HUMAN NATURE REVIEW
ISSN 1476-1084
http://human-nature.com/

Book Review

Critical Review of Philip Kitcher's *In Mendel's Mirror: Philosophical Reflections on Biology*. Oxford University Press, 2003.

By

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The philosophy of biology began to develop as a distinct field within the philosophy of science in the early 1970s, shortly before Philip Kitcher turned from mathematics and physics to biology in his thinking about general issues concerning the nature of science. Not only did the answers to traditional questions within the field seem problematic once one turned from the "hard sciences" to the softer biological sciences, but the questions themselves came to be viewed as less obviously the right ones to be asking.

At the core of the philosopher's view of science thirty years ago was a series of questions about science in general. What was distinctive about science that made it our paradigm of successful intellectual inquiry? How should we think about the nature of testing and confirmation in science? What was the character of scientific explanation? Was scientific change a rational process, and if so, how should we understand that process? (And if not--as was suggested to many by influential works such as Thomas Kuhn's *The Structure of Scientific Revolutions* and Paul Feyerabend's *Against Method*--what are the implications for our broader views of science?).

The work of the logical positivists from the 1930s on, particularly that of Rudolph Carnap and Carl Gustav Hempel, had established such questions as constitutive of the field, and much of philosophy of science in the 1960s and 1970s was either a development of positivist ideas or a reaction to, and departure from, them. The feature of positivism and the work that followed in its stead most relevant here is the presupposition that the right questions to be asking about science were perfectly general questions, questions about science *per se*. The various areas of physics and chemistry were taken as paradigms of scientific inquiry, and were drawn on extensively, even if not always in depth, both to illustrate and to test the answers given to the general questions asked.

Occasionally examples were drawn from the medical sciences, the best known of which was that of Ignaz Semmelweis's discovery of the relationship between hand-washing and the transmission of infectious disease in surgery and childbirth in Vienna in the 1840s, used by Hempel in his classic textbook *Philosophy of Natural Science* to illustrate scientific inquiry in action. But largely absent were discussions of the range of concepts and issues with a home in the biological sciences—the concept of the gene, the nature of species, the relationship between earlier and later theories within genetics, the evolution of human and non-human altruism, eugenics and social policy, the place of biology in thinking about ethics. These topics constitute the heart of the essays in Philip Kitcher's new book.

Since that time these topics and a range of others have come to define the philosophy of biology as its own field. At least four changes in the spirit of the times were instrumental in making this transition.

The first was that there was a heightened engagement between philosophers thinking about science and scientists themselves. This happened on institutional, conceptual, and methodological dimensions. Philosophers and scientists "hung out together" more frequently: in professional meetings, in the literature they read, and in the collaborations they forged. While previously many philosophers had often had substantial formal backgrounds in a physical science, philosophers now also started down the path from philosopher to scientist, taking up additional formal or informal training in particular sciences following their degree-based, disciplinary education in philosophy. Subsequently, philosophers probed more deeply into the sciences they were discussing, and began to think about them more comparatively. They moved from appealing to the sciences for illustrative examples, to taking on detailed case studies to advance broader views about the nature of science, to entering full-scale into both theoretical and practical issues within the sciences themselves.

A second shift was the growing attention to a wider range of sciences, beginning with the biological and social sciences (in general), but then focusing on particular sciences within them, including evolutionary theory, genetics, cognitive psychology, economics, anthropology, and archaeology. Philosophers of science now do not simply react to or draw on work in such fields, but often actively contribute to their development, as is illustrated by recent work in cognitive science, in developmental biology, and in game theoretical approaches to human social behavior. This has fed back, in turn, into issues about the nature of science in general by making views of scientific theories, explanation, and laws accountable not just to the physical but to the biological, cognitive, and social sciences.

A third change was the increased sensitivity of philosophers to the work of historians and sociologists of science. While "science and technology studies" (STS) often remains a turf war between philosophers, historians, and sociologists of science, philosophy of science itself has become more pluralistic as a result of

its contributions to STS and this interaction across disciplines.

Finally, the balance of power within the field has shifted from general philosophy of science to the philosophy of particular sciences. The general questions with which I began—about confirmation, explanation, and change within science—remain. But they are often taken up within the context of some particular science or episode within one science, and their discussion now competes with issues that arise only within specific sciences. While not exactly marginalized, general philosophy of science is now one amongst a number of orientations within the philosophy of science, something reflected in the range of the content of philosophy of science courses, the program of the annual meetings of organizations, such as the Philosophy of Science Association and the British Society for the Philosophy of Science, and the dissertation work of current and recent graduate students.

I have already intimated that Philip Kitcher's career spans this period of transition, a career itself that reflects the general changes in the field. Kitcher was a graduate student at Princeton working under the philosopher of mathematics Paul Benacerraf. His dissertation was written in that field (on mathematics and certainty). Like Elliott Sober, another leading figure in contemporary philosophy of biology, Kitcher entered the field shaped by an agenda in general philosophy of science set by logical positivism, and his transition to the philosophy of biology was facilitated by spending the bulk of a sabbatical leave as a guest of the population geneticist Richard Lewontin at the Museum of Comparative Zoology at Harvard University. Perhaps more than any other philosopher of biology, Kitcher has maintained his research interests in general philosophy of science, publishing *The Advancement of Science* (1993) and *Science, Truth, and Democracy* (2001) since making many of his major contributions to the philosophy of biology. But what distinguishes Kitcher's work is that, from the outset, it has straddled the divide between the theoretical orientation of much of the field, dominated as it has been by conceptual issues in evolutionary theory (what is fitness? what is a gene? what are species?), and more socially oriented aspects of the meeting place between biology and philosophy.

The seventeen essays in *In Mendel's Mirror* are representative of Kitcher's contributions to both of these strands to the philosophy of biology. As Kitcher notes in his brief but helpful introduction to the volume, "biological ideas are often introduced into discussions of social issues" (p. *xiii*), bringing a dimension to the philosophy of biology that was largely absent in earlier work in the philosophy of science. The themes in Kitcher's earlier books on socially-oriented issues--*Abusing Science: The Case Against Creationism* (1982) on creationism and creation science, *Vaulting Ambition: Sociobiology and the Quest for Human Nature* (1985) on pop sociobiology, and *The Lives to Come: The Genetic Revolution and Human Possibilities* (1996) on the Human Genome Project and the broader social issues sharpened in its wake--are reflected in the content of the last

seven essays collected here, five of which have been published in the last few years.

Kitcher has staked out distinctive positions on most of the issues he has discussed, and his stance on one topic (e.g., the levels of selection) is often closely related to that on another (e.g., the nature of species). To give the flavor of these positions and some of the relationships between them, I shall focus below on two general themes that recur across the essays: the pluralism that Kitcher has defended on a range of issues, and what has been claimed in the name of science in the human behavioral sciences. (A further theme that links essays 8-10—that of the place of modeling in articulating ideas about altruism and cultural evolution—and the essays themselves, are worthy of discussion; I omit discussion of them for reasons of time and space).

In a pair of essays on species, "Species" (1984) and "Some Puzzles About Species" (1989), Kitcher articulates and defends a view that he calls *pluralistic realism* about species. Pluralism here contrasts with monism. But to be a pluralist about species is not only to think, negatively, that there is no single conception of species that is empirically adequate. Rather, it is to hold, more positively, that we should embrace this conclusion with some enthusiasm, instead of viewing it as a philosophical failure of some kind. As Kitcher himself recognizes, characterizing realism is no small task. But at its core is the idea that our scientific theories pick out or correspond to objective structures in nature. In suggesting how to combine pluralistic and realistic views of species, he says, perhaps optimistically, that "however it [realism] is developed, it will prove compatible with pluralism about species. *Pluralistic* realism rests on the idea that our objective interests may be diverse, that we may be objectively correct in pursuing biological inquiries which demand different forms of explanation, so that the patterning of nature generated in different areas of biology may cross-classify the constituents of nature" (p.128).

Pluralism about species has *prima facie* plausibility in light of the dizzying array of species concepts out there, an array that has only broadened in recent work on species. For example, in a 1997 paper, the biologist John Mayden distinguished twenty-two species concepts to be found in the contemporary literature, and recent collections of essays on species (e.g., Howard and Berlocher 1998, Wilson 1999, Wheeler and Meier 2000) show little evidence of convergence about species concepts. Pluralism is an ecumenical view apt for capturing the diversity and complexity to biological practice, particularly as support for particular species concepts is not equally distributed across those working on the various taxonomic groups, or those adopting primarily either an ecological, molecular, behavioral, or evolutionary approach within biology. Distinctive of Kitcher's pluralism are two features.

First, it has been articulated together with the rejection of the common view that species are individuals or historical entities of some kind, introduced by

the zoologist Michael Ghiselin and later championed by the philosopher David Hull. Kitcher thinks that there is nothing wrong with the view that species are *sets* and, particularly in "Some Puzzles About Species", Kitcher critiques the chief arguments for thinking that species are historical entities, raising a series of novel objections, many of which focus on problems in understanding populations as individuals.

Second, Kitcher diagnoses a particular pattern to the plurality of species concepts within the biological sciences, distinguishing two clusters of concepts: those that invoke a structural concept of species, and those that appeal to a historical concept.

A major worry, however, concerns Kitcher's complacency about the compatibility of pluralism and realism about species. My own view is that, here as elsewhere in biology where pluralism has proven to be an attractive option, there is more of a tension between the two halves to pluralistic realism than its proponents have acknowledged. Kitcher may be right when he says that "we must recognize that there are many different contexts of investigation in which the concept of species is employed, and that the currently favored set of species taxa has emerged through a history in which different groups of organisms have been classified by biologists working on different biological problems" (p.129). But it is a further step to conclude not just that it is "different strokes for different folks", but that all of the resulting conceptions of species pick out objective structures in the biological world. Creationists and traditional essentialists about species operate within their own context of investigation. Since Kitcher's pluralism does not seem to extend to embrace their conceptions of species (a point made previously, I believe, by David Hull), more needs to be said about what privileges just the concepts that Kitcher discusses as corresponding to objective structures in the world.

This points simply to a gap between pluralism and realism. The more fundamental problem is that since many of the favored conceptions of species divide up the biological world in incompatible ways, it is difficult to see how they could all be correct (even in their own context). For example, the so-called biological species concept, defended by the ornithologist Ernst Mayr for the past sixty years, holds that species are interbreeding populations that are reproductively isolated from other such populations. Phylogenetic species concepts, of which there are several, view species as historical lineages subject to one or more constraints. Sometimes these concepts pick out the same group of organisms as conspecifics, but often they do not, and they can give very different counts for numbers of species within specific orders or families. Moreover, proponents of each of these views disagree (sometimes vehemently) about which is *the* right way to conceptualize species. Like moral relativism, pluralistic realism suggests that both are right (in their own context), and so makes this disagreement merely apparent.

In other essays Kitcher defends pluralistic views about distinct issues in the biological sciences, including about the levels of selection, functions, and genes. Some of the problems I have identified above surface in those defenses. For example, in "The Return of the Gene", a paper written with the Australian philosopher Kim Sterelny in 1988, Kitcher defends a pluralistic version of genic selection theory. Pluralism here is characterized as the view that "there are alternative, maximally adequate representations of the causal structure of the selection process" (p.108). When it is combined with the view that representations in terms of the efficacy and fitness of genic properties are always available, we get what Kitcher and Sterelny call *pluralist genic selectionism.*

This pluralism is a form of what I have elsewhere (Wilson 2003, in press) called *model* pluralism, a pluralism about the models or representations we use to express how natural selection operates. Model pluralism contrasts with *unit* or *agent* pluralism, the view that there is no one unit, level, or agent of selection, but a plurality of them. On agent pluralism, genes, individuals, and groups, for example, can all be agents of selection, and which (if any) is the agent of selection in any particular case is to be determined by the details of the case.

Model pluralism often reflects a kind of *Neckerphilia*, a love of the Necker cube analogy that Richard Dawkins introduced to discussions of the levels of selection in defending the compatibility of his selfish gene theory with standard, individual-centred Darwinian theory. Kitcher is a self-avowed Neckerphiliac (see Kitcher, in press), as is Sterelny and is their former student, the philosopher Peter Godfrey-Smith. Neckerphilia is also rampant amongst biologists themselves. The problem with model pluralism and the Neckerphilia that often accompanies it, however, is much the same as that with pluralistic realism about species concepts. Underlying the different models or representations used to characterize the operation of natural selection are distinct ontological assumptions, different views about how the world is structured. Proponents of group selection, for example, view groups not simply as the context in which genes or individuals operate, but as agents of selection in their own right. Proponents of genic selection, by contrast, typically adopt a kind of fundamentalism about the powers of genes, a view reflected in pluralist genic selectionism.

Strictly speaking, model pluralism is compatible with having no particular ontological commitments about the structure of the world, and so does not entail any form of realism. But model pluralism, as it has been articulated and defended, typically carries with it further ontological baggage that suggests that one view of the Necker cube is more fundamental than the other. For example, Elliott Sober and David Sloan Wilson argue that the exclusion of the theory of group selection from the "happy pluralistic family" of alternative perspectives "reflects a massive confusion between process and perspective. The theories that were launched as alternatives to group selection are merely different ways of looking at evolution in group structured populations" (1998:98). For them, group structure

is a fundamental feature of populations in and across which natural selection operates. The entomologists Andrew Bourke and Nigel Franks agree that one consequence of model pluralism is that "the practice of attributing traits to, say, either colony-level selection or kin selection is illogical". But they summarize their own view as implying that "colony-level, group, individual, and kin selection are all aspects of gene selection (1995:67). For them, genic selection is the most fundamental form that selection takes.

Let me turn now to a second theme in these essays, that of what has been claimed in the name of science in the human behavioral sciences. Of Kitcher's five recent essays here, two of them, "Pop Sociobiology Reborn: The Evolutionary Psychology of Sex and Violence", co-authored with A. Leah Vickers, and "Born-Again Creationism", pick up on themes treated in depth by Kitcher in his two early books on, respectively, sociobiology and creation science. Each is a response to particular works of influence. In the first case, Kitcher discusses the psychologist David Buss's claims about the origins of human psychological sex differences, and Randy Thornhill and Craig Palmer's views of the origins of human rape; in the second, Kitcher responds to the biochemist Michael Behe's new spin on an old argument for creationism that appeals to the complexity of processes claimed to evolve by natural selection, as well as to professor of law Philip Johnson's rhetorical challenges to evolutionary theory in his *Darwin on Trial*. I want to focus instead on the remaining three essays, "Battling the Undead: How (and How Not) to Resist Genetic Determinism", "Race, Ethnicity, Biology, Culture", and "Utopian Eugenics and Social Inequality", all published from 1999-2001, and some threads that run through them.

The first of these essays is a tribute to Richard Lewontin, one paid in part through criticism of Lewontin's "dialectical biology" and the critique of genocentrism in biology that it shares with developmental systems theory. Kitcher claims that both putative paradigms for biology provide off-the-mark criticisms of the role and conception of genes within biology more generally, and views them as offering little by way of a positive, alternative program for research. In making these points he relies largely on the systematic, more philosophical statements of these views. One way in which Kitcher's argument might have been strengthened would have been to consider some of the work of those within evolutionary developmental biology, such as Brian Hall, Gunther Wagner, and Scott Gilbert, or that of Eva Jablonka and Marion Lamb on non-genetic inheritance systems. Many of these researchers have recently begun to consider this work in relation to at least developmental systems theory, and it may be that Kitcher's critique is somewhat premature (see also Griffiths, in press).

In "Race, Ethnicity, Biology, Culture", Kitcher argues against eliminativist views of human racial categories, defending the idea that there is a defensible biological way of conceptualizing race. Kitcher is sensitive to the problematic character of both contemporary racial categories and the past uses to which vari-

ants of these have been put (he begins with several juicy quotes from the early twentieth-century eugenicist Charles Davenport). But he also thinks that there is a way to fashion a concept of race that reflects its historical nature and is anchored in both conceptual and empirical claims about patterns of interbreeding within sub-populations of *Homo sapiens*.

Kitcher begins with three conditions he thinks are necessary for any workable concept of race: that the species be divided into mutually exclusive subsets of individuals that belong either to one of a number of pure races, or are of mixed race; that the pure races are closed under reproduction; and that the pure races are closed under ancestry. The second condition implies that if Adam and Eve are of one pure race, then so too are all of their offspring. The third implies the converse, that if their offspring are of a given pure race, then so too are Adam and Eve and all of their ancestors. What these conditions highlight is "the idea of a historical lineage within which inbreeding occurs" (p.236). Kitcher uses this as a way to introduce the idea that races are at least approximately reproductively isolated populations. For it is this notion, familiar from the literature on species, that ensures that the distinctive characteristics of different pure races survive over evolutionary time. Kitcher then goes on to draw on data from the 1970 US Census and a 1982 paper on interracial marriage patterns to suggest that the ideal patterns demarcated by his conceptual analysis of race are at least approximated by actual patterns of ancestry and descent. There are at least three related problems with this approach to understanding race.

The first is that the notions that serve as input to Kitcher's fashioning of race—sub-populations, phenotypic differences, ancestry, descent, inbred lineages, reproductive isolation—seem too socially anemic to deliver as output even an idealization of a concept of race. As Kitcher recognizes in discussing the need to go beyond his initial three necessary conditions, were these to be the only conditions on the concept of race, there may be a plethora of "racial categories", including races distinguished by eye color (his example) or by intelligence (mine). This problem is not simply to be resolved by tightening up the original conditions through the introduction of more neutral-sounding biological relations. For the problem lies in there being nothing in what Kitcher puts into the notion of race to suggest that a few phenotypic characters, amongst which skin color is the most prominent, are crucial to anything like the concept of race that is used in everyday discourse, in social science, and in government policies, regulations, and data.

Kitcher's approach assumes that this sort of fact emerges once we move from the conceptual to the empirical realm of examining actual patterns of mating in human sub-populations. This seems to me mistaken. This is because of problems in generating categorical differences in continuous phenotypic properties such as skin color, but also because the approach is unlikely to exclude differences in other heritable but clearly non-racial characteristics. I suspect that the gene flow between Oxford professors and Broken Hill miners has been extremely

limited for the 150 years that the two groups have coexisted, and that the two groups can be readily distinguished by many phenotypic characteristics. But these ingredients take us no distance to establishing a criterion for distinguishing the two as racial groups. Curiously, in my view, Kitcher considers a similar type of case—that of an affluent class of landowners and a class of peasants deriving, respectively, from the Normans and the Saxons shortly after the Norman conquest of England—embracing the view that these classes could be races on his view. By contrast, I see this conclusion as a *reductio* of his approach to race.

A second problem lies in Kitcher's reliance on the notion of reproductive isolation. Since Kitcher points to problems with and limitations to the Mayrian biological species concept in using the notion of reproductive isolation to delineate species in both of his essays defending pluralism over monism about species concepts, it is a little surprising to see him adopt and adapt this notion so uncritically here. An additional puzzle is why Kitcher defends a monistic view of a sub-specific Linnaean rank, race, when he is a pluralist about the rank of species itself. (One wonders what a nice, pluralistic guy like Phil is doing in a dark, monistic place like this.) But the chief problems here are intrinsic to the view of race itself. Since reproductive isolation between two groups need not be absolute or universal across all possible circumstances in order to sustain the phenotypic differences between them, whether they be species or pure races, Kitcher thinks that the right way to understand reproductive isolation is in terms of stemmed gene flow and low mating probabilities between racial groups. Suppose that we grant this, that groups A and B are reproductively isolated in this sense, and that A and B are distinguished principally in terms of phenotypic characters such as skin color. Could A and B be pure races?

Possibly, but they could also be *sub-racial* groups, or kin-groups, or any other number of sub-specific groupings in either the Linnaean hierarchy or our commonsense taxonomy of the social and biological worlds. There are various levels of reproductive isolation between each of these groups, and higher levels do not always correspond to the larger of these groups. Once one loosens the criterion of reproductive isolation so that it applies to categories narrower than that of the species, then we are in a world where there is more or less gene flow amongst many sub-specific groups. It is difficult to see what could objectively pick out just some of these as racial groups, even if we help ourselves to that subset of phenotypic characters usually associated with race. We could insist on a certain level at or range over which to define the stemmed gene flow appropriate for pure races. But if anything this makes the reliance on our own subjective judgments for what groups count as racial groups all the more striking.

A third problem may be intrinsic to any position that, like Kitcher's, attempts to occupy a position that does justice to both "biological" and "constructivist" views of race. Kitcher distances himself from racist forms of the former views, and from eliminativist forms of the latter (since he articulates a conceptual

framework that purports to provide a way of acknowledging the biological reality of races). The chief reflection of constructivist views in Kitcher's account is in his endorsement of the claim that racial divisions have social causes.

One might doubt whether this makes enough of a concession to constructivist views. As Kitcher's quotations from Charles Davenport's "The Mingling of Races" remind us, many different groups of people have been regarded as being of different races in not just the distant but the recent past, and these patterns of self- and other-identity have functioned to reify existing and perceived group differences. One might aim, as does Kitcher, to at least capture the current "big three" racial groupings through a combination of conceptual analysis and empirical data. But those categories themselves, and the data collected about them, involve irreducibly subjective and culturally variable judgments.

For example, the Census data on black-white intermarriage that Kitcher uses involves self-identification in terms of a set of local rules for racial membership, and the taxonomic schemes employed by both governments and researchers typically use categories that blend together historical, biological, cultural, and ethnic categories. Moreover, as immigration policies shift, laws change, and adjustments are made in the economic realities of the lives of people with differently colored skin, not only do these categories shift, but so too do the patterns of approximate reproductive isolation that Kitcher proposes as the bases for racial categories. The problem is that there is little reason to think that these shifts are tightly correlated in the way in which they must be for Kitcher's view to be defensible.

Consider the table that Kitcher uses to illustrate some of the complexities in interracial marriage patterns in Hawaii, reproduced as Table 1 below.

Bride's ancestry	Groom's ancestry					
	Ca	Ha	Ch	Fi	Ja	Ot
Caucasian	517	230	36	86	79	52
Hawaiian and part-Hawaiian	177	515	20	121	94	72
Chinese	138	163	311	41	296	51
Filipino	114	159	26	584	69	48
Japanese	56	70	59	30	761	25
Other	201	18.5	0.21	69	127	397

The high numbers in the left to right diagonal are evidence for a preference for intra-cultural marriage. But this table also indicates no significant restrictions in gene flow between Caucasians and Asians, assuming that at least those listed as Chinese, Filipino, and Japanese are Asians. Kitcher notes that "it is unlikely that there will be a division between Asians and Caucasians that will hold across the

United States (although there might be more local divisions of this kind)" (p.244). Yet surely Figure 1 itself shows that Kitcher's concept of race does not distinguish Asians from Caucasians period, independent of what happens in other local populations. There are certainly geographically isolated communities that have distinctive phenotypic features and a high level of reproductive isolation. The reproductive isolation in such small groups is irrelevant to a concept of race, unless we entertain the hypothesis that such groups are themselves good candidates for racial groups. Taking that option seriously, however, would take us from a "big three +" view of race down the path to eliminativism, since there would be hundreds or thousands of such "racial groups".

Just as the specter of eugenics lies in the background of the topic of race, so too is it invoked by more than the title of Kitcher's "Utopian Eugenics and Social Inequality". Here Kitcher develops views that he introduced in *The Lives to Come*, beginning with the idea that since, in an age in which our genetic innocence is lost, it is impossible not to engage in eugenic decisions, the real questions concern the forms that eugenics should take. Kitcher structures his discussion around the views of "idealists", such as Richard Lewontin and Ruth Hubbard, who view reflection on genetic advances as providing a basis for the justification of broader social changes, and "pragmatists", who suppose that we can engage in what Kitcher calls *utopian eugenics* without introducing more wide-ranging and radical social changes. The rich discussion here is a fruitful blend of Kitcher's own pragmatism about the genetic age and his sensitivity to existing social injustices. These injustices are often ignored in favor of techno-hyped solutions to many similar problems raised in novel-sounding ways by advances in biological research, something that Kitcher argues is difficult to justify. I focus on just two aspects of Kitcher's views here.

The first is the repeated reminder not to fetishize distributive questions that arise in a genetic context. As Kitcher says, our "lives are the products of many lotteries, and only one of them shuffles and distributes pieces of DNA" (p.265). The distinction between remediable and irremediable deficiencies arises where there are both genetically-based and socially-based inequalities, and it is often sobering to consider attitudes toward the redistribution of resources in one of these contexts in light of the other.

The second is Kitcher's leveled response to some of the more extreme claims of critics of the Human Genome Project. He views these as the squawks of a Chicken Little inciting panic and skepticism. Kitcher takes genes as to have rightfully earned their central place in the biological sciences, and he sees little reason to think that there is more than the mere possibility of genetic technologies being misused. Kitcher expresses a conviction in the advance of scientific knowledge and technology, and the social uses to which they can be put given liberal and humane social policies, not only in this essay, but throughout much of his work. It is a conviction that, in many ways, is his trademark.

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