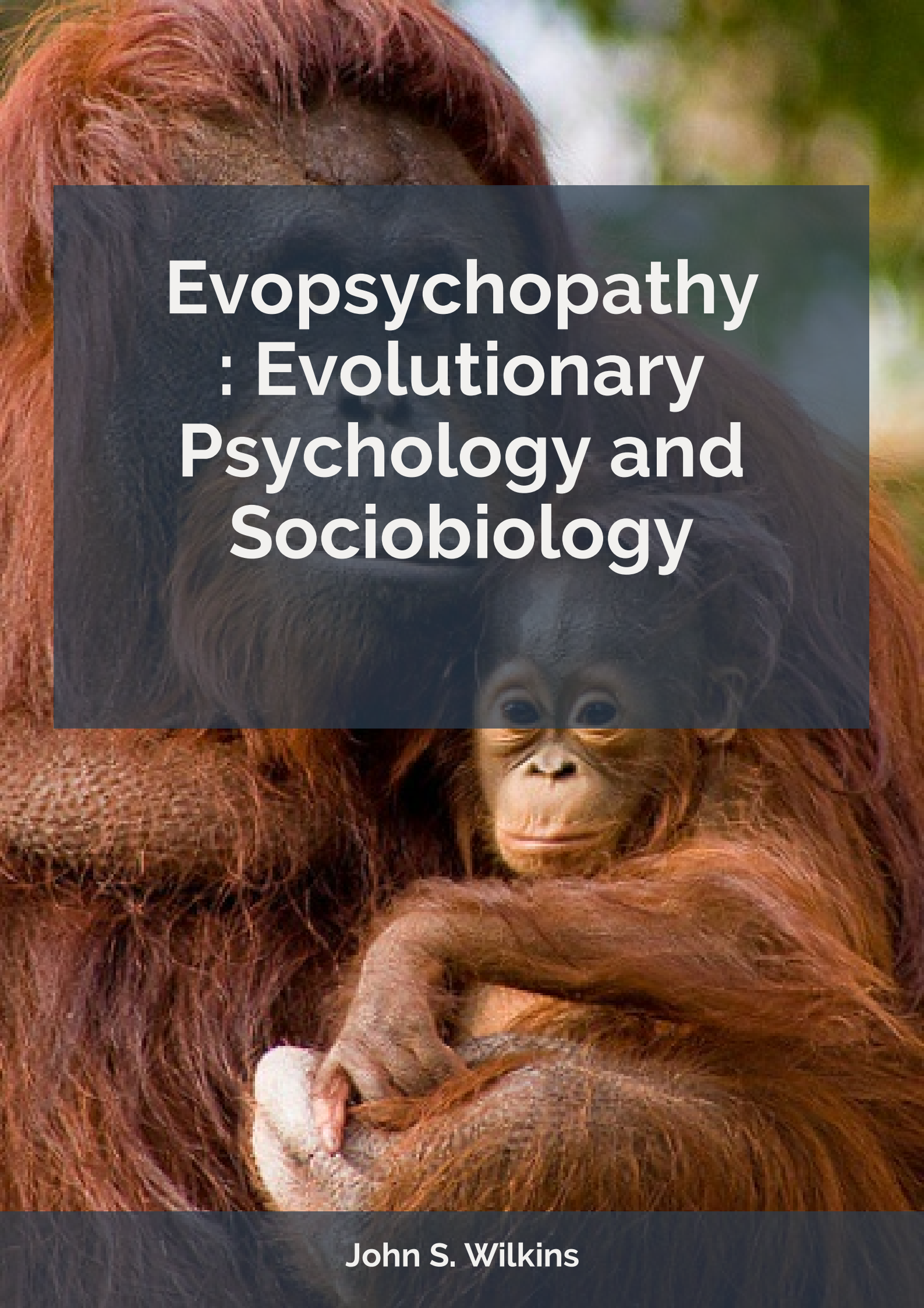

Evopsychopathy : Evolutionary Psychology and Sociobiology

John S. Wilkins

Preface

This is a series from my Evolving Thoughts blog from 2012.

I have had some requests for it in a single document, so here it is.

If you like my work, please donate any amount to my Tip Jar. I suggest this is worth $2 per user/reader.

Thanks

John

Eww, I stepped in some evolutionary psychology and other crap

Sigh

I try and try to stay out of the muck, but they keep *pulling* me back in! I saw what I thought was a careful and rather overly-documented critique by Edward Clint of a talk by Rebecca Watson against evolutionary psychology (EP). It was full of references and arguments, devoid of ad hominem, and well defended. So I linked to it on Twitter. I got these responses:

> Very BAD post. Surprised you'd recommend it.RT @john_s_wilkins @lippard @DuaeQuartunciae not recommending the individual, but that one post.
> *@pzmyers*

> @pzmyers @john_s_wilkins @lippard @DuaeQuartunciae Looked decent to me PZ, at a glance – has anyone done a response/take-down then?
> *@mjrobbins*

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> Nope. not well done most of the time & premises are false. RT @john_s_wilkins @pzmyers @lippard @DuaeQuartunciae EP is not a priori false.
> *@pzmyers*

> EEA is shockingly bad. RT @lippard @john_s_wilkins @DuaeQuartunciae massive modularity of mind? Environment of evolutionary adaptedness?
> *@pzmyers*

And off it went. There were some response articles by Stephanie Zvan (which PZ called a GOOD response), James Croft (the most measured response so far) and Greg Laden, but the raw nerves were on fire. Clint was accused of being an "MRA" (men's rights activist – a term of abuse apparently, and one I hadn't come across) and having evil motives against Rebecca. Others said that because evolutionary psychology was bad science, a post defending it must be wrong (I suspect that might be PZ's underlying enthymeme) no matter what the arguments made were.

I also discovered that while I had linked to one post, I disagreed with Clint on his treatment of agnosticism (1 and 2). I am not recommending him as an Authority, but then I don't do that.

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I am not shocked (any more) that this has descended into partisan personalities. I have come to expect this. But I am interested in the arguments made. Stephanie's post is not bad, but in the end Croft effectively says "It's okay to equivocate and cherry pick if it's for popular purposes", and that I do not agree with. If it's bad science, and we can attack antivaccinationists, homeopaths and creationists for popular bad science, then the wheel turns against us skeptics too.

Clint's defence of EP as potentially good science and not at all to be attacked because of the bad examples and bad reportage is solid, I think. The problem is that EP has its defenders who will ignore all counter evidence and counterarguments, while the opponents will ignore all evidence and arguments in its favour. I want to do something here, which I have previously alluded to: announce my being a *born-again sociobiologist* – EP is a form of sociobiology.

The criticism of sociobiology and EP is largely cultural. It tends to privilege the power structures of the people doing the research. Henrich's, Heine's and Norenzayan's recent essay on psychology focusing on WEIRD students (western educated industrialised rich democratic, if memory serves) points out that *all* psychology and social science tends to do this, by default. But we should try to remove that bias as much as possible in all science, so it is fair criticism of EP also.

But what some people, including (I know from personal contact) PZ and Larry Moran, object to about EP is what Gould called "panadaptationism" and "Just-So" storification. Here is where there is interesting and philosophical issue, and so here is where I am most compelled to comment. Forgive me in advance.

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First of all, there is the issue of when it is appropriate to use adaptationist explanations. Clint cites the leading philosopher on natural selection, Elliot Sober. Now I often disagree with Sober, especially in the assumption of optimisation studies (and of course classification), but Clint is right to cite Sober here:

> Adaptationism is first and foremost a research program. Its core claims will receive support if specific adaptationist hypotheses turn out to be well confirmed. If such explanations fail time after time, eventually scientists will begin to suspect that its core assumptions are defective. Phrenology waxed and waned according to the same dynamic (Section 2.1). Only time and hard work will tell whether adaptationism deserves the same fate (Mitchell and Valone 1990).

Opponents, largely following Gould and Lewontin's 1979 attack, tend to assert (often without consideration of the particular attempts to give adaptive explanations) that any and all adaptive hypotheses are cheap and to be avoided. This has the effect of basically eliminating natural selective accounts of anything. But we know that selection is the only process that results in complexity over any time, and the fact there are complex traits among organisms leads to the inevitable conclusion that we should be able to give selective explanations from time to time. I have argued before that we should think of adaptation as a viable hypothesis at all times; but being viable doesn't make it true. The problem is not that EP or sociobiology makes adaptive hypotheses. They should. It is that they often make them without testing them.

This is no longer the case, at least not universally. Desmond Morris is long gone from the forefront of panadaptationist thinking, and we can start to deal with the more serious claims and studies made. As Clint says

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> Although there are always going to be some flawed studies, researchers weeded out failed hypotheses and refined methodologies. The influence of evolutionary psychology has steadily grown. Evolutionary psychology theories once controversial are now accepted by mainstream psychology.

Mind, that isn't a high bar to leap. A lot of psychology is still fairly simplistic (but not most, by any means). If there's a field that is really well grounded in my subjective assessment, it is comparative psychology, which is cross-specific at comparing human cognitive development and our nearest relatives, the primates. And that gives us a constraint upon EP-style adaptationism. If it is shared across all primates, then it can't be an adaptation to an ancestral environment not shared by all primates (not unless some massively unparsimonious evolution has occurred, in which case we can't say squat about evolutionary history).

But something must have happened in our lineage to give us the traits we now have and it simply is not sufficient to say it could have been evolution by accident. Accident is an admission we cannot explain things. It is the background assumption of anything. And let us not forget that accident is the raw material of selection. Accident proposes, selection disposes. Accidental variation is the origin of things, not the reason why things are retained and built upon, at least, not always. If something can be acquired by accident, and spread through a population via drift, then it can be lost the same way. Nobody sensible would go so far as to say that there is no evolution by accident. But neither should anyone sensible suggest there is no evolution by selection either. The question, as the wit said to the society woman, is how much.

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So sociobiology *as a hypothesis* is acceptable. It need not lead to Nazism, racism, sexism or US exceptionalism. So long as there is empirical data, testing the particular hypothesis at hand, it is and can be good science. It is not the final word. And as Twain said, I wouldn't hang a dog on a newspaper report. EP like anything else can be misrepresented by various interests, just as evolution always has been.

This brings us to the formal and informal fallacies this whole subject seems to attract like things that are attracted to bad metaphors. If PZ is saying Clint's post was bad because it asserts and defends something he *knows* without argument to be false, then that is question begging and displays massive confirmation bias. This is not a good trait in scientists. If, similarly, he approves of Zvan's piece because it agrees with his belief that EP is false, then that too is confirmation bias. If he dismisses Clint's defence because Clint is an MRA or has "issues with Rebecca", that is obviously a fallacy of ad hominem, and a genetic fallacy to boot. His argument stands or falls on the merits of the case made (even if, and I can't stress this highly enough, he is massively wrong about agnosticism!).

And no, ~~Stephanie~~ *(see comments)* James, it is not sufficient to accept a bit of exaggeration or cherry picking or equivocation when *we* do it because it's entertaining or fun. It is false argument. If it's wrong to do it when you are anti vaccination, then it's wrong to do it when you are "skeptical". This is called *tu quoque* in reasoning. Rebecca equivocates between a field and reportage or misuse of a field. She is clearly trying to poison the well. Similarly, Dawkins does the same thing with religion in *The God Delusion*. It's simply dishonest argument, no matter how entertaining.

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In the past I have been challenged by PZ and Larry Moran for saying "we are all subject to our own biases". I know I am (and because they are mine I am not sure what they are, although in the case of chocolate I have suspicions), but Larry once said to me that I should show him his. Well he's not engaged on this topic for now, but here is me showing some cognitive biases of some skeptics.

I initially thought Clint's piece was overkill. Now I see that it will never be enough for some. No matter what his history or motives.

Late note: PZ has a post here and a promise of more to come.

Later note: The first of his αEP series is here.

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1. Conditions for sociobiology

Well I better put up or shut up, I guess. Here are my ruminations, excretions, and expressions regarding evolutionary psychology, or, as we might call it, evopsychopathy. I am, as I have said, a born again sociobiologist, so I guess that makes me an evopychopath.

Let's get a few things out of the way first. Evopsych, or EP, is the third version of sociobiology (SB). It is Sociobiology 3.0. Sociobiology 1.0 was developed by Herbert Spencer among others. It was not the simple point that human beings are evolved animals and so also our psychology must be evolved, which is the common thread not only to EP and SB, but to all neurobiology, and a great deal of modern psychology. It was in Spencer's and Darwin's time also the claim that humans are *sui generis* in some fashion. We had animal dispositions, yes, but something, the famous sentence "Man is the only animal that…" [blushes, uses tools, talks in English, etc.], marks us out. Moreover, in SB1.0, there were more advanced humans and less advanced humans. Often, SB1.0 was used to justify the privilege of the race or class that the SBer was a member of or identified with. Sometimes it justified the status of a race or class the SBer aspired to (as in the case of Japanese modernisers). SB1.0 was immediately applied to larger classes of people than trait groups. The industrialists of Germany before and during the second world war applied it to their nation. The robber barons (or their pundits – it is unclear to what extent the barons themselves believed this) applied it to capitalist classes. And of course it rapidly got used to justify the elimination of disabled, lower class people, and ethnic groups that were unpopular. In the United States, Canada, Australia and the rest of the Commonwealth apart from Britain itself. Germany was only catching up.

1. Conditions for sociobiology

But to what extent was SB1.0 novel? Aristocrats had been talking about "good breeding" and "good blood" since the classical times, and it was the fashion during the later medieval period and the subsequent eras to maintain what were basically breeding charts of families the way one might do for horses (unsurprisingly most of these people were equestrians). Killing ethnic groups you didn't like went back throughout history. All that was novel here was that evolutionary biology was employed to support prior views defending or attacking privilege. It would not be the last time.

SB2.0 was due to the development of genetics at the turn of the century. Although genetics took a while to get reconciled to evolutionary biology (not until 30 years passed was there a full treatment by Fisher), it immediately was adopted by eugenicists moving away from the statistical version of biological determinism that had been promoted by Galton and Perason before the turn of the century. Of course, SB1.0 remained around while SB 2.0 developed.

SB2.0 continued to develop until the 1980s. It often appealed to "genes for" this or that trait, behaviour, or more rarely disposition. No matter how often geneticists cavilled and railed against this usage, and no matter how often journalists were admonished not to use the terminology, still the popular mythos had it that there was a God Gene, a Language Gene, an Autism Gene right alongside a Cancer Gene and so on. And these genes were Selfish.

This is the backdrop to the famous glass of water in the face of Edward O. Wilson, whose book *Sociobiology, the new synthesis* set off a host of personal and political attacks. This was the 1970s (aptly and thoroughly described by Ullica Segeståle in *Defenders of the Truth*) and political outrage was the fashion. A movement known as Science for the People, including Lewontin and Gould, attacked all and every attempt to do SB as fascist and racist. Often enough for the theme to become entrenched, it was.

1. Conditions for sociobiology

So a new form of SB – SB 3.0, or EP – was invented. Like the other SBs it was often employed in defence of this or that cultural or social privilege (indeed, like evolution itself). And early work was hamfisted in an egregious manner. It attracted the considerable rhetorical eloquence of Gould and others such as Steven Rose in attacks that are required reading. Like the rejection of the existence of human races by Dobzhansky and other evolutionary and anthropological researchers after the second world war, it became the consensus that we could not do this except on pain of vapidity and pure conjecture. Adaptationism was a Bad Thing, and led to Just-So Stories.

It didn't help, as it hadn't in the early period evolutionary biology itself, that popular science writers, political writers, and ersatz philosophers had taken on EP and the older SBs as the justification for their own agendas. Writers like Desmond Morris were taken as the best of science, when in fact nearly all of the explanations offered were unsupported post hoc explanations of observed behaviour – if, indeed, it *was* observed. Nothing is so easy to think you see as human social behaviours you already think exist.

When some researchers began to argue that rape or shopping were natural and evolved behaviours, this immediately set off alarm bells. Of course, some of these researchers indeed did justify the status quo as being "natural" but as any philosophy student knows, nature is not prescription, or as we like to put it, is does not imply ought. However, the reaction to the researchers' claims was uniformly based on the notion that if rape is natural it is justified or inevitable, when in fact the researchers tried, sometimes at least, to point out that natural behaviours can be modulated by social pressures, and that being an evolved property is not itself a reason for thinking it is good. Even Darwin thought that the highly evolved behaviours of some wasps to lay eggs in living caterpillars was immoral, despite being adaptive. And Huxley had written an entire book decrying the idea that evolved = justified, back in the late 19th century.

1. Conditions for sociobiology

In this series I do not propose to defend any actual work, or to do a historical review of personalities and political games. Segerstråle's book gives a lot of this anyway. What I am going to do is strive to offer an SB4.0, evopsychopathy. The idea is this: we *did* evolve, and we do know that dispositions to behave are inherited, species typical and the result of selective pressures. So I aim to argue that we must

1. Constrain our hypotheses somehow. I will offer the *phylogenetic bracket* as such a constraint.

2. Specify the explanatory target. I will suggest that we can explain *biological dispositions* only. We cannot explain, using any form of SB, specific cultural practices, any more than a gene can explain how tall a human will grow without consideration of their upbringing, experience, parental resources and experiences, and so on.

3. Separate description from justification of behaviours.

4. Test adaptive scenarios. This means we have to think a bit about how how hypotheses are formulated and tested, whether there is a null hypothesis (there isn't by the way), and what counts as explanation in sociobiological sciences.

2. The phylogenetic bracket

As noted, SB and EP have a very unfortunate tendency to reflect the status quo in their results and research questions. This is not unique to them. History, sociology, other fields of psychology (psychotherapy for gods' sake!), and in my own profession, ethics, all have this "Pull of Privilege". Somehow the results of this research generally seem to show how natural and right things are. I am always amazed that no matter how radical the ethical foundations in philosophy, ethics always seems to end up supporting the bourgeois status quo (Peter Singer, whose approach I disagree with, is an honourable exception – he's not afraid to follow his ethical foundations wherever they lead).

This Pull is very hard to shake off. Historians of science (and more recently historians in general) have a term for it: *The Whig Interpretation of History*, AKA whiggism (also triumphalism, or presentism,). It is widely, and rightly, seen as a sin of interpretation. Why? It is because if you wish to understand the subject under investigation, rather than tell a story that makes you feel warm and comfortable about you and yours, you must to the best degree possible rid yourself of your relative attachments. You can't see animist religions in terms of Christianity, alchemical practitioners in terms of modern chemistry, or sexuality in the Azande, say, in terms of Middle American marriage practices and categories (or worse, of *penguins* in terms of those practices).

So defeating the Pull of Privilege is a serious concern in any discipline that studies human behaviours. How can we do it in SB4.0? As it happens, I have Thoughts.

2. The phylogenetic bracket

Behavior is quite labile, evolutionarily, and so there has been debate over whether it can be treated as a homology (Brigandt and Griffiths 2007; Hall 2012; Love 2007). However, classes of behaviors can easily been seen to be homologous. For example, most passerine birds have courtship displays which, while individually unique, fall into a shared class of behaviors, and moreover, these dances are very similar within groups such as riflebirds or lyrebirds (Andrew 1961). It is hard to reject the idea that these are homologies, with species-relative instantiations. The entire field of ethology is founded upon investigating both the commonalities and unique differences of behaviors in many groups of organisms.

There is, in palaeontology, a technique known as *phylogenetic bracketing* (Witmer 1995). If you need to reconstruct something that doesn't fossilise in a fossil taxon (say, *T. rex*), you can place it in a phylogenetic tree and see what its surrounding surviving relatives have in the tissues and structures that don't fossilise. By projection you can presume this is true of the extinct organism. Likewise, if you find a behaviour in known taxa, you can inductively project (Goodman 1954) from the known to the unknown if they are within the same clade. Of course, this only works if the clade happens to be relatively unspecialised, and the greater the evolutionary distance, the less specific you can get (remember: *specific* and all other words based on the Latin *spec-* root, are modifications of *species*). So you may know that all falconiformes have a recurved claw, but you may not be able to confidently predict whether an unobserved species of falconiforme is a hunter or scavenger. You'll know, though, that it eats meat.

The application of phylogenetic bracketing here should be relatively obvious. If we wish to reduce the Pull, we need to set an objective behavioural baseline for all humans and not just the WEIRDos. We cannot do this from within the milieu of a culture by an act of will or imagination. But we can bracket humans among the Hominoidea, the African Great Ape clade.

2. The phylogenetic bracket

For instance, suppose that one knew nothing else about the human species than that it was squarely nested within the Hominoidea . What would we know about that species? The inferential return on that phylogenetic investment is extensive and indefinite. We would know the species had a particular skeletal structure, with, among other things, four limbs ending in five-digit manus, or hands, and that it had a certain visual system, aural system, and so forth, and interacted with the world at a certain macroscale, in what von Uexküll called its *Umwelt* (1957), or sensed environment. It would have the primate *Umwelt*, and so interact with commonsense objects (Griffiths and Wilkins 2012). For our purposes here, however, what we would mostly know is that it was a *social* species with social dominance hierarchies.

Now it is very hard to find animal species that are not in *some* sense social. At the least they must interact during mating. But sociality comes in degrees ranging from a brief or even displaced social interaction at mating through to care of neonates and, as in chimp, gorilla, and even orang social behaviors, lifelong interaction with conspecifics of all ages. The one thing that marks all primate species, and thus all hominoids, is that they form dominance hierarchies based upon pairwise interactions, with sanctions of both a positive (reward collaborators) and negative (punish defectors) nature. As has been observed in many primate species (chimps, bonobos, various baboons and monkey species), alliances are formed and social deviants are punished (Cronin and Field 2007; de Waal 1982, 1989). We are socially normative apes. Moral strictures and social conformity is what apes *do*. Achieving high social dominance results in improved health and better mating opportunities (Burnham 2007; Creel 2001). Hence, such behaviors must be expected to play a crucial role in any social institution that may evolve generally in human, which is to say, one particular ape species', social structures.

2. The phylogenetic bracket

But it will not do to take what is observed among bonobos or gorillas and simply apply them directly to our human species. We know that the human species must typically have *some* social dominance behaviours or dispositions (why I keep referring to dispositions will become clear in a later post on selectionist explanations); we know roughly how they will be formed (through pairwise dominance displays and competitions, mate choice, etc.) and we have some reason to think they will be primarily male biased as all but bonobo dominance hierarchies are (but note: one species defeated the generalisation based on phylogenetic bracketing. This is not an infallible inference methodology, it is, as we philosophers say, *defeasible*).

Of course, there are differences between hominoid apes and humans in social dominance behavior, as there are between the non-human apes species. Common chimps tend to have a single alpha male, and the hierarchy is always determined by male status and females derive their status from their mates. Status is determined by aggressive competition and mating occurs in proportion to achieved male status. Bonobos, on the other hand have a hierarchy driven by female choice. This reflects their degree of sexual dimorphism: chimp males are on average around 125% the weight of females, while bonobo males are only slightly larger if at all than the females. Gorilla males are up to three times the weight of the females. As a result a single alpha male guards a harem of females against "bachelor" males. The hierarchy is both within the family, and between males in a territory. Humans, like bonobos, have very slight dimorphism: males weigh around 108% of female weight on average. The degree of polygyny (number of female mates per successfully mating male) roughly correlates with size dimorphism. Social hierarchies vary according to species-typical mating strategy.

2. The phylogenetic bracket

Which comes first, the strategy or the dimorphism? That is in many ways a silly question; strategies are constantly evolving, largely, I think, in response to ecological conditions, but also there is a large degree of contingency here – what one species might develop will depend on accidental factors that are largely unpredictable relative to another. Dimorphism is both a result of the evolution of mating strategies, and also a cause of it. These things evolve together. Nevertheless, when you find a fossil ape that has massive dimorphism like the gorilla, you can bet it was a harem-style (almost herd-style) social animal.

So what would we predict about humans, if we had just arrived from Mars and been given only a copy of Walker's Primates of the World without the section on humans? We would first of all predict that they would form dominance hierarchies, and that high status individuals would reward those that conformed to the group norms so formed, and punish those who defected from them. We would predict a slight male dominance over females. We would expect that the progeny of high status individuals will preferentially rise to higher status than those born of low status parents (primate societies are not meritocracies, Silk 2009). We would expect that male dominance relies upon height and musculature – the bigger males tend to gain higher status even if there is no violence in the dominance behaviours of the species. There are more things we might say, but you see how this applies.

2. The phylogenetic bracket

But what would we *not* be able to predict? Well we would not be able to predict when cultural influences modulate, moderate or even override these social dominance dispositions. We could not have predicted Elizabeth I or Benazir Bhutto (or maybe we could – both were acting as if they were sons of powerful males). We could not predict the rise of liberal democratic ideals, although once it is in play we might predict its eventual corruption and decline as plutocracy and nepotism reasserts itself. We could not predict American supermarkets, although once they are observed we can see some of the foraging dispositions (of males *and* females) of our predecessors being exploited.

All this does is set up the baseline of expectations. It is not, I think, even remotely possible to give a complete account of societal structures in terms of our shared ape heritage, although that heritage can be ignored at our peril.

I should note that the sort of explanations I am giving (sketching roughly) here are not the outcome of evolutionary psychology *per se*. Instead, it is the outcome of a number of disparate and only vaguely connected lines of research. Such research covers comparative cognitive psychology (e.g., Suddendorf 2008, a critique of Wynne and Bolhuis 2008), race psychology (Sidanius and Pratto 1999, Sidanius *et al*. 2000), the effect of status on primate testosterone levels (Anestis 2010, Eisenegger *et al*. 2011, Gray 2011), the neurology of social behaviour (Harmon-Jones and Winkielman 2007), and so on. Each of these either relies upon something like phylogenetic bracketing (as in comparative psychology) or is consonant with it (as in social dominance psychology).

2. The phylogenetic bracket

There are limitations to this method. An inference to homologous traits or behaviours is going to work just to the extent that the species does not have what cladists call an *autapomorphy* for that trait, which is to say a trait that is unique or in a unique state for that species not shared with other species. For example, the speech centres of the human brain have homologs in other primates, but *not as speech centres*. Complex grammatical speech is our autapomorphy (and perhaps was also shared by extinct species like *H. heidelbergensis*, *H. neanderthalensis* and *H. erectus*, but we are the sole possessors of it now). So we could not predict symbolic language from a knowledge of other primates.

But the real problem with sociobiological projections is what I call the problem of analogy. Previous SBers would look at eland stamping in a place to attract mates and infer that humans would have "stamping grounds"; that chickens maintained social dominance by the use of violent pecking, and assert that humans had "pecking orders", and so on. Even ants and bees were used to generate analogies of this kind. But we aren't ants, bees, elands or chickens.

To infer that we have trait *X* because all in our clade does is a licensable inference, but much of what we are looking at is not a homology at all (although every trait rests upon underlying homologous structures and systems). Instead they are themselves analogous traits (like shopping, or "rape"*) that may in fact have no homologous dispositions underlying them. Since we want to know what humans should have without ascertainment bias, we must treat these inferences as highly questionable. First you catch your homology. Some real science has to be done.

Moreover, any trait that has been the subject of a selective sweep is, *by definition*, no longer a homolog in terms of its function. So if something did occur in the X million years since we separated from taxon *Y*, it is not a homology with *Y*'s function, even if it is a structural or physiological homolog. So, for example, the role of testosterone among humans may not be (but it actually is according to the research) the same as the role it plays in other primates. You have to check.

So the *target* of explanation is crucial. I'll return to this in the next post.

Cheesy footnote

* *Rape* is a social and legal term that is often illicitly projected, whiggishly, from humans to other animals like ducks and beetles. Similar objections should be used for terms like "homosexual", "thief" and so on. Sometimes these terms are harmless, but very often they are not, and mislead us into anthropomorphisms. Caveat lector!

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3. The explanatory target

In the Bad Old Days, biologists, including Darwin, used to speak of "instinct" as an inherited trait of organisms. Darwin has a comment in his Notebooks

> It is absurd to talk of one animal being higher than another. — We consider those, when the intellectual faculties [/] cerebral structure most developed, as highest. — A bee doubtless would when the instincts were —

and he spent some time trying to work out how bees had an instinct for the formation of hexagonal honey combs. Instinct was a kind of Platonic remembrance, something that evolved before you were born but which you "knew" at birth. This is the hoary old chestnut* of nature-nurture. And it was employed at length by the nascent science of ethology that was spawned by Darwin, especially in the theories of Konrad Lorenz, who argued that the synthetic *a prioria* of Kant (things known to be true a priori that are not true by necessity) are the evolutionary *a posterioria* (1996). We are born with instincts.

Psychologist Danny Lehrman took Lorenz's idea to task in the 1950s with a direct critique (1953) followed by a series of articles that extended the research.

> To Lorenz, *the* instinctive act is a rigidly stereotyped innate movement or movement pattern, based on the activity of a specific coordinating centre in the central nervous system. [338]

3. The explanatory target

He notes

> Lorenz and Tinbergen consistently speak of behavior as "innate" or "inherited" as though these words surely referred to a definable, definite, and delimited category of behavior. [341]

He then considers cases, and ends up

> It is obvious that by the criteria used by Lorenz and other instinct theorists, [these cases] are not "learned" behavior. They fulfil all the criteria of "innateness", i.e., of behavior which develops without opportunity for practice or imitation. Yet, in each case, analysis of the developmental process involved shows that the behavior patterns concerned are not unitary, autonomously developing things, but rather that they emerge ontogenetically in complex ways from previously developed organization of the organism in a given setting. [343]

In other words, "instincts" must develop in the right environment. Change the environment during crucial developmental phases, and you do *not* get the "inherited" behaviour.

> It must be realized that an animal raised in isolation from fellow-members of his species *is not necessarily isolated from the effect of processes and events which contribute to the development of any particular behavior*.

There is "learned information", or better "acquired information" from the developmental environment. What is *inherited* is not the behaviour, but a disposition to develop it in the right circumstances (see Griffiths 2002 and Maclaurin 2002 for a discussion of this).

3. The explanatory target

Much of the trouble with SB and EP is that this fine distinction is overlooked, or if it is acknowledged, people tend to fall back into modes of expression or thought that the distinction erases. For this reason I think it is best to realise that always what we are discussing is not instinct, nor even the behaviours themselves, but a disposition, under the right circumstances, to develop typical behaviours. Let us call these *dispositional behaviours*. Henceforth, when I speak of d-behaviours, I mean dispositions to develop the behaviours.

So, we cannot speak of foraging behaviours, but we might be able to speak of a foraging d-behaviour, which is, a disposition to acquire the skills to find food. It would be odd if we did not have that. And there *may* be (although I rather doubt that there is) a difference in genders in the strength of that d-behaviour. Note, I did not say that women forage and men hunt. It would instead be only that women tend to acquire foraging behaviours more easily than men do, not that men could not, nor that women could not hunt. Given everything we know about variability of genders and dispositions, it could hardly be otherwise. Many men will learn to forage, nearly as many as women, even if there is a variation in d-behaviours.

3. The explanatory target

So marked differences in such behaviours will be the result of cultural overrides. Genes do not have culture on a leash, they merely bias the ways in which culture is acquired. This is not really genetic determinism, so much as genes as one factor among many (and not even the most significant) for behavioural development. And moreover, once you have identified that target of explanation correctly, you *cannot* justify some behaviour as "natural" therefore "justified", since the multiplicity of causes for the shared behaviour will include culture, social organisation, availability of food during childhood, the local climate and very possibly epigenetic effects of the developmental influences acting on your grandparents, none of which are heritable biologically speaking [pedants will say epigenetic effects are inherited, but I think they are simple developmental processes that cause F2 generational effects. In effect, the grandchild is, with respect to that epigenetic outcome, an extended phenotype of the grandparent].

So knowing what explanatory target is available to SB4.0 is crucial. We can explain, where and if we can explain, biological influences upon behaviour only by explaining these dispositions. Nobody is born knowing these behaviours or skills. Instead they are born able to acquire them when the conditions are right. There is no Platonic remembrance going on.

Now a second issue about targets of explanation in SB4.0 is whether or not these are homologies. Under the phylogenetic bracket approach, you can only identify a trait as a shared trait of the group (in this case of Hominoidea) if the trait is actually something that is homologous in that group. As I have said, behaviour is not always considered a homology, but given that d-behaviours are species typical and heritable, they are. In fact d-behaviours are simply another developmental disposition, like growing a spinal column (but not in folate poor environments) and so on. So it seems that it is the *dispositions to develop* a behaviour that is the target of explanation in SB4.0, *not* the behaviours themselves. Lehrman's point is correct.

* Is “hoary old chestnut” a hoary old chestnut?

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4. Adaptive scenarios

Although I think that Darwin's greatest idea was *common descent* as an explanation for the relationships of organisms, I am very much in the minority here. Most people take it as read that Darwin's best idea was *natural selection*. However, ideas very like natural selection (NS) had been around for a long time, unsurprisingly since they are employed in animal husbandry (the artificial/natural distinction is, well, artificial). What was novel about Darwin's and Wallace's use of NS was that it caused change in species, or so they thought. These days the consensus appears to be that species originate independently of natural selection processes, and selection changes the ecological adaptive features of the species rather than the isolation of species from other species.

That is by the way, however. Almost everyone in the evolutionary psychology movement and prior sociobiologies, at least after the turn of the 20th century, thinks that natural selection accounts for human behaviours. The issue is again a haggling over the price: how much and how often, and of what? So a major issue in SB4.0 is the role of adaptive explanations: what should we propose, how do we test them, and what is the default explanation: chance or selection? That is what this post is about.

EP and SB have typically presented explanations of human behaviour based upon the adaptation of those behaviours. Everything from mate choice to rape to religious belief to cognitive deficits are explained in this fashion. I do not propose to examine any of them here. What, as a philosopher, I find interesting are the assumptions underlying them, and how they might fail. Scientific research has a number of mathematical and experimental techniques to determine if something is the outcome of selection: selective sweep analysis, experimental work on cases, and phylogenetic comparisons. Each seems to have problems.

4. Adaptive scenarios

To determine if something is the result of a selection process, one has to have a good idea of what it is that could be the target of selection. The Standard Answer is that *genes* are, but often as not we do not yet have much of an idea what genes are responsible for what traits, and what the norm of reaction (the way the genetic trait is expressed) might be. This has always been a problem for evolutionary genetics, and indeed for a long time, a "gene" was simply a heritable trait (or rather, what underlay a heritable trait). As once noted by Lewontin, phenotypes are what Mendelian genetics studied.

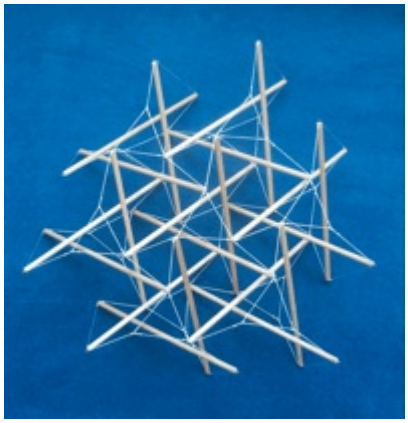
So EP/SB has to indirectly approach selectionist explanations. Often they do not, but the better studies have to face the same methodological issues that every other evolutionary explanation does:

- We do not know the genes for most traits (and few traits are single gene traits, even for metabolic diseases),
- We often do not know much about the environment in which selection is thought to have occurred, and when we do it is global rather than local (e.g., a snail is not affected by the climate of the whole world, but by the microclimate of its hillside or valley),
- Information about the past is often fragmentary, leading to easy and cheap narratives that happen to suit the researchers,
- Information about the present is also often fragmentary and partial, and
- If a trend is seen in the data, it is often unclear when this is due to drunkard's walks (chance) which can deliver directionality over moderately long periods, and when it is due to selection (which can be chaotic if the environment is).

4. Adaptive scenarios

Moreover, the standard EP approach relies upon an atomisation of traits that is more based on expectation than upon observational data. For example, and the key issue for EP for humans, the "massive modularity hypothesis" requires that the cognitive and psychological traits are parcelled into discrete and relatively independent modules. There is a parlous amount of evidence for this outside sensory modalities. Yes, vision (and possibly hearing and the other senses) is quite modular. It is also very old, going back perhaps to the Cambrian, so given that things with eyes need to see well, we might expect the visual module to be independent of the other neurological traits.

But modularity is not required by evolutionary theory; in fact it seems more to be a matter of convenience for researchers than anything else. A complex multifactorial system can be modified simultaneously in many ways by selection, so long as the factors (i.e., the underlying genetics) are heritable, because what NS requires is that things be heritable for them to be adaptively changed by NS. Think of an organism as a system that develops like a sprung mess of rigid poles connected by elastic bands. You can move several at once and the whole system will reshape (this is called tensegrity by Buckminster Fuller, as a punishment for thinking about these things). The rigid poles are the heritable genes, and the rubber bands are the norms of reaction.



[A tensegrity example, from here.]

4. Adaptive scenarios

So we might expect that you can modulate behaviours (or at any rate d-behaviours) by adjusting a few genes at once.The requirement that d-behaviours (or for that matter expressed behaviours) should be isolated and independent of other traits is not required. Gould and Lewontin referred to this as “inappropriate atomisation”. The “adaptationist programme”, they wrote (Gould and Lewontin 1979: 585), starts with this step:

> An organism is atomized into ‘traits’ and these traits are explained as structures optimally designed by natural selection for their functions. For lack of space, we must omit an extended discussion of the vital issue: ‘what is a trait?’ Some evolutionists may regard this as a trivial, or merely a semantic problem. It is not. Organisms are integrated entities, not collections of discrete objects. Evolutionists have often been led astray by inappropriate atomization, as D’Arcy Thompson … loved to point out.

The emphasis upon isolating single genes or alleles is a case in point. No gene does what it does in a vacuum (in fact, put DNA into a vacuum, and it will simply sublimate and denature). It needs not only other genes but a developmental organism in an environment it affects simply by developing. I can’t stress this truism enough. What a gene *does* depends on the system it is a part of.

4. Adaptive scenarios

Modularity, whether of genes or organs or neurological systems, is only ever a conceptual abstraction. Cognitive modularity, however, was based upon the presumption that to be the subject of selection, a cognitive process had to be isolated from other processes in order to be optimised, to be *encapsulated*: modules were *shallow* so they could produce outputs without much delay based on the types of inputs they were optimised to receive, *domain specific* to they only received one type of stimulus or input, *inaccessible* so they were not affected by content or dispositions elsewhere in the brain, and so on. Later work showed that even those exemplars of modularity, such as are tested by the Wason Test, of reasoning are able to be moderated and even interrupted by contents of the cognitive system

So, enough about what *fails* or is problematic about EP and SB. What about what works?

Some may take issue with (PZ has) my claim that only selection can generate complex d-behaviours. A brief word is due. We know that selection (as an instance of general sorting processes) can generate complexity of behaviours. We know that a lineage or population that has some suite of behaviours can randomly attain a more complex behaviour. I am not suggesting that random variations *within a population of already high complexity* cannot produce such behaviours. But over the longer term, what randomness giveth, randomness will taketh away. For a trait to persist in a population (note, in *a* population, not a lineage of species over large periods) and to go to fixation if the population is of any size it is more often than not, much more often, due to selective pressures on those behaviours than to chance. And where using chance as a "null hypothesis" generates few if any hypotheses of what did happen that are testable, selective explanations do generate many, often very specific, hypotheses that are testable.

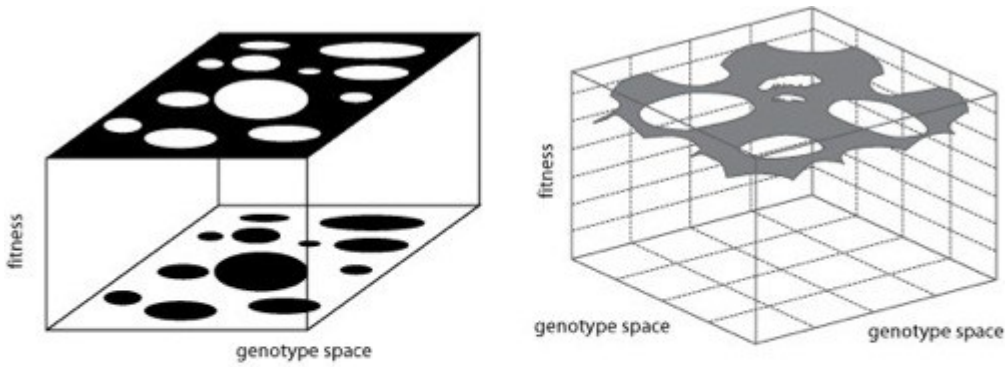
4. Adaptive scenarios

There is a real issue (see here, here and here; the work that convinced me of this is Fidler's) with null hypotheses anyway. A "null" hypothesis is, simply put, the hypothesis that the researcher or the researcher's community defaults to in the absence of anything else. In short, the null hypothesis is what the community of researchers is comfortable with, and since this is not really constrained by data (there are an indefinitely large number of potential nulls) testing between a null and positive hypothesis is at best a very localised test.

But leave this to one side: if chance explanations of a d-behaviour explain it arising, it can equally explain it disappearing. To explain the *persistance* of a complex d-behaviour, selection is pretty well it. As was long ago noted the origins of the raw material of selection are down to accidental changes, but not their retention.

4. Adaptive scenarios

But that is not the whole story. Once you *have* a reproducing populations of organisms of overall high fitness, individual d-behaviours may wander about stochastically. This can be true of a single variable, so long as the overall or general fitness of the population mean is high enough. So it comes down to being able to establish whether the d-behaviour is likely to lower the fitness enough that selection pressures will overcome the randomness of mutation and recombination. And there is no principled answer to this – it relies upon the facts of the case; the boundary conditions and the number and relation of the variables. Sergey Gavrilets (1997, 2004, see Wilkins 2007) has noted that in a correlated or smooth fitness landscape (such as we might expect nearly all species to exist in when in their ordinary environments) there are what he calls "holey landscapes" (see figure), which are components of fitness space that are of roughly equal fitness, and which are connected in various ways to higher and lower fitness components. In short, in a sufficiently high dimensional landscape, a population may drift around at about the same fitness (in other words, its d-behaviours can shift stochastically) so long as the fitness of that population remains about the same. So this implies that a d-behaviour can be both the result of selection relative to its absence, but also the result of chance relative to equally fit alternatives. The chance/selection dichotomy is misplaced. Both can be true simultaneously.



Left: From Gavrilets 1997, right from Gavrilets 2003

4. Adaptive scenarios

So to take on the adaptationist program (or "programme" as Gould and Lewontin spelled it) is not to make a mistake. It is to make an epistemic bet. Cheap and simple adaptive hypotheses must be treated with derision, of course, as they must in every aspect of science – this is not a critique of EP as such, but of bad science. But good ones must be treated as working hypotheses. There's a big literature on testing and choosing between models. I shan't bore you with that here (I find the Akaike Information Criterion approach perplexing, for example, which probably says more about me than it). The point is that EP/SB is not required not to be ~~non-~~adaptationist as a null hypothesis.

In the light of all this, I suggest that so long as we test our adaptive scenarios, and do so realistically on the basis of phylogenetic bracketing (to ensure we are actually dealing with a d-behaviour) and on the best data (especially that of neurobiology and cognitive psychology), EP/SB (or SB4.0 at any rate) has a right to be adaptationist, and that alternative approaches must be justified. Adaptation *is* the "null hypothesis" in the case of complex d-behaviours, subject to the qualifications I make above.

4. Adaptive scenarios

As once said, there's the bit where you say it and the bit where you take it back. Adaptationism does not explain, I believe, the persistence of a trait across phylogenetic bifurcations – that is, in the case of evolutionary lineages. Or rather, it can, but only in the sense of it being "evolutionarily conserved". For example, the structure of the genetic code is highly conserved. Although there are some 15 variants across the whole of life, they vary only in details. The having of the "universal" code is highly fit, and variants tend to be eliminated. Many developmental, phenotypic, physiological and cytological structures are highly conserved. Why? The answer has to be something like: if you deviate from this state, your fitness is lowered, *just because* it is the default or modal state. So the having of, say, a particular social disposition among primates can be maintained because the having of that disposition increases your fitness – deviants get fewer mating opportunities. Creatures inveterately wrong at following the norm have a pathetic, but praiseworthy, tendency to die before reproducing their kind…

Which leads us to a final point about adaptation: the what it adapts *to*. Good Old Fashioned Adaptationism (GOFA*) always presumed that what the fitness assigning process was had to do with the ecological context of the organisms. But equally, or in the case of highly adapted creatures like primates, predominantly, the fitness assigning process is social. You are fitter because your d-behaviour fits a potential mate or social conspecific. Here I think the emphasis has been more productive. Others will disagree, but as the Buss Lab's defence shows (Confer et al 2010), there are some useful results in so doing.

* Not to be confused with GOFAI, which is a position in artificial intelligence philosophy.

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Conclusion

The criticisms of evolutionary psychology and its predecessors sociobiologies 1 through 3 focus on three major points:

1. It is adaptively-biased;

2. It is gene-centric (or biological determinist, which amounts to the same thing);

3. It is culturally biased in favour of the privileged classes of the people making the claims.

I hope I have dealt with, or guarded against, each of these, but I would like to note something that any evolutionary thinking person must accept: our biological foundations for psychological and cognitive dispositions *did* evolve. *Something* like SB must be true. So what we must do is to limit the excesses (which exist in every kind of social and psychological science anyway, and must be limited in every approach), and seek to uncover what the bases of our minds are. This has to be acceptable to any naturalistic evolutionary theorist. If it is not, then one has to suspect that there is what Dennett once called "white picket fence" mentality in play: humans are more important, qualitatively different, or somehow dualistically distinct from all other living things. And to hold this view is to run contrary to all the available science. One might understand why Plantinga wants to defend this kind of qualitative dualism (for him, humans *are* different to all other living things; he is not a naturalist), but why Fodor? Why Gould? What is happening here?

Conclusion

This falls out of a larger project of what philosophers refer to as the *naturalisation project*. It is the view that everything can be given a natural account, at least if we were able to gather the right data and understand the natures involved. Most naturalists are physicalists, but naturalism is not necessarily about ontology; it is about explanations. So far as explanations rest on ontologies, naturalists are physicalists, but it doesn't do to equate the two.

Those who, like Fodor, wish to privilege human (and possibly others species') intellection and semantic reference as being irreducible to computation or to physical processes (usually relying upon a failure of denotation of terms, which is, in my view, a matter of confusing the signs for the signification par excellence; but leave that to one side for now), treat these mental events as non-physical (although they must of course exist on a physical substrate in most accounts). So EP and SB fail because they presume that the irreducibility is a failure of language not of principle, and that we are making some kind of mistake.

Others have consequentialist objections, like the apocryphal bishop's wife who said that if evolution from monkeys is true, let us hope it does not become widely known. If we have our prized characteristics by evolution and selection, then we are lessened thereby. We might find out that we are inclined to racism, sexism, and oppression. If these things were true in virtue of an evolutionary account (rather than being what we all understand from experience anyway), perhaps we might justify them thereby. But we all know (at least if we have read our Moore) that the mere fact that something evolved doesn't serve as justification any more than the success of the Romans (or the Americans) justified the Caesars' (or the Kennedys') pre-eminence.

Conclusion

If we did evolve with a predisposition towards rape (and I do not think this has even been shown to have a non-cultural component yet, so bear with me), surely to know this is not to justify it, but to forewarn and forearm? If males tend to rape, change the culture to guard against rape. If they do not, then you will find that there are other factors that explain, for example, the high rates of rape in India or other societies, and be able to look for these factors and modulate them. To know ourselves is a *virtue* not something to be feared.

As was once said by of all people a seventeenth century preacher, things are as they are, and their consequences will be what they will be. Why, then, should we seek to be deceived? Humans must be what they are via natural processes if you take the science seriously. Knowing what we are can only aid us in building a better society.

I have tried to suggest that adaptationism is not the evil demon it is sometimes painted to be, but this needs more qualification. Individual alleles or variant traits may indeed go to fixation in a population by random processes (although something like an SNP – single nucleotide polymorphism – is way below anything that would count as a psychological trait unless it happens to be akin to a single base pair defect in a psychological process~~, like Williams' Syndrome~~†). However, I regard the overall absolute fitness of modern organisms to be very high indeed. In the light of the rigid stick and rubber band metaphor I used above, we might expect that multiple-gene traits will be maintained at a high fitness. So it resolves to a question of what the explananda are. In short, how *do* we atomise the biology here?

Conclusion

We do it the way we approach any problem domain that is not already clearly atomised. We observe, try different things out and when we find a promising and productive line of research, we follow it. When we have several such lines of research we run them in parallel and wait and see. Sociobiology is one (several, perhaps) of those lines of research, and it should be followed to the degree it is both promising and productive. And it seems to be productive, whatever the promise its proponents see in it. Massive modularity is a dead issue, in my view, but we still can identify, quite clearly, heritable traits, and seek to find out if they are heritable because they are adaptive or because they are side effects of something that is adaptive. Ruling the sociobiological approach out of hand *tout court* is simply dogmatism. It is the opposite of scientific reasoning.

So I have nailed my colours to the mast. I am a born again sociobiologist. I don't like some of the tenets of other sociobiologists (such as massive modularity or group selectionism) but they aren't definitive of the approach; merely the contingent hypotheses and methodologies of some sociobiologists. If this be heresy, then you mistakenly think science is a religion or ideology.

† Clem Stanyon, who worked on Williams Syndrome and is the source of all I know about it, corrects me here: Williams' Syndrome consists of around 30 deletions. However, the point stands.