Article:
Dennis, A. orcid.org/0000-0003-4625-1123 (2018) The strange survival and apparent resurgence of sociobiology. History of the Human Sciences, 31 (1). pp. 19-35. ISSN 0952-6951

https://doi.org/10.1177/0952695117735966

The strange survival and apparent resurgence of sociobiology

Abstract

A recent dispute between Richard Dawkins and Edward O. Wilson concerning fundamental concepts in sociobiology is examined. It is argued that sociobiology has not faired well since the 1970s, and that its survival as a 'scientific' perspective has been increasingly tenuous. This is, at least in part, because it has failed to move forward in the ways its developers anticipated, but also because it has not seen the developments in natural history, genomics and social science it was relying upon. It is argued that sociobiology has become a purely utilitarian perspective, a way of looking at things, reliant increasingly on studies of the behaviour of social insects for its 'scientific' credentials. The dispute between Dawkins and Wilson is then reconsidered in this light, and it is argued that—regardless of which position prevails—sociobiology's parlous state as a means of explaining action is now difficult to disguise.

Introduction

In May 2012, Richard Dawkins (2012) wrote a coruscating review of Edward O. Wilson's (2012a) book *The Social Conquest of Earth* for the magazine *Prospect*. He concluded:

> Please do read Wilson's earlier books, including the monumental *The Ants*, written jointly with Bert Hölldobler (yet another world expert who will have no truck with group selection). As for the book under review, the theoretical errors I have explained are important, pervasive, and integral to its thesis in a way that renders it impossible to recommend. To borrow from Dorothy Parker, this is not a book to be tossed lightly aside. It should be thrown with great force. And sincere regret.

Wilson's (2012b) response was no less impolite:

> The science in our argument has, after 18 months, never been refuted or even seriously challenged—and certainly not by the archaic version of inclusive fitness from the 1970s recited in *Prospect* by Professor Dawkins. While many have protested (incidentally, not including Steven Pinker and Robert Trivers, as Professor Dawkins claims), many others of equal competence are in favour of the replacement proposed. In any case, making such lists is futile. It should be born in mind that if science depended on rhetoric and polls, we would still be burning objects with phlogiston and navigating with geocentric maps.

The online version of Dawkins's review elicited 160 comments 'below the line', many from esteemed biologists, over the following year. They demonstrated a similar failure to meet minds.

This dispute was remarkable. Firstly, the matter at hand was foundational for the perspective both Dawkins and Wilson share: at issue was what form the natural selection of characteristics takes, what constitutes the 'unit' of selection. Dawkins insisted that animals' social behaviour is determined by the degree to which individuals share genes, while Wilson was now arguing that the group (pack, colony, etc.) is the actual unit of selection. If Wilson was right, this would mean the behaviour of animals is determined by the group's composition and environment. Wilson's rejection of kin selection (Dawkins's position) placed him in opposition to what 'sociobiologists'[1] had been arguing for the previous 40 years.

More importantly, the fact the debate was being conducted in such a public forum *at all* was strange. Sociobiology has become a very marginal perspective in both sociology and the social sciences, its premises superseded by developments in the former field and rejected *tout court* by the vast majority of practitioners in the latter. Nevertheless, the dispute drew hundreds of biologists into the controversy. This mirrored the reaction to a previous paper co-authored by Wilson and published in the journal *Nature* (Nowak et al., 2010), arguing for group selection, which elicited a refutation signed by 137 leading biologists (Abbot et al., 2011), four separate critical rejoinders (Boomsma et al., 2011; Ferriere and Michod, 2011; Herre and Wcislo, 2011; Strassmann et al., 2011) as well as a number of critical papers

[1] The term 'sociobiology' is used throughout this paper to indicate the common lines of argument of Wilson's (2000, originally 1975) Sociobiology and Dawkins's (Dawkins, 2006a) *The Selfish Gene*. These are elaborated in the next section.

published elsewhere (e.g., Rousset and Lion, 2011). Given sociobiology's marginality, why did a proposed conceptual shift in its explanatory framework elicit such a strong disciplinary response?

This paper is an attempt to answer that question. It will be argued that the arguments in *Nature* and *Prospect*, although about the same topic, were actually conducted on radically different grounds: the former was a dispute about evolutionary biology, the latter about a philosophical view of how behaviour should be understood. This was the result of sociobiology's initial statement of intent being diluted over the years to a point where it only has purchase in two spheres: understanding (some of) the behaviour of social insects and providing a philosophically-grounded theory of human behaviour. This theory will be shown to be utilitarianism in a scientific costume, and—as with utilitarianism more generally—it has proved fairly resilient in the face of empirical challenges. The (attempted) movement from kin to group selection, however, poses an existential threat to the perspective and leaves it badly weakened whether or not Wilson's arguments become more widely adopted. Before this, however, it will be necessary to describe what sociobiology's primary claims are.

What is sociobiology?

Sociobiology is the 'systematic study of the biological basis of all social behaviour' (Wilson, 2000: 4), including behaviour occurring between interacting organisms of the same species, between parasites and hosts, and between predators and prey. It places a particular emphasis

on the relationships between behaviour and evolutionary trends: behaviours are understood in relation to the question 'How does doing this promote fertility, breeding, longevity, and so on?'. Animal behaviour is construed as the means by which genetic information is transmitted: animals (phenotypes) are the vehicles through which particular DNA 'codes' (genotypes) are transmitted, and their activities are therefore understood in relation to the ways in which they promote the spread of particular genes throughout a population. Genes, in turn—because they determine what behaviours will or will not be exhibited—are more or less 'successful' depending on whether the traits they control for make the organism more 'successful' reproductively, thus passing those traits on to more offspring. Such genetically-determined traits will spread through a population more than those which fail to promote reproductive 'success'. In short, sociobiology makes the theoretical assumption that all behaviour is genetically determined in the last instance.

This assumption, however, opens up two problems. Firstly, some highly social creatures do not breed, and therefore pass on their genes, at all. Female naked mole rats, for instance, are sterile throughout their lives unless their colony queen, which does reproduce, dies. In this event one or more may become fertile, leading to conflict and competition between those that do. This *eusocial* behaviour was noted by Darwin as being 'one special difficulty, which at first appeared to me insuperable, and actually fatal to my whole theory' (Darwin, 2008: 75).[2]

[2] One of Darwin's solutions to this problem was that selection operated at the group level as well as the individual: '[t]his difficulty, although appearing insuperable, is lessened, or, as I

The discovery of DNA, and an increasing research consensus around the transmission of genetic material as the key driver behind evolution, rendered this 'difficulty' even more problematic.

Secondly, some—sometimes quite a lot of—animal behaviour does not seem to be self-interested in this way at all. Some animals, for instance, place themselves in jeopardy to protect one another, as when meerkats act as 'lookouts' for the rest of their colony by watching for predators instead of feeding, grooming, mating, and so on. Lives are sacrificed to save those of others, as when bees sting animals that pose a threat to their colony. How can such behaviour be in the interest of the animal exhibiting it?

Dawkins (2006a, originally published in 1976) sought to address these difficulties by redefining the meaning of the word 'interest'. Rather than *the organism* having particular needs, requirements, preferences, and so on, 'interests' were defined in *genetic* terms. Genes, rather than organisms, seek to replicate themselves, and animals are the vehicles through which that replication occurs: animal behaviour, in short, becomes a by-product of the imperatives of genes. Both problems—eusociality and altruism—disappear if they can be shown to promote the interests of genes over and above the apparent requirements of their bearer organisms.

---

believe, disappears, when it is remembered that selection may be applied to the family, as well as to the individual, and may thus gain its desired end' (Darwin, 2008: 177).

Dawkins's work built on the work of Hamilton (1964a, 1964b), who provided the first modern (post-DNA) theoretical model of a 'limited restraint on selfish competitive behaviour and possibility of limited self-sacrifices' (Hamilton, 1964a: 1). Hamilton sought to comprehensively quantify the relationships between genetic benefit and social behaviour. This relationship was not limited just to eusocial or altruistic behaviour—indeed, it could not be, as the division of activities into categories like 'altruistic' and 'selfish' is alien to biology—but rather to *any* interactions between related organisms. 'Hamilton's rule', as interpreted by Wilson (Wilson, 2000: 3) can be summarised as $rb > c$ (behaviour is exhibited where the relatedness of the participants, r, multiplied by the benefit to the recipient, b, is greater than the cost, c, to the acting organism). The implications of this care clear:

> This means that for a hereditary tendency to perform an action of this kind to evolve the benefit to a sib[ling] must average at least twice the loss to the individual, the benefit to a half-sib must be at least four times the loss, to a cousin eight times and so on. To express the matter more vividly, in the world of our model organisms, whose behaviour is determined strictly by genotype, we expect to find that no one is prepared to sacrifice his life for any single person but that everyone will sacrifice it when he can thereby save more than two brothers, or four half-brothers, or eight first cousins… (Hamilton, 1964a: 16).

The clearest exemplar of Hamilton's argument is the behaviour of ants. In ant colonies, only the queen lays eggs. If these are unfertilised (haploid) the offspring will be male, if fertilised (diploid) female. Males mating with their mother thus produce sterile female offspring which,

on average, share 75% of their genes. Thus a high degree of relatedness between members of the colony provides both for non-reproducing members (who can ensure the propagation of their genes by maximising the survival chances of their fertile brothers and mother) and for their willingness to jeopardise themselves for other colony members.

Hamilton's rule provides for three types of social behaviour: *altruism*, *selfishness* and *spite* (Wilson, 2000: 118–9). 'Altruism' accounts for the example of ants given above, while 'selfishness' is the more routine pursuit of genetic self-interest. 'Spite' accounts for those situations in which an individual deliberately reduces the fitness of an unrelated competitor without any personal gain (even sometimes incurring a personal loss) in order to increase the fitness of one or more closely related others. Social stability is handled with the concept of the *evolutionary stable strategy* (ESS), derived from game theory.[3] In an 'altruistic' community, any one 'selfish' organism would be able to accrue disproportionate advantages to itself. This, however, would lead to the gene for that selfish behaviour spreading throughout the population—as the 'selfish' organism would breed more than its 'altruistic' counterparts—reducing the proportion of altruistic to selfish actors. If left unchecked, selfishness would thus become the predominant mode of behaviour, leading, in turn, to small numbers of mutually co-operating altruistic individuals thereby accruing disproportionate advantages to themselves. Groups would potentially oscillate between altruism and selfishness, with the minority behaviour tending to be most advantageous to those organisms

[3] This appears to be the only aspect of social scientific thought incorporated into sociobiological theory.

that exhibit it. In fact, according to Dawkins (2006a: 69), such oscillations tend towards stability.

Wilson and Dawkins together provided a comprehensive theory of social behaviour, based on Hamilton's rule—which accounts for why individuals do what they do, motivated by the demands of their genes—and evolutionary stable strategies—to account for social stability and to provide social constraints on individual actions.[4]

Sociobiology's critics

Sociobiology was never uncontroversial: both Dawkins and Wilson argued that its tenets could account for *all* forms of animal behaviour, including human actions.[5] Its survival as a

---

[4] This is very much an overview of both sociobiology's claims and the controversies it caused. Segerstråle (2001) provides the definitive overview of the perspective and its critics, while Dupré (2001, 2003) cogently outlines the philosophical issues around contemporary Darwinist thought. Both emphasise the centrality of Hamilton's arguments to the perspective's claims.

[5] Dawkins did allow for some non-biological influences on human culture, but these were mediated through the concept of the *meme*, a self-replicating unit of culture—'tunes, ideas, catch-phrases, clothes fashions, ways of making pots or building arches' (Dawkins, 2006a)—controlled by similar patterns of selection, adaptation and extinction to the gene. Those that

coherent perspective depended on its capacity to maintain this claim. Over the course of the late 1970s and early 1980s this claim was systematically demolished—or so it seemed. In fact, the early critics of sociobiological explanation failed to persuade Wilson, Dawkins, or their followers, that their perspective did not work. For the purposes of brevity four lines of criticism will be outlined: political, natural historical, anthropological and philosophical.[6]

Sociobiology's political critics focused on its congruence with free-market economics and Hobbesian self-interest. If all behaviour is ultimately 'selfish' in motivation there are biologically-driven limits on the possibilities for co-operative or progressive social change. Wilson's work in particular was construed as just another attempt to 'prove' that such social change was impossible, making inequality and social conflict out to be 'natural' states of affairs. The most comprehensive criticism of this sort was collected in the book *Not in Our Genes*, written by the biologist Steven Rose, the psychologist Leon Kamin and the geneticist

---

spread across cultures or persist over time are better 'survival machines' than their more parochial or ephemeral counterparts. Dawkins acknowledged that this was a speculative hypothesis rather than a 'finding'

[6] This is an analytical typology for explanatory purposes and should not be taken to be a comprehensive overview of the critics' positions *tout court*. Gould addressed Wilson's conceptual confusion and dogmatism as well as his explanatory form, Rose et al. carefully examined his slapdash treatment of empirical materials as well as criticising the ideology it leads to, and so on. Types of criticism have been attributed to those authors who most clearly advance them.

Richard Lewontin (Rose et al., 1984). They question the scientific precepts of sociobiology from a range of perspectives and find it wanting in all, concluding that its explanations depend on reductionism and 'vulgar' understandings of both genetics and evolutionary science:

> Sociobiology is yet another attempt to put a natural scientific foundation under Adam Smith. It combines vulgar Mendelism, vulgar Darwinism, and vulgar reductionism in the service of the status quo (Rose et al., 1984: 264).

Many natural historians argued that the materials on which sociobiology's claims rested were inaccurate. Gould (1978), for instance, cited a study of 'jealousy' in mountain bluebirds, in which a dummy male was displayed in nests at various times over the mating season (Barash, 1976). Mating males were found to be more aggressive toward this dummy before eggs were laid than after, providing an apparent warrant for the claim that aggression is most marked when there is a greater chance of the male's genes being usurped by those of another (i.e., prior to the point at which the male 'knows' the female has been inseminated). Gould simply pointed out that the decrease in aggression could equally plausibly be the result of the male having worked out that the dummy male is, in fact, just a dummy—something which fits the facts equally well.

Gould's broader critique was that sociobiological 'explanation' depended on an inversion of observation and theory, whereby the latter was used to 'make sense' of the former rather than

being derived from it. He argued that sociobiology was a form of 'just-so' explanation, little different to how fundamentalists invoke the concept of God:

> When evolutionists study individual adaptations, when they try to explain form and behaviour by reconstructing history and assessing current utility, they also tell just-so stories—and the agent is natural selection. Virtuosity in invention replaces testability as the criterion for acceptance (Gould, 1978: 530).

Social anthropological findings further seemed to confound the possibility of sociobiological explanations being applicable to humans. Sahlins (1976), for instance, considered the ways in which anthropological evidence might be used to test Wilson's argument that biological imperatives account for human behaviour. Polynesian cultures, which were most likely to exemplify kin selection (for structural and cultural reasons), proved to be ones in which infanticide and adoption were practiced more, rather than less, frequently than elsewhere. Indeed, Sahlins found no compelling evidence of social organisation that supported sociobiological principles in the anthropological literature at all:

> Whether the scientific sociobiology will succeed in its ambition of incorporating the human sciences depends largely on the fate of its theory of kin selection. This is true for several reasons. One is the significance of kinship in the so-called primitive societies, from which may be inferred its importance throughout the earlier and greater portion of human history ... But there is still another issue which makes the problem doubly critical ... [i]f kinship is not ordered by individual reproductive success, and if

> kinship is admittedly central to human social behaviour, then the project of an encompassing sociobiology collapses (Sahlins, 1976: 17–18).

Finally, philosophical critics such as Midgley (1979) and Stove (1992) exposed the conceptual confusion and fuzzy terminology in sociobiological explanations. Dawkins, for instance, carelessly applied the adjective 'selfish' to *both* genes *and* organisms, even though the latter usage makes no sense in his explanatory schema. More importantly, Midgley argued, the notion that the word 'selfish' could be used as to indicate an attribute of chains of nucleotides was absurd *tout court*: it makes *no sense* to describe genes as 'selfish': '[g]enes cannot be selfish or unselfish any more than atoms can be jealous, elephants abstract or biscuits teleological' (Midgley, 1979: 439). Stove developed this argument by pointing out that 'self-replicating' and 'selfish' are not cognate concepts: '[i]t makes no sense to say of a virus that it is selfish, any more than it makes sense to say of an electron that it is suspicious, of a triangle that it is supercilious, or of a number that it is sex-mad' (Stove, 1992: 68). He went on:

> If you cannot get, without fudging, from self-replication to selfishness even in the behavioural sense, then you certainly cannot get from self-replication to selfishness in the ordinary sense. Yet it is not really open to doubt that it was the *ordinary* sense of the word that, though constantly disavowed, 'carried' Dawkins's book with its readers. Suppose that, before publishing it, Dawkins actually *had* done what he says it was always open for him or anyone else to do: 'translate' every reference to selfishness in the book, 'back into respectable terms', about self-replication. What would have been

> the result? The title would have been *The Self-Replicating Gene*, which is about as interesting as watching paint dry or as entitling a book on cats *The Fish-Eating Cat* (Stove, 1992: 72).

Sociobiology's claims, therefore, were rejected by scholars from a variety of disciplines, taking multiple lines of attack. They were politically motivated, based on 'just-so' explanations, incompatible with the findings of social anthropology and natural history, and rested on unclear concepts and muddled thinking. This did not, however, put Wilson and Dawkins, or their followers, off. They had responses to their critics, and sociobiology was able to survive—in a way. It depended for its survival on three claims:

1. that it retained empirical veracity in accounting for (at least some) animal behaviour;

2. that it could extend this to account for (at least some) human behaviour; and

3. that it would (eventually) have its claims verified by developments in genomics.

How sociobiologists handled these claims would determine whether the perspective could survive, and what explanatory form it would take.

Sociobiology's strange survival

Sociobiological explanation still has a role in mainstream biology, which has allowed its adherents to claim it retains empirical validity. The fact that *some* animal behaviour can, very plausibly, be understood as promoting the 'selfish' replication of genes does a lot of work. The behaviour of eusocial insects has been central to this line of argument: self-sacrifice among worker ants and the organisation of fertility and reproduction in an ant colony are perspicuous examples of where 'gene-selfish' accounts of behaviour provide a best fit to the empirical data. Thus, for example, queen leaf-cutter ants of the species *Acromyrmex echinatior* mate with multiple males, while genetically similar 'social parasite' queen ants of the species *Acromyrmex insinuator* have reverted to monogamy (Sumner et al., 2004). This behaviour can readily be accounted for using Hamilton's rule: the benefits of polyandry—such as greater genetic diversity among sterile workers and the production of genetically varied daughter queens—are specific to genuinely social insects. Where insects are 'socially parasitic' (i.e., they take advantage of living in a social host colony but produce only sexually active offspring which take flight on reaching maturity) such benefits are less important, and so monogamous mating behaviour becomes more useful. Similar arguments have been used to account for, for example, the mating rituals of birds (Cronin, 1991).

These kinds of argument are relatively benign within natural history: Hamilton's rule is used as a rule *of thumb*, to see in each case if apparently strange behaviour can be accounted for on the basis of genetic determinism. In some cases, as with the leaf-cutter parasite ants, a strong case can be made; in others (e.g., Barash's mountain bluebirds) claims were weaker.

Sociobiologists were always, however, able to claim that these 'weaker' claims could be strengthened as further data were obtained: *in each case* the reason observed behaviour and gene-selfish theory could not readily be brought into alignment with one another was because more data were required to account for the behaviour. This line of argument originally derived from Hamilton himself, who argued that his model could account for Emperor penguins 'fostering' others' eggs when their own chicks failed to survive incubation:

> As regards the already mentioned fostering passion shown by Emperor Penguins that have lost their chicks, some doubt as to whether the observations have been correctly interpreted would seem to remain… But taking the statements at their face value we might suggest for instance, that it has something to do with heat-conservation. Perhaps the parent penguin is so closely adapted to living with its offspring that it is, at the stage in question, at a positive disadvantage without a chick nestling in the brood-pouch. But such a situation would hardly come into being unless there were strong general relationship within the flock. We seem to need to postulate this in any case to explain some other social behaviour of penguins, for example, the way Adelie Penguins parents are said to leave their young in the care of only a few adults while they go off on long fishing expeditions. On the other hand, some apparently social behaviour such as the formation of the crêche in severe weather is easily interpretable as being almost entirely selfish (Hamilton, 1964b: 50).

In short, because *some* animal behaviour can be plausibly accounted for by sociobiological theory, *all* animal behaviour *will* be—once we have more detailed information to show *how* it actually works.

Secondly, sociobiology needed to retain its claim to be able to account for human as well as animal behaviour. To allow one species to be exempt from genetic determinism would undermine the project as a whole, as such an exemption would complicate the explanatory system (as with Dawkins's memes) and potentially open the door to other exemptions. Accounting for human behaviour in Darwinist terms was therefore an ongoing priority, and was addressed in two ways: by reframing understandings of cognition and brain biology in evolutionary terms, and by advancing a 'scientistic' approach to the 'irrational'.

Steven Pinker's work on language acquisition is central to the first of these lines of attack. Pinker (1994) argued that language is not 'learned' but rather 'acquired' at a key stage in child development, during which humans are uniquely capable of recognising the linguistic rules of their community. No other animals have this capacity, and a failure to acquire linguistic skills at this stage of development means they will struggle to do so later on. Pinker believed that this capacity was hard-wired into the brain, in the form of a 'meta-grammar' capable of picking up the specific grammars of the community the child is a part of. He further argued that this part of the brain, and its 'activation' at a specific developmental phase, was driven by evolution: the capacity for language was selected as a means of solving the problems of communication in early hunter-gatherer societies.

These ideas are central for three reasons. Firstly, Pinker locates complex human behaviour as something evolutionarily adaptive: it is not 'social' in the first instance but 'selected' because it—at some stage in the past—maximised the chances of survival as humans (and their social activities) evolved. Secondly, he makes behaviour like speech out to be organised in parallel with brain development and activity: what people do becomes the *manifestation* of neural activity, and being able to describe that neural activity will come to account for the behaviour in total. Thirdly, he chooses language as his central area of investigation: if this quintessentially social and 'learned' activity can be shown to be evolved, biologically based and innate, it will be far easier to bring other human activities under the auspices of Darwinist explanation.

Pinker's work is also a key resource for the second sociobiological approach to human behaviour: a faith in 'progress'. Pinker (2011) argues that there has been a steady decline in violence among humans, as 'civilising' forces have become stronger and more stable. This parallels the work of Wilson (1998), on the synthesis of the social sciences and humanities (under the auspices of evolutionary biology), and Dennett (1995), on the self-interested evolution of morality and ethics. The form of 'progress' advanced by these writers is one that typically points towards scientific rationality as the high-water mark of human evolution—particularly in its Darwinist form, of course—and is strongly critical of contemporary manifestations of 'irrationality', particularly postmodernism and other forms of relativist thought. The strongly secular bent of these thinkers is revealed in the anti-religious arguments of Wilson (Sarchet, 2015), Dawkins (2006b) and others.

The sociobiological account of human behaviour, then, has relied on two lines of attack: arguing that evolutionary adaption accounts for complex social activities that *appear* to be 'learned', and reframing human history as the triumph of reason, science and order over irrationality and violence—where sociobiological explanations themselves are the key exemplars of reasonable, scientific and orderly thought.

Finally, sociobiology needed to show that genetic regularities were responsible for particular forms of behaviour. Similarly to how Pinker argued that neural development and brain structure underpinned language use, sociobiologists sought to argue that repetitive and characteristic behaviour must be the manifestation of the actions of genes which 'code' for that behaviour. As Dawkins (replying to Midgley) argued:

> I am searching for a chunk of chromosomal material which, in practice, behaves as a unit for long enough to be naturally selected at the expense of another such fuzzy unit. I agree that there are difficulties in this way of looking at evolution, but I believe I have shown them to be less great than the difficulties inherent in any other way that has been suggested (Dawkins, 1981: 569).

As long as such 'chunks of chromosomal material' could be posited to exist, to be capable of discovery, sociobiological explanations could rely on a hypothetical 'proof' to be found down the line and—at least—stave off the accusation that they had been shown to have no validity at the genetic level.

Put together, these are remarkably thin justifications for saying that sociobiology 'survived' its critics. Apart from (some of) the behaviour of eusocial insects, the perspective came to rely on a series of promissory notes: that natural historical evidence would start to reveal the evolutionary bases of other behaviour in other animals, that neurobiology would start to provide data showing the neural bases of complex human behaviour, that secular and rational social progress would continue across the world, and that the developing science of genomics would start to reveal genes 'for' behaviours in the same way it should show how there are genes 'for' physical traits.

None of these things reliably or uncontroversially came to pass. Natural historians have continued to deepen their understanding of animal behaviour, but this increasing detail has not revealed a common, genetic, form of explanation underpinning them as a whole. Developments in brain scanning have apparently revealed some parallels between emotion and brain activity (e.g., Fisher et al., 2005), but these are controversial and have had little impact on the psychology, philosophy or sociology of emotion. The world is perhaps not getting better—although in evolutionary terms there has not been enough time to determine whether or not that is the case. Finally, most troublingly, genomics has not found genes 'for' behaviour. Indeed, as the discipline has developed, increasing number of geneticists have cast doubt on the idea that there can be genes 'for' anything at all (Barnes and Dupré, 2008: 56).

What these developments reveal is not the resilience of sociobiology but its character as an *approach*, a 'correct' way to study behaviour. It is a flexible and loose rubric for putting together different claims under the auspices of a heuristic-based theory of how the world

works. To use Parsons's (1937: 51) terminology, it is a 'subsystem (or, perhaps better, an interrelated group of several sub-subsystems) of the theory of action'. It is *utilitarianism*.

Sociobiology is utilitarianism

Sociobiology has been accused of being a utilitarian perspective before, but this has generally been in the narrow sense of Hobbesian notions of self-interest (Sullivan, 1982). In a rather odd argument, Sullivan contrasts a 'Hobbesian' point of view in which all altruism is necessarily self-serving in the final analysis with a 'Marxian' perspective in which 'altruism is man's natural orientation' (Sullivan, 1982: 274). He concludes that the 'sociobiological analysis of altruism is clearly Hobbesian and not Marxist' (Sullivan, 1982: 274). The defining feature of this argument is its *shallowness*. 'Utilitarianism' and 'Marxism' are invoked as means of classifying selfishness rather than as means of examining more deeply the logic and grammar of the sociobiological perspective. In this sense, at least, Sullivan's approach shares a problem common to many of sociobiology's other critics: his analysis takes place on terrain largely chosen by sociobiology's adherents.[7] It is, however, possible to go deeper by examining sociobiology—in the form it retained—with reference to Parsons's more analytical description of utilitarianism as a programme.

[7] See also Scoccia (1990) for a more polemical reversal of this argument, in which sociobiology is used to defend 'hard' utilitarianism against its critics.

Parsons's work is particularly relevant in this regard for three reasons. Firstly, he recognised that utilitarianism as a system of thought moved beyond the narrower arguments of Hobbes and became a way of construing the nature of social life and social action *tout court*. Rather than treating it as a philosophical perspective, therefore, he examined it as a persuasive and organised theoretical movement. Secondly, he considered it not from the point of view of the claims its advocates made but in relation to the grammar and logic of its arguments: he viewed utilitarianism as a particular way of looking at things *that could be compared with competing ways* and found to rest on particular, different, analytical decisions. Finally, and for our purposes, most importantly, Parsons viewed the movement as something with a history of its own. It emerged at a particular time, for particular theoretical and practical reasons, and its decline was the result of both internal contradictions (in particular an increasing concern with troubling residual matters that it was incapable of handling) and external pressures (the growth of positivism and the voluntaristic and normative theories of Durkheim and, in particular, Weber as both developments within and responses to that growth). Using Parsons's analysis, therefore, allows sociobiology to be treated as a movement, with a particular theoretical grammar, subject to both internal and external pressures.

Parsons (Parsons, 1937: 51–60) defined utilitarianism as the theoretical action system characterised by four features: atomism, rationality, empiricism and randomness of ends. Atomism is 'a strong tendency to consider mainly the properties of conceptually isolated unit acts and to infer the properties of systems of action only by a process of "direct" generalisation from these' (Parsons, 1937: 52). Both Hamilton's account of interactional

behaviour, and the concept of ESSs, clearly fall under this rubric. The 'unit' of action in sociobiology is that between interacting organisms—evaluated by the relative costs and benefits this interaction will bring each participant, in terms of the chances of preserving and distributing their genetic make-up—rather than, for instance, those actions being understood in terms of their relevance to the costs and benefits they might bring to the group as a whole. The group is no more than the sum of the actions of its members.

Rationality, for Parsons, is a slippier concept, but—in its simplest form— can be stated thus: '[a]ction is rational in so far as it pursues ends possible within the conditions of the situation, and by the means which, among those available to the actor, are intrinsically best adapted to the end for reasons understandable and verifiable by positive empirical science' (Parsons, 1937: 58). Again, this clearly describes sociobiology. Organisms behave in ways that *make sense* in terms of their genetic imperatives and, in each and every case, their actions can be shown to be 'rational' in relation to the imperatives of their genes. Hamilton's description of Emperor penguins and Barash's account of mountain bluebirds show this rationality in full: *whatever* these creatures are doing *must* be understood in terms of a means–ends relationship where the end being pursued is to reproduce more effectively.

Here, we can see a tension in how sociobiologists have addressed criticisms of their position. Gould's argument that sociobiology is a 'just-so' explanation of behaviour illuminates the problem of *behaviour that cannot readily be accounted for within the Hamilton-Dawkins sociobiological system*, i.e., behaviour that appears not to make sense in terms of the imperatives of selfish genes. Sociobiologists have to come up with explanations for this kind

of behaviour—as non-selfish, non-rational acts are corrosive to their project—and have used Hamilton's rule as their key resource for doing this. The question is whether this *explains* such acts or *explains them away*: Gould would argue the latter, as there do not appear to be any natural phenomena that cannot be so accounted for provided sufficient imagination is brought to bear. Hamilton's rule rests on the creativity of the person applying it, and—because it is deemed to be correct, scientific and rational as an *a priori* matter—there are no 'external' criteria by which its application can be judged from the sociobiological point of view.

The (naïve) empiricism of utilitarianism, for Parsons, consists in its assumption that systems of action are no more than the sum of the acts that they consist in. 'This is the simplest and most obvious mode of employment of this conceptual scheme—the assumption, often naïvely made without full realisation of what it implies, that the concrete action systems being studied are simply aggregates of such rational unit acts' (Parsons, 1937: 59). It makes no sense to talk about the actions of, for instance, the actions of a community, an organisation or an institution—the fact that these collective nouns exist should not fool one into believing that they can, therefore, engage in actions, have agency, etc., in their own right. This is the empiricism of sociobiological explanations: while it might make sense to talk of 'colony organisation', 'pack behaviour', 'herd migration', and so on, such terms should always be understood as shorthand for the behaviour of the individuals that comprise them. This is akin to methodological individualism, but only to a certain extent: although one can talk (in sociology) about the beliefs of a church or the movement of an army, a rigorous adherence to the principles of methodological individualism would insist that such statements can—at least

in principle—be restated in terms of the co-ordinated and negotiated attitudes, actions and activities of all those individuals that make up the 'church' or 'army'. In utilitarianism, as in sociobiology, no such co-ordination or negotiation is possible: collective nouns ('colony', 'pack', 'herd', 'church', 'army') are not orientated to by individual organisms but are rather *analytical* constructs that provide for shorthand descriptions. A pack of wolves, for instance, is no more than the behaviour of its members, who *appear* to form a collective because of their repeated and regular 'altruistic' behaviour toward one another. This behaviour, however, is simply what one would expect as Hamilton's rule works itself out across a series of logically independent interactions featuring the same personnel. Its regularity should not fool one into thinking either that 'the pack' has a firm ontological status or—even worse—that parties to such behaviour orientate to 'the pack' (rather than particular other individuals) as a 'real' entity as if its apparently regular features could be relied upon to persist.

This brings us to Parsons' final characteristic feature of utilitarianism: randomness of ends. In many respects this falls out of the empiricism outlined above: to the extent that this empiricism is taken to describe what really happens in social action (as opposed to being an ideal typification, an abstraction, etc.) then there can be no meaningful co-ordination of ends without that co-ordination being in some respects teleological:

> If the concrete system be considered as analysable exclusively into rational unit acts it follows that though the conception of action as consisting in the pursuit of ends is fundamental, there is nothing in the theory dealing with the relations of the ends to each other, but only with the character of the means-end relationship … [T]he failure to state

> anything positive about the relations of ends to each other can then have only one meaning—that there are no significant relations, that is, that ends are random in the statistical sense (Parsons, 1937: 59).

To the extent that this randomness of ends poses a 'problem' for analysts, it is one that can be resolved through a probabilistic redescription. Rational self-interest manifests itself in action occurring over time and with an orientation to what others, with identical motivation, are likely to do: apparently co-operative or altruistic behaviour can thus be made out to be self-interested provided the relevant orientations of others are built into the description of the actor's behaviour. Thus, for example, one might defer immediate satisfaction if that would lead to hostility from others. Although this might appear not to be self-interested, once the temporal and social dimension of action (or, as here, lack of action) is built in its self-serving nature can be demonstrated.

In sociobiology, 'interests' are defined with regard to 'strategies' and against a background of other organisms' behaviours. Whether or not 'spiteful' behaviour serves self-interest depends on, among other things, whether other interacting organisms are also pursuing spiteful strategies. What turns out to be 'rational self-interest' can only be determined over time, with the assumption of an evolutionary stable state or strategy and the presumption of perfect knowledge on the parts of each organism. Social stability, then, is built in to the system, but only insofar as the idea of co-ordinated ends or shared goals are ruled out *a priori*.

What Dawkins versus Wilson revealed

To the extent that sociobiology is simply a utilitarian way of redescribing activities in biological terms, its claims to be 'scientific' are central to its survival. As we have seen, these claims have not weathered well. Claims about human behaviour have gained little traction outside the circle of already-committed evolutionary determinists; genomics is moving away from the idea that there are genes 'for' particular traits, let alone behaviours; and natural-historical research has not located the kinds of 'forces' Hamilton and others claimed 'underpinned' animal behaviour except in a restricted number of cases.[8]

This is why Wilson's apostasy so scandalised Dawkins. Wilson's embrace of group selection over strict kin selection represents a move away from utilitarianism and towards a mode of explanation that recognises the importance of groups and their environments. Wilson, in

[8] It is also worth noting that the version of 'science' advocated by sociobiologists and fellow-travellers is itself somewhat archiaic. In addition to their silence on developments in genomics, Wilson and Dawkins have signally failed to consider the possibility that their model of what science looks like may demand scrutiny. Dawkins, for instance, discards both Popper and Kuhn as mere relativists, used by 'truth-hecklers' to deny the existence of a real world, and refers to the philosophy and sociology of science as 'the low-grade intellectual poodling of pseudo-philosophical poseurs' (Dawkins, 2004: 16–19).

short, has suggested that utilitarian explanations should be replaced by positivist ones. As Parsons points out, this is the logical conclusion of utilitarianism:

> … the end result, or the ultimate determinant factors are the same, adaptation to conditions through, in the last analysis, the influence of these conditions themselves. Indeed, in the very last analysis even the difference of process disappears, for in so far as the 'conditions' ultimately form the sole determinants of action the subjective aspect becomes merely a reflection of these 'facts'; it is purely epiphenomenal (Parsons, 1937: 120–1).

Compare this with Wilson's advocacy of group selection:

> The fourth phase [of the evolution of eusociality] is identification of the environmental forces driving group selection, which is the logical subject of combined investigations in population genetics and behavioral ecology. Research programs have scarcely begun in this area, in part because of the relative neglect of the study of the environmental selection forces that shape early eusocial evolution (Wilson, 2012a: 186).

Dawkins's hostility to Wilson's move is not, in large part, because Wilson is arguing that a different perspective can be applied to accounting for insect behaviour. Rather, Dawkins is furious that the *last remaining claim* for sociobiology's 'scientific' status is being undermined. Following the failure of Hamilton's model everywhere else, the world's leading expert on social insects is claiming that it does not even apply in its exemplary field. Insect

behaviour, for Wilson, is better accounted for by looking at the conditions under which it occurs than with reference to the genetic imperatives of individual organisms. If he is right, the last bastion of sociobiological explanation will have fallen.

The dispute over Wilson's application of group selection among biologists—the controversy played out in the journal *Nature*—is strictly about how best to account for the behaviour of insects. Indeed, the original paper concludes '[w]e have not addressed the evolution of human social behaviour here, but parallels with the scenarios of animal eusocial evolution exist, and they are, we believe, well worth examining' (Nowak et al., 2010: 1062). The responses are not about humans at all, but about social insects.[9] The controversy is *not* about sociobiology but rather about how best to account for the 'one special difficulty' Darwin had to deal with, in a very restricted range of species. The dispute does not show that sociobiology is alive and well, but rather that Hamilton's rule may not even apply in its home domain.

Sahlins (1976: 17–18) pointed out that sociobiology's claims to account for human behaviour depend on the resilience of its theory of kin selection:

> This is true for several reasons. One is the significance of kinship in the so-called primitive societies, from which may be inferred its importance throughout the earlier and greater portion of human history ... But there is still another issue which makes the problem doubly critical ... [i]f kinship is not ordered by individual reproductive success,

[9] In one response, microbes and naked mole rats are also mentioned (Strassmann et al., 2011).

> and if kinship is admittedly central to human social behaviour, then the project of an encompassing sociobiology collapses.

More than forty years later this remains true, but the question being asked is not whether kin selection operates on humans as well as other animals but rather whether or not *it can even be used to account for the behaviour of social insects*.

Wilson may be right. Some biologists have adopted his position. Others do not see what the problem is, as selection can be construed as operating at several levels: close relatedness, environment and group-level adaptation all being relevant factors (Korb and Heinze, 2004). Others, of course disagree. Dawkins's review of Wilson's book in *Prospect*, however, is not a defence of kin selection in insects but a defence of a way of describing activities *tout court*, a restatement of Hamilton's rule and a bewildered objection to how Wilson *of all people* could doubt its veracity. Dawkins is reasserting utilitarian explanations in the face of a positivist challenge, and Wilson is threatening a further two volumes to follow his *Social Conquest of Earth* to show how his new (positivist) approach can be extended from 'where we come from' through 'what we are' and to 'where we are going' (Sarchet, 2015: 29). If Dawkins prevails, and the entomological consensus supports Hamilton's rule, the final 'scientific' strand supporting sociobiological utilitarianism will remain unbroken. If Wilson's argument wins out, it will be used to make the kind of move from utilitarianism to positivism Parsons located in nineteenth century social thought, ironically starting around the time Darwin first published his *Origin of Species*. Whether sociobiology remains an eighteenth century mode of explanation or moves willy-nilly into the Victorian era, however, its reliance on a way of

accounting for (some of) the behaviour of a (very) narrow range of species is now in plain sight.

Bibliography

Abbot, P., Abe, J., Alcock, J., et al. (2011) 'Inclusive Fitness Theory and Eusociality', *Nature* 471: E1–E4.

Barash, D. P. (1976) 'Male Response to Apparent Female Adultery in the Mountain Bluebird (Sialia currucoides): An Evolutionary Interpretation', *The American Naturalist* 110: 1097–1101.

Barnes, B. and Dupré, J. (2008) *Genomes: And What to Make of Them*. Chicago: University of Chicago Press.

Boomsma, J. J., Beekman, M., Cornwallis, C. K., et al. (2011) 'Only Full-Sibling Families Evolved Eusociality', *Nature* 471: E4–E5.

Cronin, H. (1991) *The Ant and the Peacock: Altruism and Sexual Selection from Darwin to Today*. Cambridge: Cambridge University Press.

Darwin, C. (2008) *On the Origin of Species by Means of Natural Selection or the Preservation of Favoured Races in the Struggle for Life*. Oxford: Oxford University Press.

Dawkins, R. (1981) 'In Defence of Selfish Genes', *Philosophy* 56: 556–573.

Dawkins, R. (2004) *A Devil's Chaplain: Reflections on Hope, Lies, Science, and Love*. Boston, MA: Houghton Mifflin.

Dawkins, R. (2006a) *The Selfish Gene*. Oxford: Oxford University Press.

Dawkins, R. (2006b) *The God Delusion*. London: Bantam.

Dawkins, R. (2012) 'The Descent of Edward Wilson', *Prospect*. Available at: http://www.prospectmagazine.co.uk/magazine/edward-wilson-social-conquest-earth-evolutionary-errors-origin-species/.

Dennett, D. (1995) *Darwin's Dangerous Idea: Evolution and the Meanings of Life*. New York: Simon & Schuster.

Dupré, J. (2001) *Human Nature and the Limits of Science*. Oxford: Oxford University Press.

Dupré, J. (2003) *Darwin's Legacy: What Evolution Means Today*. Oxford: Oxford University Press.

Ferriere, R. and Michod, R. E. (2011) 'Inclusive Fitness in Evolution', *Nature* 471: E6–E8.

Fisher, H., Aron, A. and Brown, L. L. (2005) 'Romantic Love: An fMRI Study of a Neural Mechanism for Mate Choice', *Journal of Comparative Neurology* 493: 58–62.

Gould, S. J. (1978) 'Sociobiology: The Art of Storytelling', *New Scientist* 530–533.

Hamilton, W. D. (1964a) 'The Genetical Evolution of Social Behaviour, I', *Journal of Theoretical Biology* 7: 1–16.

Hamilton, W. D. (1964b) 'The Genetical Evolution of Social Behaviour, II', *Journal of Theoretical Biology* 7: 17–52.

Herre, E. A. and Wcislo, W. T. (2011) 'In Defence of Inclusive Fitness Theory', *Nature* 471: E8–E9.

Korb, J. and Heinze, J. (2004) 'Multilevel Selection and Social Evolution of Insect Societies', *Naturwissenschaften* 91: 291–304.

Midgley, M. (1979) 'Gene-Juggling', *Philosophy* 54: 439–458.

Nowak, M. A., Tarnita, C. E. and Wilson, E. O. (2010) 'The Evolution of Eusociality', *Nature* 466: 1057–1062.

Parsons, T. (1937) *The Structure of Social Action: A Study in Social Theory with Special Reference to a Group of Recent European Writers*. New York: Free Press.

Pinker, S. (1994) *The Language Instinct: How the Mind Creates Language*. New York: William Morrow.

Pinker, S. (2011) *The Better Angels of Our Nature: How Violence Has Declined*. New York: Viking.

Rose, S., Kamin, L. J. and Lewontin, R. C. (1984) *Not in Our Genes: Biology, Ideology and Human Nature*. Harmondsworth: Penguin.

Rousset, F. and Lion, S. (2011) 'Much Ado About Nothing: Nowak et al.'s Charge Against Inclusive Fitness Theory', *Journal of Evolutionary Biology* 24: 1386–1392.

Sahlins, M. (1976) *The Use and Abuse of Biology: An Anthropological Critique of Sociobiology*. Ann Arbor, MI: University of Michigan Press.

Sarchet, P. (2015) 'Don't Let Earth's Tapestry Unravel', *New Scientist* 225: 28–29.

Scoccia, D. (1990) 'Utilitarianism, Sociobiology, and the Limits of Benevolence', *Journal of Philosophy* 87: 329–345.

Segerstråle, U. (2001) *Defenders of the Truth: The Sociobiology Debate*. Oxford: Oxford University Press.

Stove, D. (1992) 'The Demons and Dr. Dawkins', *The American Scholar* 61: 67–78.

Strassmann, J. E., Page, R. E., Jr., Robinson, G. E. and Seeley, T. D. (2011) 'Kin Selection and Eusociality', *Nature* 471: E5–E6.

Sullivan, R. R. (1982) 'Sociobiology and the Crisis of Public Authority', *Philosophy of the Social Sciences* 12: 271–284.

Sumner, S., Hughes, W. O. H., Pedersen, J. S. and Boomsma, J. J. (2004) 'Ant Parasite Queens Revert to Mating Singly', *Nature* 428: 35–36.

Wilson, E. O. (1998) *Consilience: The Unity of Knowledge*. London: Little, Brown.

Wilson, E. O. (2000) *Sociobiology: The New Synthesis*. Cambridge, MA: Belknap Press.

Wilson, E. O. (2012a) *The Social Conquest of Earth*. New York: Liveright.

Wilson, E. O. (2012b) 'Edward Wilson Responds', *Prospect*. Available at: http://www.prospectmagazine.co.uk/magazine/edward-wilson-social-conquest-earth-evolutionary-errors-origin-species/.