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Metaphysical Disorder and Scientific Disunity

Advocates of the disunity of science do not commonly hold this position for metaphysical reasons. One reason for this is that for those skeptical about traditional conceptions of unified science, the grand systems of traditional metaphysics are likely to seem even more dubious. More simply, recent doubts about the unity of science have developed from rather different directions. Such doubts have especially emerged from the recent tendency in science studies to seek a sharper focus on the details of science than has been customary at least in earlier philosophy of science. Thus on the one hand, to historians and sociologists looking in increasing detail at the fine grain of scientific practice, the contingency and specificity of particular projects of inquiry have made the idea of science as one grand project incredible. And on the other hand, epistemologists concerned with the claims to knowledge of particular branches of science have not easily fitted these local modes of justification into broad patterns with universal applicability.

In this essay, however, I want to argue that the picture of science as radically fractured and disunified has a role for metaphysics, and moreover that an appropriate set of metaphysical views is entirely plausible.[1] It is perhaps worth noting that the strongest antipathy to metaphysics is associated with classical positivism, which is also the source for canonical accounts of the unity of science. At any rate, the utility for a defender of the disunity of science of a metaphysics at least compatible with, and perhaps even justificatory of, that position seems to me indisputable.

One central reason why advocates of disunity should care about metaphysics is that, as I shall indicate in the course of this discussion, there is a range of metaphysical positions that both remain attractive to contemporary philosophers and argue strongly for a unified science. It is, of course, impossible for any such philosophical thesis to contradict an empirical demonstration—a demonstration derived, that is to say, from the investigation of the actual practice of science—that science is at this time in a state of radical disunity. But the deeper question is whether science is disunified simply because it has not yet been unified, or rather because disunity is its inevitable and appropriate condition. Historical or sociological investigations might indeed motivate or suggest arguments on one or the other side of this question. If the principles, methods, forms of argument, and everyday practice of different sciences, and even the same sciences at different times, prove to be radically diverse, this will certainly present difficulties for the believer in scientific unity. Nevertheless, no amount of such evidence can rule out the possibility that this diversity reveals only the immaturity of most of science. If one is interested in whether disunity is an inescapable attribute of science, one must attempt some more abstract philosophy.

Reductionism

The philosophical position most generally associated with the unity of science is reductionism. This is, in the first place, an epistemological rather than a metaphysical position. The kind of reductionism I have in mind, at any rate, is a view about the nature of scientific theories, that they must aim to explain the behavior of complex objects in terms of the behaviors of their constituents. This is associated with scientific unity because it mandates strong theoretical links between each science and the science that investigates the objects that are the structural constituents of the objects of the first science. Thus all sciences are linked in a vertical, perhaps branching, structure. In its stronger versions, theories of objects at the higher levels are shown by the process of reduction to be redundant, and thus unity is established in the very strong sense of uniqueness: in the end the only theory we need is the theory that describes the behavior of the smallest objects that there are (if, indeed, there are any smallest objects).

Although, as I have said, this kind of reductionism is primarily an epistemological thesis, it is laden with metaphysical presuppositions. Most significant among these is the hierarchical ontology into which everything—or at least everything amenable to scientific investigation—is to be confined. The world is seen as composed of objects belonging to one of a sequence of levels: elementary particles, atoms, molecules, and so on. The phrase "and so on" conceals the fact that I do not really know how to continue the series. *One* continuation, though surely not the only one, might run through biology, which is itself often supposed to have such a hierarchical structure. Thus, perhaps: cells, organs, multicellular organisms, ecological communities. But though these hierarchical classifications make a point, they are vastly oversimplified. Even the first stretch is not without its difficulties. Elementary particles have become an increasingly heterogeneous bunch since the good old days of electrons, protons, and neutrons, and some of them seem to fail to be elementary by being composed of simpler entities, quarks. The sense of "composed," however, is obscure, as quarks are generally said not to exist other than in such "compositions."

But even if the picture of hierarchical levels can be sustained at these low levels, this amounts to little more than the (admittedly important) discovery that the world is composed of a relatively homogeneous set of structural constituents.[2] At the biological level such a division into discrete levels is much more clearly artificial. The fundamental unit for those areas of biology that deal with interactions between organisms is, obviously enough, the individual organism. But individual organisms span the entire hierarchy of structural complexity, from viruses, simpler than most cells, to the most complex multicellular organisms, such as apes, elephants, and octopuses.[3] And the structural constituents of a complex organism are not remotely homogeneous with respect to their positions on such a hierarchy. While there are certainly parts of an elephant that can be neatly differentiated as organs (heart, liver, brain, etc.), there are also fluids (blood, lymph), and chemical species from the very large (hormones, hemoglobin, neurotransmitters, etc.) to the very small (various essential ions). In summary, though one can roughly *classify* objects in terms of their structural complexity, such classifications need not correspond to the basic individuals of any particular area of investigation, nor do they identify the constituents from which more complex objects are uniquely composed.

The ontological hierarchy just discussed provides a useful way of raising a rather different question. I have argued that the organization of objects into such a hierarchy is not something forced on us by the way the world is. But that does not mean that there might not be reasons for looking at the world in these terms for purposes of some scientific investigation. The question thus raised is whether scientific fields must be determined simply by recognition of the different areas of phenomena that there are, or whether there might not be a much wider range of more pragmatic reasons for distinguishing areas of scientific investigation. In the latter case, it might seem quite plausible that physical size itself could provide an abstraction suitable for defining a scientific field. And at least for the extremes of size this does indeed seem to be the case. Anything below a certain size is an object of study for particle physics, regardless of whether that field is ever likely to come up with a unified theory, or even identify a set of smallest existing objects. And anything larger than a certain size belongs in the domain of astrophysics. Things close to our own size, on the other hand, especially if they resemble us in other ways, get much more detailed attention, and an abstraction as crude as gross physical size has little relevance to an area such as biology. And this anthropocentric focus on things of about our own size would remain true, I think, even if we were to find out (somehow) that there were little people living on electrons, or that our galaxy was a tiny part of the fingernail of some gigantic being.

Natural Kinds

The preceding discussion raises the question whether, or in what sense, the kinds of things investigated by science exist independent of and antecedent to such investigation. By that I do not mean to raise a doubt as to whether the individual things exist, but only as to whether it is an objective fact that they belong to those particular kinds. Answers to this question have tended to take extreme dichotomous forms. At one extreme it is held that for every individual thing of interest to science there is an objective answer to the question what kind of thing it is. Such a view will typically require that there be some feature of a thing, its essential property, that will determine unequivocally what this kind is. In the tradition of Western science such essential properties have generally been assumed to be structural properties; thus this picture of natural kinds determined by structural essences dovetails perfectly with the ontological hierarchy presented in the preceding section. At the other extreme are various nominalistic theses that deny any objective reality to anything but the individuals themselves.

I would like to propose a view distinct from either of these. I suggest that many individual things are objectively members of many individual kinds. Thus I, for example, am a human, a primate, a male, a philosophy professor, and many other things. All, or at least many, of these are perfectly real kinds; but none of them is *the* kind to which I belong. Since I deny that any of these kinds is privileged over the others, I must, of course, deny that I have any essential property that determines what kind I *really* belong to. This I am happy to do. So far I do not take the kind of pluralism here adumbrated to be particularly novel. However, it is my impression that most theorists who advocate such pluralistic positions think that the admission of equal status to so many kinds must amount to denial of any real status to any. But I see no reason why many overlapping and intersecting kinds might not be equally and genuinely real. This would preclude the general possibility of answering one kind of question to which a theory of kinds has traditionally seemed relevant, questions as to what (unique) kind a particular individual belongs to. But I see no reason why there should be any answers to such questions, any more than there need be an answer to the question what color something is (think of rainbows or peacocks). Indeed, in the special case of the classification of people it is very important to deny that there are any answers to such questions.

This combination of pluralism and realism, what I have sometimes referred to as promiscuous realism, provides the starting point for seeing the robust metaphysical basis that I suggest underlies disunified science. For if there are numerous distinct ways of classifying objects into real kinds, any one of which schemes of classification could provide the basis for a properly grounded project of scientific inquiry, then there can be no reason to expect a convergence of these projects of inquiry onto one grand theoretical system. The question that should attract one's attention is rather what grounds justify pursuing one particular project of inquiry rather than any of the many possible alternatives. The general form of the answer to such a question seems clear: we should select that project that best serves the goals that motivate our inquiry or, at any rate, whatever other goals may be potentially served by such a project. A vast body of contemporary work in the history, philosophy, and sociology of science has shown how fruitful for the explanation of scientific activity and belief investigations of the goals and motives of scientists can be. Where I differ from many of the exponents of such research is that while such an approach does presuppose that the direction of (proper) scientific research is not simply dictated by the way things are, I do not take this as contradicting the claim that good scientific research can, nevertheless, describe the way things objectively are. It is just that a particular scientific project can describe only one of the many ways things are. I shall conclude this section by mentioning one reason why I take it to be of great importance to distinguish my position from the purely skeptical interpretation of recent research into the interests guiding and motivating science. The idea that motivation is *all* that can be investigated in looking at a contemporary or historical scientific research project necessarily treats all such projects as on an equal footing. Indeed this is a fundamental methodological precept of the sociology of knowledge movement.[4] But the conjunction of the ideas that scientific research can be explained in terms of the motivations of its instigators, but also in terms of the objective reality that it discloses, also opens up the possibility that some research that can be explained in the first way cannot be explained in the second. There can, that is to say, be both good science and bad science. Since it seems to me very clear that both these categories are well represented in the past and the present, I take this conclusion to be a very important one.

Supervenience and Causal Completeness

Reductionism as a comprehensive account of the aims of science, a program of explaining phenomena at every level in terms of the properties of their constituents, is no longer widely defended. Without going into much detail about the reasons for this, I think it is safe to generalize that the difficulties with reductionism are seen as, in some sense, merely practical. For example, it is widely perceived that macroscopic phenomena are usually, from a microscopic point of view, much too complex for there to be any hope of providing tractable analyses. To make sense of macroscopic phenomena we must categorize them in terms that are, from the microscopic point of view, radically heterogeneous. In sciences such as biology, sociology, or economics nobody seriously supposes that the relevant classificatory kinds will be microphysically homogeneous. And therefore the laws of these sciences will be untranslatable into the terminology of microphysics.

That these problems are widely perceived as "merely practical" is revealed, I think, in the proliferation of supervenience theses. Such theses are often intended to indicate precisely the failure of reductionism, as for example Davidson's well-known version in the philosophy of mind.[5] But I think it is clear that supervenience inherits the metaphysical spirit of reductionism.[6] According to supervenience theses, the microscopic determines the macroscopic, at least in the sense of providing a sufficient condition for any macroscopic property. Thus if this dependency is not to be wholly mysterious, there is presumably some set of facts that *could* be known that would permit the inference of the macroscopic from a sufficient knowledge of the microscopic. Perhaps we could not, even in principle, know these facts. But God, I suppose, would need merely to exist in order to know them.

To explain in more detail how supervenience preserves the metaphysical spirit of reductionism, I must elaborate a little on the account I have so far offered of supervenience. So far I have described supervenience in a merely instantaneous sense. That is, the macroscopic properties of a thing at time t are said to be dependent on its microscopic properties at t. But it is also widely supposed by devotees of the microphysical that the microscopic properties of a thing at time $t + 1$ are dependent on the microscopic properties at t. Then since the macroscopic properties at $t + 1$ are dependent on the microscopic properties at $t + 1$, the former must depend ultimately on the microscopic properties at t.[7] Of course, to the extent that this sounds like a statement of determinism, it is almost entirely discredited at the microphysical level. However, even within an indeterministic theory, it is possible to preserve the idea that a complete causal story can be told relating the situation at t to the situation at $t + 1$. Indeterministically, this would specify only a probability distribution over possible states at $t + 1$ as a function of the state at t. This is a complete story, in the sense that nothing other than the state at t is relevant to what happens at $t + 1$. And the way that the state at t influences that at $t + 1$ is fully and quantitatively determinate. This is what I mean by the assumption of causal completeness, an assumption of which determinism is merely an extreme limiting case.

Such dependence on antecedent microphysical states leads to a problem. For much of our scientific and everyday belief consists of more or less firmly entrenched hypotheses about the causal relations between macroscopic properties of things and events. But if the macroscopic state of a thing at a certain time is dependent on its immediately preceding microscopic state, then there is an obvious problem of reconciling such truths with the causal knowledge that we take ourselves to have at the macroscopic level. It appears that our macroscopic causal beliefs can be true, or even approximately true, only to the extent that they somehow shadow the underlying microphysical processes. It may be true, for example, that my intending to hit a certain key on my word processor causes me to hit that key. (Or more likely, some nearby key.) It seems to follow from this that my intention to hit the key causes, in addition, the movement in the direction of the keyboard of a particular electron in my fingernail. But the idea of microphysical causal completeness implies that the causes of events at the microphysical level are fully specifiable at the microlevel. So the causal efficacy of my intention in moving the electron had better be *consistent* at least with such microlevel causal facts. The consistency of the macroscopic causal statement with all the billions of such microlevel causal processes appears to require either in-principle reducibility, or divinely preordained harmony. At any rate, the causal completeness of the microphysical *directly* contradicts the supposition that my intention could be *necessary* for the movement of the electron, except insofar as it is itself a necessary consequence of events at the microlevel. So even the supervenientist's denial of (in-principle) reductionism seems to leave the macroscopic realm causally inert. Thus it is natural to take supervenience to involve at least God's-eye reductionism.[8]

Perhaps in an unfashionably positivistic vein, I am suspicious of the assertion of facts that God alone could know. Since I suspect that God lacks that notorious perfection, existence, the facts alleged by the supervenientist have the peculiar property of being, if, as is generally supposed, unknowable by us, unknowable. Nevertheless having presented some widely held views that appear to assume the existence of such facts, I cannot deny them without some suggestion as to which of these views I propose to reject.

The argument for the causal imperialism of the microphysical involves two premises that might reasonably be questioned: instantaneous supervenience, and microphysical causal completeness. While instantaneous supervenience does violate my mildly positivistic intuitions about unknowable facts, it may seem to follow from a metaphysical view with which I am sympathetic, the nonexistence of immaterial things. If a thing is exhaustively composed of the particles of microphysics, it may be plausible that its properties must ultimately depend, in the sense of instantaneous supervenience, on the properties and arrangement of those particles. As a matter of fact, I think this is less plausible than it seems at first sight. The intuition on which it is grounded must surely depend on two further assumptions, that it is possible sharply to distinguish the intrinsic from the relational properties of a thing, and that the relational properties of things can be reduced to intrinsic properties of the things related. For surely the relational properties of a thing cannot supervene on the microphysical properties of that thing alone. If these additional assumptions are rejected, then we will be driven to a merely global supervenience, in which the total state of the universe is held to supervene on its microphysical state. And this would surely be a thesis of such blinding epistemological vacuity as to add nothing to the thesis of the nonexistence of the immaterial.

For present purposes, however, I am more concerned with the second premise, the causal completeness of the microphysical, and I will focus on causal completeness for the remainder of this section of the essay. Why should anyone believe that microphysics describes a realm of entities about which complete causal stories can be told? Certainly such a belief is not derived directly from scientific success, since it is admitted that these stories rapidly get too complicated to tell when, for instance, the number of characters exceeds about two. Presumably it is the impressive precision with which we can tell very short stories about very few characters that encourages us to conclude that these must be part of a larger story that itself is precise and causally complete. On the other hand, I can predict very precisely what will happen if I put a hungry fox in a small enclosure with a rabbit. Nobody would be inclined to take this as showing that there must be precise laws governing the ecology of the Amazon rain forest. I suppose that moving from the kinds of experiments performed by particle physicists to, say, the microphysics of a human brain would be a considerably larger shift in complexity.

There is one fashionable move that might seem to dispose immediately of an important part of the thesis of causal completeness. This is the combination of indeterminacy (say quantum indeterminacy) with the ideas developed in chaos theory. Chaos theory has emphasized the existence of mathematical functions the evolution of which is indefinitely sensitive to the initial values of parameters (though sometimes curious or wonderfully intricate patterns emerge from such functions). If such functions should best reflect aspects of physical reality—a possibility, incidentally, discussed by Duhem—then even within a fully deterministic system, prediction could present conceptual problems even for God. If, considering quantum mechanics, initial parameter values should be indeterministic, God's problem would, if possible, be exacerbated.

Though I think this is a scenario that should give serious pause to supporters of even the weakest reductionist theses, I shall not attempt to pursue it here. For it must be noted that this possibility not only lacks obvious relevance to the question of instantaneous supervenience, but also does not throw doubt on causal completeness. Indeed, the functions studied in chaos theory are typically deterministic. It does not, therefore, reveal any way of circumventing the preceding arguments about supervenience, or, therefore, of affirming the genuine causal efficacy of the macroscopic. It is, in effect, merely an extreme way of proposing *practical* obstacles to reductionism. It is, indeed, part of my thesis that there are likely to be parts of nature that are not susceptible to systematic analysis in the canonical style of science, and the existence of natural chaotic systems would concretely illustrate one form of this possibility. But for the reasons just indicated I shall not rest my argument on this possibility, but will focus instead on causal completeness.

Microphysical causal completeness, to repeat, is the idea that at least in a hypothetical divine mind there is a complete causal truth to be told about the influences acting on microphysical objects. Since this is not strictly an empirically grounded belief—nobody, for example, has tried to investigate the forces acting on an electron in my, or anybody else's, fingernail—it is perhaps most plausibly diagnosed as deriving from ideas we have about macroscopic causality. The most plausible paradigms here are the macrophysical sciences such as thermodynamics or mechanics; or perhaps the structural accounts of complex functional objects, as in physiology.

An obvious feature of such domains is that they seem amenable to analysis in terms of very general laws (Newton's laws, or Maxwell's equations, for example). One point about such laws is that their generality and abstractness by no means imply that they ever provide causally complete accounts of concrete situations. As Nancy Cartwright has emphasized, by virtue of their simple form and exclusion of many factors known to be potentially relevant, without an open-ended *ceteris absentibus* clause, such laws are typically, if not always, strictly false.[9] The move to causal completeness is rather attempted in their application to the complex task of constructing gadgets or experimental setups. And we all know that this attempt is arduous, and never fully successful. (Our cars sometimes break down, for instance.) But more important than the correct analysis of these technical-scientific projects is the question whether they provide an appropriate model for very different projects of inquiry. My thesis will be that, on the contrary, they can be highly misleading.

Parallel to the success of applied science in guiding the construction of gadgets is the attempt to explain the structural basis of the properties of complex, organized objects, notably in physiological accounts of the properties of biological organisms. Success in such investigations may also be of central importance in inspiring confidence in the unlimited potential of science, and thereby in the assumption of causal completeness. An important feature of such investigations is that the *explanandum* provides us a powerful criterion for distinguishing those events in which we are interested. For example, we know that organisms are very good at respiration, and we seek out those structural elements that make this possible. In contrast, for many classes of phenomena, those typical of evolutionary biology, economics, sociology, or meteorology, for example, no such built-in teleology is available. Part of the investigation is precisely to decide what, if anything, such systems do. Our goal may remain that of seeing how interactions at one level (people performing economic exchanges, organisms consuming one another, etc.) produce global results. In the absence of a clearly defined relevant set of effects, the assumption of causal completeness is considerably less compelling.

We need a rather different picture of how we should study interactions of this latter kind, and here again I think physics provides us with an unhelpful paradigm. The most famous paradigm of causal interaction is provided by mechanics, the collision of billiard balls. And statistical mechanics gives us a model for deriving macroscopic properties from the aggregation of similar interactions. I think certain features of this paradigm can be seen to dominate scientific investigations of the upshots of multiple interactions, and I believe that this influence is often pernicious.

There is no doubt that when dealing with enormously complex phenomena (societies, ecological communities, etc.) we will get nowhere without radical strategies for simplification. Mechanics works by giving very simple characterizations of the interacting entities (mass, velocity, position, etc.) and providing general laws for the outcomes of interactions of entities described in terms of such parameters. Just such a procedure well describes typical methodology in much of evolutionary biology and economics (not to mention the rest of the social sciences insofar as they are threatened with cannibalization by these disciplines). In major parts of economics, for example, agents are characterized by income, indifference curves, and a crude sort of instrumental rationality, and a general account of the nature of exchanges between such agents is offered. Aggregated models of these interactions offer accounts of larger-scale economic processes. In evolutionary theory we have selection coefficients, reproductive rates, and so forth. Characterizing interacting populations as homogeneous except in respect of the values of a few key parameters is certainly a powerful form of simplification, and one that sometimes works well. A metaphysical danger it tends to carry with it is that the formal relations between these abstract entities, as displayed in the formal models of economics or population biology, tend to look like universal laws, failing to be true only because of the irritating intervention of further causal influences too numerous to be conveniently included in the model. Thus such models may suggest that we have the beginnings of a causally complete account, denied us in full detail only because of practical obstacles.

My central point, though it is an admittedly controversial one that cannot be adequately defended here, is that nothing in the practice of this kind of scientific methodology either presupposes or implies that whatever regularities are found to correspond to these abstract models are the consequences of determinate propensities characterizing the particular events that constitute such regularities. It is true that the belief in such propensities would encourage optimism in the search for regularities. But first, even a guarantee that there were such probabilities would not guarantee that regularities were empirically accessible; the number of factors relevant to the strength of the propensity might prevent any intelligible regularities from emerging. Second, such optimism may be misplaced. The empirical success of abstract modeling in science has been, at best, modest. The assumption that it is an appropriate approach to any domain for which it might be feasible would be grossly premature. And third, even where empirical regularities of the right sort can be found, this in no way requires that they be grounded in underlying single-case propensities.

This last point can usefully be illustrated by looking briefly at a topic about which there has been a great deal of investigation of statistical regularities, though without much effort to construct elaborate theoretical models, the game of baseball. The performances of baseball players are subject to analysis in terms of a battery of statistical measures, the most familiar being the batting average and the pitcher's earned-run average. There are serious and well-known limitations to such statistics as measures of the skills of players. Batting averages, for example, even ignoring their obvious failure to measure various batting skills (power, knowledge of the strike zone), are sensitive to the degree of threat presented by following batters, the frequency with which preceding batters are on first base (taking the first baseman out of optimal fielding position), and various features of the home ballpark in which much of the average is compiled. Equally clearly, they convey a good deal of useful, if fallible, information. Over a number of years earned-run average will very reliably distinguish an outstanding pitcher from a marginal pitcher. The same handful of batters average over .300, or drive in 90 runs, with considerable consistency.

But whereas with sufficient time such statistics can give a good idea of the capacities of baseball players, this possibility does not depend in any way on the assumption that the particular events codified by such statistics are subject to any fully determinate and completely specifiable causal influence. Evidence in favor of causal completeness here is precluded by the fact that the number of potentially relevant factors is so large as to reduce each case to causal uniqueness. This is somewhat amusingly illustrated by the frequent production by baseball commentators of facts such as that a certain hitter is batting .750 against a certain pitcher during day games, that is, 3 for 4. There are many statistical patterns, from the larger-scale patterns concerning batters in general or batters of a certain kind to those concerning particular batters in particular situations. But as we move from the smaller-scale patterns to specific events, there is no finest-grained general pattern to be found. We do not aspire to the complete causal story; we move from general knowledge to the specificity of historical narrative. This uniqueness *might* just present an epistemological problem; but I can see no reason why it should not equally well reflect the metaphysical fact that the regularities in question emerge only over time.

There remains the last-ditch defense of causal completeness, the appeal to a God's-eye story in terms of the instantaneous physiological state of the pitcher and the hitter, the air density and movements between the two, and so on. On this I have just two comments. First, such a story plays no part in our everyday understanding of statistics and is in no way presupposed by such understanding. And second, the appeal to such a story depends on the microphysical reductionism that, I have claimed, we have excellent independent reasons for rejecting.

I suggested earlier that our belief in causal completeness could be grounded only in our understanding or experience of macrophysical phenomena. But I have just been arguing that at least for a wide range of interesting events, this conception has no useful role to play. I suggest, in fact, that we have no good reason to believe in causal completeness as characterizing either the microphysical or the macrophysical. One conclusion from this is that the consequences of supervenience I discussed do not follow. We are relieved of the threatened tyranny of the microphysical. If there is no complete and all-encompassing causal nexus determining the movements of microphysical particles, it is even possible that my intention to hit a key really does cause the movements of the electrons in my fingernail.

The most general conclusion I want to draw from the preceding discussion is the following. Rejecting all forms of reductionism, and rejecting the assumption of a complete causal nexus, leaves it entirely open how much order there may prove to be in the world. There may be many kinds of phenomena in which no interesting patterns can be found; and even when there are patterns, it remains an open question how pervasive they may be. This leads naturally to the question how we evaluate various scientific projects, and thereby leads me to the final section of this essay.

The Unity of Scientism

In this final section I shall say something about why it matters whether scientific disunity is inescapable. The central answer to this question is that the political power of science rests in considerable part on the assumption that it is a unified whole. Thus "scientific" has become an honorific applying to anything that satisfies even the thinnest sociological criteria of being a part of science. If science is instead portrayed as a miscellaneous assortment of diverse investigations with only loose relations and interconnections, then particular appeals to the authority of science must stand on their own merits. This is a major step toward increasing the social accountability of scientific claims.

The semiserious title of this section suggests two distinct though related claims. First, I want to claim that the belief in a unified project of science itself helps to support a number of projects that I wish to characterize, in a broadly derogatory way, as scientistic. Second, I think that these scientistic projects involve rather typical though misplaced assumptions about what constitutes a properly scientific approach to a domain of inquiry; thus these characteristics might semifacetiously be said to constitute a genuinely unified project of scientism. Due to limitations of space, this part of the discussion must remain programmatic. I shall outline four main points that I take to be major practical consequences of the metaphysical views just discussed.

1. A belief in the unity of science tends to distribute the epistemic credentials earned by genuinely successful scientific inquiries across the entire range of practices that satisfy merely sociological criteria of scientificity. This is particularly unfortunate in tending to legitimate resistance to powerful contemporary critiques of particular areas of scientific theory. I think especially of recent feminist critiques of substantial parts of evolutionary theory, economics, psychology, and other sciences.[10] It sometimes seems as if, to be taken seriously, such critiques must demonstrate the ideological biases in, say, quantum mechanics. Scientific disunity entails that adequate defenses of particular scientific theories must be local and specific.

2. The thesis of disorder gives no reason to suppose that there is one correct way of categorizing and describing a particular domain. There may rather be a number of possible descriptions, any of which could reveal limited degrees of order and intelligibility. The necessity of providing a criterion for choosing between such possible descriptions suggests a deep sense in which values can become embedded in scientific projects. For example, the expression "positive [i.e., nonnormative] economics" might well be considered close to an oxymoron. Income, growth, and the like are not objectively inescapable characteristics of economic systems, but represent particular choices of how to attempt to describe such systems and, implicitly, to evaluate their success. If we were to change our views as to the goals of economic systems, a description in such terms could be seen as largely irrelevant.

3. I have not discussed in this essay versions of the unity of science that are purely methodological. On the whole, apart from rather vague demands for empirical accountability, such claims have been rendered extremely problematic by recent work in the history of science. However, such theses survive in often inchoate and unarticulated forms, perhaps deriving some of their credibility from a process parallel to, or even parasitic upon, that described in (1) above. One that seems at least implicitly to be widely accepted is the idea that scientific credibility is largely contingent on the extent to which claims are expressed in a quantitative, mathematical form. This, for example, underlies the rather bizarre impression

that economics is the most "scientific" of the social sciences, and also the attempts of game theorists, optimality theorists, rational-choice theorists, and suchlike to colonize the remainder of the social sciences. I say "bizarre" because much of mathematical economics, as also various other curious mathematical practices such as formal population genetics, has done little even to meet what I referred to as the "vague demand for empirical accountability." One may well suspect that these uses of mathematics have more to do with providing barriers to entry to lucrative professions than with illuminating the natural world. The fetishistic reverence for formal methods, finally, is not merely a harmless academic foible. One might argue that the growing throng of homeless people on the streets of the United States are partly indebted for their plight to the mathematical diversions of influential economists.

4. Finally, and going one step beyond the point made in (2) above, a metaphysics of disorder implies that there is no presumption that there is *any* analysis of a particular domain in the canonical style of science. While it may always be possible to provide some illumination of the small-scale events that provide the substance of the interesting processes in a domain of scientific inquiry, there can be no *a priori* answer to the question at what point there will be convergence onto pure historical contingency. One important area to which this issue is highly relevant is the case of evolutionary theory. While there is certainly some demonstrated scope for understanding particular microevolutionary processes, generalizations about the overall patterns of macroevolution might be few and far between. Similar, and perhaps more urgent, questions arise concerning the point at which social science ends and human history begins.

As I have indicated, these concluding remarks are highly programmatic. Each of my points requires much more defense and elaboration. I hope, however, that they are sufficient to show the potential for substantial practical consequences, and for the motivation of various important critical projects in the philosophy of science, provided by what might at first sight seem a rather abstract set of metaphysical issues.