

Interview

INTERVIEW WITH PHILIP KITCHER

Philip Kitcher is Professor of Philosophy at Columbia University and one of the most influential philosophers of science of the past two decades. His writings have been distinguished by the depth and clarity of analysis and the broad range of the questions on which he has written. Kitcher has published numerous papers on the philosophy of biology, works on foundational epistemological and metaphysical issues related to science (*The Nature of Mathematical Knowledge* [1983], *The Advancement of Science* [1993]), and several books dealing with hot-button issues such as creationism (*Abusing Science* [1982]), sociobiology (*Vaulting Ambition* [1985]), and genetic engineering (*The Lives to Come* [1996]). His most recent books are *Science, Truth and Democracy* (2001) and *In Mendel's Mirror* (2003), a collection of his most important papers recently reviewed by *Human Nature Review* (<http://human-nature.com/nibbs/04/rawilson.html>).

Human Nature Review associate editor Phil Gasper interviewed Kitcher by email recently.

PG: You dedicate your new collection of essays, *In Mendel's Mirror*, to Richard Lewontin and the late Stephen Jay Gould, but in two papers you also take issue with some of Lewontin's ideas, lightheartedly describing yourself as playing Keir Hardie (the founder of the British Labour Party) to Lewontin's Lenin. Why did you choose this dedication and what are your disagreements with Lewontin?

PK: I dedicated *In Mendel's Mirror* to the memory of Steve and to Dick (originally to Steve and Dick) because of the important role they have played in my own thinking about biology, and, of course, in that of many other philosophers of biology. Both have been immensely hospitable to philosophers, and both have written a great deal that is of philosophical interest. Of course, from time to time, I have disagreed with both of them, and, in Dick's case, this has taken a particular form. Although we have been on the same side of disputes about the credentials of human sociobiology (say) his stance has often been more radical than mine. He's chided me for not recognizing the systematic way in which the conclusions

we both oppose are generated; I've seen him as importing an overarching scheme where there is none to be found. It seemed to me that the contrast between the left-wing approaches of Keir Hardie and Lenin captured this in a mildly amusing way.

PG: We might distinguish, first, the claim that there are social factors, such as the interests of dominant groups, which might systematically favor the repeated emergence of hypotheses which seem to favor those interests despite little scientific merit and, second, the claim that there are features intrinsic to our current biological paradigms that effectively serve the same purpose. The first claim is about factors external to science, the second about internal factors. I take it that you wouldn't necessarily deny the first claim, but that your dispute with Lewontin is about the second. Perhaps he would maintain that the rot runs deeper than the first claim by itself recognizes, and that the social factors have affected the way in which fundamental biological issues are conceptualized. Does that seem a fair way of capturing your disagreement?

PK: The principal difference between Lewontin and me seems to be that he inclines to accept general explanations for what he sees as bad trends in biological research. In the case of reductionism or sociobiology, for example, he sees a global position lurking in the background - possibly one that reflects a political ideology or something that has become embedded in our culture. I'm more inclined to suppose that the misadventures arise piecemeal, needing to be tackled on a case-by-case basis.

PG: In your now classic paper "1953 And All That: A Tale of Two Sciences", you argue forcefully against the idea that classical genetics can successfully be reduced to molecular biology. You even suggest that "higher level" states or events may sometimes explain "what goes on at a more fundamental level." But in a later paper "The Return of the Gene", you and Kim Sterelny argue that evolutionary change can always be adequately explained in genetic terms, and that it's simply a pragmatic matter whether or not to offer an explanation in terms of gene selection or selection at some higher level (whether organism or group). So in one case you defend a strongly anti-reductionist position, but in the other you don't. Isn't there some tension between what you say about these two issues?

PK: I don't see any conflict between the two papers. The facts that you can't identify the notion of *gene* in the terms of physics and chemistry and that you can't explain regularities about genes by constructing derivations from physico-chemical principles don't imply that there must be similar explanatory failures

when you try to understand natural selection in terms of genic fitnesses. Reductionist programs stand or fall with the details of the particular cases. The more interesting tension I find between the papers is in my defense of realism about genes, and the kind of anti-realist conventionalism I think holds in the theory of natural selection. I think biologists and philosophers have overinterpreted Darwin's metaphor. They think there must be a particular level at which the selecting finger points. But there isn't. I discuss this in a forthcoming essay in *Biology and Philosophy* where I try to evaluate some of Gould's claims about hierarchical selection.

PG: Samir Okasha recently contributed a helpful survey of the levels of selection debate to HNR (<http://human-nature.com/nibbs/03/okasha.html>). But aren't the issues which you distinguish really two sides of the same coin? Realism about genes makes sense because there are explanatory patterns at the genetic level that we miss if we focus only on the underlying physico-chemical principles. True, for every specific genetic process there is an underlying physico-chemical story that might in principle be told, but that underlying story lacks the explanatory power of the story we tell at the genetic level. What strikes me is the similarity between this case and the debates about natural selection. Pluralists about selection like Gould (and Elliott Sober and David Sloan Wilson), who hold that sometimes (or often) selection is best seen as operating, not at the genic, but at the organismic or group level, don't disagree that there is some genic level story to be told, but maintain that it can lack the explanatory power of looking at things from a higher level. I guess that in principle it would be possible that, unlike in the case of genes and their physico-chemical constituents, no relevant explanatory patterns emerge at the level of organisms or groups, but that claim seems to me sufficiently implausible to create what I called a "tension" between your two papers. Why not think that just as the genetic story is sometimes superior to the physico-chemical one, the organismic or group story is sometimes similarly superior? Or am I missing some significant difference between the two cases?

PK: I see an enormous difference between *entity-realism* and attempts to identify the *real* locus of causation in selection processes. Realism about genes can be motivated by reflection on the precision and scope of our interventions in processes of heredity and development: frankly it would seem quite miraculous that we be able to manufacture all the special-purpose organisms we do unless our views about genes and their molecular structures were fairly accurate. The units-of-selection controversy is a very different story. One can tell all the facts about how genotype and phenotype frequencies change across the generations - including the causal explanations of the changes - without any commitment to a definite level at which selection acts. For example, it makes no difference whether one thinks that

a particular allele's production of a specific protein initiates a causal chain that makes it likely that that allele will have an enhanced chance of finding its way into the gene pool or whether one believes that some phenotypic trait raises the probability that an organism will have increased reproductive success. Natural selection, we should always remember, is a *metaphor*, inspired by artificial selection. When breeders select, there is something they have in mind, and it makes sense to talk about a locus of selection or a level of selection - the breeder wants organisms with that allele or with that that phenotypic trait. But nature doesn't have intentions. There are processes of differential proliferation, and one can describe them *completely* (identifying all the causal facts) by supposing that the unit of selection is this or that. The units of selection controversy started with worries about the validity of group selection, and has become a philosophical exercise in irrelevant metaphysics, precisely because those with "realist" sympathies have overstretched Darwin's metaphor.

PG: Many biologists and social scientists have argued that there is no biological basis for the division of the human population into different races. You take issue with these arguments. Why?

PK: I'm very much on the side of biologists and anthropologists who argue against straightforward phenotypic accounts of race. What interests me is the possibility of finding within our own species the kinds of "local races" that biologists discern in nonhuman species. If there are such divisions, then, because culture makes such an enormous difference to our mating patterns, it will be correct to say both that racial divisions are biological (they represent a biological pattern of decreased mating) and that they are socially constructed (that pattern comes about because of the history and development of our society and culture). So I definitely want to get beyond the old-fashioned approaches to race, looking at the ways in which biological facts and cultural facts intertwine and reinforce one another.

PG: You've been a close observer of the Human Genome Project and its ethical, political and social implications. Now that the human genome has been mapped, what are your hopes and fears for the way in which that knowledge may be used?

PK: I hope very much that the development of rapid sequencing techniques will be deployed to address significant biological questions about development and intracellular metabolism. The answers to those questions can be expected to pay great dividends down the road. I hope also that there will be some serious attempts to come to grips with the elementary problems to which the success of the HGP adds new twists. It's become ever clearer to me that those problems are matters of social justice. The first step, one that should have been taken a decade ago,

is to put in place a universal health care system. But there are many other social protections needed to make sure that the potential harms of the new knowledge we have are avoided. I also hope that scientists in the affluent world will start to think seriously about how to use our new knowledge to tackle problems of infectious disease that kill and disable millions of poor people.

PG: In your recent book *Science, Truth, and Democracy* you were rather pessimistic about these possibilities, at least in the short term. You discuss the failure of the HGP's ELSI program (set up to address the potential ethical, legal and social implications of genomic research) and propose that leading genomic scientists should publicly abandon their funding and pursue other lines of research. Is there anything more that scientists as individuals or as a group can do? Do we need to reconceptualize the role of science and scientists in our society?

PK: I think we do need to reconceptualize the role of science and scientists in our society. In *Science, Truth, and Democracy*, and in some more recent writings, I've been suggesting that philosophers of science ought to think about the functional role of science as an institution, and about the responsibilities that role brings for scientists and science policy-makers. Many people find these questions uncomfortable. It seems to me that they are unavoidable in a world in which the interests of many people are simply overlooked or marginalized in the current pursuit of scientific research. (See, for example, James Flory and Philip Kitcher "Global Health and the Scientific Research Agenda", *Philosophy and Public Affairs*, 32, 2004, 36-65, Philip Kitcher "What Kinds of Science Should be Done?" in Alan Lightman, Daniel Sarewitz, and Christina Desser *Living With The Genie* (Washington D.C.: Island Press, 2003) 201-224, and Philip Kitcher "Responsible Biology", forthcoming in *Bioscience*).

PG: In the 1980s, you were one of the leading critics of the excesses of human sociobiology, in particular what you called "pop sociobiology". In the 1990s, sociobiology was reborn as evolutionary psychology. You've argued that some of this more recent work (most notably Thornhill and Palmer on rape) repeats all the old mistakes of pop sociobiology. First, what are those mistakes? Second, do you think that more careful work in evolutionary psychology represents an advance on old-style sociobiology?

PK: The mistakes made by pop sociobiology that are repeated in the more flamboyant forms of evolutionary psychology are: (1) no attempt to think seriously about whether there could be a genetic basis for the traits considered; (2) a penchant for substituting casual and vague general claims about fitness for detailed exploration of precise models - this often leads to neglect of alternative hypothe-

ses; (3) no careful consideration of whether cultural transmission might play a significant role in the phenomena. At its worst, the new evolutionary psychology is orders of magnitude less sophisticated than the work of some of the pop socio-biologists whom I criticized. I think of E. O. Wilson, Richard Alexander, Mildred Dickemann, and others as being in a quite different intellectual league from Randy Thornhill and David Buss. *In Mendel's Mirror* not only contains my critique of Thornhill and Palmer on rape, but also an essay suggesting how the evolutionary exploration of human behavior might be done better ("Developmental decomposition and the future of human behavioral ecology"). So far as I know, nobody lives up to the standards I set there. But it should be recognized that some people do better than Thornhill, Palmer and Buss: Cosmides and Tooby, for example, are clearly more careful.

PG: Another debate you've recently returned to is evolution versus creation. The final essay in your collection takes on the "intelligent design" creationism of Michael Behe and Phillip Johnson. Why does this controversy refuse to die in the US, and do you have any hope that it can be put to rest in your lifetime?

PK: I find the American yen for primitive religion deeply puzzling and disturbing. Recently I've been reading William James and John Dewey on religion, and it's evident that they are responding to a public religious culture that is far more sophisticated. I suspect that the need to rebut fallacies about evolution will continue for a very long time.

PG: Philosophy of science has changed enormously over the past forty years. Do you think there has been genuine progress in the discipline? And what contributions do you think philosophers of science can make to scientific research and to the broader understanding of science?

PK: Philosophy of science has made great progress in tackling problems of the special sciences. Much less has been done to come to terms with science as a social phenomenon, to understand the ways in which scientific claims are affected by and affect the kinds of societies we have, and to integrate philosophy of science with social and political philosophy. This is an area in which I think that a large amount of philosophical work needs to be done. Perhaps in twenty years time there will be as many papers on the appropriate goals for biomedical and environmental research as there are now on much-discussed puzzles in confirmation theory.