The Lure of the Simplistic

John Dupré†
University of Exeter

This paper attacks the perennial philosophical and scientific quest for a simple and unified vision of the world. Without denying the attraction of this vision, I argue that such a goal often seriously distorts our understanding of complex phenomena. The argument is illustrated with reference to simplistic attempts to provide extremely general views of biology, and especially of human nature, through the theory of evolution. Although that theory is a fundamental ingredient of our scientific world view, it provides only one of a number of perspectives that are required for an understanding of biology in general, and human behaviour in paticular. The argument is connected to the replacement of views of science in terms of universal laws with views that emphasise ranges of models more locally suited to specific phenomena.

My title, I hope, speaks for itself. It is related to something much more innocuous, the attraction of the simple. Simplicity is often a good thing: it avoids the hazards of the over-sophisticated, the baroque, or even the decadent. And many attractive things are worth pursuing. But when something attractive becomes a lure, it is leading us somewhere we shouldn't want to go—into a trap, or up the garden path, for instance. And when the simple becomes simplistic it becomes naive, unsubtle, or just misguided. Simplicity has often been suggested as a virtue of scientific theories or scientific explanation, and perhaps it is. But this is also a domain in which the simple all too often merges into the simplistic. Or so I shall claim.

As theologians have long been aware, one path to simplicity is unity. Some philosophers and scientists still maintain that there is only one fundamental law of nature. Everything else is just details. (Recalling the parallel with theology, there is just one true God, and the devil is in the details.) I shall not be concerned here with such a rarified claim to the simplicity of nature, but rather with narrower, if still ambitious claims for

†Send requests for reprints to the author, Department of Sociology, University of Exeter, EX4 4RJ, UK; e-mail: J.A.Dupre@exeter.ac.uk.

Philosophy of Science, 69 (September 2002) pp. S284–S293. 0031-8248/2002/69supp-0024$10.00

smaller, if still substantial, domains of the natural world. My main topic will be the theory of evolution.

Theodosius Dobzhansky famously remarked that nothing in biology makes sense except in the light of the theory of evolution. Perhaps so. But that is very far from the claim that everything in biology does make sense in the light of the theory of evolution. We, the members of *Homo sapiens,* are surely included within the broad umbrella of 'everything in biology'. And I suppose that we don't make much sense apart from the theory of evolution. That theory is, after all, our best account of how we came to be here at all. But I have been arguing for some years that the light of evolutionary theory, though it may wonderfully illuminate our origins, does little to help us understand what we currently do or why.[1] Simplifiers or unifiers will hope to show that one fundamental theory will provide the key to a whole domain such as that of Life on Earth, or even just Life. Anti-simplisticators will insist that even a theory as profound as this one will illuminate only certain aspects of a domain of this complexity.

We should begin by reflecting briefly on what exactly the theory of evolution is. It is, in part, a series of theses. The first, and perhaps central thesis, is that life came into being from non-living systems through a long sequence of intermediate forms. A second is that most, at least, of the living forms that now exist have common ancestors within this long and diverging sequence. So far we have nothing that holds out any promise of telling us anything much about the final products of the evolutionary process. The step that enthusiasts hope will accomplish this, is a third thesis, that changes between successive stages in the evolutionary sequence are to be explained by the process of natural selection. Of course no one holds that every such change is a product of natural selection, and I do not want to get involved here in the controversial question of exactly how much of evolutionary change is to be explained by natural selection. I shall rather concede, for the sake of argument, a slightly weaker thesis, that all biological adaptation is to be explained by natural selection. That is to say, the features of organisms that contribute to their survival and reproduction are the products of natural selection. (As will become clear later, I take this last thesis to have serious problems of interpretation and even truth.)

Such theses are hardly sufficient to provide the kind of unity or simplicity that is the topic of this paper. For that we need to extract from these theses something more like a traditional theory. And of course the theory in question will be the, or a, theory of evolution by natural selection. What I mean by a 'traditional theory' is some statement or statements of universal form that might figure in a covering law explanation of the

1. See, for instance, Dupré 1998, and in greatest detail, Dupré 2001.

presence of any adaptive feature of an organism. But here deep problems begin to appear. First, no one has succeeded in formulating a convincing universal statement of the theory of evolution by natural selection. This is not, I think, an accident. One reason why the task is likely to be impossible is that evolution by natural selection, unlike, perhaps, gravitational attraction or heat diffusion, is not itself one process. It is at least two processes, namely natural selection and inheritance.

Evolution by natural selection occurs through repeated cycles of selection (some members of a population are more successful than others at surviving and leaving offspring) and inheritance, the tendency of offspring to be more similar to their parents than to other members of the population. And neither of these subprocesses is itself plausibly a simple, homogeneous process. Selection is increasingly recognized as occurring on various different levels of organization, and perhaps often doing so simultaneously. Inheritance is much messier than that. By inheritance I definitely do not mean simply transfer of DNA. I mean to include the whole process of development by which an organism finally ends up with features more or less correlated with those of its parents; and no one now supposes that the provision of DNA is the sole means by which parents cause offspring to have features resembling their own. So inheritance, in this sense, is only very loosely referred to as a single process. It is rather the upshot of a vast series of interactions involving genes but also features of the environment that include anything from chemicals in the egg or mother to interactions with other whole organisms.

The relevance of pointing to the complexity of the processes that are involved in evolution by natural selection is to indicate their diversity. The processes of selection and inheritance will vary from particular case to particular case, and different aspects of each side of the process will be more or less significant in different cases. Hence any attempt to provide a general account of the theory of evolution by natural selection must necessarily involve abstraction away from the particularities of individual cases in a way that must make it false of most of those cases. And hence, finally, we cannot expect any such universal theory to provide the sort of D-N explanation machine envisaged in the simplistic unified vision.

It is considerations such as these that have led the majority of philosophers (and perhaps biologists) to see evolution not as providing us with some kind of universal law in the style of Newtonian mechanics, but rather as providing a large and open-ended family of models, a tool-kit with which we can hope to get some understanding of the processes involved in particular cases. But this move is fatal for unifying aspirations. A universal law carries with it, so to speak, the instructions for applications to new cases. You fill in appropriate values for the parameters and churn out a result. Models aren't like that. A model is more or less isomorphic to

the situation it is supposed to model. If you have succeeded in finding a model that represents the most important factors in a concrete situation, the model may give you useful information about that situation; if not, not. Models are only transportable to new situations to the extent that the same factors are important and no new ones are important. This is a very restrictive condition, and one that needs to be specifically justified in any particular case. And this is of course why we aim to assemble a tool-kit of models. The more diverse our tool-kit, the better chance we have of finding something that may serve the case in hand.

The best cautionary tales against the violation of these methodological precepts can be found in what I have in the past referred to as 'imperialist economics'. Economics is a model-based science if anything is. The particular factors relevant to real situations are enormously varied and only a fat and well-stocked tool-kit will give you much chance of dealing with particular cases. Unfortunately, however, economics has canonized one particular super-model and given it a role somewhat like that of the happily non-existent general theory of evolution by natural selection. This is the model of the perfectly competitive market. This is in reality a model of very limited application. Real markets generally have limited numbers of buyers and sellers, differentiated products, imperfect information, indivisible goods, and so on, and thus diverge very considerably from the assumptions of this ideal model. A good deal of respectable, if sometimes only modestly successful, work in real economics involves developing more sophisticated models to take account of these factors. But when economists venture into the more dubious terrain of markets for sex, children, marriage partners, and so on, since there is in fact no real market at all to suggest realistic restrictions on the model, what tends to get imported into the discussion is the pure concept of market from the super-model. The concept of 'market', one might say, goes on holiday. And the sort of nonsense generated, even by Nobel laureate economists,[2] is sadly predictable.

It is also relevant at this point to mention a rather different context in which unification has been promoted, the accounts of explanation as unification associated with Philip Kitcher and Michael Friedman.[3] Very crudely, this is the idea that we explain phenomena by assimilating them to other similar phenomena. The larger the groups of phenomena that we can thus assimilate, the more satisfactory are our explanations of the particular phenomena within the group. It will be clear from what I have said so far that the goal suggested by this admittedly crude statement is one

2. For example Becker 1991. This critical argument is developed in detail in Dupré 2001.

3. See Kitcher 1981; Friedman 1974. Both of these papers are reprinted in Pitt 1988.

that I would be very reluctant to endorse. Models, on my view, serve very limited explanatory roles, and we should not be drawn into trying to apply them very widely. But perhaps this is to look at explanatory unification at the wrong level. Perhaps it would be better to see it as a theory ranging not over explanations but, at a higher level, over tool-kits. The idea then would be that taking a model out of a very fat tool-kit, with models in it suitable for a wide range of explanatory tasks, is a more satisfactory activity than coming along with a very small tool-kit, containing perhaps just one model specially designed for the problem in hand (designed, we might say, ad hoc). Then a task for philosophers might be giving an account of why a certain large set of models, despite their variety, all belong in the same tool-box. This is perhaps a plausible interpretation of, for instance, Kitcher's (1993) schemata for evolutionary explanations. I must say, however, that I am inclined to suspect that, even for tool-kits, size doesn't matter. It strikes me, at any rate, as largely a pragmatic matter. Obviously a well-stuffed tool-kit with something for every job is a more useful thing to carry than one of those apocryphal devices on Swiss army knives for removing stones from horses' hooves. But if you should happen to find yourself desperate to remove a stone from your horse's hoof, then that curious little gadget may prove to be just the thing.

I should now return to my main topic of simplistic evolutionary explanations. I want to say something of why the allegedly 'universal acid' (Dennett 1995) of evolutionary explanation is often such an unhelpful reagent for dealing with human matters. I must now distance myself a little from the third thesis above about evolution. This was the thesis that adaptive features were always to be explained by natural selection. I want to point out two problems with this, neither of which is especially original or unfamiliar.

The first problem, made famous by Steven Jay Gould and Richard Lewontin's classic paper, "The Spandrels of San Marco" (1979), is that, for the purposes of such a thesis, it is very difficult to know what is a feature. We are interested in explaining those features that resulted through natural selection. We feel fairly confident that the shape of the lens of the eye, say, is the right kind of feature for such explanation and that having a scar over the left eye is not. In between there are all kinds of features that are on the one hand inherited, but on the other lack obvious functionality, and for which it is entirely unclear whether they are naturally selected adaptations or not. The basic problem is that the development of an organism is a highly integrated process, and much that happens must be a side-effect of other processes rather than an adapted feature in its own right—if indeed, this distinction does not already imply an indefensible atomism. Gould and Lewontin's famous example was the human chin, which is a novel feature of human evolution but which, they

claim, is not a consequence of natural selection for chins, but an epiphenomenon of other changes taking place in the human lineage.

A second problem, particularly salient to the human case, is that natural selection is not the only source of adaptation. Humans have developed enormously elaborate systems for surviving and functioning that have nothing to do with evolution by natural selection. Being a professor of philosophy is a decent way of making a living, and for all I know philosophy professors may have an above average fecundity. But not even the most rabid Panglossian supposes that there are genes for being a philosophy professor. It may well be that to become a philosophy professor requires a functioning brain, perhaps even an above average functional brain. And though, of course, the brain evolved, it did not evolve so that people could become philosophy professors. Being a philosophy professor is, to use the very useful terminology developed by John Searle (1995), a social status, something that depends on the consensus of a society, not merely the physiological features of an organism.

We can now see more clearly the kind of misguided simplification involved in recent attempts to apply evolutionary models to human behavior. Begin with a paradigm of the model of natural selection, the lens of the eye. We have a great deal of confidence that we know the optimal state of the lens, through application of a very well entrenched scientific practice, geometrical optics. We could very probably establish that the development of the lens was largely invariant with respect to environmental features. Not of course entirely invariant: inadequate diet, or nasty parasites could no doubt derail it. But under most reasonably favorable conditions for human development most humans will develop functioning lenses. The speculation that before the introduction of corrective lenses, greater visual acuity would have been an aid to survival, while still a speculation, is a plausible one. Though we can imagine discoveries that would refute this, the thesis that the lens is a device for seeing better and that it evolved by natural selection in favor of individuals who could see better is a highly convincing one.

Now consider the application of this kind of model to human behavior. The first problem, typically more severe even than the corresponding difficulty for physiology, is one of identifying features. A competent anatomist should have no great difficulty finding the lens in the eye and speculating that this is an organ with a specific function. No such organs of behavior emerge when the human brain is dissected. Evolutionary psychologists have responded to this by imaginatively dissecting brains into discrete behavioral organs or psychological modules. But here the organs are not independently discoverable entities but artifacts of the theory. There is of course fascinating work in neuropsychology in which behavioral deficits are correlated with particular injuries to the brain. But, first,

this work does not identify social exchange modules or rape modules, but neural structures necessary for particular tasks such as recognizing objects or words. And even the discovery of a neural lesion that caused incompetence in, for instance, acts of social exchange, would hardly prove the existence of a module the function of which was to regulate such acts. It is one of the most characteristic errors of popular behavioral genetics to suppose that identifying some feature of the brain without which a task cannot be performed is a way of identifying a feature with the function of performing that task. One need only compare much less complex structures. If one removes the cylinders from a car engine the car will not start. But it is not the function of the cylinders to make the car start. So even where neural damage can be correlated with particular behavioral deficiencies, not the slightest evidence is thereby provided for the identification of modules with particular psychological functions. There is, in short, no evidence whatever for the modules of evolutionary psychology, so there are, as far as we know, no features for the models to explain.

But even supposing one found a group of brain cells that lit up on one's Magnetic Resonance Imaging screen every time someone was engaged in, say, buying something, would we have identified a suitable target for an explanation by natural selection, a buying module? Hardly. First, we know that most people in contemporary societies often buy things, presumably a condition for any plausible account of the buying module (bizarre and sometimes offensive stories about, for instance, rape modules, notwithstanding). We know therefore, by a familiar philosophical maneuvre, that they are able to buy things. We suppose (ignoring for now some of the more extreme variants of contemporary shopping) that their brains are somewhat involved in this capacity. We play around a bit with our MRI machine and discover that a particular part of the brain is typically engaged when we are making financial transactions. And that, as far as I can see, is that, though of course we might imaginably go on to tell long and fascinating stories about the mental processing that was involved in buying a bottle of shampoo. There is perhaps a development of the story that would lead us to think that we had discovered a shopping organ, but before we take such a possibility seriously we need a lot more detail about what such a devleopment would involve. The great majority of work in evolutionary psychology is concerned with arguing that particular modules would have been useful to our distant ancestors. Much less attention has been devoted to the question of how we might now hope to detect such modules, or even what it would take for such a thing to exist.

The main moral so far is just to reinforce the claim that as models move away from the phenomena that they were originally designed to explain, the abstractions they involve become ever greater impediments to truth or illumination. But it will perhaps be objected that evolutionary models were

introduced to explain the features of biological organisms and human behavior is, in the end, a collection of features of biological organisms. It may be that we are not yet very good at sorting those features in a way that is well-suited for providing convincing explanations, but the modules of contemporary evolutionary psychology are at least an attempt in that direction and should be applauded. If that project is to be rejected, I need to say more about what, if anything, is fundamentally different about that case. One response, drawing on my reference to general critiques of adaptationist models by Gould and Lewontin and others, is simply to insist that such models are not universally suited to the broad range of phenomena just indicated, and so their inapplicability to human behavior needs no further explanation. More specifically, I might point to the fact that there is no reason to think that human behavior evolved to do the kinds of things people do today, and indeed every reason to think the opposite. No one in the stone age spent much time shopping at the local mall or writing philosophy papers, and evolutionary considerations can at best provide us with some of the necessary conditions for the performance of such activities, hardly a full or satisfying explanation.

Though I think all this is perfectly correct, I would like to gesture toward a deeper problem. The deficiencies of evolutionary psychology derive also from a more traditional expression of the unifying and simplifying urge, reductionism. The picture of explanation in such models is thoroughly bottom up. The human agent is seen as a complex mechanism emitting bits of behavior in response to external stimuli. To understand why particular bits of behavior are emitted we must understand the detailed structure of the mechanism. It seems to me that the application of this picture is not merely empirically unsuccessful, but conceptually confused. It makes no sense to describe a piece of machinery that is designed, whether by God or Mother Nature, to emit shopping behavior when confronted by mall-stimuli.

This is simply because shopping is an essentially social phenomenon, or better, in terms of the useful analysis by Searle to which I have already alluded, an institutional fact. Shopping essentially involves such things as money, shops, customers, sales assistants, and so on. Nothing can satisfy any of these descriptions in the absence of a society that recognizes these various statuses. And in fact, as Searle convincingly argues, a society can only recognize these statuses when it has a language, or at any rate a symbolic system, through which they can be represented. To take just the most obvious example, nothing can be money simply by virtue of its physical properties. To be money is, at least, to be socially recognized as money. Whether or not Robinson Crusoe could learn to talk to himself, he must wait for Man Friday before he has any chance of going shopping.

There is a rather different question to be asked, to what extent the

theory of evolution can help us to understand the possibility of shopping. No doubt we evolved complex brains over millions of years, and without fairly complex brains we could not form the kinds of society that make shopping possible. Hypotheses about how we came to acquire such hypertrophied cerebra thus have some distant relevance to the possibility of shopping. There is also the much more controversial hypothesis that we wouldn't have developed such a practice as shopping if we had not, perhaps in the Stone Age, developed specialized mental equipment for evaluating exchanges (or whatever), and that these modules are now engaged when we enter the mall. It will be clear that I consider such hypotheses as wholly ungrounded speculation. But my point here is that, even if true, they would still be of only distant relevance to understanding the possibility and nature of shopping. To do that we must understand the social conventions and institutions that underlie this complex social practice.

All this is of course just what we should expect. Evolutionary theory is concerned with the origins of biological phenomena. But to assume that understanding the origins of a phenomenon is all there is to understanding it is to commit a crude instance of the genetic fallacy. And in this case we have compelling reasons to conclude that committing the genetic fallacy is indeed committing a fallacy.

This, finally, illustrates the more general point of this paper. As a general background to biology, as that without which nothing in biology makes sense, the theory of evolution is little more than a few theses about the origins of life. These are very important theses for our understanding of our place in nature, but very sparse as the basis for specific evolutionary explanations. For the latter we have a well-stocked tool-kit of explanatory models. But even this well-stocked tool-kit, when we confront real situations with complications including genetic linkage, pleiotropy, polygeny, epistasis, complex developmental interactions with the environment, culturally mediated learning, and so on, may prove of only limited efficacy. In the case of human behavior we are not only confident that all these complications obtain, but we have a domain of phenomena constituted not by the biological but by the social. The relevance of biological models struggling with this array of complexities, and aspiring at best to provide explanations of hypothetical biological precursors of the social behavior that is our ultimate concern, is, to say the least, marginal. Only the intoxication with the vision of grand theories, the lure of the simplistic, could inspire us to devote much energy to such a misguided project.

Let me conclude with a more ambitious proposal. Human behavior is in one sense biological, but it is also fundamentally social. It is culturally diverse. On occasion it is economic, religious, domestic, competitive or cooperative, and so on. It is, in short, exceedingly complex. Any grand unifying theory of human nature can be confidently predicted to distort

many or most of these complexities. It seems to me that here we should not even aspire to approach the topic with a unified tool-kit. Indeed, the more diverse and varied the contents of our tool-kit, the better chance we have of coming to grips with the really interesting problems about human behavior. Just as simplicity or simplisticity is naturally connected with unity, so complexity cries out for plurality. The only route to a deeper understanding of ourselves is through radical epistemological pluralism.

REFERENCES

Becker, G. S. ([1981] 1991), *A Treatise on the Family.* Cambridge, Mass.: Harvard University Press.

Dennett, D. (1995), *Darwin's Dangerous Idea.* New York: Simon and Schuster.

Dupré, J. (1998), "Against Reductive Theories of Human Behaviour", *Proceedings of the Aristotelian Society* 72 (Supplement): 153–171.

——— (2001), *Scientism and Human Nature.* Oxford: Oxford University Press.

Friedman, M. (1974), "Explanation and Scientific Understanding", *Journal of Philosophy* 71: 5–19.

Gould, S. J. and R. C. Lewontin (1979), "The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme", *Proceedings of the Royal Society of London* 205: 581–598.

Kitcher, P. (1981), "Explanatory Unification", *Philosophy of Science* 48: 507–531.

——— (1993), *The Advancement of Science: Science Without Legend, Objectivity Without Illusion.* New York: Oxford University Press.

Pitt, J. C. (ed.) (1988), *Theories of Explanation.* New York: Oxford University Press.

Searle, J. R. (1995), *The Construction of Social Reality.* London: Penguin Books.