly genealogical. Relationships are posed on the basis of family trees where a 'family' is taken as several generations of progeny. Phenetics: This approach emphasises phenotypic similarity. Relationships are posed on the basis of gross physical characteristics. Taxonomists: This approaches attempts to balance genealogy and adaptive similarity. Relationships are posed via family trees of progeny that are similarly adaptive. Thus a behavioural element is included.

[5] We will look further at some of the issues that arise from the idea that our ancestors were not perfectly adapted even to their own environments in chapter II "Adaptations, Exaptations, Spandrels and By-products".

[6] Indeed, it may be the case that we pick the right lineage only to find it in the *Homo sapien sapien* form drifting toward its own extinction. I am assuming here that the current western trend toward something less than replacement value continues.

[7] And, albeit from a slightly different perspective, Kavanhah (1996). But see Badcock (in press) for an alternative view from the standpoint of genomic imprinting.

[8] Whiten and Byrne's two volume series *Machiavellian Intelligence I* and *II* rest on the entirely correct assumption that evolution is, from bottom to top, about competition, and this manifests itself in tactical deception in the great apes and very probably humans.

[9] The notion of Machiavellian intelligence is, in part, attractive because it does not simply type sociality, it also offers a reason for the brain growth that is such a distinctive feature of *Homo* evolution. It achieves this by stating that an evolutionary arms race has taken place. More specifically, the arms race was one between intellects. But it also to be noted that the hypothesis is generous enough to allow a quasi-Machiavellian intelligence which is not centered on those behaviours that Byrne and Whiten favour. For example, we may suppose an equally flexible, duplicitous intelligence to have focused on coalition building rather than coalition manipulation. There is a healthy banality about the rudiments of Bryne and Whiten's approach.

[10] This fact withstands the technical possibility that hominid brains could have expanded and contracted in size several time over *c.*6 million years (Pinker, 1997) for such fluctuations in selection pressures would probably have rendered a given lineage extinct.

[11] There is an analogous argument that illustrates this criticism. Suppose we were to say that no word is to be understood less an understanding of at least one other word. Thus, *w1* is dependent on *w2*. The implication that *w2* is prior to *w1* is obvious. Reapplying the principle for *w2* generates a dependence on *w3*. And so on. To my knowledge, this argument is not in fact deployed in the psychology of language or linguistics.

[12] "[the fossil record of apes] after about 8 million years ago includes scanty remains of Pleistocene gibbons and oran-utans, dental remains of a recently extinct Pleistocene giant ape (Gigantopithecus) and Pliocene fossils of uncertain affinity, all from south eastern Asia. There is no record of chimpanzees or gorillas at all. (Kelly, 1992: 224) . . . the fossil record of the gorillas and chimpanzees is a complete blank (ibid, 227)."

[13] Although the distinction is common, the terminology is not entirely fixed. For example, "Distal" is sometimes used in place of "ultimate" (Dewsbury, 1999). Mayr (1993) suggests we use "evolutionary cause" in place of "ultimate".

[14] In practice, it appears to be the case that debates in psychology (and probably in other social sciences) are not, in fact, between null and experimental/research hypotheses. The debates are between competing research hypotheses, often derived from distinct theories.

[15] As we said, the organism in question is typically a hominid and the environment is the group in which that hominid functioned.

Chapter 7 Conceptual problems created by the EEA

Summary of chapter 7

Analysis of the EEA is supposed to tell us what modern humans are adapted to and for. This chapter builds upon the last and looks at the two basic ways of viewing the past. First the proposal that we assume the past and the present are identical is considered. The difficulty with this proposal for evolutionary psychology is that it is not evolutionary. Instead it implicitly encourages us to continue with the plurality of approaches that constitute current social psychology. Second the proposal that the past and present are distinct is considered. The difficulty with this proposal for Tooby and Cosmides' adaptationist program based on the EEA is that many behaviours routinely performed by modern humans cannot be said to depend on adaptations.

7.1 Introduction

Taken as natural history, natural selection is an explanation of how novelty can occur. Any given species is, by definition, novel (Foley, 1987a; Mayr, 1996; Noble & Davidson, 1996), and if it isn't novel it isn't new. That natural selection can generate real novelty – that is, novelty of type and not just variation – is, arguably, the most fundamental and one of the most controversial of Darwin's claims in his time (Desmond & Moore, 1991).

In accounting for the existence and generation of novelty, evolutionary theory enjoys a peculiarity as a scientific theory. As John Dewey (1922) and George Herbert Mead (1934) pointed out, at any given point in time one could not predict what species would be extant at a future point in time (provided there was a sufficient lapse of time to allow, in principle, sufficient genomic change to come about)[1]. Plotkin (1997: 11-15) discusses this issue in terms of the difference between evolutionary theory and theory in "classical" physics and chemistry. He maintains that the latter are concerned with "now and forever causal explanations" and that such explanations are inappropriate and inadequate in evolutionary biology. The reason for this is because local and temporal causes generate change (effects) such that the original causes cease to be either efficacious or even extant. Thus, we may postulate the cause of a given adaptation knowing that the cause may no longer exert itself.

This chapter is largely concerned with the apparent fact that evolutionary psychology does not appear to know what do about speciation and the concept of novelty. Whilst it gestures towards an acceptance that invocations of the past beyond the emergence of *Homo sapien sapien* necessarily invokes another species (as we have seen any invocation of the Pleistocene does this), in practice it ignores the concept and implications of speciation. In the parlance of biological anthropology, Tooby and Cosmides are radical "lumpers"[2]. As indicated in the last chapter, I am of the view that the

assumption that pre-modern forms were psychologically identical to earlier forms of *Homo* is anthropomorphic. It also ignores the orthodox view that those forms were distinct species and it ignores the implications of speciation. When a new species comes into existence, at the very minimum this involves a new combination of previously evolved adaptations. Given that an organism is a concert of adaptations, the new arrangement implies novelty on some criteria.

Still, evolutionary psychology does accept that something about the past and present are different. This is shown by its use of Sperber's term "domain mismatch" (introduced in chapter 6, section 3, 'The EEA and the minds of its occupants: the plurality of our ancestors'). In this chapter I want to look further at the notion of a domain mismatch. First we will look at the consequences of underplaying it. Following that we will look at the difficulties that arise for the adaptationist approach by invoking a domain mismatch so to accommodate the idea of maladaptive behaviour. This will lead us onto alternative terms to describe psychological mechanisms as producers of organised behaviour.

7.2 The null hypothesis: the EEA as a version of the present

As we have seen, the EEA is the main way by which evolutionary psychology distinguishes itself from Darwinian social science (also called the "correspondence program" Tooby & Cosmides, 1990b; Badcock, 2000). Tooby and Cosmides accuse Darwinian social science of resting on a logical flaw (see also Symonds, 1992). Barkow (1992) calls this the flaw of functionalism. The flaw resides in the assumption that events (or outcomes) precede their causes. Thus, looking to current behaviours that are fitter than others (in that they generate more reproductive success), and then supposing that there are adaptations underlying these behaviours, generates a cause from an effect. This discovery, the cause, is then traced forward to reproduce the present. The charge is that Darwinian social science is circular[3].

Tooby and Cosmides do not deny that there is some utility in what Sherman and Reeve (1997) have called the backwards method, but they caution against the investment of too much faith in it (Tooby & Cosmides, 1990b). Their argument is that evolutionary psychology can escape any suggestion of circularity via recourse to the EEA. Rather than looking to the present for clues as to what is adaptive and building the past accordingly, one begins with a scrutiny of the past and builds the present accordingly (Tooby & Cosmides, 1990b, 1992). In the first instance, the search is not for adaptive features of extant organisms but for adaptive features of non-extant organisms, i.e. the now-extinct hominid predecessors of *Homo sapien sapien*. One generates one's hypotheses according to what unknown members of another extinct species consisted of and did. Such a scheme carries no

obligation to specify any extant feature or behaviour as adaptive. Extant features or behaviours are not to be categorised according to their current effectiveness toward the end of inclusive fitness. Rather than taking the present as an effect into the past to determine its cause, the past as a cause is brought into the present and its effects are assessed. This method has one obvious difficulty: is it plausible to hold that accounts of the EEA are free of implicitly or explicitly held assumptions about modern humans? The concern hinges on the likelihood that the encephalisation of modern minds is due to sociality. Here we can build on the discussion in chapter 6, section 4, 'The physical and the social EEA'.

The view that *Homo sapien sapien* brains are as they are because of sociality implies that our lineage has been engaged in what we might call a "social arms race". This phrase in and of itself reveals little. At a minimum it refers to the problem of assessing the intentional states of other equally sophisticated systems. (Tooby and Cosmides are themselves confident that this is so; "humans everywhere include as part of their standard conceptual equipment the idea that the behaviour of others is guided by invisible internal entities, such as "beliefs" and "desires" – reflecting what Dennett calls "the intentional stance", Tooby & Cosmides, 1992: 89). Should such an arms race be responsible for the particular character of modern minds it suggests that, if the evolved psychology of modern humans should exhibit one thing, it would be the tendency to devote energy and time into having some reliable knowledge as to the intentions of important others. Moreover this tendency is compulsive – it is what modern humans do. The consequence of this is that a proposal which demands a method of thought that the theory in question considers unlikely, if not contradictory, must be treated with scepticism. It is not plausible to think that we can go to the past in an open-minded condition in light of evolutionary accounts of encephalisation. This may be why the tendency to treat the past as a version of the present is evident in the literature of evolutionary psychology. Consider the following passage:

> . . . the size of intimate human groups has changed little across evolutionary time . . . The absence of genetic kin is probably the greatest difference between the [typical academic] department's social organization and that of a hunter-gatherer tribe. . . In terms of the sociality of the situation, the way kinship, reciprocity, group size, resource distribution, and so on impact on social organization of a typical group, the sociality of a modern academic department may differ little from that of our Pleistocene ancestors (Crawford, 1998: 287).

Whilst one may be amused by the analogy Charles Crawford promotes here, amongst other things, it is remarkable that an author who subscribes to inclusive fitness theory should so casually dismiss the fact that a hunter-gatherer tribe and a modern academic department differ on the dimension of their constitution of kin. However, such an attitude toward some of the fundamentals of evolutionary

theory is not uncommon. Consider again the examples of the natural history and our *Homo* ancestors given in chapter 5. For Buss, the occupants of the EEA were, at bottom, us, *Homo sapien sapien.* In this vein, Crawford argues that, "the most plausible hypothesis about the EEA current environment differences is that ancestral and current environments do not differ vis-à-vis any particular adaptation, and that the proper course of action is to make a null hypothesis" (Crawford, 1998: 285). In taking this view toward proposed differences, Crawford is saying that the past explains the present because the past just is the present and the present just is the past. And he endorses this view as a methodological assumption.

We can see this assumption at work in the typing of the EEA as that which we see exhibited by modern hunter-gatherers (Tooby & Cosmides, 1992). It is at work in the characterisation of the pre-modern mind as essentially modern in its approach to mating behaviour (e.g. Buss, 1989, 1992, 1994; Daly & Wilson, 1992). And it is at work in the characterisation of *Homo* aesthetics (Orians & Heerwagen, 1992; Kaplan, 1992).

So what is wrong with this approach? Why not, in effect, class as the EEA only the period from the emergence of *Homo sapien sapien* to, say, the beginnings of agriculture *c.*15 000BC? Such an approach would rebut many of the objections raised in the preceding chapter. For example,

- We overcome the difficulties of typing the EEA as the set of problems faced by hunter-gatherers. The new EEA allows us to define our "natural" method of subsistence as that portrayed by *Homo sapien sapien* for most of its history since its emergence *c.*100 000 years ago.
- We find anatomical and cranial stasis in the fossil record. This allows us to define the minds of the occupants of the EEA as that which occupy our own heads and quashes the shortcomings of the comparative method and difficulties about suppositions concerning, for example, natural affordances and meaning.
- We have a more tractable task in specifying the physical conditions of the EEA. This enables us to be more precise about lifestyles and diet.

Unfortunately this move fails for three reasons. One, because Tooby and Cosmides work on the assumption there is no evidence that there has been evolution within *Homo sapien sapiens,* the EEA, construed as the past of modern humans, as opposed to their lineage, loses its force as an evolutionary argument. Evolutionary psychology then becomes a form of history as opposed to an exercise that draws upon natural history. Two, there is no non-question begging means of selecting which behaviours are and which are not "natural": the diversity of *Homo sapien sapien* environments (e.g. technologies, institutional behaviours, social systems, religions) makes it impossible to choose

between them in any principled manner without importing notions that pre-date *Homo sapien sapien* (see Bernard, 1924[4]; Foley, 1996a). Should we select certain features of *Homo sapien sapien* environments (e.g. monogamy) we are back with the piecemeal accumulation of detail about the self-contained system of cause and effect. We continue with the more convention activity of generating explanations around "an axis of variation rather than type" (Foley, 1996a: 195). And three, should we ignore the problems of doing so, and assume the null hypothesis of their being no difference between our ancestors and ourselves aside certain environmental differences, we find that the EEA amounts to a form of the SSSM. It differs from the SSSM in that the cluster of causes are located in a pre-agricultural past rather than the immediate past and present.

Let us consider in more detail the grounds for making this third point. I am saying that if we ignore the idea that in using the term "Pleistocene" we simultaneously invoke non-modern ancestors, as does evolutionary psychology in practise, then evolutionary psychology is in the same position as any other psychological/behavioural science – searching for intra-species universals (see Daly & Wilson, 1988). What we add are some general statements to the end that, say, humans want to eat, have sex and form friendships because of evolution.

When it is said that the EEA invokes a mismatch the mismatch is typically between late Pleistocene and modern industrial environments. To use a favoured example, one difference between then and now is in the abundance of sweet and fatty foods. The same brain that took all it could in the way of such foods in the past was being adaptive. The same brain that does it now is, unwittingly, being maladaptive. However, in order to generate the basic adaptation, an assumption must be made as to the prior condition of the mind – about its initial condition prior to being shaped by the EEA. Accordingly, evolutionary psychology assumes that the mind prior to being shaped by the EEA is, on some criterion, simpler or devoid of certain content-dependent mechanisms.

Now, when we look to be more precise about the source of the change that brings about a given adaptation than the catch-all " the EEA", Tooby and Cosmides point outward from the mind. The sources of change are problems posed by the world, by changes to the existing mode of life. Marx appears to sum-up the position quite nicely. Consider the following passage in light of the insistence of evolutionary psychology that the modern mind is a relic of the hunter-gatherer past.

> The social relations within which individuals produce, the social relations of production, are altered, transformed, with change and development of the material means of production, of the forces of production. The relations of production in their totality constitute what is called the social relations, society, and, moreover, a society with a unique and distinctive character. Ancient society, feudal society, bourgeois (or capitalist) society [i.e. any society], are such totalities of relations of

> production, each of which denotes a particular stage of development in the history of mankind (Marx, 1849: 156).

My point here is straightforward. Whilst not a *tabula rasa* in its developed condition, the concept of the EEA invokes a mind that is relatively free of content-dependent mechanisms in its initial condition. The forces that impinge on this relatively simple mind and render it complex in a human sense human are social and environmental.

7.3 Domain mismatches and maladaptive behaviour

According to Tooby and Cosmides, "For a Darwinian, there is no escaping the past" (Tooby & Cosmides, 1990b: 420). This idea legitimises their claim that,

> Although widespread current behaviours have consequences in terms of inclusive fitness at the present time, these present consequences are entirely irrelevant to explaining the adaptations that produce these behaviours. If these adaptations are to be explained . . . they must be explained in terms of their contribution to inclusive fitness during past encounters with ancestral conditions. . . For this reason, human behaviour is not well explained by attempts to show how it corresponds to contextually appropriate fitness pursuit (ibid).

It is the distinction between the past and the present that creates room for the notion of a domain-mismatch[5]. And it is the notion of mismatch that creates room for the possibility that the present as input into adaptations generates maladaptive behaviour because the present misleads adaptations in some sense. This reasoning allows Tooby and Cosmides to discuss maladaptive behaviours in adaptationist terms.

However, the notion of maladaptation - the idea that adaptations generate non-adaptive behaviours or trains of thought - admits of only one sort of change. The change is in the problems faced by the organism. The change is out there in the environment, outside the organism (Kohn, 1999). Tooby and Cosmides construe the EEA as continuous with the present in terms of what exists in human heads. This is why the issue of a "domain-mismatch" rather than a "mechanism mismatch" is an issue in evolutionary psychology: there is no talk of maladaptive behaviour stemming from changes to adaptive mechanisms with the result that they become non-functional in the same environment (unless adaptations are damaged). In accordance with my claim that evolutionary psychology does not appear to understand the evolution of novelty, its message is that you can take the woman out of the EEA but you can't take the EEA out of the woman. Accordingly, the maladaptive move is an admission that there are important differences between the past and the present. But the failure (or unwillingness) to concede that the difference is in the heads of forms of *Homo* forces evolutionary

psychology into admitting that the novel behaviour exhibited by extant humans are not the product of adaptations. However, the insistence that novel behaviours be anchored to adaptations creates a serious difficulty for adaptationist accounts. To see what this difficulty is we will need to be clear as to the meaning of the terms exaptation, spandrel and by-product, and to refresh our definition of adaptation.

7.3.1 Adaptations, Exaptations, Spandrels & By-products

Whatever may be said to be the origin of purposive action (of whatever form or nature), psychological and behavioural science does not seriously doubt that action has its seat in the human brain. This is simply to say that "no brain, no purposive action" and it is a demonstration the basic commitment to materialism in psychology. Evolutionary psychology shares this basic commitment. *Prima facie*, that human behaviour is, to use the term favoured by Hinde (1958) and Bowlby (1969), "labile", implies that adapted psychological mechanisms are functionally labile. However, whilst the emphasis Tooby and Cosmides place on the tightness of fit between the past and psychological adaptations wins them the notion of maladaptive behaviour, a cost is an implicit denial that adaptations are so labile as they would appear to be. In this section we will look at some of the difficulties this position creates for adaptationism. The heart of the matter is that, if one insists on saying that extant modern humans are adapted to an environment other than that which they inhabit and a style of subsistence other than that which they pursue, many of their current behaviours defy an adaptationist account. It will take some time to work through the terms. The reader may take this as a reflection of the muddle that ensues if one is serious about the past as an explanation for the present.

Much of our focus will be Buss, Haselton, Shackelford, Bleske & Wakefield's paper *Adaptations, Exaptations & Spandrels* (1998). This paper, together with peer commentaries and a reply to those commentaries, is the most recent and fully articulated defence of evolutionary psychology against the claim that exaptations and spandrels are lethal to the enterprise as presented to date (e.g. Tooby & Cosmides, 1992; Buss, 1995).

The first task of the analysis is to define the terms adaptation, exaptation, spandrel and by-product. The second is to trace out some of the implications of these definitions. And the third is to show why the terms exaptation, spandrel and by-product jeopardise evolutionary psychology as promoted by Tooby and Cosmides. Before we go on let us briefly consider why Tooby and Cosmides and evolutionary psychology as part of the adaptionist program concede that humans mind are comprised of anything other than adaptations.

7.3.1.1 Scruffy engineering

The concession amongst evolutionary psychologists that natural selection does not produce optimally "engineered" organisms is not new. We can trace the idea back to Alfred Wallace's 1896 paper *The Problem of Utility: Are Specific Characters Always or Generally Useful?* This paper was Wallace's response to George Romanes' volume *Darwin and after Darwin* in which, in Wallace's words, Romanes' argued "that the majority of specific character are not and never have been useful, but . . . persist and constitute the main external differences which we observe between species" (Wallace, 1896: 4). Wallace argued against this view. His contention was with the term "majority". He accepted that "certain growths, appendages [and] markings, which are of no use to the organism, do occasionally appear" (ibid: 6), but he argued that for them to constitute the majority of a species defies the logic of specialisation inherent in natural selection. He emphasised the role of "appended" characteristics such as shape, colour and size as being precisely the features that distinguish one species from another. But he also conceded that apparently useless appendages can become useful over time and with changing circumstances.

The first explicit consideration of the possibility that psychological adaptations may undergo a change in function appears to have come from Chauncey Wright. In a paper titled *Evolution of Self Consciousness* (1873), Wright argued that the opportunism and economy of evolution that he took Darwin to have clearly demonstrated suggested that changes of adaptive function for any given adaptation may be regarded, in principle, as common place.

> New uses of old powers arise discontinuously both in the bodily and nebtal natures of the animal, and in its individual developments, as well as in the development of its race, according to the theory of evolution, although at their rise, these uses are small and of the smallest importance to life (Wright, 1873: 246).

There are two points to be made with regard to this debate, especially in light of the extensive use Wallace, Romanes and Wright make of Darwin's considerations. The first is that "the problem of utility" – the question as to what characteristics of what organisms are to be regarded as adaptations – has long been an issue in evolutionary theory. And the second point, and this is the key to Wallace's objection to Romanes, is that "although non-utilitarian characters do undoubtedly appear in the normal course of variation, no agency has yet been detected adequate to the extension of these useless peculiarities" (ibid: 9). In other words, Wallace is claiming that natural selection only covers those characteristics of known function. We can refer to feature as "scruffy engineering". It denotes nothing more than the fact that organisms are other than optimally designed.

By being "other than optimal" a design is other than perfectly suited and matched to the problem is solves. As we have seen, evolution can be expected to generate other than optimal adaptations. It does so in three ways. Firstly, evolution predicts variation within a species. In principle, then, only one of these variations could even be considered as optimal. Secondly, natural selection, being "blind", cannot anticipate changes that will impinge on the operation of otherwise optimal solutions. Thirdly, natural selection tinkers with extant designs with the result that they tend to be adequate rather than optimal. In contemporary parlance the concepts that are used to unpack the notion of scruffy engineering are referred to as exaptations, spandrels and by-products.

7.3.1.2 Definition of terms

Adaptations: Tooby and Cosmides (1992) define an adaptation, "stripped of complications and qualifications" as,

> (1) a system of inherited and reliably developing properties that recurs among members of a species that (2) became incorporated into the species' standard design because during the period of their incorporation, (3) they were coordinated with a set of statistically recurrent structural properties outside the adaptation (either in the environment or in other parts of the organism), (4) in such a way that the causal interaction of the two (in the context of the rest of the properties of the organism) produced functional outcomes that were ultimately tributary to propagation with sufficient frequency (i.e. solved an adaptive problem for the organism) (Tooby and Cosmides, 1992: 61-62).

Following Tooby and Cosmides, Buss *et al* (1998) state that an adaptation is, "an inherited and reliably developing characteristic that came into existence as a feature of a species through natural selection because it helped to directly or indirectly facilitate reproduction during the period of its evolution". The solving of an adaptive problem is the function of the adaptation. If it fails to solve a given problem, it is not an adaptation by definition. In accordance with Williams' (1966) discussion, Tooby and Cosmides and Buss *et al* agree that the necessary and sufficient conditions for a feature to be properly labelled as an adaptations are that the feature in question be (a) complex, (b) economic, (c) efficient, (d) reliable, (e) precise and (f) functional.

(a) By complex it is meant that the feature exhibits non-accidental design characteristics.

(b) By economic it is meant that the feature exhibits material/metabolic parsimony.

(c) By efficient it is meant that the feature exhibits algorithmic parsimony.

(d) By reliable it is meant that the feature exhibits iterated performance.

(e) By precise it is meant that the feature exhibits discrimination.

(f) By functional it is meant that the feature exhibits a specific fitness utility.

There are two other important aspects of adaptations so far as evolutionary psychology is concerned. The first is that they attain their status as adaptations before the present: they are products of the past and they are not to be judged by their ability to fulfil the set of conditions detailed above in the present. The second is that adaptations are a sort of singularity. This is not to say that they map to single genes (although they might, adaptations are not to be defined in terms of the number of genes responsible). It is to say that they are individual and individuated modules. In sum, adaptations are always a product of a problem. Problems are always to be viewed as historical. Adaptations are always to be viewed as features of the phenotype by virtue of the fact that they are genetically heritable.

Exaptations: Exaptations are defined by Buss *et al* (following Gould, 1991, 2000) as an inherited and reliably developing characteristic that originally came into existence as an adaptive feature of an organism but the characteristic has since been,

a. further modified to fulfil some further distinguishable function, or,

b. co-opted to fulfil some further distinguishable function without distinguishable modification

The certain dependence of an exaptation on a prior adaptation and the need for an exaptation to fulfil the necessary and sufficient conditions of an adaptation (conditions (a) to (f) as defined above) lead Buss *et al* to label exaptations as co-opted adaptations.

Spandrels: The source of the term "spandrel" is to be found in Gould and Lewontin's 1979 paper, *The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptionist Programme*. Gould and Lewontin define a spandrel as an inherited and reliably developing characteristic of an organism that originally came into existence by virtue of an adaptive feature but the characteristic is not itself adaptive. This definition has since been reiterated (i.e. Gould, 1991, 2000). Following Gould, spandrels are defined by Buss *et al* as being, in the first instance, a non-functional by-product of an adaptation. Their caveat is that such features have since been,

a. co-opted by the organism to fulfil a function in light of an additional adaptive problem, but has not been,

b. modified in order for it to fulfil that further distinguishable function

The certain dependence of a spandrel on an adaptation and the need for a spandrel to fulfil the necessary and sufficient conditions of an adaptation (conditions (a) to (f) as defined above) lead Buss

et al to re-label spandrels as co-opted spandrels. Again, there is the further question as to when these conditions need to be fulfilled: in the present or in the past. I will return to this point in due course.

By-products: By-products are defined as incidental, non-adaptive features of adaptations. Given that, from a functional point of view, exaptations just are adaptations aside their necessary dependence on adaptations and the possibility that they may be specified as such under present conditions, I suggest that we allow a second form of by-product. This second form is defined as incidental, non-adaptive features of exaptations.

Similarly, given that, from a functional point of view, spandrels just are adaptations aside their necessary dependence on adaptations and the possibility that they may be specified as such under present conditions, I suggest that we allow a third form of by-product. This third form are defined as incidental, non-adaptive features of spandrels.

A by-product of either form may be negatively characterised as exhibiting none of the features of either adaptations or exaptations. That is to say they are not complex, economic, efficient, reliable, precise or functional.

7.3.1.3 Implications of the definitions

The first and foremost implication of the definitions given of adaptations, exaptations, spandrels and by-products is that any given feature of an organism or species may be properly characterised as one or another (in the logically exclusive sense of the term "or"). This is to say that organisms can be exhaustively accounted for in terms of adaptations, exaptations, spandrels and by-products. The second implication is that any given feature is not attributed a given definition *a priori*. This is to say that whilst an organism can be exhaustively accounted for in terms of adaptations, exaptations, spandrels and by-products it is an empirical issue as to which features or proposed mechanisms are given which label. The third implication is that any given feature may at, any given prior point in time, be alternatively defined. This is to say that domain-mismatches permit of changes in which ascription we give any given feature or mechanism. None of these implications breach selectionist constraints. To say that any given feature of an organism or species may be properly characterised as one or another is simply to say that it must fulfil the conditions of the selectionist term. To say that any given feature is not attributed a given definition *a priori* is simply to say that any given feature needs to be shown to fulfil the conditions of the term and does not do so axiomatically. And to say that any given feature may at, any given prior point in time, be alternatively defined is simply to say that the function of any given feature may change in terms of its contribution to fitness.

It is the third implication - changes in the definitional ascription of features - which I want to discuss next for it is the most obvious consequence of granting Tooby and Cosmides the notion of maladaptive behaviour by recourse to a domain mismatch. This discussion will take the form of an instance-by-instance examination of the plausibility and general conditions under which any given feature may undergo a change of ascription from one definition, as being an exaptation for example, to being another, for example a by-product.

7.3.1.4 Changes of ascription

As has been shown in the discussion of the EEA, adaptionist accounts have the problem of balancing the changes in environments that give rise to adaptations and specifying sufficient stasis at some point in time in order to take an inventory of an organism's adaptations: the inventory is the adaptionist account. To make this task clearer let us take as an example the germane case, the *c.*6 million year period that has seen the emergence of humans from chimpanzees.

To bridge the gap, or difference, between *Pan troglodytes* and *Homo sapien sapien* adaptionist accounts have to posit new or changed adaptive problems in order to generate a causal engine for those differences. And it then needs to "stop" or freeze the change in order to fix the adaptation and allow it to go to fixation in the population. If one accepts the idea of the ARE this process of start-stop may need to be reiterated for each individual adaptation (Buss *et al*, 1998; Irons, 1998). The number of iterations is conceptually, if not technically, irrelevant on the assumption that the general procedure will yield a specification of an adaptation.

The thrust behind the concept of the EEA comes from the fact that in order to generate and specify the basic characteristics and proper definition for a given feature one must show what caused or brought it about; i.e. one must specify the problem. As Plotkin (1995) suggests, problems are either lethal to a species because it cannot cope with or adjust to them, they are lethal to strains of a species, lethal to individuals of a species or they are solved by virtue of a feature. The evolution of a species is, on the one hand, the history of its problems, or, on the other, the history of the features generated by those problems. The two histories are complementary but not symmetric. They are complementary in the sense that problems and features enjoy a certain goodness of fit - if this fit cannot be shown then one is matching the wrong problem to a given feature or vice versa. They are not symmetric in that, whilst consecutive problems are not constrained by the speed at which an organism can adapt, adaptations are. If we are to avoid tautologies on the one hand and change constrains on the other, in order to offer an evolutionary explanation of a feature we must be prepared to specify a change of ascription for any given feature. This does not violate selectionist

thinking. Changes of ascription are just stages of evolution so long as we limit ourselves to ascriptions that are naturally occurring products of the evolutionary process. Let us now return to the notion of maladaptive behaviour and see how it depends upon adaptations functioning in ways other than those they are designed for.

Tooby and Cosmides argue that maladaptive behaviour occurs because we are not wholly adapted to current environments. Indeed evolutionary psychology is, in part, founded on a rejection of the idea that we are well suited to the present. It accuses such thinking as being the fountainhead of tautological "just-so" stories (e.g. Symons, 1992) and as responsible for "hyperadaptiveness" and "instantaneous Lamarckianism" (Tooby and Cosmides, 1990b). It argues that humans are adapted to an environment - and thus a suite of problems - other than that which the bulk of us currently inhabit. But the implied mismatch demands that we be prepared to ascribe common current behaviours that are either neutral (watching television perhaps), apparently adaptive (say, reading) or simply very well organised (say, driving a vehicle) to mechanisms that are not best described as adaptations. Here is the argument that allows for changes of ascription:

(a) On the (questionable) assumption that an organism is wholly constituted of adaptations to specific problems, and,

(b) On the assumption that the range of problems it faces change, and,

(c) On the assumption that the organism continues to thrive (that the change in problems is not lethal)

(d1) It follows that the organism does not continue to thrive by virtue of adaptations properly defined, or (in the logically inclusive sense) it does so by virtue of features other than adaptations properly defined

In other words, in order to account for the outcome - the still thriving but mismatched organism - within a selectionist framework one must assume that the original adaptation is now either an exaptation, a spandrel, or a by-product.

To allow for cases other than adaptation to spandrel/exaptation/by-product re-ascription we need only weaken assumption (a) by employing (d1). Here is the argument that follows:

(a2) On the assumption that an organism is wholly constituted of adaptations, exaptations, spandrels and/or by-products, and,

(b) On the assumption that the range of problems it faces change, and,

(c) On the assumption that the organism continues to thrive

(d2) It follows that the organism does so by virtue of changes of ascription of adaptations, exaptations, spandrels or by-products properly defined

In other words, in order to account for the outcome - the still thriving organism - within a selectionist framework one must assume that at least one adaptation, exaptation, spandrel or by-product is now properly described as either an adaptation, exaptation, spandrel or by-product.

7.3.1.4.1 How do changes of ascription work?

I hope to have shown that changes of ascription must necessarily come about if humans continue to thrive in conditions other than those to which they are adapted. This is the consequence of positing a domain mismatch. I suggest that evolutionary psychology and its invocation of the EEA suggests that *Homo sapiens sapiens* are an exemplary case of such changes having come about. What I will now do is take each possible case of a change of ascription and specify under what conditions each might come about. As we will see, certain cases are more plausible than others and certain cases are, by virtue of definition, impossible. I will go onto to argue that the adaptionist program as instantiated in evolutionary psychology must show that certain reascriptions have not come about in order to support the general hypothesis that humans minds are a suite of adaptations.

By a change of ascription I mean a re-labelling of

1 an adaptation to an exaptation	2 an adaptation to a spandrel
3 an adaptation to a by-product	4 an exaptation to an adaptation
5 an exaptation to a spandrel	6 an exaptation to a by-product
7 a spandrel to an adaptation	8 a spandrel to an exaptation
9 a spandrel to a by-product	10 a by product to an adaptation
11 a by-product to an exaptation	12 a by-product to a spandrel

The above is an exhaustive list of the possible changes of ascription.

7.3.1.4.1.1 From an adaptation to an exaptation

This is the prime instance of a change of ascription. That evolution is said to be a tinkerer of existing design is to say that it builds on existing adaptation to cope with new or changed adaptive problems. Exaptations depend upon adaptations: this is to say that exaptations are former adaptations that have

been co-opted to perform some other/additional/expanded/distinct function. The motto is, "in the beginning there was an adaptation".

We may follow Dennett (1995) and say that all adaptive features of humans are, in fact, exaptations from previous adaptations. To achieve this we simply have to choose a sufficiently distant comparison point - fish for example. (However, we may say that this is trivially true because it is merely the postulation of evolutionary change). But to capture the import in the term exaptation we need to suppose a much sharper disjunction. We need to be able to show that feature *x* was an adaptation in the full sense of the term and that feature *x* at some future but sequentially immediate point fulfilled all the general criteria of being an adaptation but the functional specification of each is distinct. For example, as an adaptation feature *x* is complex in manner *y*, and as an exaptation feature *x* is complex in manner *w*, and so on.

7.3.1.4.1.2 From an adaptation to a spandrel

It is not possible for an adaptation to become a spandrel. The prerequisite for a spandrel is that it is first a by-product of an adaptation not an adaptation *per se*.

7.3.1.4.1.3 From an adaptation to a by-product

On the definition of a by-product, an adaptation cannot be become a by-product directly. However, we may say that an adaptation becomes redundant. To say that a feature goes from being an adaptation to being a by-product requires that the adaptation is first exapted and in the process some feature of it no longer has a function.

7.3.1.4.1.4 From an exaptation to an adaptation

As in the trivial case of adaptations becoming exaptations by definition, exaptations proper just are adaptations. The only instance in which an exaptation could become an adaptation would be where a feature reverts to its original function. We might say that this happens all the time. For example, at this moment in time I am "using" my eyes to trace and read these words. When I prepare a cup of coffee at the end of this section I will use them to detect edges.

7.3.1.4.1.5 From an exaptation to a spandrel

On the definition given, it is not possible for an exaptation to become a spandrel. The prerequisite for a spandrel is that it is first a by-product of an adaptation. It is possible, however, that in the process of becoming an exaptation an adaptation generates spandrels. This is to say that in the process of becoming an exaptation, what were by-products of the adaptation now become spandrels by virtue of the new problem the exaptation is co-opted to solve.

7.3.1.4.1.6 From an exaptation to a by-product

On the definition given above, an exaptation cannot become a by-product. The prerequisite for a by-product is that it is first a functionless element of an adaptation, an exaptation or a spandrel. Again, as with an adaptation, however, we may say that an exaptation becomes redundant. We may also say that exaptations generate by-products in the case that the exaptation undergoes a further co-option and in the process some feature of it no longer has a function.

7.3.1.4.1.7 From a spandrel to an adaptation

On the definition given above, it is not possible for a spandrel to become an adaptation except in the trivial case whereupon a spandrel just is an adaptation.

7.3.1.4.1.8 From a spandrel to an exaptation

On the definition given above, it is not possible for a spandrel to become an exaptation other than in the trivial sense that a spandrel as an exaptation just is an adaptation, and other than in the trivial case that a spandrel just is an additional use of an adaptation. However, we may say that a spandrel arises from an exaptation when in the process of co-opting an adaptation an exaptation produces a by-product that is subsequently co-opted to produce a spandrel

7.3.1.4.1.9 From a spandrel to a by-product

It is possible for a spandrel to become a by-product when it becomes redundant. This can occur when, as a feature of an adaptation that becomes an exaptation, it becomes a by-product of that exaptation.

7.3.1.4.1.10 From a by-product to an adaptation.

On the definition of by-product given above, is not possible for a by-product to become an adaptation. However, we might suppose that a by-product, properly defined, might become a functional aspect of a given feature. This would require that the specification of the adaptation remains the same (or not so dissimilar that is becomes an exaptation). Again the utilisation of a by-product would enhance the economy of the adaptation.

7.3.1.4.1.11 From a by-product to an exaptation

On the definition given above, it is not possible for a by-product to become an exaptation. However, it is possible for a by-product of a given adaptation to become a functional part of an exaptation. This may, in fact, be a parsimonious way for an adaptation to become an exaptation. We need only assume that a given adaptation has a by-product attached and that in the process of exaptation this by-product is utilised. The economy of the original adaptation is actually enhanced and no additional

engineering is required. The question is do we classify such a change in ascription as an expansion of the adaptation or as a bona fide exaptation?

7.3.1.4.1.12 From a by-product to a spandrel

Specifying a reascription of a feature from it being a by-product to being a spandrel is to detail the conditions of a spandrel. It is to define a case of a spandrel. We need only assume that, in the first instance, a given adaptation has a by-product attached and that in the process of conversion this by-product is utilised. The economy of the original adaptation is actually enhanced and no additional engineering is required.

7.3.1.5 Implications for evolutionary psychology

The point of the preceding sections is simply to show how a given or proposed feature or mechanism goes from being classified as one sort of product of evolution to another by virtue of changes in the problems an organism faces. What I want to do in this section is draw out three implications of the possibility – indeed, the probability given a domain mismatch - that mechanism can change "jobs" or be applied to tasks other than that which they were originally selected for.

7.3.1.5.1 The relative importance of exaptations and spandrels

A noteworthy feature of Buss, Haselton, Shackelford, Bleske and Wakefield's *Adaptations, Exaptations & Spandrels* (1998) is that it accepts and resists exaptations, spandrels and by-products. Following Tooby and Cosmides (1990b), Buss *et al* accept that the concepts are respectable, *viz.*, "The evolutionary process produces three products: naturally selected features (adaptations), by-products of naturally selected features, and a residue of noise" (Buss *et al*, 1998: 537). But they are anxious to squash any claim to the end that other products of the evolutionary process "may be a more important concept for the emerging paradigm of evolutionary psychology" (ibid: 533) than adaptations. That they countenance the idea at all flows from their acceptance of a domain-mismatch.

Buss *et al's* strategy is as follows. First, they suggest that Gould, the main proponent of spandrels, is an unreliable voice on matters evolutionary. They do this by accusing Gould (1982, 1991) of being inconsistent in his usage of the term adaptation (they, in turn, are accused of being inconsistent with Gould by one of their commentators, i.e., Beauchaine, 1998). Second, having doubted the doubter, as it were, Buss *et al* look to capitalise by marginalising the "other products". While accepting that there are constraints on optimal design (i.e. time lags, local optima, constrained genetic variation,

fitness costs, and modular co-ordination), they suggest that such constraints may only give rise to characteristics that appear to be exaptations and spandrels, and that by-products or noise are simply flaws in adaptations. What they say is that one can account for more or most behaviours by recourse to adaptations supported by accounts of the EEA. Thirdly, Buss *et al* look to make exaptations and spandrels tractable in term of the adaptationist program by relabelling them. Exaptation, defined as a redeployed adaptation, is called a "co-opted adaptation". Similarly, spandrel, defined as deployment of a hitherto redundant by-product, is called a "co-opted spandrel". What is the same in each instance is that exaptations and spandrels "must possess a biological function that contributes to fitness to qualify" as either (ibid: 539). The gain here is that each term carries the obligation to be functional. True to the concept of the EEA, Buss *et al* invoke objections to Darwinian Social Science (Buss, 1995; Symons, 1992) to repudiate the utility of the concepts of exaptations and spandrels, given that they are said to apply to current fitness. In doing this they ignore the probability that exaptations are also features of the past and would themselves have exerted selection pressures on con-specifics. Having relabelled exaptations and spandrels in adaptionist terms, Buss *et al* suggest a return to normal business by saying that ". . . the component parts of a species can be analyzed, and empirical studies can be conducted to determine which of these parts are adaptations, which are by-products and which are noise" (ibid: 537). We will turn again to Tooby and Cosmides in the next section to see how this analysis of determination is to be conducted. But first let us consider obvious problems for Buss *et al's* scheme.

Firstly, how important does one take those behaviours that humans routinely perform in an organised manner, that are not obviously maladaptive and that we cannot be said to have been selected to perform? I have in mind such behaviours as, say, driving, reading and forming large abstract coalitions (e.g. national unions or multinational corporations). How are such activities to be explained in terms of the past?

Secondly, what in addition to an adaptationist description of our predecessors do we need to produce in order to make sense of the fact that activities such as reading and driving are so routinely achieved by us? Surely the more complex, economic, efficient, reliable, precise and functional the mechanisms we take our predecessors to have been comprised of, the more difficult it is to explain how we achieve such complex tasks efficiently, reliably, precisely and functionally. We can attempt to tackle these questions. Or, alternatively, we can take a more relaxed attitude toward adaptationism.

7.3.1.5.2 How do you tell an exaptation from an adaptation?

In this section I want to point out two difficulties that arise when we try to discern exaptations from adaptations. One centers on Tooby and Cosmides' proposal for the analysis of adaptations. The other centers on the experimental method as a means of ascertaining adaptations.

In order to establish a psychological adaptation, Tooby and Cosmides present "the five structured components that can be fit together in such an analysis" (Tooby & Cosmides, 1992: 73).

1. *An adaptive target*: a description of what counts as a biologically successful outcome in a given situation. . .
2. *Background conditions*: a description of the recurrent structure of the ancestral world that is relevant to the adaptive problem . . .
3. *A design*: a description of the articulated organization of the recurrent features in the organism that together comprise the adaptation or suspected adaptation. . .
4. *A performance examination*: a description of what happens when the proposed adaptation mechanistically interacts with the world. . .
5. *A performance evaluation*: a description or analysis of how well (or how poorly) the design, under circumstances paralleling ancestral conditions, managed to produce the adaptive target (the set of biologically successful outcomes). The better the mechanisms performs, the more likely it is that one has identified an adaptation (ibid: 73-74).

As the central place of the EEA in evolutionary psychology would suggest, Tooby and Cosmides' preferred method is to specify an adaptive target (i.e. an outcome known to have been regularly achieved in the past), construct the requisite background conditions, including the condition of the species in question at the time, and hypothesise a design. However, it is obvious that this approach is not open to us if we are testing to see if the design underlying a behaviour is an exaptation. To look for the background conditions for, say, driving, would, of course, be senseless. This leaves any search for an exaptation reliant on steps 1, 3 and 4. But, in light of considerations of our knowledge of the EEA, I suggest that steps 1, 3 and 4 are all we have at our disposal when searching for adaptations also. The result is that we can articulate a proposed design (step 3), examine its performance in the world (step 4) and decide that it is biologically successful (keeping in mind that a biologically successful outcome for Tooby and Cosmides can be remote from the production of offspring). Whilst our hunch might be that a performance that amounted to, say, the consumption of a macrobiotic diet was more likely to be the result of an adaptation(s), we cannot say that it has a better outcome (in Tooby and Cosmides sense of the term) than being, say, a good carpenter. I have

picked two ends of a fairly mundane spectrum to illustrate the point that we cannot attribute many day-to-day behaviours to adaptations less detailed and unsupported assumptions about the past.

The second point of confusion that arises if we attempt to discern adaptations via Tooby and Cosmides' proposed method arises from the means we may use to do so.

First of all, allow me to reiterate: It is respectable to accept that human minds are comprised of adaptations and exaptations. Any activity in which they engage that was not explicitly generated by the EEA is achieved courtesy of an exaptation, a spandrel or a by-product. Reading is the obvious example we have been working with and it is apposite to the present point. An adaptionist account of reading can go no further than saying that the EEA must have been comprised, in part, by problems for the visual system and cognition that have subsequently allowed for or permitted reading to become a common and organised behaviour. It cannot reasonably equate the functional value of the adaptations that allow for reading with the fitness consequences of reading. To do so would implicitly assert that reading is an adaptation.

An odd result of the fact that reading is not an adaptation arises for any method for finding a psychological adaptation that depends upon the ability of subjects to read. Arguably the most influential experiment in the literature of evolutionary psychology, Cosmides use of the Wason selection task (Wason, 1969; Cosmides, 1985, 1989; Cosmides & Tooby, 1992) is just such an example. Whilst only a majority of participants could respond correctly to the social contract version of the selection task, all participants could read it. The wider point that many psychological mechanisms may best described in exaptationist terms arises from the ability of humans to complete experiments designed to show adaptations. Any method that depends on abilities that cannot be responses to ancestral problems is, unwittingly, a demonstration and study of exaptations.

7.3.1.5.3 How do you do scruffy engineering?

A possible way out of the difficulties that arise from trying to accommodate a domain-mismatch within the framework of psychological adaptations, as Tooby and Cosmides' define them, is to embrace the idea that adaptations are "scruffily engineered". This need not necessarily mean that they do not have background conditions or adaptive targets. But it does mean that they may produce quasi-functional behaviours and outcomes. This then presents problems when we attempt to evaluate their performance. The basic problem is how do we do scruffy engineering? For example:

- Whilst Tooby and Cosmides claim that "an evolutionary functional analysis consists of asking a series of engineering questions" (ibid: 75; see also Dennett, 1995), the stance required must set scruffy questions.
- If we expect sub-optimal performance from an evolved design, how scruffily engineered need it be before we take it as an adaptation?
- Should a proposed design produce an outcome that hits the adaptive target with very great precision and reliability, does this imply that the proposed design is too good?
- Is there any reason to believe that all adaptations are similarly scruffy? Given the assumptions of modularity and specific problem/solution couplings, should we not expect that adaptations are variably scruffy? What performance standard do we set in order to evaluate each supposed adaptation and how do we set this standard?
- Accepting that adaptations are (more or less) scruffily engineered and scruffy in their individual instantiations raises the very possibility of by-products and spandrels: we may assume that optimal designs would produce fewer of either. But it also raises the possibility that certain exaptations and spandrels may perform their functions better than do certain adaptations. All that is required is that the new problem meshes with the extant feature more precisely or closely than did the old problem. How would a performance evaluation settle the issue as to the proper status of a supposed design feature?

7.4 Backwards, forwards but no further ahead

In this chapter we have looked at two construals of the EEA. The first is that it was the same as is the present. The second is that it is not. What is the same in both cases is the questionable assumption that it is not minds that have changed since the beginning of the Pleistocene but the environment in which they operate. We examined some of the evidence that makes that assumption questionable in the last chapter.

With regard to the first proposal, the benefit of assuming that our hominid predecessors occupied essentially identical environments or worlds, physical and social, to ourselves is that the present is as good as the past so far as discerning adaptations are concerned. However, if we take the past and the present to be homogenous or seamless we have to justify why one form of life and social living – typically hunting and gathering – is taken as being primary, natural or, in some or another sense, more important than any other. If, in effect, we are going to ignore *Homo* outside of *Homo sapien sapien* surely we have to take into account the fact that most human beings have not lived that form of life. As "design propagators", human beings are more successful when out of their "natural"

environment. Does evolutionary psychology wish to stipulate the conditions in, say, modern India as the proper domain?

With regard to the second proposal, the benefit of assuming that our hominid predecessors occupied non-identical environments or worlds, physical and social, to ourselves is that we gain the notion of maladaptation. Describing behaviour as maladaptive allows evolutionary psychology to explain functional and non-functional outcomes in terms of adaptations. Non-functional outcomes are the result of a mismatch between the past and the present confusing or confounding adaptations. However, two lines of reasoning present difficulties for this proposal.

First, many behaviours of modern humans that, *prima facie*, have the hallmarks of being complex, economic, efficient, reliable, precise and functional cannot have been selected for in the Pleistocene, e.g. reading. Second, Tooby and Cosmides' use of terms such as "engineering", "architecture" and "mechanism" notwithstanding, evolutionary theorists from Wallace onwards accept that natural selection does not produce optimally designed organisms. From within the Darwinian tradition, Gould and Lewontin (1979; Gould & Vrba, 1982; Gould, 1991, 2000) have pressed this point and have won the concession that modern humans may not be exhaustively described in adaptationist terms. Tooby and Cosmides and other evolutionary psychologists accept this but they do not embrace it. One of the ways of avoiding the charge of teleology and keeping to a selectionist account of how species cope with domain-mismatches is to resort to the concepts of exaptations, spandrels and by-products. These two lines of reasoning issue the result that the behaviours of extant humans in modern environments, including those that look adaptive, are not the direct product of adaptations. Furthermore, depending on the interest of the researchers, behaviours that cannot be explained in terms of the EEA may be of greater importance or interest than those that can.

I suggest that a way out of the difficulties created by construing a very tight fit between psychological mechanisms and the past is to embrace the notion of scruffy engineering. However, such a move demands that we abandon Tooby and Cosmides' definition of an adaptation and the method by which we ascribe a proposed feature of mind as such.

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[1] Dewey's argument run's as follows: Because modern man has such a wide behavioural repertoire at his disposal, and that whatever the repertoire of our ancestors we cannot be sure of its psychological foundations, we cannot know the relationship between our psychology and theirs. Though we know that *sapien sapien* is novel, and that one cannot predict species X at time Y preceding it, the EEA cannot contain what we would need to know about subsequent events and species if we construct it from $time_1$ through to $time_n$. However we define its duration, the past can tells us nothing definite and specific about the novel species that emerged from it by virtue of their novelty. This idea can also be expressed using Dennett's notion of "design space" (Dennett, 1995). By design space Dennett simply means all viable possibilities for phenotypes at time X in environment

Y. He points out that there are a finite but "Vast" number of possibilities but only a tiny sub-set of actualities. Evolutionary novelty is really nothing more (!) than the computational difficulty (or impossibility?) of correctly predicting X at time Y at some point prior to either.

[2] Palaeoanthropologists divided themselves into "lumpers" and "splitters". Lumpers minimise the number of different hominid species, although very few suggest there are less than five (Tattersall, 2000). Splitters think that there were many more and interpret what for a lumpers is variation as type (ibid).

[3] A circular argument is an argument wherein one (or more) of the premises used to deduce the conclusion is itself supported by the conclusion. This is why circular arguments are often labelled question begging or taken to beg the question.

This is the structure of the argument.

X is an extant human mind

All extant features (y) of X are adaptations

all adaptations are product of the past

all adaptations (strictly conceived as being successfully functional) are in the present

The argument promotes two conclusions

if feature of the past then an extant feature

and

if and extant feature then a feature of the past

[4] Bernard (1924) cited Thorndike's survey of the field in *The Original Nature of Man* (1913) as well as his own analysis of instinct in use to support his view that the predominant tendency amongst instinct theorists was to think of instincts as being for something, as being functional. However,

> The difficulty of this method [of classification] is that it describes our attitude toward the activity process or our understanding of its value in society, rather than the process itself. Such a definition does not enable us to distinguish one instinct or set of instincts from another, so far as origin or structure are concerned . . . (Bernard, 1924: 148)[4].

Bernard labeled the functional approach "instinctivism". More precisely, he was concerned about the plausibility of constructing an unbiased account of the present:

> . . . what adherents to this brand of determinism hope to accomplish . . . depends primarily upon what their reason, traditions and experiences have taught them to consider desirable. One cannot avoid observing that a conscious ideal or principle or convention has justified the social or individual "necessity" of monogamy, or of freedom of sexual relationships, or of any other "ism" before instinct is invoked in its support . . . Thus, in the case of so-called instinctive determination of social principles and attitudes, we have an interesting example of the influence of environment

itself. The environmental pressures have . . . determined what the instinctivists wish society to be and have moulded [sic] their attitudes towards, or theories of, the instincts and their importance accordingly (Bernard, 1924: 33).

In chapter XIII, "A Reductio Ad Absurdum" Bernard developed this line of criticism. He sought to show how the muddle that had come about in attempts to numerate instincts flowed from the tendency of functional account to talk of instinct being for something or to something. The example he chose was the instinct for criminality (or to criminality). Bernard's strategy was to decompose any such claim by arguing that it cannot possibly refer to any given inherited fact of physiology. Given that criminality is not a biological unit but a synthesis of units, Bernard showed how different forms of criminality – i.e. murder, robbery, arson – themselves are comprised of different units.

> If we select shooting as the type of murder which we desire to analyse into its elements, we discover a great variety of instruments by means of which one may be shot. But this analysis into the instrumental mechanisms is only one phase of the divisibility. We must also take into consideration whether the shooting was intentional or purposive, whether it was socially justifiable or not, what the other circumstance of light, position, provocation, etc., are. All of these considerations have some effect, sometimes a profound effect, upon the final classification of the act from neural and from muscular standpoints, that is, as an act physically speaking, - the only sort of an act which can conceivably be inherited (Bernard, 1924: 308-309).

Bernard took this decomposition as a demonstration that any claim of the type "instinct for . . ." was, in fact, a synthetic claim, and "that an abstract idea (such as criminality) cannot be inherited, because neurologically it does not represent a unit act or organisation".

> It stands for a synthetic valuation of act with a certain similarity of social and moral significance, when viewed in relation to certain results. It is a conceptual fact rather than an overt of neuro-muscular act (ibid: 310-311).

This analysis was the basis of Bernard's claim that instincts defined by their practical results,

> Instead of representing definite internal biological unit structural organizations which remain constant, as instinct or inherited traits necessarily must, they represent varying syntheses of biological (predominantly neurological) complexes and dispositions which have only a nominal constancy, one which exists in their meaning rather than in their structural organization. That is, their unity is a varying unity with regard to the time and space elements and we are able to give them a constant or permanent title only through a process of abstract or conceptual syntheses of functions. Their unity and identity are social and conceptual rather than biological (ibid: 303).

On the same theme he also wrote, "Using the universality of a practise as an argument in support of its instinctive character would get us into all sorts of difficulties" (ibid: 223). The "difficulties" are three. First,

Bernard argues, it is easy to be deceived as to what is universal if one is selective about the time and place one chooses to look as one is bound to be due to operational constraints. Second, apparent universals, such as the belief in a god, can force one into stipulating instincts for religion if one is true to ones method. Third, and most pertinently, Bernard believed that functional accounts of instincts underplayed both the power and the ubiquity of "biological and anthropogeographical environments".

> Any practise which can be acquired can be made universal if the proper environmental conditions for stamping in or inducing the practise exist . . . countless numbers of attitudes and practises have grown up because of their survival value under favourable environmental conditions. These attitudes and practises have tended to universalize themselves for the same reason that they appeared in the first place. We may expect any of these to undergo further modification under varying environmental pressures, understanding by environment the psycho-social environment quite as much as the biological and anthropogeographical environments (Bernard, 1924: 229).

Notice that Bernard did not denying that humans are motivated to survive, or that they behave functionally. What he argued was that specific instincts are difficult to demonstrate as being functional in light of shifting means toward the end of survival.

[5] As it turns out, the notion of domain-mismatch was first articulated by one of the most insistent critics of instinct theory, Kuo. (Unlike, say, Dewey who gradually abandoned his positive view of instincts, Kuo appears to have never entertained the possibility that the concept had any use or validity.) To set up his domain-mismatch, Kuo relied on what today critics of evolutionary psychology call "ultra-adaptionism" (Rose & Rose, 2000. And see below, 'Adaptations, Exaptations, Spandrels and By-products). Kuo begins his criticism by rejecting what he saw as one of the ". . . motives which have led the psychologists to insist on the existence of instincts and their significance in behaviour . . . the notion that every instinct has an adaptive function" (Kuo, 1921: 653).

> Biased by the Darwinian theory of natural selection, students of psychology are apt to interpret every spontaneous reaction of the organism in terms of biological value. They argue that instincts play a very important part in the preservation of the organism and the species. These instincts, because of their adaptive value, are preserved in the race through natural selection and are handed down from generation to generation (ibid: 653).

Kuo levels had two complaints with this view. The one of interest to our present concerns was that,

> . . . supposed instincts might be adaptive in certain generations; but there is no guarantee that they will be adaptive in all generations and in every circumstance . . . If instincts persist in form from generation to generation, they, instead of being adaptive instruments for racial or individual preservation, will become mal-adaptive in a new environment . . . Should we have inherited the same instincts as our ancestors of a few thousand years ago, how awkward we would be in adapting ourselves to modern society (ibid: 654).

We don't have to take this criticism seriously to take the point. It was Kuo's view that either instinct theorists should concede that the ability of instincts to cope with such radically altered environments implied that their flexibility under the rubric of habit was the essential phenomena of interest, or that they should show that, contra the very notion of historical development, no change of environment had taken place.

Part III: Evolutionary Social Psychology without Adaptations

Chapter 8 Instincts and Adaptations

Summary of chapter 8

Instincts and adaptations invite comparison. Definitions of both employ the notions of inheritance, selection, function and species typicality. With regard to the application of each to wider issues of social behaviour and the generation of culture, authors of different generations have illustrated the similarity by making similar claims for each. Whilst comparisons of instincts and adaptations are readily made, there are differences between the two. Whereas evolutionary psychology seeks to specify the adaptations that comprise humans by reference to their ancestral environments, instinct theory, whilst insisting that instincts have an adaptive function, did not explicitly press questions as to their origin in the natural history of humans. I suggest that instincts theorists were unable to avail themselves of a developed palaeoanthropology to differentiate an account of human origins from philosophical speculations about the "primeval" origin of man. Reasons for the fall of the instinct concept are examined and a further comparison is made with adaptations. I argue that Tooby and Cosmides drive the concept of a psychological adaptation into the difficulties that beset the notion of psychological instincts.

8.1 Introduction

In this chapter I will argue that there are lessons that Tooby and Cosmides and evolutionary psychology can take from the non-Standard Model paradigm in psychological thought that I have called the instinct debate. The first of these is the affinity between the earlier notion of instinct and the current one of adaptation. In the first part of chapter 8 we will see how close the affinity is by comparing Tooby and Cosmides' overall objective with that of William McDougall. More broadly it can be seen that instincts and psychological adaptations are, if you will, of the same species as concepts.

The second lesson that evolutionary psychology can take from the instinct debate concerns the fate of the latter. Forty years of effort, expended by many of the major psychological thinkers of the period, did not - just as Tooby and Cosmides' thesis of the SSSM suggests - result in a mainstream social psychology explicitly founded in Darwinism. It is important for social psychologists inclined toward an evolutionary approach to ask why the idea of social instincts failed.

In the later part of the chapter we revisit the instinct debate and look at some of the reasons why it came to an end. I hope to show that the notion of an adaptation could not be successfully de-coupled from its foundation in anatomy and physiology so to provide a satisfactory notion of a psychological instinct. A noticeable feature of the debate was the switch of emphasis from instincts conceived of as

physiological phenomenon and framed in the terminology of reflexes, to an emphasis on their psychological manifestation and properties. It was this switch which encouraged the reification objection[1], the criticism that comparative analyses of psychological content rested on anthropomorphism[2], and accusations that psychological instincts were teleological[3]. In my view, the attempt to discuss instincts in terms of psychological content also led to the demise of the concept. It did so because instinctive psychological states could not be adequately defined with the result that what counted as a psychological instinct could not be constrained.

We will look at how psychologists tried to psychologise instincts. The exercise will show that once one decouples the notion of adaptation from anatomy or physiology one seriously weakens the tether to natural selection and natural history. We will also consider Luther Bernard's influential critique of the psychological instinct concept in use. In the final section of the chapter I will explicate Tooby and Cosmides attempt to psychologise adaptations and suggest that their formulation too cannot be constrained.

8.2 Compare . . .

As suggested in chapter 4, instincts never entirely ceased to be of issue in psychology and social psychology. On the one hand, the concept was broken up. It buckled under the weight of its own obligations but lived on in a lower case existence and eventually faded from view as an unuttered assumption in drive theories. On the other hand, it lived on in ethology. A sideshow of the social sciences, the leading advocates of ethology went on advertising the wares of instincts to those most interested in the human drama. In this section I will directly compare evolutionary psychology's claims concerning the power of adaptations to shed light on social psychology and society with those made by instinct theory. More precisely I will compare Tooby and Cosmides with McDougall.

Tooby and Cosmides are forthright and firm in stating the propositions that constitute the project of evolutionary psychology and the aim of the enterprise. They list eight propositions as a termination to their introductory remarks in *The Psychological Foundations of Culture*. Below I have quoted the list in full. After each proposition I have quoted phrases from Chapter II of McDougall's *Social Psychology*, "The Nature of Instinct". (McDougall is quoted in italics so to aid clarity). The object of the exercise is, I hope, self-evident.

> a. the human mind consists of a set of evolved information-processing mechanisms instantiated in the human nervous system; (Tooby & Cosmides, 1992: 24)

The human mind has certain innate or inherited tendencies which are the essential springs or motive powers of all thought and action, whether individual or collective . . . (McDougall, 1908: 19).

b. these mechanisms, and the developmental programs that produce them, are adaptations, produced by natural selection over evolutionary time in ancestral environments (ibid: 24);

*These all-important and relatively unchanging tendencies . . . [*arise*] out of the constitution of mind and the nature of mental process in general, when mind and mental process attain a certain degree of complexity in the course of evolution (ibid: 20) . . . [*these tendencies*] are common to all members of any one species, racial characters that have been slowly evolved in the process of adaptation of a species to their environment (ibid: 20-21).*

c. many of these mechanisms are functionally specialized to produce behaviour that solves particular adaptive problems, such as mate selection, language acquisition, family relations, and cooperation (ibid: 24).

In the typical case some sense-impression, or combination of sense-impressions, excites some perfectly definite behaviour, some movement or train of movements which is the same in all individuals of the species and on all similar occasions; and in general the behaviour so occasioned is of a kind either to promote the welfare of the individual or of the community to which he belongs . . . (ibid, 26).

d. to be functionally specialized, many of these mechanisms must be richly structured in a content-specific way; (ibid: 24).

. . . the psycho-physical process that issues in an instinctive action is initiated by a sense-impression which, usually, is but one of many sense-impressions received at the same time; and the fact that this one impression plays an altogether dominant part in determining the animal's behaviour shows that its effects are peculiarly favoured, that the nervous system is peculiarly fitted to receive and to respond to just that kind of impression (ibid: 27).

e. content-specific information-processing mechanisms generate some of the particular content of human culture, including certain behaviours, artifacts, and linguistically transmitted representations; (ibid: 24)

*. . . that human nature has everywhere and at all times this common native foundation [*affords*] a much-needed basis for speculation on the history of the development of human societies and human institutions (ibid: 19)*

f. the cultural content generated by these and other mechanisms is then present to be adopted or modified by psychological mechanisms situated in other members of the population; (ibid: 24)

. . . social psychology has to show how, given the native propensities and capacities of the human mind, all the complex mental life of societies is shaped by them and in turn reacts upon the course of their development and operation in the individual (ibid: 18).

g. this sets up epidemiological and historical population level processes; and

h. these processes are located in particular ecological, economic, demographic, and intergroup social contexts or environments (ibid: 24)

. . . the understanding of the life of society in any or all of its phases presupposes a knowledge of the constitution of the human mind. . . (ibid; 18)

The similarity is undeniable. Tooby and Cosmides restate McDougall. The former place psychological adaptations at the centre of the enterprise to understand mind, behaviour, social relations and society, the latter placed instincts at the centre of the same enterprise. Let us take this demonstration of similarity and look further into a comparison between adaptations and instincts.

8.3 Instincts as adaptations

It is, I think, difficult to avoid the conclusion that the instinct theorist of the late nineteenth and early twentieth century took instincts to be adaptations in the Darwinian sense. This raises the question as to whether instincts just are adaptations. In this discussion we need to follow Bateson's (2000)[4] suggestion and ignore the fact that "instinct" has become an anachronism at best and a dirty word at worst[5]. We need to consider not the term itself but its definition(s).

Any comparison of the constituent propositions that comprise definitions of instincts and adaptations shows them to be very similar. Instincts have many of the properties one finds in a definition of adaptation, most notably, of course, the claim that instincts are the adaptive consequences of natural selection. Whilst I do not wish to say that "instinct" and "adaptation" are equivalent terms, to a first approximation I do wish to say that all instincts are adaptations but not all adaptations are instincts: that instincts are a sub-set of adaptations.

That a definition of "instinct" does not exhaust a definition of adaptation is readily apparent. Tooby and Cosmides take the same view.

> The study of adaptations can be broken into two halves . . . the study of adaptations that regulate behaviour – and physiology – the study of the morphological structures and processes . . . (Tooby & Cosmides, 1990: 383).

Adaptations include anatomical features and behaviours (broadly conceived as processes), whereas, as Darwin's definition suggests, instincts do not include anatomical features; i.e. the pelvis

in bipedal hominids is an adaptation but it is not an instinct. Thus, at a minimum, we may say that for the set A, Adaptations, we have the sub-sets a_1, instincts, "Adaptations that regulate behaviour", and a_2, anatomy, "morphological structures and processes".

However, this is too simple a picture. As we saw in chapter 7, section 3 'Adaptations, Exaptations, Spandrels and By-products', not all anatomical features are adaptations. A frequently offered example is the colour of bones (e.g. Buss, Haselton, Shackleford, Bleske, & Wakefield, 1998). Accordingly, anatomical features, taken as a set, includes the set of features that are not-adaptations. Similarly, not all instincts are adaptations when reflex actions are included as instincts. (For example, what is adaptive about laughter provoked by being tickled? Or something like the "ouch" response to pain?) Let us now look at one obvious implication of saying that all instincts are adaptations but not all adaptations are instincts. The implication according to an elementary account of sets is that any definition of a sub-set constitutes part of the definition of the set itself. In the scheme presented above,

$$def\text{:-}\ A = \{a_1, a_2 \ldots\ldots\ldots a_n\}$$

It follows that should the definition of any sub-set be incoherent or inadequate - in this instance, the definition of instinct, a_1 - the set itself is incoherent. In this instance, adaptation. These considerations, together with the similarities between Tooby and Cosmides' and McDougall, prompts the suggestion that evolutionary psychology, as an enterprise centred on the notion of psychological adaptation, has inherited the problems that beset the notion of a psychological instinct. It has inherited the state of affairs that obtained *c.*1930. There are, of course, obvious rejoinders to such a claim. One is that evolutionary psychology appears to have no place for the term instinct[6]. A second is that instinct theorists worked with a different version of evolutionary theory than do we today (see Appendix 2). However, I do not think that either response makes a comparison insensible. A third rejoinder to the proposal that Tooby and Cosmides' and McDougall argue much the same thing is that instinct theorists did not deploy (or were not in possession of) the central conceptual plank of the EEA. It is this third consideration that I want to address in the remainder of this chapter.

8.4 . . . and contrast

In his introduction to the third edition of *The Expression of the Emotions in Man and Animals,* Paul Ekman argues that the study upon which Darwin embarked in that volume was "rejected or simply ignored" because subsequent psychological and behavioural researchers asked questions differing in orientation.

> Darwin asked a question about emotional expression that few other scientists asked in his own time or since. Most scientists studying emotion and expression address the "what, "how" or "when" question. *What* expressions are shown for each emotion? *How* are they produced? *When* do they occur? Darwin also deals with these, but he was one of the first, and for a long time, the only scientist, to ask the "why" question: *Why* do expressions occur in a particular form? (Ekman, 1998: xxiv).

Ekman implies that the neglect of the "why?" question amounts to a repudiation of Darwin's approach. What needs to be pointed out is that the "why?" here has two related but slightly different senses. On the one hand it equates to function. On the other it equates to origin. As we have seen, for evolutionary psychology the two senses are combined. The EEA tells us about the origin of function.

As I hope to have shown, instinct theory was Darwinian and, so it follows, those who promoted it cannot reasonably be said to have rejected Darwin. Certainly, the instinct theorists were concerned with the adaptive role of instincts. However, if we take Ekman's sense of "why?" to be about origin and apply it to the instinct debate he may be correct. Whilst various contributors approached a discussion of mind in historical terms - that is, in terms of it having a particular natural history - the instinct debate is notable for its lack of interest in the actual natural history of the mind. Many instinct theorists insisted that instincts, and the minds that embodied them, were adapted. But there are very few arguments or concrete claims about the origin of these adaptations. Few depictions of what made it the way it is. The interest focussed on how it is now. Comparisons are almost invariably horizontal across extant species, and not vertical within genera. Only occasionally are primates or apes invoked as explanatory devices or examples. Let us briefly review Darwin's position regarding the relative importance horizontal and vertical explanation before looking at why instinct theorists eschewed the latter[7].

Darwin, although of the opinion that the resemblance of instincts and habits (i.e. unlearned vs. learned, inherited vs. acquired processes and/or behaviours) was "so close as not to be distinguishable", he also insisted on a distinction between the two by appeal to their origin. This was achieved by tying instincts to "corporeal structures" and, in light of doing so, allow for natural selection to act upon instincts and shape them just as it did anatomy.

> No complex instinct can possibly be produced through natural selection, except by the slow and gradual accumulation of numerous slight, yet profitable, variations (Darwin, 1859: 193).

The implication is clear. If one wishes to account for the presence of an instinct one must specify its adaptive role. But to discern its role as an adaptation, Darwin emphasised the need to specify the natural history of an instinct.

> Hence, as in the case of corporeal structures, we ought to find in nature, not the actual transitional gradations by which each complex instinct has been acquired – for these could be found only in the lineal ancestors of each species – but we ought to find in the collateral lines of decent some evidence of such gradations . . . (Darwin, 1859: 193).

The two key points here are the emphasis on the development of an instinctive behaviour over historical time – the past of the instinct in question explains its current exhibition, and the dependency on comparative evidence to show this. Darwin accepted that only in the "lineal ancestors of each species" can the proper origin of instincts be "ascertained without offering a solution to the case whereupon adequate collateral lines of decent" are absent. *The Expression of Emotions in Man and Animals* is a record of how Darwin deployed his suggested method. The accusation against Darwin's claims in that volume by parties particularly interested in the human case was anthropomorphism. Darwin invited the charge by implying a privilege to an analysis that relies upon the ancestors of the species of interest.

The obvious question, then, is why did instinct theory not attempt to reconstruct the past? I do not propose to offer a definitive answer to this question. However, two related possible reasons present themselves. First, I suggest that speculation as to the "original nature" of humans appeared to psychologists in the early part of the twentieth century as an outmoded and discredited exercise. Quite simply, it had too close a similarity to what we might call the "primeval" device so frequently used in social and political philosophy. Whether they agree or disagree on who founded social science and social psychology, where and when, most historians agree that by the late nineteenth century what had been the social philosophies wanted to become the social sciences (e.g. Allport, 1954/1968; Collier, Minton & Reynolds, 1991; Farr, 1978, 1991, 1995; Jones, 1985; Leahey, 1994; Manicas, 1987; Manstead & Hewstone, 1997; Robinson, 1981). The lynchpin of the change was the adoption of experimental methods (Farr, 1983b, 1985a). But other aspects of more traditional social philosophy were also rejected. Robinson (1981) suggests that one of them was an appeal to the dark and distant past of mankind that so characterised the influential philosophies of, for example, Thomas Hobbes, Jean-Jacques Rousseau and Karl Marx. Probably the most successful theorist in the twentieth century to rest so much of his case on a primal setting or scene is Freud.

Second, the knowledge base in what today we call palaeoanthropology did not encourage such as exercise. At the turn of the nineteenth and through the first quarter of the twentieth century palaeoanthropology was in its earliest infancy. The picture we have today of the *Homo* lineage is very largely a product of discoveries that have been made and techniques that have been developed since the second world war (Ardley, 1961; Rensch, 1972; Tattersall, 1995; Trinkhaus & Shipman,

1993.) In short, it seems that the instinct theorists, for two related reasons, thought it unscientific to construct the lineage of humans. This is one way in which evolutionary psychology and instinct theory contrast.

8.5 Physiological and psychological definitions of instinct

A major division which came to split the vote amongst advocates of instinct in the psychological and behavioural sciences centred on the psychological status of instincts. There was that school which took instincts to be essentially physiological in character, and another which took them to be marked by their psychological manifestation. This is not to say that theorists insisted on their being one thing or the other. Most took them to be both. The dividing issue was how they were best or most fruitfully characterised in the human case.

As our parade of definitions of instinct in chapter 4 showed, as well as being inherited, unlearned and adaptive, there was considerable agreement that an instinct manifests itself as or in a process – this is what was taken to distinguish an instinct from an anatomical adaptation. It taken to follow from this distinction that an instinct was not to be discerned in anatomy alone but in activity tied to anatomy (James, 1890). It was in this view that the material nature of instincts was emphasised. And it was implicit in this view that any given instinct may be described in terms of more than one type, or level, of process. This opened the way for classifications of physiological, behavioural and psychological instincts.

Just as did the majority of instinct theorists of the early twentieth century, today those who take a Darwinian view of mind and social behaviour take it as axiomatic that sex is fundamental to sexual reproducers including humans. In the first instance, this was indicated to instinct theorists by human anatomy: *viz.*, it was accepted, on the materialist conception and on the morphological stance inherent in Darwinism, that physiology was fundamental to behaviour. However, the precise nature of the sex instinct in humans – and not just its general presence in sexual reproducers – is specified by particular processes. It was at this point that the process and its characterisation became open to disciplinary orientation. So, for a physiologist, for a zoologist and for a psychologist the type or level of process differed. Following Darwin, psychologists typically introduced mind in the form of intelligence and/or emotion (e.g. James, 1890; see Fletcher, 1957). As we will see, the invocation of intelligence saved the psychologist from having to demonstrate instincts by recourse to specific behavioural patterns - intelligence allowed one to retain the notion of instinct in the absence of behavioural rigidity.

McDougall changed the nature of the instinct debate by capitalizing on, pressing further and emphasising Darwin's suggestion that mental states were to be considered instinctive. The problem McDougall introduced centered on the validity of invoking a primary place for instinctual mental states and their role whilst retaining the general argument for their initial presence. In other words, McDougall wanted to retain the authority of evolutionary materialism for a special case. He wanted to retain the orthodox standpoint of explaining the presence of humans and their characteristics in terms of an appeal to other species - i.e. ascribing to humans that said to obtain in non-humans – whilst qualifying the very quality of the ascribed common feature.

James, of course, preceded McDougall in claiming that instincts were exhibited in consciousness and subject to volition. This claim rested on the utility of consciousness in relation to the demands of instinct. In chapter 4, we left James' discussion at the point where he had emphasised the role of mental processes in instincts. I will gloss over James' argument as to how this state of affairs comes about,[8] but he is quite clear as to why it comes about: it is adaptive to be adaptable. For James, actual situational and situated adaptability demands that instincts be governed. It is through this reasoning that James came to pose the conscious mind as a product of natural selection.

> Thus, then, without troubling ourselves about the word instinct and reason, we may confidently say that however uncertain man's reactions upon his environment may some-times seem in comparison with those of lower creatures, the uncertainty is probably not due to their possession of any principles of action which he lacks. *On the contrary, man possesses all the impulses that they have, and a great many more besides.* In other words, there is no material antagonism between instinct and reason. Reason, *per se*, can inhibit no impulses; the only thing that can neutralise an impulse is an impulse the other way. Reason may, however, make *an inference which will excite the imagination so as to set loose* the impulse the other way; and thus, though the animal richest in reason might also be the animal richest in instinctive impulses too, he would never seem the fatal automaton which a *merely* instinctive animal would be (ibid: 393. Italics in the original).

Thus, for James, instincts in human beings were only going to be fully intelligible when their rational management is considered and the manner of this management is understood. This enterprise is possible by virtue of it being carried out consciously, for James took the management of instincts to be open to view.

Extending James' analysis, McDougall claimed that instincts gave human consciousness its particular flavour(s). It rested on the association of instinct with emotion, taking its support from Darwin's *The Expression of the Emotions in Man and Animals*. This is how McDougall reached his position:

> . . . instincts are more than innate tendencies or dispositions to certain kinds of movement. There is every reason to believe that even the most purely instinctive action is the outcome of a distinctly mental process, one which is incapable of being described in purely mechanical terms, because it is a psycho-physical process, involving psychical as well as physical changes, and one which, like every other mental process, has, and can only be fully described in terms of, the three aspects of all mental processes – the cognitive, the affective, and the conative aspects; that is to say, every instance of instinctive behaviour involves a knowing of some thing or object, a feeling in regard to it, and a striving towards or away from that object (McDougall, 1908: 26).

Opposed to a psychological account of instincts and prominent amongst those who sought to exhaust instincts via a physiological account were Thorndike and Watson. Alongside Loeb, both eschewed the notion that volition or experiential considerations play any role in the expression of instincts, holding instead to a mechanistic view. Although both subscribed to a reflex definition, they did not agree that an instinct was a reflex. Thorndike (1913) argued that as a reflex an instinct was a specifiable and specific nervous reaction. Watson argued that as a reflex an instinct was a behaviour.

> The term reflex is a convenient abstraction in both physiology and behaviour. . . We mean by reflex when used in this way [neurology] that action takes place under appropriate stimulation in some fairly circumscribed glandular or muscular tissue. It is an abstraction because reflex action in the eye, the leg, the hand or foot can never take place in isolation. Action is altered in other parts of the body as well (Watson, 1929: 264).

Watson's point in opposition to Thorndike was that instincts are reactions of the whole organism as opposed to abstracted parts thereof. But both maintained that instincts must remain tied to anatomy and physiology and that no amount of physiological complexity makes an instinct subject to conscious control and no inspection of consciousness would render an account of their operation.

Dunlap (1919) made a similar point. He also pointed out the consequences. Dunlap saw the move from taking physiological patterns as instinctive (wherein an instinct was inherited, adaptive and species typical - a proposition with which he had no contention) and the assumption that these patterns were grouped according to broad end states, to specifying instincts as psychological realities as illegitimate. In other words, he argued that one cannot build psychological content out of known physiology and supposed purpose without tying those purposes to specific physiological process. It was Dunlap's view that if this dictum is disobeyed any given physiological process can be used again and again for different purposes. The result at the psychological level being that although differentiated functionally, the psychological account has no definite physiology. Dunlap insisted on the link between anatomy and process as had Darwin.

It is important to make it clear that whilst some authors sought to emphasise the physiological and others the psychological aspects of instincts, most were content to entertain both aspects (see Helson, 1951; Klineberg, 1954; O'Donnell, 1985; Roberts, 1987). A typical accommodation between mechanistic and conscious, physiological and psychological aspects was the claim that the relative importance depended on the particular instinct in question. However, for those seeking a definitive, or "meta", definition, this accommodation blurred the issue (e.g. Kuo, 1921) and encouraged recourse to the anatomical and physiological evidence.

The invocation of the "distinctly mental" as opposed to the "purely mechanical" led to scepticism of instincts as psychological phenomena for much the same reasons that introspectionism was rejected (Collier, Minton & Reynolds, 1991). This was despite the fact that, whilst not so antagonistic toward introspectionism as was behaviourism, instinct theory sought to account for mind in a distinctly different way. Indeed, the allusions of James notwithstanding, as Charles Myers argued, instinct were *prima facie* instances of mental content not open to introspection:

> Man is never aware that he is acting instinctively . . . When a mother sacrifices her life to save her child, does she recognize that she is acting instinctively or unintelligently? . . . From our own introspection we can only answer negatively (Myers, 1910: 215).

Nevertheless, Watson's took McDougall to be an introspectionist. Their exchange in 1929 shows this clearly. Watson began his critique of McDougall under the heading "The Religious Background of Introspective Psychology". The one phrase of McDougall's that Watson quoted was, "Psychology is the description and explanation of states of consciousness as such". In the remainder of the paper Watson is adamant that any psychology, regardless of the quality of its underpinnings, is lost once it centres its attention on "the soul". Watson did not deny instincts, but he did reject the notion that they are felt sensations open to the view of the possessor. The approach to instincts that saw them as mental exhibits was a casualty of behaviourism and its influence.

The majority of instinct theorists and their critics were content to say "without hesitation" that instinct was "any responses which have not been learned" (Dunlap, 1919: 307). Similarly, instinct was taken to be a biological adaptation (Kuo, 1921; Bernard, 1924). To say that an instinct was an unlearned response and an adaptation was to say what it was, but critics of psychological instincts drove proponents back from that position and demanded to know what an unlearned response was and what a biological adaptation was in psychological terms (Dunlap, 1932). To say that a thought was an adaptation or a sequence of thoughts was an unlearned response was deemed illegitimate because such notions could not be anatomically or physiologically specified.

Whilst the physiologically and the behaviourally inclined authors were by no means in agreement on the proper definition of instinct, there was very considerable overlap in what sorts of phenomenon qualified, e.g. sex, aggression, fear, greed, parental solicitude and altruism. I would contend that most took it that physiological evidence would eventually settle the issue of proper definition, classification and numeration. This follows from the basic and global acceptance that there were such things as instincts – naturally selected, inherited, functionally discrete, goal directed features that achieved their ends via behaviour. The acceptance of instincts prior to empirical verification of any given one of any given type in any given species was licensed by Darwin's own approach: one is able to make sense and build classifications of species and their behaviours subsequent to the acceptance of naturally selected "collateral" forms. I would also contend that those psychologists who promulgated instincts were, in general, similarly inclined to accept those points. That is, they too took instincts that manifested themselves in mental content to be naturally selected, inherited, functionally discrete, goal directed features that achieved their ends via behaviour[9].

But those who promulgated instincts as a variety of mental content injected heat into the debate theretofore not present. Animal behaviour – and we must be careful here not to conflate the study of animal behaviour pure and simple with the study of animals for comparative purposes – at the turn of the nineteenth century was very similar to modern ethology in that it did not impute cognition in its object(s) of study. The basic and default assumption was that should there be any cognition in the animal it was epiphenomenal.

I specify the turn of the nineteenth century purposely. As Richard Richards (1987) has detailed, French zoologists in the 18th century had reacted against the Cartesian notion of reflex and the dichotomy it presented with mind. Extrapolating from what they took to be Locke's position on animal intelligence, the "Sensationalists" claimed that what appeared to be instinctive behaviour exhibited intelligence and reason and not mechanical inflexibility. Their challenge to the Cartesian view was simple: prove that physiological reflexes can result in elaborate and extended courses of action that differ from case to case, from organism to organism, from time to time (e.g. day to day, week to week, month to month, year to year). This challenge was posted as a bluff – a bluff based on parsimony. That is to say, whilst there were undoubted problems in investing reason in "lower" animals – how much? of what sort? toward, precisely, what end? – the supposition of rationality was, nevertheless, a more elegant solution than building reflex chains of indefinite length for indefinite instances of each type of behaviour.

Richards pre-history of the term instinct offers a clue as to why the proponents of psychological instincts injected so much heat into the debate of the early twentieth century. Whilst there were

thorny empirical issues embedded in the claim that a resolutely materialist standpoint could account for mind, to the materialist mind-set of the immediate post-Darwinians there were as many thorny philosophical issues embedded in the claim that mind was an element in natural selection. One reason why James Drever (1917) was so anxious to detail what I have called the pre-history of instincts was due to his wish to avoid a repetition of what he took to be a sterile debate.

Despite the best efforts of James and McDougall, those accounts of instincts that laid emphasis on mind and its role were seen as being at odds with the basically mechanistic tenet of evolution. The invocation of the mental was taken as retrograde and as flowing from an underestimation of the complexity of naturally occurring physiological systems. What I have called the psychological version of instinct theory was rejected not because it sought to accommodate flexibility but by the concepts it employed to do so. The failure of those who deployed this concept to agree on its nature and role seems to have been taken by opponents as evidence of its vacuity. Bernard's (1924) insistence that terms such as intelligence, affect and emotion be purged and replaced by "mechanisms" more or less fixed or more or less fluid arose from what he took to be confusion over the place of the conscious element in instinct. He saw the confusion as being between consciousness as epiphenomenal and consciousness as a modifying force which adjust instincts to particular environments. If it is seen as the former it is scientifically redundant; if it is the latter then instincts only occur once in a "pure form", leaving the rest of the organisms behaviour open to an alternative account.

We will look at Tooby and Cosmides notion of a psychological adaptation shortly. Before doing so let us look at the treatment the idea of a psychological instinct received at the hands of Luther Bernard.

8.6 How many instincts are there?

Bernard's *Instinct: a study in social psychology* (1924) – a volume that Collier, Minton and Reynolds (1991), and Richards (1987) judge to be amongst the most influential critiques of instinct theory – zeroed in on the decoupling of instincts and anatomy. It showed what happens to a concept that is, essentially, biological when it ceases to be constrained by biology.

McDougall had anticipated the central problem. In Chapter II of *An Introduction to Social Psychology* he takes to task unspecified "contemporary writers" "of considerable philosophical culture" for making use of the terms instinct and instinctive, "but, with very few exceptions, they use the them so loosely that they have almost spoilt them for scientific purposes".

> On the one hand, the adjective "instinctive" is commonly applied to every human action that is performed without deliberate reflexion [*sic*]; on the other hand, the actions of animals are popularly attributed to instinct . . . Hundreds of passages might be quoted . . . to illustrate how these two words are used with a minimum of meaning . . . (McDougall, 1908: 21).

McDougall's strategy against those he accuses of misusing the term instinct was a form of *reductio ad absurdum.* He went on to offer "obscure" and "incoherent" examples. These include the instinct of ancestor worship, the teetotaller instinct, an acquired political instinct amongst Russians and the instinct of contradiction. He moved on to plea that others acquire a habit of restraint when using the terms because, "there can be no understanding of the development of individual character or of individual and collective conduct unless the nature of instinct and its scope and function in the human mind are clearly and firmly grasped" (ibid: 22). But this plea for constraint was met with objections to the terms and conditions of definition that McDougall then offered. Most of these objections took the form of alternative proposals and alternative lists of instincts. Following through on McDougall's suggestion that "hundreds of passages might be quoted . . . to illustrate how these two words are used with a minimum of meaning", Bernard sought to show that the term instinct when conceived of as psychological produced vacuous science.

In chapter VII, "Usage of the term Instinct", Bernard attempted to filter out those usages that specified "acts which are definitely inherited and which may be properly termed instinctive" from those that were "a general and indefinite employment . . . not necessarily descriptive of a concrete act at all", "cases covering automatic and habitual actions", and "stimulus-response activity". Given that he had not room enough to simply display the instances and definitions that he had discovered, Bernard devoted chapter VIII, "The Classification of Instincts", to a discussion of his method of condensation and classification. He points out that the main difficulty in constructing a method flowed not just from sheer volume but also from the absence of any discernible scheme for classifying instincts in general and even such an absence in the writings of individual authors[10].

> In many cases it is a sort of catch-all for vague and indefinite ideas about the causes or relationships of activities. Writers, unable to account clearly for the occurrence of a particular behaviouristic phenomena on a purely objective basis, bring in the term instinct and use it as a charmed word, thus sidetracking further responsibility for an explanation (Bernard, 1924: 172).

Bernard claimed that, against his better judgement, he was forced to order his data at the risk of misrepresenting it because he identified so many instincts in the literature he surveyed that in a volume that runs to five hundred and fifty pages, he was unable to find room enough to print them all. Furthermore, he was at pains to point out that the survey was not exhaustive,

> On the contrary . . . it has been his [the author] experience that the list of classes and cases grows constantly, with almost undiminished rapidity, as additional authors are consulted. In the industry of collecting instincts there seems to be no law of diminishing returns (Bernard, 1924: 180).

He reinforces his point by calling the chapter "Some Results of Investigation".

The two main forms of tabulation that Bernard employed were "Classification of Instincts by General Group" and "Specific Instincts Classified into Groups". In the classification of instincts by general group – what he called the "classification of classifications" he presents,

> No specific instincts . . . all references are to instincts in the plural, that is, to groups or classes of instincts . . . No attempt is made in this classification to distinguish between groupings of instinct on the basis of the general or specific functions served by them; that is, whether they are specific to the function or form from which they are named or whether they serve their function derivatively and by adaptation (ibid: 180).

In effect Bernard's method was to take the 14, 046 individual instance of instinct (gleaned from 412 authors and 495 books) and group them on the basis of synonymy. That is, he grouped individual instances on the basis of the meaning of the term attached to the term instinctive. For example, individual instances "bad, base, debased, debasing, depraved, evil, immoral, undesirable, vicious, wrong were thrown together" (ibid: 177). The "Classification of Instincts by General Group" lists a total of 185 general types of instinct. In order to nuance these types in tune with authors own usage, most of these types were presented by Bernard as consisting of more than one class. For example, as a type, the "male" instinct has two classes and the "female" three classes. The type with the most classes is "human" with 25. Types with only one class include "Bohemian", "Healthy", "Mechanical", "Spiritual" and "Vitiated". In "Miscellaneous" types there are 139 classes. The total number of classes of instinct is 849 presented by 250 authors. Bernard concludes the presentation of his classification of classifications by reminding the reader that it is highly condensed.

The tables that comprise "Specific Instincts Classified into Groups" employ an alternative method. Seeking to preserve as much original and actual terminology as possible, and guided by the rule that the specific instincts included were "specific inherited activities of individuals . . . of a biological character" (ibid: 188) for editorial purposes, Bernard constructed groups of instincts oriented toward some intelligible general activity. For example, in the group "The Aesthetic Instincts" Bernard includes the specific instincts of "artistic", "for the beautiful" and "rhythmic". The "Specific Instinct Classified by Groups" yields a total of 23. Here is the full list. Three examples of the specific instincts that comprise each groups are added for illustration (following Bernard's advice, it may be

helpful for prefix each example with "instinct for . . ." or "instinct . . . of"). The total number of instances (or classes) for each type are added after the examples.

The Aesthetic Instincts – artistic, for the beautiful, rhythmic	51
The Altruistic Instincts – affection, pity, sacrifice	44
Anti-Social Instincts – aggression, criminal, selfishness	100
Instincts of Disgust or Repulsion – avoidance, rejection, retching	39
Economic Instincts – accumulation, acquisition, thrift	60
Ethical Instincts – fair-play, honesty, justice	27
Family Instincts – nesting, defence of family, filial	83
Fear and Flight Instincts – clinging, escape, shyness	87
Food Instincts – chasing, gathering, hunting	64
Gregarious or Social Instincts – association, conformity, fidelity	149
Intellectual Instincts – curiosity, juridical, ratiocination	106
Imitative Instincts – mimicry, suggestion[11]	16
Migratory and Climatic Instincts – home-finding, nomadic, running away	31
Play Instincts – adventure, gambling, recreation	44
Recessive and Repose Instincts – asceticism, solitude, sheltering	26
Religious Instincts – faith, regard for a higher power, worship	30
Retaliative Instincts – anger, defiance, revenge	41
Self-Abasement Instincts – acquiescence, confession, submission	53
Self-Assertive Instincts – action, ambition, elation	170
Self-Display Instincts – adornment, rank, vanity	37
Sex Instincts – carnal, chastity, courting	130
Workmanship Instincts – construction, manipulation, planning	63
Miscellaneous Instincts – heliotropic, thalassophilia, throwing things	144

In the grand "Summary of the Classifications of the Specific Instincts" the 23 types break down into 1594 classes and 5684 individual cases gleaned from 388 publications written by 323 authors.

Bernard offered two further alternative methods of sorting instincts for classification, the "Indefinite and peculiar instincts" and "Instinctive attitudes". By the former he meant "not specific instincts . . . but characterisations of the qualities or nature of origin of such instincts" (ibid: 219) and reports that this method yields 843 types and 2238 individual cases. By the latter he meant "all activity or attitudinal processes containing the terms "instinctive" and "instinctively", instead of "instinct"" (ibid: 219) and reported 2474 types and 3585 individual cases. These last two surveys enjoy a fraction of the space and discussion afforded to the first two.

It is in the "Summary of the Classifications of the Specific Instincts" that Bernard presented the upper and lower estimates of instincts in the literature he surveyed. The lower estimate of 23 discernible groups (keeping in mind that the 23rd is a catch-all group containing 19 types and a further miscellaneous of the miscellaneous type comprised of 96 classes and 126 individual cases) carried the disadvantage of doing violence to the actual texts that supplied the data. The upper estimate is 5684 individual cases carried the advantage of preserving each application of the term he came across. It was in the distance between the two estimates that Bernard displayed "the indiscriminate chaos of instincts".

Bernard appealed to the exhibition of psychological instinct in use to show the concept to be absurd. Moreover, he also quantified the absurdity. As a scientist, his form matched the function of his argument. Bernard believed in instincts but (and this may come as a surprise to Tooby and Cosmides given that he was a sociologist) he thought that the place to start was in the nervous system and not with psychological content. *Instinct: a study in social psychology* was put forward by Bernard as an account of what happens when the link between instinct and physiology is broken. Whilst there were those who objected to the concept outright (e.g. Ross, 1908; Kuo, 1929), Bernard showed that the difficulties of constraint, definition and demonstration were generated by proponents. Eventually the seeming inability of psychological theorists to constrain the concept of instinct or to discipline its employment led critics into rather caustic language.

> Man is impelled to action, it is said, by his instincts. If he goes with his fellow, it is the "herd instinct" which actuates him; if he walks alone, it is the "anti-social instinct"; if he fights it is the instinct of pugnacity; if he defers to another, it is the instinct of self-abasement; if he twiddles his thumbs it is the thumb-twiddling instinct; if he does not twiddle his thumbs, it is the thumb-not-twiddling instinct. Thus everything is explained by magic – word magic (Holt, 1931: 4. Cited in Klineberg, 1954: 66).

8.7 Adaptations as Idealisations

It is, perhaps, a peculiarity of the instinct debate that the usefulness and tractability of the concept of psychological instinct deteriorated with discussion. However, the discussion did persist, and persists today in the form of "adaptionism", because of the sheer and apparent obviousness of the reality of instincts in man and animals. In light of Darwin it seemed, and in light of the modern synthesis it has become obvious, that distinct, enduring and species-typical anatomical features of organisms are "for" something. However, in animals such features, in order to be for something, need to do something.

The doing is what was and is typically called physiology. Physiology is the study of process. It seeks to provide law like generalisations about what anatomy does. By virtue of physiological processes, organisms behave. Behaviours that were thought to be distinct (i.e. distinguishable), enduring and species-typical formed, and form, the basis of the term instinct (McFarland, 1993). The formal or technical use of the term instinct was reserved for those behaviours that could be unproblematically tied to anatomy and physiology and, thus, to heritability and natural selection.

All behaviours depend upon anatomy and physiology, and the working default assumption is that all anatomy and physiology has a natural history of selection. The concept of instinct rested on the dependence of behaviour on anatomy and physiology. This dependency licensed the inference that behaviours too were heritable and naturally selected. However, the concept and mental states that appeared to be candidates could not be distinguished from behaviours that could not be candidates. The result were monsters such as, say, the instinct of filial piety (Bernard, 1924). The long career of the reflex arc shows how keen was the desire to establish behaviours as adaptations. Instinct theorists, and William McDougall in particular, shared with Tooby and Cosmides a dislike for the notion of adaptiveness because, rightly in my view, a species of instinct fell because proponents were unable to show human behaviours to have the same properties that permitted anatomy and physiology to be classed as heritable and naturally selected. The notion of a psychological instinct fell on the same sword. Instinct theorists could not show that behaviours and mental states as processes were amenable to law like generalisations and, accordingly, could not show what they were for and about in the functional terms demanded by Darwinism. Behaviours or a phenomenon is not properly evolutionised (that is, in the rubric of an evolutionary account) *post hoc* (that is, after the fact whereupon, ultimately, the "fact" is the established reproductive success of the organism or definite contribution to reproductive success of a given facet or behaviour). Because human social behaviour would not bend to the conditions demanded by the term instinct, no general species-specific account of what to expect of and from humans in the way of social behaviour was

forthcoming. This encouraged the retreat into the head and the search therein for instincts initiated by James and exemplified by McDougall.

At this point I want to pick up the contemporary story again. I want show how Tooby and Cosmides and evolutionary psychology address the problem faced by earlier proponents of psychological instincts.

Tooby and Cosmides are unequivocal in calling themselves adaptationists and calling for psychology to be about adaptations. Similarly they are unequivocal about the universality of adaptations: if a proposed mechanism is not universal it is not an adaptation. In this section I want to address the consequences of these claims for researchers wanting to construct an evolutionary social psychology. I will argue that the price of universality in light of the fact that manifest human behaviours do differ is idealisation, and price of idealisation is that social psychology is uncoupled from behaviour. I will recommend that evolutionary social psychology rejects the notion of adaptation as construed by Tooby and Cosmides.

From a Darwinian perspective, the answer to the question as to how universal is universal is two-fold. One the one hand, it is not a question at all: human nature is universal to all those organisms properly classed as modern humans. Our confidence in this statement is derived from and equivalent to our confidence in our taxonomy. On the other hand, the question invites the reply that there is no strict equivalence between individuals by virtue of recombination. Courtesy of sexual reproduction and drift we are all of us genuinely unique. In the hands of Tooby and Cosmides, the term universal is closest to the first of these responses, but it is not exactly the same. They explicitly reject the latter position. Let us first look at their reasons for doing so.

For Tooby and Cosmides that which is universal is some or another idealisation of the human genome conceived as a design. As a design, the idealised genotype and consequent phenotype and behaviour does not permit of variability – or, at least, the variability is inconsequential in comparison to that about which variability is hinged. Thus,

> Nearly all of population genetics consists of the elaboration of a mathematics to describe the varieties of genetic change and ongoing selection. . . In population genetics, designs show up purely as some allele or combination of alleles, that is, as part of some system of genetic variation. As alleles become fixed they tend to disappear from the analysis, leaving the accumulated uniformity of the evolving organisms complex design invisible to these tools of mathematical analysis (Tooby and Cosmides, 1990: 380).

Notice the distinction between "designs" and their exhibition – combinations of alleles. Tooby and Cosmides argue that too close an interest in actual genotypes inevitably leads to a misleading concentration in that which varies between individuals or samples. A consequence of this for social and behavioural science has been that,

> . . . empirical studies tended to focus on related phenomena that were observable: the distribution of genetic variation; the relationship between genetic variation and phenotypic variation; the patterns of variability within and between populations; fitness differentials between individuals. . . For this reason, there are many studies of such phenomena as environmental gradients associated with genetic or phenotypic gradients. But when a gene reaches fixation it no longer creates heritable differences between individuals; at that point it disappears from the analytic scope of the study of variation. Consequently, present variation in design and ongoing selection was visible to these methods, whereas the uniform design reflecting already completed selection was invisible. Unfortunately, the vast preponderance of organic design representing the accumulated effects of four billion years of selection reflects completed rather than ongoing selection. To study variation is to bypass most of the structure of complex functional design (Tooby and Cosmides, 1990: 380-1).

Their point is that one cannot find adaptations in the differences between genotypes because,

> An adaptation is more than a mere collection of phenotypic properties which, in a particular individual, happen to have the effect of enhancing reproduction . . . An adaptation is a recurrent design that reappears across generations and across individuals . . . This means that the phenotype of an individual organism must be carefully distinguished from the design of the phenotype – fitness should be assigned to designs, not to individuals (ibid: 394).

Furthermore, by arguing that it is "logically necessary . . . [to] redescrib[e] the variable and the transitory [i.e. the actual] in terms of that which is recurrent and stable . . . Thus, individual phenotypes are instances of designs, not designs themselves . . . to recover adaptive design out of behavioural or morphological observations, one needs to determine what is variable and what is invariant across individuals" (ibid: 395), Tooby and Cosmides relegate actual properties of actual organisms in favour of recurrent composites derived from the natural history of species. This approach leads to a situation whereupon adaptations may be specified in the absence of any extant instance of such an adaptation. Accordingly, the price of universality is idealisation.

As idealisations, adaptations are robust against variations in "output" or performance, they are robust against variations in output due to temporal and spatial exigencies – i.e. against the fact that they are subject to different "input conditions" due to historical, geographical and cultural variation – and they are robust against individual differences in performance despite the reasonable expectation that they are individually instantiated organism to organism. Additionally, adaptations as idealisations are robust against outputs that, *prima facie*, may be described as maladaptive. There is no obligation to show any design to be extant. Adaptations as idealised designs do not need to be defined ostensively. As a consequence, whilst the claim is that adaptations are a universal property of human design, they need not be treated literally. Tooby and Cosmides are asking us to refrain from

surprise if, in actuality, we unable to show a concrete instance of a proposed psychological adaptation.

The cost, in my view, is not acceptable. To cast psychological adaptations as idealised designs isolates and inoculates them from descriptions of actual minds and the output of those minds – and there is the rub for social psychology because surely social psychology is interested in output, in behaviour. This interest in not just an obligation imposed by method (our window into heads is behaviour) and the need to substantiate psychological theories by recourse to eliciting, observing and analysing behaviour. The interest in behaviour stems from the fact that social psychology in concerned with the interaction, action and reaction of actors.

I am of the view that adaptations as presented by Tooby and Cosmides will run into the same sand as did instincts as presented by McDougall. Conceptually decoupled from definite behaviours, all constraints are off aside the need to depict them in mechanistic counterfactual term. The key danger is that there is no way of keeping a lid on the number of adaptations that may be posited. I suggest that the eventual result will be a volume in the spirit of Bernard: "Adaptations: a study in social psychology".

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[1] The reification (or hypostatization) objection: This objection takes two forms. The first of these is the claim that in naming a phenomena, i.e. a pattern of behaviour taken to be species wide, species specific, heritable and, against some criteria, adaptive, one does not thereby explain it. The second form of the hypostatization objection is the claim that the act of classification is taken as a fact of that classified that then requires explanation.

In the general case, hypostasis it is said to be fallacious because it supposes that a concept of a set containing specific sub-sets plays a causal role in the explanation of those sub-sets. The application of the general case to instincts involves, in the first instance, the claim that organisms are instinctive[1]. This broad characterisation is then substantiated by appeal to individual example of instincts e.g. sex, fear, self-preservation. The fact that organisms are instinctive is taken to be responsible for the individual cases.

The commonest expression of this "error", and the one which Thorndike (1913) highlighted, involved the demarcation of certain behaviours (and, often, mental states or dispositions that support them) under an instinctive rubric – sex for example. This process yields the claim that behaviours/dispositions *a, b, c . . . z* equal the sex instinct, *S*. Explanatory force is then sought in the claim that *S* causes *a, b, c . . .z*. As Thorndike suggests, this slippage gives the impression that the causal factors for given behaviours are established when all that has actually been achieved is a categorisation of those behaviours as being of a type.

This criticism does not necessarily amount to a denial of instincts – Thorndike did not deny them. What it does deny is that naming and knowing are equivalent, and in doing so it cast doubt on the scientific utility of

the concept of instincts. It eventually led others, such as Bernard (1924), to cast doubt on the activity of classification given that it could not yield causal accounts – an enterprise Katz (1937) came to see as "irritatingly sterile". But not all took that view. Whilst labelling behaviours and dispositions as being instinctive did not thereby explain them instinct theory remained influential because it stipulated that to be explained. This moves us on to the second form of the hypostatization objection; acts and facts of classification.

We can look to Dewey to clarify the distinction between the act of classification and the facts supposedly grasped. Casting a net beyond psychology and the instinct debate, Dewey was of the view that, "The tendency to forget the office of distinctions and classifications, and to take them as marking things in themselves, is the current fallacy of scientific specialism" (Dewey, 1922: 131). He took this tendency to be at its height amongst instinct theorists with the result that a most obvious fact was ignored:

> Man has been resolved into a definite collection of primary instincts which may be numbered, catalogued and exhaustively described one by one. Theorists differ only or chiefly as to their number and ranking . . . But in fact there as many specific reactions to differing stimulating conditions as there is time for, and our list are only classifications for a purpose (ibid: 132).

Dewey argued that the root of the "mythological psychology" of instincts resided in the mistake of reifying abstract classes of properties from what are actually lists of tautologies. Here is how he treats the instinct of "self-love".

> Animals, including man, certainly perform many acts whose consequence is to protect and preserve life. If their acts did not upon the whole have this tendency, neither the individual or the species would long endure. The acts that spring from life also in the main conserve life. Such is the undoubted fact. What does the statement amount to? Simply the truism that life is life, that life is a continuing activity as long as it is life at all. But the self-love school converted the fact that life tends to maintain life into a separate and special force which somehow lies back of life and accounts for its various acts (ibid: 135).

The accusation is clear. Dewey argued that in the process of stipulating what animates life – instincts – one needs to go beyond the evidence and assert that life is constituted by discrete forces not witnessed in the actual ongoing behaviour of the organism in question. To follow the example, to say that a given behaviour has the effect of preservation does not legitimate the claim that the organism is comprised, in part, of a force to the end of self-love. It was Dewey's contention that such a move results only in propositions that not only assume the validity of the conclusion from which they are drawn but that are repetitions of it.

Dewey's criticism resolved into a question concerning the sense of what we might call a compositional account of mind and behaviour. Whilst he accepted there to be some analytical sense in supposing that organisms are comprised of discrete dispositions and behaviours, the utility of this approach is brought into perspective and curtailed should one try to conceive of an organism complete except a given instinct. His

question was, can one deduct a discrete instinct from a given organism? He suggested that we would have no idea as to what an organism would behave or think like. He took it as an indication that the proposition of discrete instincts is incoherent.

It is often said that academic psychologists in the first part of the twentieth century were averse to the past of their discipline. This may add substance to Fletcher's (1957) claim that the hypostatization objection was most influential in the guise presented by Field (1921). In a manner comparable to that of Falk (1981) sixty years later in the paper *Is Sociobiology Neophrenology?*, Field's *Faculty Psychology and Instinct Psychology* accused the latter of being equivalent to the former. The difference between the two being that faculty psychology was probably better attested empirically and was not plagued by anthropomorphism.

[2] Anthropomorphism: Formally, to anthropomophise is to make ascriptions of mental state known or said to hold in humans to animals (or to inanimate objects). Darwin's thesis in *The Expression of the Emotions in Man and Animals* was attacked on this point even by those who accepted species continuity. The logical flaw of anthropomorphism resides in the inconsistency of holding that two entities are at once distinct and identical. Such an objection may, in principle, be raised against any form of comparative analysis.

Although to anthropomophise is to ascribe properties said to obtain in humans to non-humans, there are closely related varieties of the same reasoning. For example, a comparative analysis between cultures said to be distinct involves the ascription to one culture properties that are held to obtain in another. But the variety that I wish to focus upon for present purposes involves a simple reverse of the definitional case: The ascription to humans of some property said to obtain in non-humans.

Sometime after the debate in psychology, with hindsight buttressed by advances in ethology Beech claimed,

> . . .that this war over instinct was fought more with words and inferential reasoning than with behavioural evidence . . . most of the battles of the campaign were fought from the arm chair in the study rather than in the laboratory (Beach, 1955: 409).

Fletcher (1957) and Richards (1987) make comparable claims. But they are not entirely accurate. Whilst statements about the human case were openly theoretical, there is a striking similarity with contemporary discussions in that evidence was liberally cited and examples given from species other than humans. The most popular source species were insects and birds. Very few authors support an argument, definition or classification by recourse to evidence from primates. Carr (1910) saw the problem as obvious. The need to suppose that humans shared with other (mostly distant) species certain key properties in order to explain pertinent human behaviours and thought indicated to him nothing so clearly as it did the absence of evidence that humans consisted of and were determined by instincts. Carr took the appeal to other species as an admission that human were a distinct phenomena and that this admission left comparisons open to conscious or unconscious anthropomorphism. His tone is essentially one of incredulity.

> I cannot understand what is meant if it really be asserted that the instincts of ants and bees have a concomitant aspect of intelligence. There is nothing in our own intellectual life at all analogous to

these activities, and they are completely different to any aspect that our own intelligence can be made to present (Carr, 1910: 231).

[3] Critics did not object to the idea human beings and other species were driven, orientated or organised toward the achievement of, say, sex: i.e. that sex was a goal. However, they did object to the view that these goals were a psychological fact. This objection rested on the supposition that goals did not seem to be self evident by virtue of anatomy or by virtue of their not being species or group universal. The idea of a pre-specification of psychological function provoked the objection that such a method was teleological.

For example, Dunlap (1919) was keen to emphasis that whilst instincts could be classified according to different stances, not all such classifications made sense. The classifications he saw as most important were what we might call "instinct for" and "instincts as". Dunlap maintained that instincts taken as being for something led to them being defined teleologically, whereas instincts taken as being constituted by something led to them being defined physiologically.

> It makes considerable difference whether, for example, we consider the "feeding instinct" as made up of the activities which result in food being secured; or consider it as a certain physiological group of activities which . . . is defined by its actual reaction character, and in that way distinguished from other physiological groups (Dunlap, 1919: 307).

Dunlap's later work also criticised what he called the "teleological approach" to instincts on the grounds that it presumed the very thing that general psychology sought to discover. To post physiological and/or psychological processes as being in the service an instinct defined by its function was to presume that the function was established. Seeing McDougall as the main culprit, Dunlap wrote,

> The constant tendency in social psychology is to consider these convenient groupings, arbitrarily made, as if they were series of natural and generic distributions on the psychological level, and to deduce an important set of deductions from the classification adopted (Dunlap, 1919: 309).

Dunlap's objection to functional, or "end state", accounts of instincts fed upon what he saw as "plain and deplorable fact" that the teleological classification inherent in a psychological accounts of instincts hinged on the outlook and taste of the classifier. Dunlap took the different classifications of different psychological accounts as evidence that even the most paired lists of instincts reflected the values of the classifier, not any basic consensus on the nature of humans and their needs or wants.

[4] Bateson's paper is titled *Taking the Stink Out of Instinct*

[5] Bateson provides a list of properties that either singly or in various combinations amount to an instinct. It tallies neatly both with a summary definition of instinct *c.*1925 and with contemporary definitions of instinct.

1. Present at birth
2. Not learned

3. Develops before it can be used

4. Unchanged once developed

5. Shared by all members of the species (or the same sex and age)

6. Organised into a distinct behavioural system (such as foraging)

7. Served by a distinct neural module

8. Adapted during evolution

9. Differences between individuals are due to genetic differences

[6] A brief inspection of the indices of contemporary texts and compendia of evolutionary psychology and social science suggests that I am mistaken in this claim. There is no entry for "instinct(s)" in the indices of the major compilations of evolutionary psychology; e.g. Barkow, Cosmides & Tooby, 1992; Betzig, 1997; Simpson & Kenrick, 1997; Crawford & Krebs, 1997. If and when the term is discussed at all it is typically done so in terms of species considerably removed from humans (e.g. insects) and it is done so in term of algorithms (e.g. Pinker, 1997).

For evolutionary psychology, then, instinct is not an issue if we take as evidence the virtual absence of the term in its literature. But, again, we need to go beyond the label itself and look at its definition and its perceived utility. As we have seen, aside being a concern of Darwin, Wallace and those in the psychological and behavioural sciences in the succeeding era, and aside it being described in terms of natural selection, instincts were taken to be important because there represented the "live" element of that which had evolved. Psychological and behavioural authors were interested in instincts because it was processes and not primarily anatomy that were of interest.

I take it essentially on trust that more is known and understood now about physiology and anatomy than was known in the first quarter of the twentieth century. Taking this as the case we may say that that part of the definitional ledger of "adaptation" which owes itself to physiology and anatomy is, in some sense, better than it was. We may also say that because behavioural processes are dependent upon physiology and anatomy, developments in the understanding of the latter sharpen and impose more precise constraints on our conception of the former. However, they do not dissolve the problems. The issue as to what constitutes a psychological adaptation is the same problem as the issue as to what constitutes an instinct on the assumption that a psychological adaptation is not comprised of anatomy (i.e. a given partition of the cerebral cortex) and/or neurophysiology (i.e. a particular pattern of activity with the cerebral cortex).

[7] Here are some examples of the how instinct theorists viewed the importance of an analysis of the past. We can see from them how close the debate came to postulating a natural history of human psychology. With a view to getting a better grasp of the number a type of instincts in humans, Lloyd-Morgan suggested that psychologists should follow the lead of biologists;

> . . . for the biologist, a criterion of instinctive behaviour is that it is serviceable on the first occasion. But the biologist . . . will ask: Serviceable for what end? Most broadly and generally serviceable for survival . . . In further detail serviceable for avoiding danger by shrinking, quiescence, or flight; serviceable for warding off the attacks of enemies; serviceable for obtaining food, capturing prey, and so forth; serviceable for winning and securing a mate, for protecting and rearing offspring; in social animals, serviceable for co-operating with others and so behaving that not only the individual but the social group shall survive. Under each head diverse modes of behaviour may be grouped – modes of behaviour which have this in common that they severally subserve what *we* regard as one end (Lloyd-Morgan, 1910: 226-227).

He went on to say that such an analysis offers up a power to predict what sort of behaviours to expect from a given species, including humans. Predictive power rests on a clear specification of the set of "potentialities" that comprises a species. For Lloyd-Morgan, one of these potentialities was intelligence itself;

> . . . what I mean by potentiality is an actually existent structure in virtue of the possession of which the organism does functionally respond in specific ways under the appropriate circumstances. There is an innate potentiality, then, a so-called instinctive faculty, dependent on the inherited make-up of the organism, to respond with the behaviour which I have defined as instinctive . . . If you know your bird, including its past history, you can bet on its intelligent procedure with as confidence as on its instinctive behaviour (Lloyd-Morgan, 1910: 227-228).

The salient thing in Lloyd-Morgan's view, then, was "the past history" of the species. However, he refrained from offering such a history for the human case.

Also from a 1910 edition of the *British Journal of Psychology*, the paper by Stout is most replete with allusions to the functional nature of instinct and intelligence as biological adaptations. Stout subscribed to the notion of "mental anticipation" and a view of the organism as "striving after ends". He binds instinct to intelligence as opposed to "Reflex Action" by seeing instincts and intelligence in explicitly function terms.

> The marks by which we recognize an action as instinctive rather than reflex are precisely the same marks which show the presence of intelligent consciousness, - conative impulse, unity and continuity of attention, perseverance with adaptive variation of behaviour corresponding to felt success or failure . . . The differentia of Instinct, then, as contrasted with a series of reflex actions, however complex, is that in Instinct congenital prearrangements of the neuro-muscular mechanism for special modes of behaviour do not of themselves suffice to explain the animals conduct. Their biological utility depends from the outset on their operation being sustained, controlled and guided by intelligent interest in the pursuit of ends (Stout, 1910: 244).

Pillsbury (1932) adopted a comparable position. Of the three definitions of instinct Pillsbury offered, the one he preferred emphasised the purpose they serve: Instincts can be thought of and distinguished in terms of their functional yield. From within a more overtly behaviourist perspective, Thorndike's *The Original Nature of*

Man (1913) sought to introduce order into the proliferating and confused systems of instinct classification by arguing that one could not know a behaviour to be instinctive unless one knew under which conditions it appeared and operated. However, he did not privilege a specification of these conditions over other means of defining instincts (such as the stimuli which elicit them, the conditions that satisfy them or their ontogeny).

Perhaps the closest of approaches to those we see now regarding the point, purpose and fruit of the analysis of historical origin was expressed by Bernard. One of Bernard's complaints about others (e.g. Dunlap) was not that they failed to consider "lower animal forms, to discover if there are corresponding definite and apparently unlearned action patterns in these primitive types" (Bernard, 1924: 7), but that they think of "action pattern merely in terms of structure, instead of in terms of origins";

> Long ago we learned that we do not content ourselves with the anatomy or cross section view of any subject or organ or organization. We are primarily interested in what the thing does, although a study of the anatomy or structure may be of the greatest use value in solving this problem of function. Function and origins are inseparable in our thinking. . . We examine it [the object of study], not alone in terms of its structure or organization, but we study its origin and its application and we consider that we do not comprehend the one until we have discovered the other. (ibid: 7).

In the absence of a direct reference it is reasonable to assume that he has Darwin in mind here. What is clear, and he reiterates the point, is that instincts are only to be properly understood and distinguished from habits and other forms of acquired behaviours by reference to their source in natural selection and their role in evolution. Where Bernard appears to have failed is in the point of making this distinction. Other psychologists took the means of sorting the distinction as redundant if they did not share Bernard's purpose in making it.

[8] " . . . as her [Nature's] children get higher, and their lives more precious, she reduces the risks [by] implanting contrary impulses to act on many classes of things (ibid)

[9] The obvious exception to this – and it is by no means a trivial exception – is Freud.

[10] One cannot help but be reminded of Tooby and Cosmides accusation that the social sciences are "an enormous mass of half-digested observations, a not inconsiderable body of generalizations . . . expressed in a babel of incommensurate technical lexicons".

[11] Only two specific types are listed. The third is "miscellaneous".

Chapter 9 Adaptations and Fixed Functions

Summary of chapter 9

The contemporary dispute between those of the adapted mind persuasion and those of the adaptive mind persuasion echoes the attempt to accommodate instinct and intelligence. We see in this chapter how tortured the attempt became. There appeared to be and appears to be an inverse relation between adapted and adaptive behaviour. Playing-up mechanistic instincts/adaptations highlights non-mechanistic adaptiveness. Playing-up adaptiveness seems to defy the nature of instincts/adaptations. A way through the conundrum is proposed. We can retain the notion of an evolved adaptation in social psychology only if we accept that this adaptation is for social living and has no rigidly fixed function. What is known about the natural history of humans offers license for such an approach.

9.1 Introduction

I am of the view that Tooby and Cosmides' insistence on a machine approach to the mind and the antagonism toward the term adaptive that flows from it (nicely illustrated in the demand for a switch from an "adaptive mind" stance to the "adapted mind" stance) presents a number of problems for evolutionary social psychology. We have already looked at two of these problems. In chapter 8 we looked at some of the implications that follow from a minimisation of the domain mismatch between the past and the present. One of those implications is that the notion of a maladaptive behaviour evaporates for if there is nothing new under the sun then we must consign all that would appear to be maladaptive to the dustbin of scruffy engineering. However, just as Tooby and Cosmides suggest when arguing for the explanatory usefulness of maladaptive behaviour, and as we discussed under the heading 'Domain mismatches and maladaptive behaviour', there are good grounds for thinking that the world inhabited by humans in the industrialised west today is not that inhabited by our predecessors. One of the implications of the acceptance of a domain mismatch (even if we ignore the view that the mismatch between the past and the present includes a speciation event) is that modern humans operate courtesy of exaptations and spandrels and that such notions are not amenable to a seamless adaptionist account. Either view – that we function courtesy of scruffy adaptations or that we do so courtesy of exaptations and spandrels – suggests that Tooby and Cosmides' notion of a psychological adaptation is of limited value. We have to reject the idea that psychological mechanisms are rigid and embrace the notion that minds are adaptive.

In this chapter, after a review of attempts by instinct theorist to accommodate the idea of adaptability within the concept of an adaptation, I present an alternative view of a social adaptation. This view has already been intimated in the literature and it has to be accommodated if evolutionary social psychology is to progress. The tenor of Tooby and Cosmides' adapted mind thesis

notwithstanding, humans are adaptive in the biological sense of the term. They have to and do adapt to one another. Resting on the view that what we have left when a comparative analysis between humans and other species is exhausted is our relatively elaborate social lives, I argue that evolution has produced an adaptation for nothing in particular. This "adaptation" is about other people. But because others are not and have not been entirely predictable it is not best described in mechanical or counterfactual terms. Natural history suggests to us that we cannot exhaustively detail this adaptation because we cannot exhaustively detail social life.

9.2 Adaptiveness - "the unexplained remainder"

The preparedness to move away from a strictly reflexive account of instincts was an expression of the desire of physiologically inclined researchers such as Watson to accommodate what they took to be the flexibility of instincts. However, Watson and Thorndike wanted to go no further than an appeal to the improbability of identical organisms being faced with identical stimuli so producing identical instinctive patterns. As we saw in the discussion of 'Physiological and psychological definitions of instinct' in the last chapter, James and McDougall appealed to the role of consciousness, reason and volition to explain the indefiniteness and variability in the behavioural expression of instincts. Whilst those who subscribed to a psychological account of instincts did not all agree on the nature or role of instincts as psychological content, in the effort to accommodate the evident flexibility of human behaviour there was some convergence on the notion that instincts were intimately connected with intelligence. The connection was grounded in the properties that the two shared; both were inherited, adaptive, species typical and naturally selected.

The invocation of intelligence as inherited and adaptive implied that natural selection had produced a phenomenon that is, in some sense, "above" instincts as they were conceived in philosophies of reflex and the chain-reflexes of behavioural patterns. In effect, authors from James to Drever, implied that instincts could be and had been inadequate as adaptations. This was a radical move in the sense that it sought to explain away classical dualism - it took the very thing which defied a wholly mechanistic view into account via a mechanistic account. More pertinent to our present concern is that this view seems to have been taken as opposed to the Darwinian orthodoxy of the time in saying that in the hierarchy of reflex-behaviour-mind, mind was master. By its advocates, this view was seen as the most viable solution to the problem of flexibility.

That the presence of intelligence and its relation to instinct was taken seriously as a problem for evolutionary accounts of psychology and social behaviour is evidenced by volume III, part 3 of the *British Journal of Psychology* published in 1910. The whole issue consists of papers titled "Instinct

and Intelligence". We will largely restrict ourselves to its contents to see how a previous generation of evolutionary psychologists handled the problem of flexibility by recourse to intelligence. The opening salvo from Charles Myers set the tone:

> Instinct and intelligence are generally regarded as two distinct modes of mental activity. In the following paper I hope to give adequate reasons for abandoning this view. I shall endeavour to show that instinct and intelligence are everywhere inseparable, and that in every so-called instinctive or intelligent act, a concomitant aspect of intelligence or instinct may be obtained. I regard the separation of instinct and intelligence as a purely artificial act of abstraction – convenient, no doubt, for the purposes of psychological science, but resulting merely from regarding the mental from two different points of view (Myers, 1910: 209).

Myers argued that this act of abstraction is a "necessarily 'anthropo-psychic' interpretation of one and the same problem". Behaviour is seen as being necessarily intelligent when conceived of from the standpoint of the organism, and necessarily instinctive when simply observed: "instinct regarded from within becomes intelligence; intelligence regarded from without becomes instinct" (ibid: 221). Myers went on to recommend that each of these positions be allowed to dilute one another as the evidence suggests: let instinct dilute our ascription's of intelligence and intelligence our ascription's of instinct. The result is that the dichotomy will dissolve.

Of the four papers that follow, three agreed with the proposition that any metaphysical distinction between intelligence and instinct is untenable and must be dissolved. All four disagreed with Myers solution. And all four disagreed with one another.

Using Darwin's terminology, Lloyd-Morgan argued that intelligence is a property of individual instincts that hones them to specific conditions:

> The products of natural selection are practically serviceable, not theoretically perfect . . . It affords the rude outline sketch of that far less imperfect behaviour, the finishing touches of which are supplied by practise under the guidance of intelligence (Lloyd-Morgan, 1910: 225-226).

Lloyd-Morgan called intelligence the "unexplained remainder" of any mechanistic account of instincts which seeks to specify their behavioural characteristics in advance of their actual exhibition by particular organisms.

Stout joined Myers and Lloyd-Morgan in seeing instinct "as essentially a form of biological adaptation" (Stout, 1910: 237). But his elaborated position is a hybrid of the two. He did not believe that intelligence is just instinct viewed from the organisms point of view as did Myers. And, in opposition to Lloyd-Morgan, he did believe that instincts in their mature expression are the result of intelligence. In tune with Darwin, Stout took instinct and intelligence as conceptually distinct but

practically indistinguishable. His criticism of what we might call Myers "double-aspect" view, or mirage, is that it denies that the basic nature of instinct is something unlearned and that intelligence is something learned. His support for the view that instincts are given shape by intelligence rests on his conviction that "all intelligent activity, including that of animals, is directed to the attainment of future results" (ibid: 239).

Stout set up his position via a contention with the "typical" definition of intelligence as learning by experience.

> An animal in consequence of a train of previous experience, intelligently modifies its behaviour from the outset, when it is again confronted with a similar situation. This implies what we call learning by experience. . . but in the first execution of a train of instinctive movements, there can be no learning by experience, because the required experiences are themselves absent . . . when does the animal learn its lesson? Does the actual process of learning take place on the second occasion or on the first? Plainly it takes place on the first and not on the second. On the second occasion the lesson is utilised: but in order to be utilised it must already have been learned. Thus if the actual process of learning involves intelligent consciousness, intelligence must accompany every instinctive act which leads to intelligent modification of behaviour on its repetition in a similar situation (ibid: 238).

However, Stout argued that the intelligent modification of behaviour based on prior instances is not in itself sufficient to explain the quality of fit between organisms and their environments. The revival of past experience cannot accommodate the flow of uncertainty that an organism faces. It is at this point that Stout introduced the notion of "mental anticipation". In a manner not dissimilar to that espoused over eighty years later by Henry Plotkin (1995), Stout sought to reconcile instinct and intelligence by positing both as forms of knowledge. This knowledge is what the organism inherits.

> . . . I see no intrinsic absurdity in the assumption that even in the commencement of the first performance of an instinctive action, the given situation may be apprehended as about to have a further development. Such anticipation, if it exists, is not wholly indefinite; for the mental reference is to a coming change and development in a certain specific situation, and is therefore, to that extent, itself a specific anticipation of the future (ibid: 240).

In support of this attribution Stout wrote;

> Animals in their instinctive actions do actually behave exactly as if they were continuously interested in what is for them one and the same situation: they actually behave as if they were continuously attentive looking forward beyond the ignorant present to meet what is coming (ibid: 242).

Stout differentiated himself from Myers and Lloyd-Morgan by casting his analysis beyond the confines of any given act. It is the ability of humans to do likewise that marks them out as intelligent. For Stout, to anticipate is to think.

McDougall begins his contribution by saying that he agrees with Myers and Stout if his reading of them is correct in that they regard "instinctive processes and intelligent processes as of essentially similar nature, as involving the same fundamental modes of mental activity" (McDougall, 1910: 250). But he did not agree with any view to the end

> . . . that we cannot properly and usefully distinguish between the mental processes that are conditioned wholly or mainly by innate dispositions on the one hand, and on the other hand such as are conditioned by dispositions that have been largely built up through the experience of the individual" (ibid).

Thus McDougall wanted to say that instinct and intelligence are identical in so far as they amount to evolved psychological processes but distinguishable in their specific service to the organism.

McDougall sided with Stout in believing that "animals are guided by anticipatory representations of the ends to be achieved, representations that owe little or nothing to previous experience" (ibid: 251), and against Myers and Lloyd-Morgan in their view that intelligent behaviour is determined by "factors of revival" (i.e. memory).

McDougall was, perhaps, the most resolutely psychological of the instinct theorists. We have seen how closely tied is his conception of instinct to that of felt emotion, and in the 1912 paper he objected to the, ". . . usage of the word instinct to denote an instinctive action. . . to describe any particular action as an instinct is, I submit, a loose and confusing usage against which we must set our faces".

> We ought rather to use the term an instinct to denote that feature of the innate constitution of any organism, that inherited disposition, in virtue of the possession of which the organism acts instinctively . . . the former member of the pair of terms denotes some enduring condition of the mode of activity denoted by the second (ibid: 253).

Thus it is not behaviour *per se* that is the product of evolution but the disposition to behave. It is from this stance that McDougall distanced himself from the other authors. His basic point was that intelligence is to be understood as volitional and volition is to be understood as instinctive. He objects to the dividing up of instincts into "dualistic" oppositions and contrasts between the mechanical and the situational.

> I would say that each innate specific conative tendency has at its service an innately coordinated system of motor or efferent nerve channels; that these belong together functionally and phylogenetically as one feature, one psycho-physical disposition, of the inherited constitution of the organism . . . instinctive activity always involves their cooperation (ibid: 263).

By removing instinct, as opposed to the instinctive exhibitions, from the realm of the directly observable and into a "conatative tendency" served by a "perceptual disposition" and "coordinated motor channels", McDougall's strategy was to dissolve the opposition by refusing to acknowledge it. Like Tooby and Cosmides, in attempting to psychologise instincts McDougall idealised them.

I have discussed the papers in order save the contribution by Carr. The reason for this is that he was the odd man out. This is reflected in the fact that he was summarily dismissed by Myers, Stout and McDougall, and totally ignored by Lloyd-Morgan. Following the theory of Henri Bergson, Carr argues for a parallel in the evolution of instinct and intelligence. For Carr instinct and intelligence are both "natural" products of evolution but they are fundamentally distinct.

> When we consider two such outwardly dissimilar instances of behaviour as, let us say, that of a paralysing wasp, and that of an admiral directing a naval engagement, have we before us only two different aspects of a mental behaviour that is essentially identical? . . . Or, are they two modes of psychical activity, fundamentally distinct, different in the nature of their mentality, in the direction of their activity, in the kind of knowledge that each is fitted to receive and use? (Carr, 1910: 232).

Carr's answer is that they are.

> The fundamental difference [between instincts and intelligence] is one of kind, and lies in the mode of apprehension of reality, and the kind of knowledge that serves the activity of each. It is this essential difference that accounts for the degree of consciousness or unconsciousness, plasticity or fixity that characterises each, and not *vice versa* (ibid).

He took to task the idea that the behaviour of different species must be driven and organised by a single unified system of instinct with attendant intelligence. He suggested that the muddling of the two stems from the very power of each to fulfil the goals of different types of organism. Carr invested more in the potential of evolution to compose different systems than do the other contributors. In effect, he accused them of being conservative in their application of natural selection. He concluded by saying that the apparent contradiction between instinct and intelligence is, as the evidence from comparative psychology suggests, real and not merely redundant metaphysical dualism.

In this brief review of four sophisticated and psychologically orientated positions on the relationship between instinct and intelligence, I hope to have conveyed three main points. One, that

there was a common belief that both "faculties" were the product of evolution. Two, that the commitment to a naturalistic account leaves adaptability, the basis of which for Myers, Stout, Lloyd-Morgan, McDougall and Carr was intelligence, as an "unexplained remainder" for strictly instinctive accounts. And three, that, ignoring any erstwhile motivation to place humans at the pinnacle of evolution, the comparative approach adopted by all four authors leads to the conclusion that the exemplar of relatively flexible behaviour is man. This point is worth further elaboration.

The desire to place their claims within the confines of Darwinism and to be scientific led Myers, Stout, Lloyd-Morgan, McDougall and Carr to cite examples of animal behaviour. The examples are cited both to support their claims that instincts and intelligence are exhibited in the natural world and to generate a model of their operation. So, in discussing the nesting behaviour of the moorhen, for example, Lloyd-Morgan seeks to show the origin and aetiology of a known behaviour in term of the functional modification of an inherited tendency. Each author then "runs" his model with a view to offering an account of instinct, intelligence and their relation in humans – they are, after all, psychologists. With the partial exception of some tentative remarks by McDougall, they do not give examples drawn from human behaviour and excuse themselves from doing so because they see any attempt to do so as premature. The issue of the relation between instinct and intelligence needs to be settled before it can be applied. All except Carr agree that the relation between instinct and intelligence in humans is different from that in, say, ants. But the difference is one of degree and not kind. The outcome of this approach is summed up in a representative statement by Stout;

> In the case of human beings, this function of instinct, is in the main, superseded by Instruction. All that either Instinct or Instruction can do is to supply appropriate experiences. . . The importance of Instinct as a substitute for the lessons of experience, become greater as we descend the scale of animal intelligence. But the more instinct serves as a substitute for experience, the more fixed and specialised must the instinctive equipment be, in order to provide in advance for the special exigencies which arise in the life-history of the animal. On the contrary, in proportion as the educational function of instinct becomes more pronounced, instinctive endowment becomes less fixed and specialised. . . in the case of human beings there are hardly any well marked instincts as distinguished from special capacities for learning by experience in certain directions (Stout, 1910: 249).

In other words, the claim is that instinct and intelligence in humans is to be considered in relation to other animals but that nature may be viewed from humans downward, and that that comparison yields the result that humans are less instinctive.

> Vital adaptation, in the first instance, calls in psychical adaptation to help it; but, in the sequel of biological development, it is gradually ousted from its place and function by its servant and ally.

> The climax of this process is reached in man, where instinctive endowment dwindles to the minimum of complexity and specialisation, so that careful scrutiny is required to detect its presence at all (ibid: 245).

The extent of agreement is evident it the following claim by McDougall;

> In the young mammal the instincts ripen slowly and successively at considerable intervals . . . This period of youth, characterised by the accumulation of experience in preparation for the serious tasks of life, reaches its maximum in the most developed forms of human life . . . the vast accumulation thus rendered possible modifies so greatly the innate dispositions and their modes of operation as to obscure completely for some of us the fact of their existence (McDougall, 1910: 257).

Despite variations there was also considerable consistency of approach: thoroughly comparative, avowedly anthropocentric and devoid of sentimentality. We also reach a conclusion as to the role of intelligence very similar to that outlined by James. We can see here elements of the SSSM in the assertion that there "are hardly any well marked instincts", but there is too what we can only call an adaptionist approach.

If for Theodosius Dobzhansky nothing in biology made sense except in the light of evolution, for many social psychologists at the turn and in the first quarter of the twentieth century, nothing about behaviour made sense except in the light of the relation between instinct and intelligence. The reception that Carr's view received shows that instinct theorists were not interested in separating out the origin or the practical exhibition of the two concepts. This was taken as being antithetical to Darwinism and evolution. But, in retrospect, it appears that the failure of instinct theorist to do more than allude to and gesture toward the origin and nature of adaptable instincts and behavioural flexibility opened the way for the widespread consideration of determinants other than instincts on behaviour. The remarkable fact of flexibility – a phenomenon "so wonderful that [its] development will probably appear to the reader a difficulty sufficient to overthrow my whole theory" (Darwin, 1859: 191) - led to the neglect of the determinants of behaviour outside of its varieties within the lifetime of given organisms.

9.3 The uncertain futures problem

With the debate over the problem of flexibility and the nature of intelligence in mind, let us now consider what Plotkin calls the "uncertain futures problem" (Plotkin, 1995).

Plotkin sets up his discussion with a definition of evolution as the response and solution to uncertainties in the ecological niche of organisms. In short, evolution is driven by change and

uncertainty in environments for perfect stasis demands no change in viable organisms. Here is how he defines the uncertain futures problem:

> The uncertain futures problem concerns an organism going through life, equipped only with instructions given at conception (and hence perhaps only correct at that time) on how to survive, and having to interact with a world that may be different from that in which its life began (Plotkin, 1995: 144).

Plotkin argues that there are several forms of uncertainty. The first and most prevalent form - indeed inevitable should anything else in the environment of a given species be evolving even if by random drift alone - is essentially historical in nature. This is natural selection in its widest and most inevitable form - the "great digital river out of Eden" (Dawkins, 1995).

The fossil record is a demonstration of the uncertain futures problem. It tells us that the great majority of species that ever lived are now extinct – including all but one species of hominid – and that genes, the organisms and species that they build, do not cope well with the uncertain future that is natural selection. Constrained by the possible rates of evolution, genes are frequently over run by uncertainty.

> Extinction is always caused by an inability to cope with the uncertain futures problem, and given the estimates that well in excess of 98 per cent of all species that have ever existed are now extinct, it is clear that, in the end, all life succumbs to its uncertain future (Plotkin 1995: 144-5).

A second form of uncertainty is local and specific to species, populations and, ultimately, down to individual phenotypes. This uncertainty takes the form of specific new or novel problems within the lifetime of a given organism. Any deviation from the norm that a given organism is adapted to encounter amounts to such uncertainty. Uncertain futures within the lifetime of an organism leave genes exposed to what Lorenz (1966) called "generational deadtime". This phrase refers to the exposure of genes between replications. Plotkin discusses generational deadtime in term of the interlude between dips into the gene pool that comprises a species. Any given gene is locked into a given genotype and at the mercy of the subsequent phenotype until (indeed if) it recombines courtesy of the reproductive success of the organism. Generational deadtime at one extreme of the $r - K$ continuum can be minutes, at the other it is years. In highly K-selected modern humans generational deadtime exceeds a decade.

But certain characteristics of animals suggest that genes have learned that the future is uncertain and have responded. Plotkin points out two ways in which genes have come to cope with generational deadtime and the subsequent exposure to uncertainty. We can call one type physical uncertainty, the other chemical uncertainty.

Just as Buss (1995) has suggested, although not by using the terminology, a physically uncertain future and its solution is the human capacity to develop protection of exposed skin surfaces with calluses. Genes reach into generational deadtime of modern humans via the capacity of phenotypes to develop such features should they be required. Perhaps more sophisticated is the solution to chemically uncertain futures represented by the immune system. Seen as biochemical systems, humans inhabit a sea of biochemical systems that replicate at their expense. Humans cope via their immune system. The immune system is another way in which genes defend against their exposure to the uncertainties of the world between replications. Both of these solutions to uncertainty are called a "tracking option" by Plotkin. By tracking option Plotkin means that genes build a capacity for change or alteration within individual phenotypes.

A third, and, for our purposes, more interesting sort of tracking option that responds to and solves uncertain futures problems is psychological. Plotkin is of the view that what we typically and commonly call intelligence and rationality is an adaptation to solve for uncertainty during generational deadtime. As a tracking option, intelligence allows organisms to adjust their behaviour according to "wobbles" in the world. As an adaptation intelligence is for nothing in particular other than serving inclusive fitness. Before its actual exhibition one cannot say what it is intelligence will do, or how it will adjust unless one is able to specify exactly what uncertainty will be faced during generational deadtime.

To develop this discussion and conclude this dissertation we can now return again to the idea that what it is that is truly distinctive about humans is their sociality.

9.4 Social life is not discrete

In chapter 6, section 4, 'The physical and the social EEA', we looked at some of the arguments and evidence put forward to support the claim that sociality flavours the depiction of humans as "another unique species". The core of the position rests on a conundrum common to all social species. The conundrum is this: social animals must cope with the hawkish individuals or Machiavellian Intelligence of individuals around them. They must do so using a brain of comparable sophistication to that possessed by conspecifics. It would appear to be the parity of sophistication that renders stability in the population. This stability can become profound. Thus, we see stasis in brain volumes in very many species over time (and very probably in complexity of organisation also).

However, this is decidedly not the case in the lineage *Homo*. As we have discussed, our relatively swift transition from *pan troglodyte* to *Homo sapien sapien* is flavoured by instability. Our lineage would appear to have been embroiled in an evolutionary arms race. What makes it so acute and

dramatic is that the "weaponry" has been, so far as we know, the most sophisticated pieces of Darwinian engineering on earth at each stage in the race (Buss, 1997). The sociality argument for encephalisation says that key adaptive problems have been set by one another, by social life. On this account, then, the EEA - the environment(s) whose flux amounted to the suite of adaptive problems that issued *Homo sapien sapien* - was a social psychological phenomenon. Buss concurs:

> Humans are probably unique in the duration and complexity of the social relationships they form. Humans sometimes form lifelong mating relationships, develop relationships that last for decades, and maintain contact with their brothers, sisters and other relatives over great expanses of time and distance. Because social adaptive problems were so crucial for human survival and reproduction, many of the most important features of our evolved psychological mechanisms will necessarily be social in nature. Social adaptive problems have been so important over evolutionary history that many of the dedicated psychological mechanisms currently studied by cognitive, personality, and developmental psychologists, in addition to those studied by social psychologists, are inherently social. (Buss, 1995: 9).

What Plotkin adds to this analysis is the claim that sociality presents a particular form of uncertainty. What I want to do is look at the implications of saying that what it is that is distinctive about human minds – what is left over, if you will, once what can be said from a comparative stance has been exhausted – is best considered as a response to socially uncertain futures and the ability of humans to deal with them.

First, Tooby and Cosmides insist that we talk about psychological adaptations as mechanisms. They give no examples of these mechanisms other than as counterfactual devices or algorithms: That is, devices that manipulate statements via counterfactual propositions. For example;

> *If* your rival is larger than you *then* don't attack,

and,

> *If* retinal displays of naked nubile females *then* move toward source (Tooby & Cosmides, 1990b: 405-6).

In chapter 6, section 5, 'The EEA and ARE', I argued that Tooby and Cosmides readily confuse ultimate and proximate causes. They confuse and conflate the principles that we take to comprise evolutionary forces with the actual species-specific adaptations that solve for selection pressures. For example, the imperative "have sex" – an imperative that applies to any and all sexual reproducers – tells us nothing more than that humans will seek to have sex. It tells us nothing about the actual psychology of sex and mating other than that we should expect the imperative to be instantiated in minds and exhibited in behaviour. Here I want to draw attention to the clauses (propositions) that plug into counterfactual propositions. Tooby and Cosmides call these "ancestral cues" (ibid: 406).

Recall that ancestral cues are the statistical composites of past environments that provide the antecedent clauses for adaptive (counterfactual) mechanisms: ancestral cues provide the content after the operator "if" (ibid). Being composites these cues are particular in that they are about sorts or classes of problem or stimuli. But, being composites, ancestral cues are not definite or tightly tied to particular instances of stimuli.

Although, in Tooby and Cosmides' hands, evolutionary psychology is all about adaptations, what is clear is that the notion of ancestral cues actually relegates mechanisms in favour of meanings (and the ability to generate or derive them), and it places the source of meanings "out there" in the world. If the meanings – the antecedent clauses in counterfactual mechanisms - are about social problems then the cues we are looking for are also social. I am of the view that an emphasis on cues, or "fitness tokens" (Daly & Wilson, 1988) is right despite its formidable difficulties. (Philosophical and psychological literature is littered with theories and unsettled discussions of the nature of isomorphism, prototypes, affordances, and the necessary and sufficient conditions required to classify instances of kinds into classes of kinds.) Despite the repeated emphasis on adaptations, if we want to do evolutionary social psychology according to the cannons of Tooby and Cosmides, the pursuit of adaptations in the first instance won't help because external cues – the trigger for decisions and the driving stimuli behind behaviour – are not adaptations.

Secondly, as we have seen, encephalisation suggests that as social "place" or phenomena, the EEA was uncertain. Encephalisation seen as a response to an increase in the complexities of social living, defines it as such. This is because in a psychological arms race the source of the key adaptive problems – other brains – is as sophisticated as the means of the tool used to generate solutions. A mind engaged in the social EEA that was, as a mechanism, adapted to some concrete characterisation of social living would be vulnerable to socially uncertain futures. A mind that was attuned to various non-species-specific social problems - sexually dimorphic mating, male kin bonded groups, mating mechanisms, iterated reciprocal altruism - but capable of negotiating the potentially endless forms that they can take is a different prospect. Viewed this way the adapted social mind is characterised by its ability to adapt. I am of the view that we can only make sense of brain growth and the development of cognitive sophistication in the lineage *Homo* by presupposing indeterminacy of both problem and psychological process. On the adaptionist account of the EEA the capacity of humans to negotiate social uncertainty entails that the EEA and its selection pressures were themselves uncertain. The sociality that was the selection pressure behind human brain growth was variable. Although a "fact", these selection pressures cannot be specified precisely. If we say that our brains are there to "solve" sociality within generational deadtime we must accept that the problems were and are undetermined and fluid. What our brains tell us about the past is that it was socially rich.

Accordingly, searching for variation from an idealised hunter-gatherer-like arrangement is not going to be profitable. The ability to negotiate others and the attendant uncertainty is what we mean when we talk of humans being adaptive as opposed to adapted. From the point of view of social psychology, humans are adapted to adapt to others.

Finally, one may insist that the ability to solve for social uncertainty is itself an adaptation. On Tooby and Cosmides' account this leaves us with the obligation to specify in what way this adaptation is economic, efficient, reliable, precise and functional. Although we may be able to point to behaviours and say, yes, that was or is economic, efficient, reliable, precise and functional (and we may be able to help ourselves in doing this by invoking the non-optimal nature of adaptations thus permitting performances or output that is mostly or quasi- economic, efficient, reliable, precise and functional) we can only ever do so after the fact. We cannot say in advance what such an adaptation is for because it isn't for anything in particular. We can go no further than saying that the adaptation is for sociality (and all the uncertainty that that entails) and that it serves inclusive fitness.

Concluding remarks

The dissertation began with an introduction to John Tooby and Leda Cosmides' project for evolutionary psychology as it is presented in their influential essay *The Psychological Foundations of Culture*. Other than to introduce the adapted mind view that underpins what we can call mainstream evolutionary psychology, the aim of chapter 1 was to show what is and what is not new in evolutionary psychology as presented by Tooby and Cosmides. I argue that the central place given to John Bowlby's notion of the Environment of Evolutionary Adaptedness and the history of social science called the Standard Social Science Model mark evolutionary psychology out from its immediate predecessor, sociobiology. By isolating the EEA and the SSSM I simultaneously sought grounds to justify the title of the dissertation and the concentrated analysis the history of social psychology and natural history receive in Parts I and II.

Chapter 2 is purely expository, but the quality and fairness of the exposition is critical. I try to capture in two propositions Tooby and Cosmides' thesis of the Standard Model paradigm. I call these propositions the 'culture as cause commitment' and the '*tabula rasa* commitment'. Chapter 3 rests upon the adequacy of my exposition. The obvious danger is that I refute a straw man of my own making – I knock down an argument that nobody has made. Of course, I do not think that this is the case. The extensive quotations from *The Psychological Foundations of Culture* show this.

Those who are alive to the history of social psychology may not have needed to read chapters 3 and 4 to find that the Standard Model thesis is not reliable as a history of ideas. Still, one of the aims of chapter 3, and the whole thrust of chapter 4, was to go a little further than showing up the inadequacies of Tooby and Cosmides' history. The object is to show that Darwinism has not just been of issue in social psychology, it has, in the past, been a dogma.

My strategy in chapter 3 was to map the two propositions that comprise the Standard Model thesis onto the long-standing division between sociological and psychological forms of social psychology. The adequacy of that mapping is key to the success of the chapter. As a starting point, I accept the possibility that the tradition of sociological social psychology might show the culture as cause commitment to be applicable to social psychology in general. And I accept the possibility that the psychological form of social psychology might show that the *tabula rasa* commitment has dominated social psychology. The weight of historical opinion and the evidence suggests that the dominance of a psychological over a sociological emphasis in social psychology enforces the conclusion that social psychology cannot be said to be well characterised by the culture as cause commitment. I went on to argue the other possibility, that psychological social psychology has been governed by the *tabula rasa* commitment, is not a view that withstands scrutiny because social

psychology has never been dominated by behaviourism (indeed, nothing has been as dominated by behaviourism as the Standard Model thesis implies). Tooby and Cosmides' thesis does appear to hold, however, for the tradition in psychological social psychology we now call social cognition. The cognitive turn in social psychology in the 1950's did import the general-purpose machine view along with the computational metaphor. The general conclusion of chapter 3 was that a qualified version of the Standard Model thesis holds for social psychology since the nineteen sixties.

If chapter 3 is about cleavages – both in the form of my stark reduction of Tooby and Cosmides' thesis of the SSSM to two propositions and my ready acceptance of the division of social psychology into SSP and PSP – I may be accused of ignoring another that is common in histories of social psychology. I have in mind the distinction between American and European forms of social psychology. While it is true that I have not made an issue of this distinction, I do not think that it would rescue the larger part of Tooby and Cosmides' argument. Recent commentaries (e.g. Farr, 1996; Manicas, 1987) suggest that, as a whole, American social psychology has expunged the social and individuated the discipline. Developments in Europe have sought to redress the balance. The key points are that social psychology as an enterprise is much larger in America than it is in Europe and that American social psychologists have also been resistant to the *tabula rasa* view.

The role of chapter 3 in my overall argument is to weaken confidence in the Standard Model thesis and in doing so begin the process of weakening confidence in the scholarship of Tooby and Cosmides' essay. Chapter 4 takes the first of those goals to its conclusion and develops the second. The long and intense effort to establish instinct as a basic unit of analysis and explanation in the psychological and behavioural sciences shows that the SSSM did not dominate the century of social psychological theorising that preceded *The Psychological Foundations of Culture*. However, Tooby and Cosmides are not entirely consistent in their claims for the duration of the Standard Model. Accidentally, or by design, they also suggest that it took hold at just about the time the instinct debate abated. Should we accept the more modest time-scale, my claim that the instinct debate refutes the Standard Model thesis loses force. What is not lost is the remarkable neglect of the instinct debate shown by Tooby and Cosmides in their history of social science. I argue that the integrity of Tooby and Cosmides' work is damaged by their seeming ignorance of the many others who would not have objected to being labelled "evolutionary psychologists". Additionally, and this is a theme that I develop in Part III, by taking no backward glance at their predecessors, Tooby and Cosmides' flawed account of intellectual history mitigates against the learning of lessons from the failure of instincts to win a central place in contemporary social psychology.

Having accused Tooby and Cosmides of an omission I must highlight one of my own. Appendix 2 shows that no one was more cited and referred to in social psychology through the twentieth century than Sigmund Freud, yet I do not discuss his definition(s) or account of instincts. I omit Freud for two reasons. One, there is nothing in Freud that would rescue Tooby and Cosmides' thesis - he consistently argued in favour of instincts and was an unashamed biological materialist. And two, whilst my argument that the instinct debate refutes the SSSM would be further strengthened, I do not think that the conclusion needs the additional support a consideration of Freud would give.

The analysis that constitutes Part I may be regarded as largely irrelevant to the enterprise that Tooby and Cosmides promote as opposed to that which they reject. Whilst I recommend that the Standard Model thesis be abandoned by evolutionary psychology because its promulgation does not aid or add to the prospects of a wider adoption of a Darwinian view in social psychology, the future of evolutionary psychology will not be determined by the Standard Model thesis. It is in Part II of the dissertation that I turn to the positive project that Tooby and Cosmides present. Particularly, I turn to an analysis of the EEA, the concept that Tooby and Cosmides place at the heart of evolutionary psychology.

Like chapter 2, chapter 5 is largely expository. I show that, in practice, the EEA refers to the set of conditions responsible for modern humans. We saw that the role of the EEA is to furnish us with a specifically human psychology. And it was pointed out how Tooby and Cosmides use the EEA to mark a distinction between the adapted mind approach, i.e. evolutionary psychology, and the adaptive mind approach, i.e. Darwinian social science. In chapter 5 we looked at the concept of the EEA in use, and unearthed some of its assumptions about the natural history of the human lineage.

Having elucidated the justification for and conceptual role of the EEA, chapter 6 turns to a fuller explication and consideration than has hitherto been offered of the actual evidence and facts of our natural history. I presented the (more or less) conventional view of palaeoanthropology. This view suggests that evolutionary psychology is radically wrong in assuming that modern hunter-gatherers give us a window into the Pleistocene. *Homos* prior to *Homo sapien sapien* were not hunter-gatherers in the same sense as are modern humans. Furthermore, several hominid species were in existence in the Pleistocene and it is not known which of them evolved into *Homo sapien sapien.* These considerations mitigate against the assumption that the Pleistocene was a seamless continuity that issued modern humans.

The latter part of chapter 6 sounds a rather pessimistic note. Based on the orthodox belief that modern humans are a product of social selection pressures, I argue that what we would need to know about the EEA to construct an evolutionary social psychology is probably not knowable. But if this

position is pessimistic it too is common. The recovery of the social-behavioural ecology of our predecessors is not a tractable exercise when one considers the kind of detail required to flesh-out the information processing mechanisms at the centre of Tooby and Cosmides' enterprise. One way in which the dearth of detail expresses itself is in what I called a "slippage" between ultimate and proximate causes; i.e., the tendency to take as natural history the constituent propositions of evolutionary theory. Aside pointing out some of the accepted facts of our natural history and how Tooby and Cosmides appear to underestimate the difficulty of ascertaining the further facts their program demands, I also point out how evolutionary psychology tends to disobey the dictum of conceptual integration by overlooking the fossil record.

The strength of chapter 6 is the emphasis placed on the material evidence taken to demonstrate that modern humans have evolved. The emphasis the EEA concept places on natural history begs the sort of analysis of the evidence I provide. The fossil and archaeological records do not support the rather static and simple picture that evolutionary psychologists use as an adaptation generator. However, the weakness of chapter 6 also comes from my emphasis on the evidence. For example, I rely, perhaps too heavily, on the absence of evidence to make the point that pre-modern forms differed substantially from *Homo sapien sapien*. And I presuppose that evidence to come; i.e. the actual lives of millions of humans, will show modern forms to be more markedly different from their predecessors than even now seems to be the case.

Whilst I do not pretend to be a palaeontologist, my treatment of the palaeontological literature must be trusted if chapter 6 is to work as a criticism of evolutionary psychology. There is an important aspect of that literature so far as evolutionary psychology is concerned that I do not address. Although, by making an issue of speciation within the hominid lineage prior to *Homo sapien sapien,* I am a "splitter", I also adopt an orthodox "Out of Africa" view of the lineage after *Homo sapien sapien* appears. Tooby and Cosmides' do not present their case for universal adaptations in these terms but, in practice, they too assume a common ancestor for modern humans. Alongside Tooby and Cosmides, I assume that modern humans are of a common stock that originated and radiated out of Africa and subsequently developed superficial morphological characteristics in situ that are of no matter to social psychology. The alternative to this view is the so-called "multi-regional hypothesis". The multi-regional view argues that populations of a *Homo* prior to *Homo sapien sapien* left Africa and evolved into modern humans independently. Should it be true, the importance of the multi-regional account for evolutionary social psychology is potentially profound. It weakens the likelihood that current races of humans are psychologically identical. It weakens Tooby and Cosmides' idea that human nature is everywhere the same. I do not pursue this line of inquiry

because different strands of evidence converge to support the Out of Africa hypothesis at the cost of the rival account. As a social psychologist I have sought at each turn to keep to an orthodox path through the palaeontological literature.

Chapter 7 moves the analysis of the EEA on and looks at its conceptual coherence. There are two basic ways of viewing the EEA as an explanation of the present. The first of these is the idea that the past and the present differ in no important respect so far as Tooby and Cosmides' enterprise is concerned. The second is the contrary view that the past and present do differ in some respect that impinges upon our explanations of the present. I take issue with both views. If the past just is the present then the dictum "the past explains the present" is equivalent to the 'past explains the past' and 'the present explains the present'. The difficulty with either approach is that they are more akin to the quasi-historical stances that constitute the so-called Standard Model so despised by Tooby and Cosmides. On the other hand, the idea that the past and present are distinct leads us to conclude that many of the routine performances of modern humans cannot be said to depend on adaptations and cannot, therefore, be explained in adaptationist terms. An insistence on couching, say, driving or reading in adaptationist terms reveals as much, perhaps, about the explainer as it does the phenomena.

In my concluding comments on the status of the Standard Model thesis I borrowed Daniel Dennett's term and called the SSSM a "luscious slogan". My overall treatment of the EEA in chapter 6 invites a reuse of the term. A strength of chapter 7 is the further analysis of some of the concepts which the EEA encourages or gives rise to. The idea of a domain mismatch is, in my view, true, but I ask if it is useful. Similarly, the demand that an engineering viewpoint is taken toward adaptations seems essential, but I ask if it is plausible. By looking at the EEA as a device and not just as natural history its utility is brought into doubt.

The main problem with chapter 7 is that it is obtuse. Whereas chapter 6 takes virtue from evidence and examples, chapter 7 eschews them. For example, in the discussion of how we might re-ascribe adaptations, exaptations, spandrels and by-products I offer no illustrative cases or instances. This makes the chapter more difficult to read. My defence is this: examples would clutter the argument and would mislead. Clutter would come in the form of the inevitably analogous nature of any examples, and the presentation of analogies would mislead because they would suggest that we know that changes of ascription in the uses of mind are, somehow, just like changes in the use of anatomy. I take the view that illustrations and examples would engender a sense that we know answers to the very questions that arise from invocations of a domain mismatch between the EEA and the present. As Stephen Jay Gould had repeatedly pointed out, apparent discontinuities in natural history suggests

to us the need for the concept of exaptation. The features of the hominid line that have undergone changes of function is a central issue and not a starting point.

In Part III I look to redress an imbalance that may have arisen from the content and tone of Parts I and II. My issue in the dissertation is not with Darwin but with the Darwinism of Tooby and Cosmides and the evolutionary psychology they have successfully promulgated. Precisely, I criticise their claim that social psychology has had no room for Darwin and the key notion that they add to contemporary evolutionary theory, that the past explains the present. Part III looks again at both points.

In the early parts of chapter 8 I try to show how, luscious slogans and developments in evolutionary theory aside, old notions concerning the explanatory power of psychological instincts and new ones concerning the power of psychological adaptations are similar. In the middle sections of the chapter I show how and why the descent of the concept of psychological instinct into confusion occurred despite Darwin's confidence that instincts presented no particular problem for a naturalistic account of human psychology. Proponents of psychological instincts could not meet the demand that such entities must sustain a tie with physiology. Without this constraint the concept of a psychological instinct was used too freely for the tastes of psychologists anxious to be called scientists. Critics rendered the apparently fecund notion of psychological instinct vacuous and forced an abandonment in the nineteen thirties. Alongside the overarching aims of McDougall's social psychology, Tooby and Cosmides have resurrected the idea of discrete, universal, non-material psychological entities. I argue that the price of such entities remains the same: idealisations that cannot be constrained.

Based as it is on the claim that instincts and adaptations are comparable concepts, chapter 8 might be criticised for not pursuing the comparison more fully, for not extending to other authors the match-up I present between McDougall and Tooby and Cosmides. In part, the second section of chapter 9, 'Adaptiveness – "the unexplained remainder"' is a reply to such a criticism. There my aim is to continue the comparison of instinct and adaptation by inviting the reader to further consider mutual difficulties the two concepts face. I show how the contemporary dispute between those of the adapted mind persuasion and those of the adaptive mind persuasion echoes the past attempt to accommodate instinct and intelligence and I give some idea as to how tortured the attempt became.

It appeared to instinct theorists that there was an inverse relation between functionally appropriate behaviours that could said to have been unlearned and those acquired or modified by experience. An emphasis on mechanistic instincts served to highlight non-mechanistic patterns of activity that mapped onto contingent needs. On the other hand, an emphasis on adaptability served to marginalise

the explanatory power and scope of unlearned instinct. I suggest that the conundrum is mirrored in the tension between evolutionary psychologists and Darwinian social scientists.

The final two sections of the dissertation look to find a way through these positions for social psychology. The cornerstone of the argument is an appreciation of novelty that I think Tooby and Cosmides and evolutionary psychology lacks. I take seriously Darwin's argument that natural selection generates real novelty. In phylogenetic terms we call this process speciation and the product species. Modern humans are novel. This is trivially true by virtue of them being a species. But what it is that is novel about them is not a trivial truth of taxonomy. Our novelty as mammals, as primates, and, most saliently for evolutionary psychology, as hominids, is our ability to thrive in the face of novel problems. Most importantly, the novel problems are other people. Whilst I think that labelling the novel features of mind which distinguishes us from other species an adaptation is problematic, I argue that we can retain the notion of an evolved adaptation in social psychology. However, the cost of doing so is an acceptance that this adaptation is for social living and has no rigidly fixed function.

Of course, the obvious problem for an evolutionary social psychology resting on the claim that the key human adaptation is for nothing in particular is that the claim is an oxymoron: adaptations are, by definition, for something very particular. My argument has been that both the history of the concept of instinct/adaptation and the natural history of encephalisation tells us that that formulation is not useful.

A peculiar outcome of my examination of the Standard Model thesis is the attention it draws to the fact that two (or even three) generations of social psychologists have already tried to incorporate psychological adaptations into the discipline. Tooby and Cosmides take us back to a dormant debate but they do not resuscitate it. The prominent role of the EEA concept is new. However, the inquiry into our natural history it invites suggests that the aspect of mind of most interest to social psychology is not a mechanism in Tooby and Cosmides' sense of the term. It does not get switched on by stimuli *x*, it does not get switched off by stimuli *y*, it does not have predetermined decision rules or algorithms. This is not to say that some adaptations are not for something very particular. It is to say that not all adaptations – not all naturally selected and heritable features of a species – are equally particular.

Whether this analysis matters or not depends on what it is one is interested in. The sort of adaptation that accommodates social uncertainty may not be of interest to, say, the vision researcher who uses evolutionary theory and natural history to construct a psychological model of how we accomplish movement through undulating terrain. Similarly, on my account, social uncertainty does

not encompass the sort of stimuli that provoke invariant reflex-like responses. However, for those who are interested in the social thought and behaviour of extant humans, the past does not explain enough of the present to warrant the place imagined for it by evolutionary psychologists.

I said in the general introduction that, before beginning this dissertation, I wanted to do some evolutionary social psychology. I wanted to test hypotheses derived from evolutionary theory. In the event I have spent my time reading and thinking about evidence and arguments for and against the very prospect of an evolutionary social psychology. I want to close with some comments that indicate why I, like so many others, ended up writing about rather than adding empirical results to evolutionary science.

The following is a statement about material objects: all human beings are products of evolution. This is not a controversial statement in the psychological and behavioural sciences. The following is a statement about behaviour: the behaviour of all humans is precedented in evolution. The following is a statement about thought: the thought of all humans is precedented in evolution. These are controversial statements in the psychological and behavioural sciences. The first statement is not controversial because psychologists and behavioural scientists have been persuaded by the argument and the evidence. The other two statements are controversial because psychologists and behavioural scientists have not yet been persuaded by the arguments or presented with sufficient evidence. The old literature on instincts and the new literature on adaptations points to an underlying frustration in Darwinian thought in the social sciences. The frustration is with those who accept the claim that all human beings, as material objects, are products of evolution but do not, at the same time, accept the claim entails that all human thought and behaviour is explicable in terms of natural history.

To date, the frustration has not been productive. In the psychological and social disciplines the power of evolutionary theory seems to create distortions in our thinking and rhetoric in our writing. Luther Bernard's *Instinct: a study in Social Psychology* reads like a farce – the story of a concept gone mad - but it is no such thing. What it is is an account of the immodest application of a good idea. In a similar vein, Philip Kitcher's discussion of the immodesty of sociobiology is ideally titled *Vaulting Ambition*. Today, the apparent result that sociality is responsible for the brains that so clearly mark modern humans out even with our own genus, makes evolutionary social psychology particularly vulnerable to overheating. If an evolutionary approach to psychology, behaviour and society is to have anything like the sort of future imagined by thinkers such as William McDougall, Edward Wilson, John Tooby and Leda Cosmides we must be more circumspect. With an eye toward the false starts of the past, in looking to avoid the pitfalls of our own ambition we can help ourselves:

we must always stop and ask what it is that the terms such as "evolution" or "Darwinism" adds to our accounts of social thought and behaviour.

For example, let us suppose that David Buss is correct and we say that males and females are equipped with two sorts of mating strategy (Buss, 1989a, 1990, 1992a, 1994). As a matter of fact, and as are all other empirical findings in the psychology of human beings, this finding is derived from extant populations. In what way is the finding altered, what else do we know, if we say that the finding is evolutionary? Does it always follow that we know something over and above, something extra or more if we attach tags such as "evolution" or "Darwinian" to results? The danger is that such tags are used to legitimise or add weight to otherwise ordinary, day-to-day, research. Generating hypotheses coloured by an overt allegiance to evolutionary theory does not guarantee their quality. The truth value of evolutionary theory does not transfer to appended propositions. The brand name of Darwin will not rescue questionable methods or results. Such practices lead to common dogma. It may be exasperating, but I think that we still are not thinking hard enough about evolution.

Appendix 1

The influence of *The Psychological Foundations of Culture*

At the time of writing John Tooby and Leda Cosmides' *The Psychological Foundations of Culture* (1992. In, Barkow, J., Tooby, J. & Cosmides, L (eds) *The Adapted Mind: Evolutionary Psychology and the Foundations of Culture*. OUP) had been cited in 202 journal articles (Web of Science citation search, 24/05/01).

The three papers that precede and presage *The Psychological Foundations of Culture* are;

Tooby, J. & Cosmides, L. 1989a. Evolutionary Psychology and the Generation of Culture: Part, Theoretical Considerations. *Ethology and Sociobiology*, 10, 29-49

Tooby, J. & Cosmides, L. 1990a. On the universality and the uniqueness of the individual: The role of genetics and adaptation. *Journal of Personality*, 58, 17-67

Tooby, J. & Cosmides, L. 1990b. The past explains the present: Emotional adaptations and the structure of ancestral environments. *Ethology and Sociobiology*, 16, 375-424

At the time of writing these papers had been cited by other authors in peer reviewed journals 111, 119, and 131 times respectively (Web of Science citation search, 24/05/01).

A list of some books and book chapters in which *The Psychological Foundations of Culture* has been cited.

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Appendix 2

Influential authors in social psychology 1908-1989.

The following details and tables have been extracted from work by Collier, Minton and Reynolds (1991). Collier *et al* compiled a list of theorists and researches most frequently cited in social psychology texts books between 1908, the date of publication of two of the earliest and most influential textbooks on social psychology, and 1989. Frequency of citations are ranked in time slices, these being, 1908-29, 1930-42, 1948-53, 1959-69, 1970-79, and 1980-89. Each period is headed by the most frequently cited theorist according to the disciplinary orientation of the author(s): sociology or psychology. For example, in the period 1908-1929 William McDougall was the most frequently cited by psychologists writing social psychological texts, and Edward Ross was most frequently cited by sociologists writing social psychological texts.

Table One

Period	*Sociological authors*	*Psychological authors*
1908-29	E.Ross	W. McDougall
1930-42	F. Allport	F. Allport
1948-53	G.Murphy	G. Allport
1959-69	S. Freud	T. Newcomb
1970-79	G. Mead	L. Festinger
1980-89	E. Goffman	L. Festinger

Additionally, Collier, Minton and Reynolds point out that some theorists appear in the top ten citations list of both sociologically inclined and psychologically inclined authors. These are presented in table two.

Table Two

Period	*Theorists*
1908-29	McDougall, F.Allport, Tarde
1930-42	McDougall, F.Allport
1948-53	Murphy, Newcomb, Freud, Sherif
1959-69	G. Allport, Newcomb, Sherif
1970-79	Freud, Festinger
1980-89	Kelley

And the final list derived here is that of those authors who are cited in the top ten of both sociologically inclined and psychologically inclined authors in three or more periods. These are as presented in table three.

Table Three

Theorist	*Period*					
F.Allport	08-29	30-42	48-53			
G. Allport		30-42	48-53	59-69	70-79	
Freud	08-29	30-42	48-53	59-69	70-79	
Mead		30-42	48-53	59-69	70-79	
Sherif			48-53	59-69	70-79	
Newcomb			48-53	59-69	70-79	80-89
Kelley				59-69	70-79	80-89
Festinger				59-69	70-79	80-89
Schachter				59-69	70-79	80-89

This leaves us with the following list of theorist that, if citations in general social psychology texts are any guide, have been important in the discipline. G.Tarde, E.Ross, W.McDougall, F.Allport, G.Allport, S.Freud, G.Mead, G.Murphy, T.Newcomb, M.Sherif, H.Kelley, E.Goffman L.Festinger, and S.Schachter.

Appendix 3

The development of Darwinian theory in the twentieth century

It is often said that Darwin retreated from the position he set out in *The Origin of Species* (e.g. Badcock, 1994). The validity of this claim depends on what one takes to be the fundamental core of his position. I agree with that his position was essentially Malthusian (Dennett, 1995; Rose, H. 2000) and not, as is sometimes supposed, Mendelian. Darwin did retreat on his original anti-Lamarkian view on inheritance. Without a mechanism for selection he incorporated a sort of morphological learning process that could be transmitted through generations (see Darwin, 1859: 191-195). The modern synthesis is the death of that idea. It is the death of Lamarkism. But this "concession" did not affect his conviction that adaptation (via whatever biological means) was a mathematical necessity. This conviction followed from his reading of Malthus (1798, 1821).

The ideological motivations of Thomas Malthus are variously depicted. Events showed his political economy to be wrong, but his mathematics were sound and simple. Malthus claimed that organisms capable of geometric reproduction would, sooner or later, saturate and exhaust available resources. At the point of exhaustion, some factor or another would militate against population growth rates as had been typical to date. It was the militating (or mitigating) facets that caught the attention of Darwin. Darwin saw that the great "crunch" that Malthus envisaged need not be the supposed cataclysm. It could also be a feature of day-to-day existence. The existence that both Malthus and Darwin had in mind was that of the individual actor – the organism. The difficulties that this view created for Darwin have been extensively discussed (e.g. Sigmund, 1995). It is called the problem of altruism (i.e. Brown, 1985). The solution to the problem of altruism has shifted the focus concerning the unit of natural selection.

Darwinism as was

As is frequently pointed out (Desmond & Moore, 1991; Richards, 1987), the idea of evolution was in the air long before 1859. Darwin's own grandfather had been a proponent (Darwin, E. 1799). On the production of his theory, and the collected evidence in support of it, the general argument was won in the scientific community. The great debate that took place in Oxford in 1860 between T.H. Huxley and the Bishop of Oxford indicated the most acute contention. That contention was between the world view of which natural selection was of a piece and religion (Rose, 1985). There is some very definite sense in which the scientific world was not surprised or inclined to dispute the necessary features of what Darwin had to say.

Still, what Darwin presented was not complete. The rump issue, not fully decided until 1964 and not widely understood until 1976, was: What is the unit of selection? The obvious answer (and the

one behind the social Darwinism of Herbert Spencer for example) was the individual organism. This was certainly Darwin's view and he never abandoned it. Survival, and hence reproduction and extension through generations, was a matter of fitness as specified in the individual organism.

This hard-edged approach, aside its political implications in the dawning age of collectivist thinking (Rose, N., 1985), did not appear to fit the evidence that began to pour out of comparative zoology and ethology. Whilst it was accepted that the physical and mental condition of individuals within a species carried implication for the ability of that organism to survive, it also carried implications for the group in which the individual was nested.

Following the synthesis of Mendel and Darwin (after the former was rediscovered in 1900) the mathematics inherent in the ideas of both men were twined and formalised by Sir Ronald Fisher. Fisher's *The Genetical Theory of Natural Selection* (1930) might be better titled "The Chromosomal Theory of Natural Selection". This is because the level of hard detail stops at the chromosomal level. At the time, chromosomes were the lowest level of specification and identification of species. This work, together with the apparent (one could say obvious without being anthropomorphic) co-operation amongst members of species as diverse as ants and birds, offered what seemed to be sound reasons for believing that individual characteristics pertaining to fitness were pertinent to the survival of the species as a whole. Thus, group selectionism took hold (Wynn-Edwards, 1964).

There is little doubt that group selectionism offered a softening of the darker tones in individual selection. (One hundred years before, Mill's *On Liberty* (1859) seemed to have a similar effect on the doctrine of utilitarianism in that the will of the individual could have a salutary affect on a wider group). On a human level, group selectionism seemed to offer the prospect that the good of the species orchestrated some of the most acute human behaviours such as aggression and altruism.

The strength of appeal that group selectionism offers has not been laid to rest. As recently as 1994 a target article in *Behavioural and Brain Sciences* argued that the idea still has relevance (Sober & Wilson, 1994). A recurring theme amongst those who responded to that article was that the idea brought about a collapse in the constraints the gene selectionist view offered.

It might also be noted that group selectionism held sway over most of the period of the supposed SSSM hegemony. This helps us to put Tooby and Cosmides' thesis into perspective. Because Tooby and Cosmides are so vociferous in their account of the SSSM as an anti-thesis to evolutionary accounts of human mind and social behaviour (as well as a doctrine in its own right), I want to point out that there has, in the shape of group selectionism, been plenty for the SSSM to feed upon. In claiming that individual human destiny is inherently and biologically bound up and dictated by the destiny of the group, Darwinism before Hamilton built the group into the human mind. Right or wrong, group selectionism says that the point and purpose of a species being is not decided in the

individual but imposed by the wider group. In order to understand and find out what is in the interest of the group, highly flexible organisms such as humans referred and deferred to the prevailing notion of the group. It is but a short step from group selectionism to the SSSM. And group selectionism dominated evolutionary biology close to the centenary of scientific psychology.

Darwinism as is

The problem of altruism haunted Darwin (Desmomd & Moore, 1991). The fundamental conundrum was this: If an organism sacrificed itself or any resource (including time) to the benefit of another then that proclivity would, *axiomatically*, not be passed on to others (and, incidentally, this holds for Lamarkian inheritance as well). This is because the bottom line in evolution is differential reproductive success.

The notion that organisms behave according to the good of the whole species had been weakened by the time Hamilton published his solution to altruism. This is indicated in the term "group" selection rather than species selection. The narrower term reflected evidence from the field. It had been noticed that groups, often made up of blood kin, exhibited more of the selfless behaviour that seemed to contradict Darwin and Mendel. Hamilton accounted for this by following through on Fisher's formulation but doing so at the level of the gene. The key notion that Hamilton put forward was that any given gene is selfish but not necessarily unique. A gene could be extant in a number of individuals. The chances of this are a function of the degree of relatedness. The formalisation of this is as follows: **Br>C**

Where **B** = benefit to the reproductive success of the actor

Where **C** = cost to the actor in terms of reproductive success, and ,

Where **r** = the degree of relatedness between the benefactor and the benfitee.

So, provided that there is some degree of relatedness, there is always some potential that it will benefit the actor to jeopardise its own reproductive success provided the success of the other is enhanced. This formula was the beginning of the rigorous application of game-theory mathematics in evolutionary theory (Maynard-Smith, 1981; Sigmund, 1993).

Accounting for kin altruism in this way allowed for both the individual selfishness insisted upon by Darwin and pro-social behaviour to evolve and settle into what Maynard-Smith (1982) termed an evolutionary stable strategy (ESS). The selfish-gene account of altruism achieves this synthesis by concentrating on outcomes and multiple iterations. Altruism need not be explicitly intended to be what amounts to the case in fact; altruism without a representation of it, if you will. And it can fail any number of times but still be selected for when it occurs quite blindly. Altruism works not in spite of selfish genes but because of them.

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