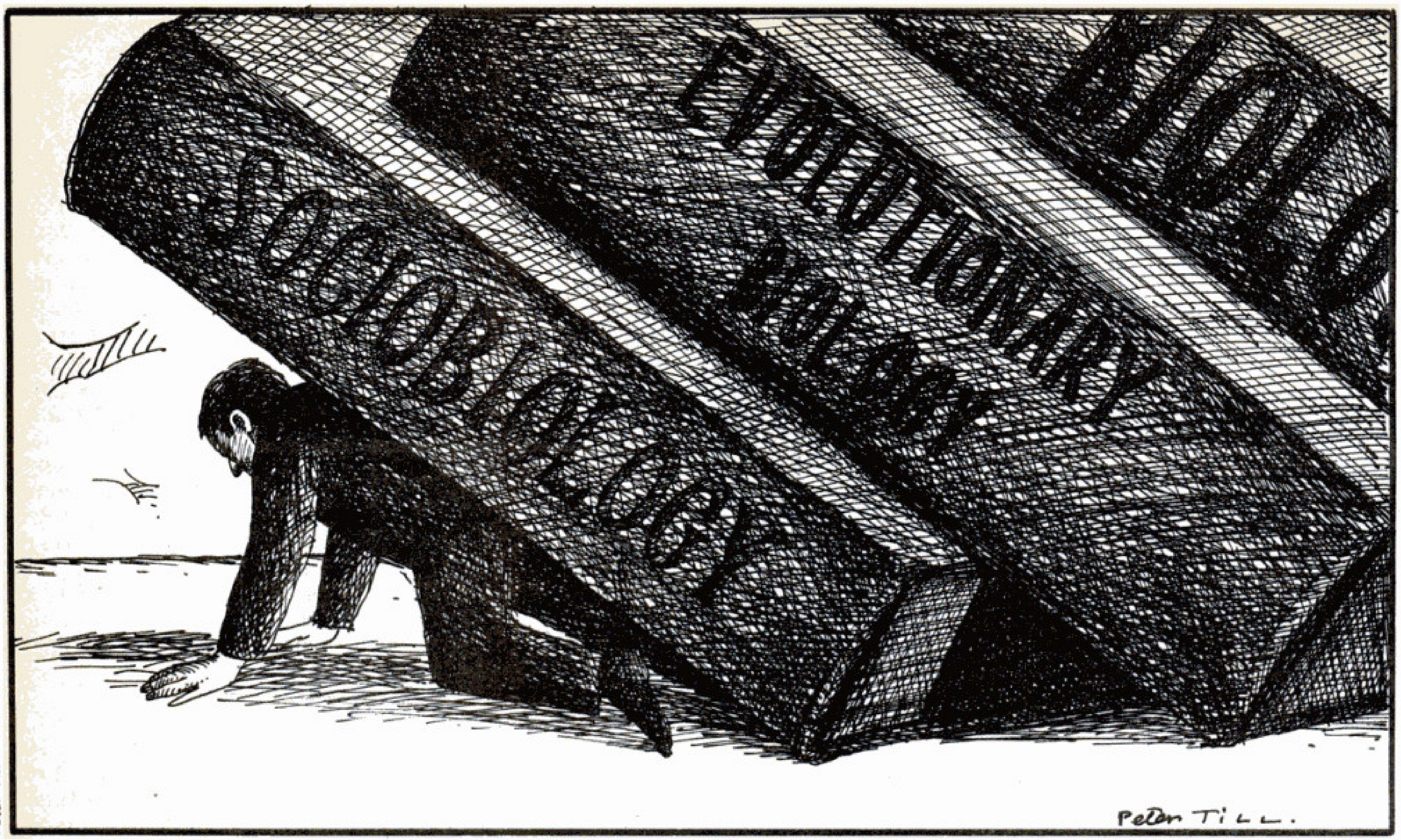

Peter Till

Sociobiology: the art of storytelling

Too many sociobiological explanations of behaviour come into the category of "Just So" stories: they may be plausible but are less than rigorously supported by solid evidence

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Ludwig von Bertalanffy, a founder of general systems theory and a holdout against the neo-Darwinian tide, often argued that natural selection must fail as a comprehensive theory because it explains *too* much—a paradoxical, but perceptive statement. In 1969 he wrote: "If selection is taken as an axiomatic and *a priori* principle, it is always possible to imagine auxiliary hypotheses—unproved and by nature unprovable—to make it work in any special case . . . Some adaptive value . . . can always be construed or imagined.

"I think the fact that a theory so vague, so insufficiently verifiable and so far from the criteria otherwise applied in 'hard' science, has become a dogma, can only be explained on sociological grounds. Society and science have been so steeped in the ideas of mechanism, utilitarianism, and the economic concept of free competition, that instead of God, Selection was enthroned as ultimate reality."

Similarly, the arguments of Christian fundamentalism used to frustrate me until I realised that there are, in principle, no counter cases and that, on this ground alone, the theory is bankrupt.

The theory of natural selection is, fortunately, in much better straits. It could be invalidated as a general cause of evolutionary change. (If, for example, Lamarckian inheritance were true and general, then adaptation would arise so rapidly in the Lamarckian mode that natural selection would be powerless to create and would operate only to eliminate.) Moreover, its action and efficacy have been demonstrated experimentally by 60 years of manipulation within *Drosophila* bottles—not to mention several thousand years of success by plant and animal breeders.

Yet in one area, unfortunately a very large part of evolutionary theory and practice, natural selection has operated like the fundamentalist's God—he who maketh all things. Rudyard Kipling asked how the leopard got its spots, the rhino its wrinkled skin. He called his answers "Just So stories". 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. This is the procedure that inspired von Bertalanffy's complaint. It is also the procedure that has given evolutionary biology a bad name among many experimental scientists in other disciplines. We should heed their disquiet, not dismiss it with a claim that they understand neither natural selection nor the special procedures of historical science.

This style of storytelling might yield acceptable answers if we could be sure of two things: first, that all bits of morphology and behaviour arise as direct results of natural selection, and secondly, that only one selective explanation exists for each bit. But, as Darwin insisted vociferously, and contrary to the mythology about him, there is much more to evolution than natural selection. (Darwin was a consistent pluralist who viewed natural selection as the most important agent of evolutionary change, but who accepted a range of other agents and specified the conditions of their presumed effectiveness. In chapter seven of the *Origin of Species* (sixth edition), for example, he attributed the cryptic colouration of a flatfish's upper surface to natural selection and the migration of its eyes to inheritance of acquired characters. He continually insisted that he wrote his two-volume *Variation of Animals and Plants Under Domestication* (1868), with its Lamarckian hypothesis of pangenesis, primarily to illustrate the effect of evolutionary factors other than natural selection. In a letter to *Nature* in 1880, he used the sharpest and most waspish language of his life to castigate Sir Wyville Thompson for caricaturing his theory by ascribing all evolutionary change to natural selection.)

Since all theories cite God in their support, and since Darwin comes close to this status among evolutionary biologists, the panselectionists of the modern synthesis tended to remake Darwin in their image. But we now reject this rigid version of natural selection and grant a major role to other evolutionary agents (genetic drift, fixation of neutral mutations, for example). We must also recognise that many features arise indirectly as developmental consequences of other features directly subject to natural selection. Moreover, and perhaps most importantly, there are a multitude of potential selective explanations for each feature. There is no such thing in nature as a self-evident and unambiguous story.

When we examine the history of favoured stories for any particular adaptation, we do not trace a tale of increasing truth as one story replaces the last, but rather a chronicle of shifting fads and fashions. When Newtonian mechanical explanations were riding high, G. G. Simpson wrote (in 1961), "The problem of the pelycosaur dorsal fin . . . seems essentially solved by Romer's demonstration that the regression relationship of fin area to body volume is appropriate to the functioning of the fin as a temperature regulating mechanism." Simpson's firmness seems almost amusing since now—a mere 15 years later with behavioural stories in vogue—most palaeontologists feel equally sure that the sail was primarily a device for sexual display. (Yes, I know the litany: It might have performed both functions. But this too is a story.)

On the other side of the same shift in fashion, a recent article on functional endothermy in some large beetles had this to say about the why of it all: "It is possible that the increased power and speed of terrestrial locomotion associated with a modest elevation of body temperatures may offer reproductive advantages by increasing the effectiveness of intraspecific aggressive behaviour, particularly between males." This conjecture reflects no evidence drawn from the beetles themselves, only the current fashion in selective stories. We may be confident that the same data, collected 15 years ago, would have inspired a speculation about improved design and mechanical advantage.

Most work in sociobiology has been done in the mode of adaptive storytelling based on the optimising character and pervasive power of natural selection. As such, its weaknesses of methodology are those that have plagued so much of evolutionary theory for more than a century. Sociobiologists have anchored their stories in the basic Darwinian notion of selection as individual reproductive success.

Sociobiologists have broadened their range of selective stories by invoking concepts of inclusive fitness and kin selection to solve (successfully I think) the vexatious problem of altruism—previously the greatest stumbling block to a Darwinian theory of social behaviour. (Altruistic acts are the cement of stable societies. Until we could explain apparent acts of self-sacrifice as potentially beneficial to the genetic fitness of sacrificers themselves—propagation of genes through enhanced survival of kin, for example—the prevalence of altruism blocked any Darwinian theory of social behaviour.)

Thus, kin selection has broadened the range of permissible stories, but it has not alleviated any methodological difficulties in the process of storytelling itself. Von Bertalanffy's objections still apply, if anything with greater force, because behaviour is generally more plastic and more difficult to specify and homologise than morphology. Sociobiologists are still telling speculative stories, still hitching without evidence to one potential star among many, still using mere consistency with natural selection as a criterion of acceptance.

David Barash, for example, tells the following story about mountain bluebirds. (It is, by the way, a perfectly plausible story that may well be true. I only wish to criticise its assertion without evidence or test, using consistency with natural selection as the sole criterion for useful speculation.) He reasoned that a male bird might be more sensitive to intrusion of other males before eggs are laid than after (when he can be certain that his genes are inside). So Barash studied two nests, making three observations at 10-day intervals, the first before the eggs were laid, the last two after. For each period of observation, he mounted a stuffed male near the nest while the male occupant was out foraging. When the male returned he counted aggressive encounters with both model and female. At time one, males in both nests were quite aggressive towards the model and less, but still substantially aggressive towards the female as well. At time two, after eggs had been laid, males were less aggressive to models and scarcely aggressive to females at all. At time three, males were still less aggressive towards models, and not aggressive at all towards females.

Is consistency enough?

Barash concludes that he has established consistency with natural selection and need do no more: "These results are consistent with the expectations of evolutionary theory. Thus aggression toward an intruding male (the model) would clearly be especially advantageous early in the breeding season, when territories and nests are normally defended . . . The initial, aggressive response to the mated female is also adaptive in that, given a situation suggesting a high probability of adultery (that is, the presence of the model near the female) and assuming that replacement females are available, obtaining a new mate would enhance the fitness of males . . . The decline in male-female aggressiveness during incubation and fledgling stages could be attributed to the impossibility of being cuckolded after the eggs have been laid . . . The results are consistent with an evolutionary interpretation. In addition, the term 'adultery' is unblushingly employed in this letter without quotation marks, as I believe it reflects a true analogy to the human concept, in the sense of Lorenz. It may also be prophesied that continued application of a similar evolutionary approach will eventually shed considerable light on various human foibles as well."

Consistent, yes. But what about the obvious alternative, dismissed without test in a line by Barash: male returns at times two and three, approaches the model a few times, encounters no reaction, mutters to himself the avian equivalent of "it's that damned stuffed bird again," and ceases to bother. And why not the evident test: expose a male to the model for the *first* time *after* the eggs are laid.

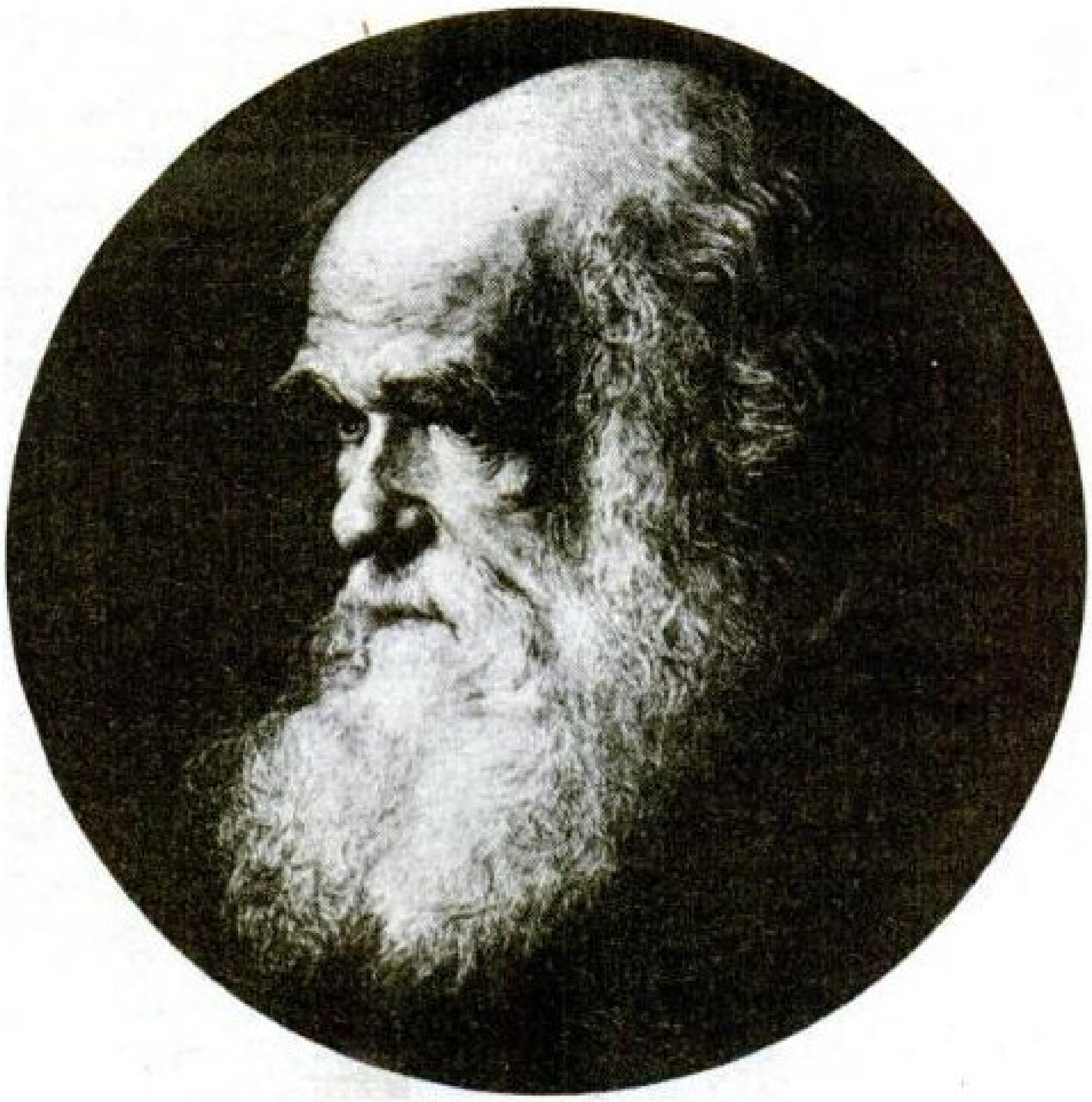
We have been deluged in recent years with sociobiological stories. Some, like Barash's are plausible, if unsupported. For many others, I can only confess my intuition of extreme unlikeliness, to say the least—for adaptive and genetic arguments about why fellatio and cunnilingus are more common among the upper classes, or why male panhandlers are more successful with females and people who are eating than with males and people who are not eating.

Not all sociobiology proceeds in the mode of storytelling for individual cases. It rests on firmer methodological ground when it seeks broad correlations across taxonomic lines, as between reproductive strategy and distribution of resources, for example, or when it can make testable, quantitative predictions as in Bob Trivers and Hope Hare's work on haplodiploidy and eusociality in *Hymenoptera*. Here sociobiology has had and will continue to have success. And here I wish it well. For it represents an extension of basic Darwinism to a realm where it should apply.

Sociobiological explanations of human behaviour encounter two special difficulties, suggesting that a Darwinian model may be generally inapplicable in this case.

● *First* we have very little direct evidence about the genetics of behaviour in humans; and we know no way to obtain it for the specific behaviours that figure most prominently in sociobiological speculation—aggression and conformity, for instance. With our long generations, it is very difficult to amass much data on heritability. More importantly, we cannot (ethically, that is) perform the kind of breeding experiments, in standardised environments, that would yield the required information. Thus, in dealing with humans, sociobiologists rely even more heavily than usual on speculative story-telling.

At this point, the political debate engendered by sociobiology comes appropriately to the fore. For these speculative stories about human behaviour have broad implications and proscriptions for social policy—and this is true quite apart from the intent or personal politics of the storyteller. Intent and usage are very different things; the latter marks political and social influence, the former is gossip or, as best, sociology.


Charles Darwin

The common political character and effect of these stories lies in the direction historically taken by nativistic arguments about human behaviour and capabilities—a defence of existing social arrangements as part of our biology.

In raising this point, I do not act to suppress truth for fear of its political consequences. Truth, as we understand it, must always be our primary criterion. We live, because we must, with all manner of unpleasant biological truths—death being the most pervasive and ineluctable. I complain because sociobiological stories are not truth, rather they are unsupported speculations with political clout (again, I must emphasise, quite apart from the intent of the storyteller). All science is embedded in cultural contexts, and the lower the ratio of data to social importance, the more science reflects the context.

In stating that there is politics in sociobiology, I do not criticise the scientists involved in it by claiming that an unconscious politics has intruded into a supposedly objective enterprise. For they are behaving like all good scientists—as human beings in a cultural context. I only ask for a more explicit recognition of the context—and, specifically, for more attention to the evident impact of speculative sociobiological stories. For example, when the *New York Times* runs a weeklong front page series on women and their rising achievements and expectations, spends the first four days documenting their progress towards social equality, devotes the last day to potential limits upon this progress, and advances sociobiological stories as the only argument for potential limits—then we know that these are stories with consequences: "Sociologists believe that women will continue for some years to achieve greater parity with men, both in the work place and in the home. But an uneasy sense of frustration and pessimism is growing among some advocates of full female equality in the face of mounting conservative opposition. Moreover, even some staunch feminists are reluctantly reaching the conclusion that women's aspirations may ultimately be limited by inherent biological differences that will forever leave men the dominant sex" (*New York Times*, 30 November, 1977).

The article then quotes two social scientists, each with a story. First, "If you define dominance as who occupies formal roles of responsibility, then there is no society where males are not dominant. When something is so universal, the probability is—as reluctant as I am to say it—that there is some quality of the organism that leads to this condition." Secondly, "It may mean that there never will be full parity in jobs, that women will always predominate in the caring tasks like teaching and social work and in the life sciences, while men will prevail in those requiring more aggression — business and politics, for example—and in the 'dead' sciences like physics."

● *Secondly,* the standard foundation of Darwinian just-so stories does not apply to humans. That foundation is the implication: if adaptive, then genetic—for the inference of adaptation is usually the only basis of a selective story, and Darwinism is a theory of genetic change and variation in populations.

Much of human behaviour is clearly adaptive, but the problem for sociobiology is that humans have developed an alternative, non-genetic system to support and transmit adaptive behaviour—cultural evolution. (An adaptive behaviour does not require genetic input and Darwinian selection for its origin and maintenance in humans; it may arise by trial and error in a few individuals who do not differ genetically from their groupmates in any way relevant to this behaviour spread by learning and imitation, and stabilise across generations by value, custom and tradition.) Moreover, cultural transmission is far more powerful in potential speed and spread than natural selection—for cultural evolution operates in the "Lamarckian" mode by inheritance through custom, writing and technology of characteristics acquired by human activity in each generation.

Thus, the existence of adaptive behaviour in humans says nothing about the probability of a genetic basis for it, or about the operation of natural selection. Take, for example, Trivers's concept of "reciprocal altruism". The phenomenon exists, to be sure, and it is clearly adaptive. In honest moments, we all acknowledge that many of our "altruistic" acts are performed in the hope and expectation of future reward. Can anyone imagine a stable society without bonds of reciprocal obligation. But structural necessities do not imply direct genetic coding. (All human behaviours are, of course, part of the potential range permitted by our genotype—but sociobiological speculations posit direct natural selection for specific behavioural traits.) As Benjamin Franklin said: "Either we hang together, or assuredly we will all hang separately."

The grandest goal—I do not say the only goal—of human sociobiology must fail in the face of these difficulties. That goal is no less than the reduction of the behavioural (indeed most of the social) sciences to Darwinian theory. Edward Wilson presents a vision of the human sciences shrinking in their independent domain, absorbed on one side by neurobiology and on the other by sociobiology.

But this vision cannot be fulfilled, for the reason cited above. Although we can identify adaptive behaviour in humans, we cannot tell if it is genetically based (while much of it must arise by fairly pure cultural evolution). Yet the reduction of the human sciences to Darwinism requires the genetic argument, for Darwinism is a theory about genetic change in populations. All else is analogy and metaphor.

My crystal ball shows the human sociobiologists retreating to a fallback position—indeed it is happening already. They will argue that this fallback is as powerful as their original position, though it actually represents the unravelling of their fondest hopes. They will argue: yes, indeed, we cannot tell whether an adaptive behaviour is genetically coded or not. But it doesn't matter. The same adaptive constraints apply whether the behaviour evolved by cultural or Darwinian routes, and biologists have identified and explicated the adaptive constraints. (Steve Emlen tells me, for example, that some Indian peoples gather food in accordance with predictions of optimal foraging strategy—a theory developed by ecologists.)

But it does matter. It makes all the difference in the world whether human behaviours develop and stabilise by cultural evolution or by direct Darwinian selection for genes influencing specific adaptive actions. It makes a great difference because cultural and Darwinian evolution differ profoundly in the three major areas that embody what evolution, at least as a quantitative science, is all about:

1. *Rate*. Cultural evolution, as a "Lamarckian" process, can proceed orders of magnitude more rapidly than Darwinian evolution. Natural selection continues its work within *Homo sapiens*, probably at characteristic rates for change in large, fairly stable populations, but the power of cultural evolution has dwarfed its influence (alteration in frequency of the sickling gene *v.* changes in modes of communication and transportation). Consider what we have done in the past 3000 years, all without the slightest evidence for any change in the power of the human brain.

2. *Modifiability*. Complex traits of cultural evolution can be altered rapidly; Darwinian change is limited to much slower rates of spread of alleles by natural selection.

3. *Diffusability*. Since traits of cultural evolution can be transmitted by imitation and inculcation, evolutionary patterns include frequent and complex anastomosis among branches. Darwinian evolution is a process of continuous divergence and ramification.

I believe that the future will bring mutual illumination between two vigorous, independent disciplines—Darwinian theory and cultural history. This is a good thing, joyously to be welcomed. But there will be no reduction of the human sciences to Darwinian theory and the research programme of human sociobiology will fail. The name, of course, may survive. It is an irony of history that movements are judged successful if their label sticks, though the emerging content of a discipline may lie closer to what opponents originally advocated. Modern geology, for example, is an even blend of Lyell's strict uniformitarianism and the claims of catastrophists. But we call the hybrid doctrine by Lyell's name.

I welcome the coming failure of reductionistic hopes because it will lead us to recognise human complexity at its proper level. For consumption by *Time's* millions, my colleague Bob Trivers maintained: "Sooner or later, political science, law, economics, psychology, psychiatry, and anthropology will all be branches of sociobiology" (*Time*, 1 August, 1977, p 54). It's one thing to conjecture, as I would allow, that common features among independently developed legal systems might reflect adaptive constraints and might be explicated usefully with some biological analogies. It is quite another to state, as Bob Trivers did, that the entire legal profession, among others, will be subsumed as mere epiphenomena of Darwinian processes.

I read Trivers's statement the day after I had sung in a full production of Berlioz's *Requiem*. And I remembered the visceral reaction I had experienced upon hearing the four brass choirs, finally amalgamated with the 10 tympani in the massive din preceding the great *Tuba mirum*—the spine tingling and the involuntary tears that almost prevented me from singing. I tried to analyse it in the terms of Wilson's conjecture—reduction of behaviour to neurobiology on the one hand and sociobiology on the other. And I realised that this conjecture might apply to my experience. My reaction had been physiological and, as a good mechanist, I do not doubt that its neurological foundation can be ascertained. I will also not be surprised to learn that the reaction has something to do with adaptation (emotional overwhelming to cement group coherence in the face of danger, to tell a story). But I also realised that these explanations, however "true", could never capture the meaning of that experience.

And I say this not to espouse mysticism or incomprehensibility, but merely to assert that the world of human behaviour is too complex and multifarious to be unlocked by any simple key. I say this to maintain that this richness —if anything—is both our hope and our essence. □

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