

Ontological Progress in Science

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Introduction

In the grand tradition, philosophical ontology was considered logically and epistemologically prior to scientific ontology. Like many contemporary philosophers of science, we consider this a mistake. There is a two-way traffic between philosophical and scientific ontologies; the more we learn about what there actually is, the more we learn about what can be and what must be, and vice versa. But that is not the present topic; here we focus on the ontology or ontologies of science, not traditional philosophical ontologies or the traffic between scientific and philosophical ontologies.

As will soon be obvious, the problem of ontological progress in science, thus restricted, still offers plenty to hold us. We will address the following four theses, which we will support with some fragmentary arguments:

1. The ontology of science is intensely compositional and hierarchical.
2. Although much of science is reductionistic, the reductionism in question is generally not eliminative.
3. Current philosophers' treatments of the ontology (or ontologies) of science are far too intimately tied to the latest or the 'best' theory. Finally,
4. A central form of ontological progress in science—we believe *the* central form—consists in obtaining significant contact with, and accurate characterizations of, entities, processes, properties, events, and states (henceforth, simply 'entities'), further and further removed from those that are perceptually available to us.

We shall utilize this framework of theses to suggest some preliminary responses to the sceptical arguments of such philosophers as Larry Laudan and Bas van Fraassen against the idea that there is ontological progress in science. There is not space on this occasion to attempt a proper rebuttal of the arguments they employ to undercut scientific realism, but the general line that we would pursue in constructing such rebuttals will become clear in the course of this paper.

We have been greatly influenced by many recent writings attacking and defending scientific realism¹ and by discussions with many friends concerning this topic.² However, in order to carry out the constructive program of the paper we shall make but scant reference to the recent literature or the views of others. On occasion we will indicate certain of the differences between the position here espoused and those put forward by some of the leading authorities who have published on this topic.

Scientific Ontologies

A plausible defense of scientific realism will be philosophically complex. Ultimately, the best evidence for scientific realism depends on the results of some *collection* of secure, well-developed theories. At the same time, scientific realism ought not be tied too tightly to particular scientific theories, especially speculative ones at the pioneering frontiers of research. One reason for this is that scientific realism, at least as we conceive it, is less closely allied to familiar styles of philosophical or "formal" ontology than is usually recognized. Most scientists' answers to questions like "What kinds of things have been discovered by your science?" or "What sorts of entities are required by the best theories in your field?" do not have the sort of (intended) ultimacy that characterizes the answers to questions in formal ontology.

A scientist often answers to such questions by speaking of some sorts of entities—e.g., of quarks, of electrons and protons, of different isotopes of the same chemical element, of DNA and RNA molecules, of membrane bound and soluble proteins, of the Golgi apparatus and ribosomes, of tectonic plates, of dynamic equilibria, or of spiral galaxies, black holes, and quasars. Such answers usually are not (and certainly need not be) so closely tied to a particular account or theory of the structure or the workings of the entities in question as to rule out alternative theories of the same entities. Indeed, answers of this sort can be put forward without any commitment to what philosophers would consider an account of the 'ultimate ontological status' of the entities in question. (We mean to include here such matters as whether the entities are

¹. A representative sample of the literature may be found in the following books: Nancy Cartwright, *How the Laws of Physics Lie*, (Oxford and New York: Oxford University Press, 1983); Paul Churchland and Clifford Hooker (eds.), *Images of Science* (Chicago: The University of Chicago Press, 1985); Arthur Fine, *The Shaky Game* (Chicago: The University of Chicago Press, 1986); Bas van Fraassen, *The Scientific Image* (Oxford: Oxford University Press, 1980), and *Laws and Symmetry* (Oxford: Oxford University Press, 1989); Ronald Giere, *Explaining Science* (Chicago: The University of Chicago Press, 1988); Ian Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983); Larry Laudan, *Progress and its Problems* (Berkeley: University of California Press, 1977); Jarrett Leplin (ed.), *Scientific Realism* (Berkeley: University of California Press, 1984); Joseph Margolis, *Pragmatism Without Foundations* (Oxford: Basil Blackwell, 1986); and W. H. Newton-Smith, *The Rationality of Science* (London: Routledge and Kegan Paul, 1981).

². We include particularly Richard Boyd, Michael Krausz, Larry Laudan, Jarrett Leplin, Joe Margolis, Deborah Mayo, Alan Musgrave, Joe Pitt, Bob Richardson, and Nils Roll-Hansen as well as various members of the Piedmont Philosophy of Science Discussion Group. We are also grateful to discussants at seminars in the Departments of Philosophy of Virginia Polytechnic Institute and State University, the University of California, Davis, the University of Hawaii at Manoa, The Ohio State University, and the Universities of Tromsø, Trondheim, and Oslo, and to many others for constructive suggestions.

simple or complex, if complex whether they are 'mere aggregates' or essentially structured, how they fit into the relevant system of categories, and so on.)

It follows almost immediately that the ontological status of scientific entities cannot simply be read off a theory that countenances and describes them, not even when it is the latest and best such theory. For what the scientist is committed to (rightly or wrongly) is the existence and causal importance of entities that might well come to be located or individuated in ways not yet available. It might well turn out that the entities in question are seriously misdescribed by *all* currently available theories, e.g., that they belong to different formal ontological categories than current theories suggest.³ To the extent that this is correct, we should be mistrustful of philosophical attempts to analyze the ontological commitments of a scientific discipline (or of, say, the atomic theory of matter, the gene theory of heredity, or quantum electrodynamics) by means of an analysis of the (supposedly) best theories on the forefront of the relevant discipline.

In fact, there are at least three distinct reasons for distrusting analysis of the latest and best theories of science as the central tool by means of which to tease out its ontological commitments. The first is that theories themselves are embedded in a larger context. Evaluation of the claims implicit in theories often requires considerable knowledge of that context. Many theories, after all, are "models," "working theories," or "idealizations," not meant to be definitive. Often theories incorporate, in one way or another, idealizations and approximations that limit their application within narrow limits. For example, the treatment of a gas as a cloud of molecules is sometimes meant to serve as a useful model, as a means of making predictions or constructing tests of a theory, rather than as a literal description. In such cases serious mistakes often follow from reading ontological significance into an analysis of the theory in question, especially since (as the next two points make clear) contextual knowledge often contains independent information bearing on the evaluation of the ontological status of the entities in question. There may, for example, be experimental or observational means of securing reference or obtaining access to the entities or interactions of concern independent of that particular theory.

The second reason for caution is that the theory as a unit of analysis is (at least usually) too small. By this we mean not only that theories typically constitute attempts to explain or understand a domain of phenomena that is at least in part independently characterized and identified, but also that the very same science contains (or can properly come to contain) alternative theories that attempt significantly different accounts *of the same phenomena or entities*, perhaps even linking different subsets of the entities to different domains. To the extent that this is correct, the science has (evolving) ontological commitments not fully or decisively reflected in whichever 'best' theory one examines.

³. Thus for more than a decade, W. Bateson and T. H. Morgan differed about the ultimate structure of genes. Bateson thought that they were some sort of stable resonances that could not be located on chromosomes while Morgan thought them to be material particles with chromosomal locations. In disagreements of this character there is no way to be certain which party, if either, will prove to be right, though (of course) both cannot be. Nonetheless, the fundamental disagreements between these two theorists did not prevent them from securing thorough, experimentally anchored agreement about the referent of such terms as 'the gene for dwarfing in peas'. Such agreement could discriminate between distinct genes with similar or identical effects. For a few more details and some references, cf. Richard Burian, "On Conceptual Change in Biology: The Case of the Gene," in D. Depew and B. Weber (eds.), *Evolution at a Crossroads* (Cambridge, Ma.: MIT Press, 1985), pp. 21-42, esp. pp. 26 ff.

Finally, we often have access to the phenomena of a domain with which a theory is concerned that is (at least in good part) independent of that theory or of the differences between it and various alternative theories. At points where theoretical domains make contact, different theories may refer to the same entities—as when ions referred to in chemistry are also referred to in membrane biology, for example, to describe the process of ion transfer across cell membranes. It is for this reason that multiple independent confirmation and theoretical integration are such potent sources of evidence for scientific realism about entities. In such cases, the process of developing and testing alternative theories to explain the phenomena of the domain can sometimes take advantage of the avenues of independent access to resolve experimentally the disagreements that arise, including disagreements over the correct ontology and perhaps the very integrity of the domain in question. But this means that our analyses must cope with ontological disagreement *within a science* or *between sciences* and must account for our occasional ability to resolve such disagreements by use of experiments. We are sometimes in a position to resolve such questions as whether electrons "really" can be spatio-temporally localized within an arbitrary tolerance. If we peg the ontology of a science to its latest best theory, we cannot properly parse the issues in cases of this sort, nor will our conceptual apparatus allow us to perform a satisfactory analysis of the procedures that achieve resolution of such disputes.

It follows that the relationship between our theories and our assessments of the ontological status of the theoretical entities they purport to designate is far more complex than has been acknowledged in traditional versions of scientific realism, tied as they are to the analysis of well formulated versions of particular theories. In particular, the formal characterization of the relevant theoretical entities within a theory is not (and ought not be) the sole means of identifying, localizing, or characterizing the entities in question; otherwise it would be impossible to construct tests of claims about the principal features alleged by the theory to characterize those entities. The fact that successful tests of such claims are not just possible but actual, indicates that scientific realism about entities depends at least in some measure on our knowledge of true, non-accidental generalizations—in short, knowledge of laws—concerning those entities.⁴

Against Eliminative Reduction

Beyond the excessive theory-centrism of some versions of scientific realism, the current literature on this topic is flawed by the implicit commitment of many realist authors to some form of eliminative (micro)reductionism. At stake is the notion that the ontologically important entities discovered by science are physically smaller constituents of familiar or larger-scale objects and events. Whatever the importance for formal ontology of a successful mereological analysis of an entity or class of entities (i.e., an exhaustive analysis of those entities into a set or structure of "lower level" parts), a scientific analysis of an entity into its constituent parts does not and should not *by itself* count as the reduction of the "higher level" entity to those constituents.⁵

4. We mean 'law' in no stronger or more committed sense than that used here.

5. Proper development of this line of inquiry properly requires us to face a number of issues regarding the various modes of construction of wholes out of parts. The topic, however, is too large for the present occasion. Some useful preliminary steps have been taken by Robert C. Richardson in "Grades of Organization and the Units of Selection Controversy," in P. Asquith and T. Nickles (eds.), *PSA 1982*,

One reason for the insufficiency of a mereological analysis to eliminate "higher level" entities is that such analyses are (typically) contingent and substantive in character. That is, they claim that *independently identified* higher level entities are composed in thus-and-such a way out of thus-and-such lower level entities. Supposing such an analysis to be correct, it still might have turned out differently in much the way that the same phenotype can be produced by organisms with different genotypes. The results of mereological analysis are contingent, not analytic or conceptually necessary. The same claim can be made within an older terminology: the "bridge laws" required for reducing one theory to another are likewise contingent; at very best they are physically, not conceptually necessary—and we would argue from cases that they are very seldom necessary at all. The structure and composition of organisms (or of pieces of jade,⁶ or whatever) could be different than they are. Establishing structure and composition is a substantial scientific accomplishment, but not a philosophical one. By themselves, therefore, mereological analyses do not in any substantial sense eliminate higher level entities.

To drive this point home, notice that the issue of emergence is left open by mereological analyses even when those analyses are entirely correct. For present purposes the problem of emergence can be glossed in terms of the question whether the "higher level" entities make an independent causal contribution to the interactions of concern. A sufficient (though by no means necessary) condition for a class of higher level entities to be counted in the ontology of a science as of a given time is that they enter seriously into the causal hypotheses of that science and that their contribution to the relevant processes or interactions cannot be wholly accounted for by a lower level analysis. This means that the behavior of the higher level entities cannot be derived from their composition out of lower level components, the structure of those components, and the laws and relevant lower level boundary conditions pertaining to the behavior of the components out of which they are composed.⁷ Thus, for example, organisms must be taken

Vol. 1 (East Lansing, MI: Philosophy of Science Association, 1982), pp. 324-240. The supervenience literature is concerned with the relation of parts to wholes and with the construction of the latter from the former. For an overview by a major figure in the supervenience literature, see J. Kim, "Supervenience as a Philosophical Concept," *Metaphilosophy* 21 (1990): 1-27; another valuable survey can be found in Paul Teller, "A Poor Man's Guide to Supervenience," *The Southern Journal of Philosophy*, supplement to volume 22 (1984): 137-162. Orthodox philosophical treatments of mereological analysis are provided by P. Strawson, *Individuals*, (London: Methuen, 1959); D. M. Armstrong, *Nominalism and Realism: Universals and Scientific Realism* (Volume I), (Cambridge: Cambridge University Press, 1978) and *A Combinatorial Theory of Possibility* (Cambridge: Cambridge University Press, 1989); D. K. Lewis, *The Plurality of Worlds* (Oxford: Blackwell, 1986), esp. pp. 211-213, and *Parts of Classes* (Cambridge, MA: Blackwell, 1991); and R. M. Chisholm, *Person and Object* (La Salle, IL: Open Court, 1976), esp. Appendix B.

⁶. This example is chosen with malice aforethought. Jade has two alternative mineral compositions. Contrary to the views of Hilary Putnam and others, we need not employ the chemical (mereological) analysis according to which jade must count as two distinct substances. It is up to us whether we do so or not. Our choice ought to depend on our purposes. What this shows is that the way in which jade enters into certain transactions may or may not be a function of its mineral composition and that, in certain instances at least, that composition is not the decisive issue to be faced *even in the causal order*.

⁷. With the resulting failures of micro-explanations for macro-phenomena. These failures of reductive "micro-explanations" are elegantly accounted for by Alan Garfinkel in *Forms of Explanation*,

account of as (causally emergent) wholes within evolutionary biology insofar as evolution genuinely involves a two-step process—selection acting on phenotypic differences *between whole organisms* and variation at the genotypic level.⁸

The question of emergence goes unresolved in mereological analyses because those philosophical analyses are typically insensitive to the taxonomic criteria of identity applied in the sciences and to the multiple pathways by which "the same" higher-level result can be brought about. On our view, it is the multiply-established persistence of emergent kinds in scientific taxonomies that justifies ontological commitment to such "higher-level" entities.

Scientific Realism and Emergence

These results affect the content of realism in a variety of ways. For example, although it is widely recognized that such objects as quasars, tectonic plates, and demes (as potential units of biological evolution) are among the novel entities that scientists may well claim to have discovered and characterized, such entities do not enter seriously into most philosophical discussions of the ontology of science. On an eliminativist view, of course, such entities are not serious candidates for fundamental ontological status. Because so much of the philosophical debate has been focussed on eliminationism, even those philosophers who do not consider a well-grounded mereological analysis sufficient to demonstrate the secondary status of higher level entities have tended to concentrate their attention on the "lower level" or supposedly "fundamental" ontological commitments of science. In consequence, putative higher level entities like those just mentioned have been largely ignored in the philosophical literature.

At a minimum, then, philosophical and scientific treatments of ontology are often at cross purposes. But the situation is worse; standard philosophical approaches yield a serious mistreatment of the character and evidential status of the ontological claims of workaday science. Geologists and astronomers will (and should) count tectonic plates and black holes as "real" whether or not they are "reducible" to and exhaustively constructed out of lower level entities in any of the senses here discussed.

(New Haven, CT: Yale University Press, 1981). When attempting to account for "higher-level" phenomena, such a micro-explanation

"gives us a false picture of the sensitivity of the situation to change. It suggests that, had the specific cause not been the case, the effect would not have occurred. This is false in such cases because there is redundant causality operating, the effect of which is to ensure that many *other* states, perturbations of the original microcause, would have produced the same result. Microreductions cannot take account of this redundancy and to that extent cannot replace upper-level explanations." (p.62)

⁸. Ernst Mayr has elaborated the interpretation of evolution as a two-step process since at least 1964. Cf., e.g., "The Evolution of Living Systems," *Proc. Natl. Acad. Sci., USA*, 51 (1964): 934-941, esp. 940-941 and many subsequent writings. One way in which Mayr reinforces his perspective is nicely brought out by his implicit criticism of T. H. Morgan's explanation of sexual dimorphism. Mayr argues that the existence of distinct tissues in male and female organisms cannot be explained by the difference in male and female responses to hormonal influences. It is not the proximal mechanisms, but the evolutionary and ecological context and the distinctive roles of males and females that must be understood if the existence of sexual dimorphism is to be (adequately) explained. (Cf. *The Growth of Biological Thought* (Cambridge, Ma.: Harvard University Press, 1982), p. 73.).

To enforce this claim, it is worth elaborating on the world picture of contemporary science as best we understand it. That picture is intensely hierarchical and compositional without being easily treated by any of the old familiar accounts of reduction. What this means needs considerable unpacking.

DNA is composed of hydrogen, carbon, nitrogen, oxygen, and phosphorus. The atoms of each of these elements are composed of electrons, protons, and neutrons. Neutrons (at least) are composed of further subatomic particles. And, more generally, subatomic particles may well be something like stable or quasi-stable resonances of sub-subatomic particles. This illustrates what we mean by the world picture of science being compositional and hierarchical. For the purposes of this paper, it is particularly important to recognize that the hierarchy can be continued indefinitely in the direction of larger and more comprehensive entities. DNA molecules are but one among the many kinds of molecules of which cells and organisms are composed.

Indeed, the current standard view is that cells and organisms are composed wholly of molecules, always (well, virtually always) including some DNA, some RNA, some proteins, some lipids, etc., organized in certain ways. These molecules are organized so as to form a variety of structures, including various organelles, that count as intermediate steps between molecules and cells. And there are many intermediate steps, including tissues and organs, between cells and multicellular ("higher") organisms. A species is composed of organisms that share genealogical ties (and perhaps somewhat more). The boundaries between cells and organisms and between organisms and species are not wholly determinate; one cannot always determine whether a molecule belongs to a cell in which it is embedded, whether a cell is part of a particular organism, or whether an organism belongs to a particular species. One reason for this is that the unity of cells, organisms, planets, and the like is in part historical—using shorthand, these are historical entities. Issues about the boundaries of such entities are often largely semantic and conventional, but in favorable cases they are not wholly so. (There are substantive, and not just conventional-semantic reasons for holding that various bacterial invaders in our gut tissues are not—or not yet—part of me.) Such issues are most easily illustrated in the life sciences, but they also arise in the physical sciences; nothing of importance turns on the fact that our illustrations concern organisms.

An important traditional account of reduction⁹ would have it that if one but knew the laws governing the behavior of the entities at some lower level, those laws—by themselves or in conjunction with an account of relevant boundary conditions *pertaining to parameters at the lower level*—would suffice to yield a scientifically complete explanation of the behavior of all entities at the upper level. (One would, of course, need to include a structurally adequate statement of the composition of the upper level entities *in wholly lower level terms*.) It is at this point that the hierarchical aspects of our current account of the structure of the world enter into the matter. Very roughly, what is at stake is the need for laws and regularities over and above (and not reducible to or deducible from) those that pertain to the lowest level(s).¹⁰ As we indicated at the close of the section on scientific ontologies, the version of realism we defend can accommodate this need, because it sustains a commitment to (an appropriate conception of) laws.

⁹. See Chapter 11 of Ernest Nagel's classic, *The Structure of Science* (Indianapolis, IN: Hackett, 1961).

¹⁰. An argument with a similar conclusion can be found in Elliott Sober and Richard Lewontin, "Artifact, Cause, and Genic Selection," *Philosophy of Science* 47 (1982): 157-180.

Consider what would be involved in a biochemically reductive explanation of the manner in which differences in DNA content determine that two organisms will have alternate forms of a particular trait. For concreteness consider eye color in, say, wild dogs. In elaborating an explanation, one would have to proceed wholly at the biochemical level.¹¹ Specifically, an account in terms of the genetic code would have to be replaced by an account in terms of the statistical affinities between sequences of nucleotides, taken three at a time, and the relevant molecules that play a role in transcription of DNA relative to the concentrations of those molecules in physiological conditions. This would have to be followed by a strictly biochemical account of the transport of the resultant RNA molecules through the nuclear membrane, plus a strictly biochemical account of the manufacture of the relevant pigment precursors, plus a strictly biochemical account of the eventual alteration and distribution of those precursors. Furthermore, it would be necessary to provide a strictly biochemical account of the factors in the relevant physiological contexts that altered the rates of all of the relevant reactions only in the epithelial cells of the iris of the eye. To mention only a few that are relevant to the transcription of DNA to RNA, these include the presence or absence of enhancer sequences hundreds of nucleotides away on the DNA molecule, the location of the (strictly biochemically described!) 'start transcription' and 'stop transcription' signals on the DNA molecule, the presence or absence of stretches of DNA at appropriate locations in the nucleus that compete for the RNA precursors or alter the configuration of the DNA in ways affecting reaction rates, and so on and so forth.

But even if all of this were done, there would still be an issue to be resolved regarding the traditional accounts of reduction. Applying those accounts to the present case, laws and boundary conditions *at the biochemical level* are supposed to bear the primary burden for explaining the laws, regularities, occurrence of particular events, and presence or absence of particular states at the 'higher' level. And in the present instance it is not at all clear that they can bear this burden, for it is not clear that the biochemical account can adequately characterize the informational content of DNA.

A great deal, of course, depends on what questions we ask and what we may legitimately hold fixed as relevant background information or conditions. In order to make the issue stand out more clearly, let us switch the example slightly to consider a fairly straightforward evolutionary question about, say, the prevalence of the wild-type over a mutant type of eye color.¹² What is to be explained is the changing distribution of eye colors in the population over time. In some instances it may be the case that such changes are connected with a physiological effect brought about by some detailed quirk in the biochemical machinery. For the sake of the argument let us grant (what we think false) that when this is the reason the prevalences in question can be given a narrowly reductive explanation.¹³ But there are cases that involve such

¹¹. On the difficulty of substituting a biochemical for a functional explanation of such matters, an illuminating discussion can be found in Chapter 3 of Alexander Rosenberg, *The Structure of Biological Science* (Cambridge: Cambridge University Press, 1985). For an important statement that classical genetics is not reducible to molecular genetics, see Philip Kitcher, "1953 and All That: A Tale of Two Sciences," *Philosophical Review* 93 (1984): 335-373.

¹². One reason for switching examples is to take advantage of Mayr's apparatus for arguing the insufficiency of explanations of evolutionary phenomena in terms of proximate mechanisms. Cf. n. 8 above.

¹³. More precisely, the reductionist must hold that there is no information bearing on the changing proportions of the relevant phenotypes that cannot be adequately reparsed in physico-chemical

factors as, for example, mate choice by females; let us say that males with the mutant eye color are rejected at a higher rate by females than males with the wild-type color. In such a case, the behavior of the female typically cannot be given a narrowly biochemical description or explanation, and so, *prima facie*, the evolutionary question cannot be given a reductionist answer.

What are the reasons for this? Among the many, we shall focus on one that leads us to a general moral. If the suggested account for the prevalence of the wild-type eye color is correct in some particular case, what matters is not the ultimate biochemical constitutions of the females in question, but their behaviors. Robert Brandon¹⁴ has developed an account applicable to such cases using the technical apparatus of "screening off" devised by Wesley Salmon,¹⁵ but for present purposes we think a simpler account is feasible. The causally relevant behaviors of the various females cannot be reduced to their biochemical compositions if, and only if, the prevalence of the wild-type eye color tends to be altered in the right circumstances by the distribution of female behaviors in the population in a way that cannot be accounted for by their biochemistries.¹⁶ In other words, if the probability of wild-type eye color's being at least such-and-such a percent is greater relative to *both* the behavior pattern and the biochemical composition of the females than it is relative only to their biochemical composition, then, subject to all of the standard difficulties about basing causal judgments on correlations (which none of the parties to these disputes can escape), the behavior is a (positively) relevant causal factor that cannot be explained by or reduced to the biochemical composition of the females in question. More generally, if A is a positive causal factor for E over and above B, then we must have

$$p(E \square A.B) > p(E \square B) \quad (1)$$

where E is the condition to be explained, $p(E \square X)$ is the probability of E relative to X, A is a "higher level" circumstance or condition (e.g., female behavior) supposed to contribute to E, and B is a "lower level" description the sufficiency of which to account for A or for E is under examination. In the present hypothetical instance, it is possible in principle (though for most higher organisms not in practice) to perform experimental manipulations or to employ "natural experiments" to determine whether learned behaviors potentially of evolutionary significance are irreducible in the sense of equation (1) to biochemical conditions. Should a real case turn up in which mate choice behavior is a positive causal factor partially independent of biochemistry in this sense—an empirical matter not to be decided from an armchair!—the natural account to

terms. Since the issue is *not* whether such reparsing could be carried out in practice (all sides grant that it cannot), but whether it can be carried out "in principle," the issue is not easily resolved. Rosenberg, in Chap. 6 of *Structure*, provides a useful discussion of one of the principal reasons for scepticism in this regard, namely the supervenience of fitness on lower level properties.

¹⁴. E.g., in "The Levels of Selection," in Peter Asquith and Thomas Nickles (eds.), *PSA 1982*, Vol. 1 (East Lansing, Mi.: Philosophy of Science Association, 1982), pp. 315-323, and section 3.2 (pp. 82ff) of *Adaptation and Environment* (Princeton: Princeton University Press, 1990).

¹⁵. Wesley C. Salmon, *Statistical Explanation and Statistical Relevance* (Pittsburgh: University of Pittsburgh Press, 1971).

¹⁶. Thus a higher-level cause must satisfy one of the pragmatist slogans -- it must be a difference that makes a difference. The matter is slightly trickier than appears at first glance. Because of the (ineliminable) phrases "tends to be" and "in the right circumstances," some version of a propensity interpretation of probability is required in order for the formula presented below to work properly.

offer would be that the differences in female behaviors, like the differences in female biochemistries, are causally relevant to the outcome, but that the behaviors are not reducible to the biochemical differences precisely because they add a component of causation over and above that which is determined by the biochemistry involved.

The general moral here is that the mereological status of a class of entities (here wild dogs which, viewed compositionally, are made up wholly of complexly ordered and structured molecules) is not sufficient to determine whether or not those entities or their behaviors are causally relevant units in some class of interactions or in some domain of phenomena. The hierarchical ordering of nature that (we maintain) is implicit in contemporary science involves the adding of novel causal interactions (e.g., behaviors between organisms) at higher and higher levels of aggregation—interactions that at least sometimes cannot be fully explained by an account of the entities and laws pertaining to the lower level(s) alone.

This hierarchical ordering provides some grounding for our account of the ontologies of various sciences. Various sciences seek, among other things, to identify, locate, characterize, and determine the *modus operandi* of entities that are causally relevant to the phenomena of concern. Whether such entities turn out to be simple or complex, should they turn out to be emergent causal factors in ways suggested by our hypothetical example they belong in the ontology of the relevant science not as mere aggregates but as causally relevant higher level units that serve as sources of interaction. (Coherent higher level entities can also, of course, act as causal factors in certain interactions even when they are aggregates rather than some form of emergent entity.)

Ontological Progress

The central concern of this paper is the discovery of entities at levels far removed from human perception. Very roughly, the "level" to which an entity or process belongs depends on its place in the causal order—what sorts of entities it interacts with, what sorts of forces it is subject to, and what sorts of perturbations it can withstand. If a putative entity really exists, is not perceptible by unaided human senses, and is causally relevant to a class of reliably characterized interactions, *prima facie* it ought to count as a "discovered" entity of the sort intended.

"If a putative entity really exists"—aye, there's the rub. The epistemic challenge to realism put forward by figures such as Larry Laudan and Bas van Fraassen denies that we can justify the required existence claims.¹⁷ At the very least, if one is to establish that water is composed of molecules which, in turn, are composed of two atoms of hydrogen and one of oxygen, or that the material inserted by bacteriophage into the bacteria they infect consists of one long DNA molecule, or that certain quasars are interacting with immense gas clouds in a process that will yield new galaxies, it is necessary to establish (by some reasonable standard) that each of these esoteric entities exists. Can this be done?

We shall argue the affirmative. But first we must enter two caveats that temper and clarify our claims. These caveats place the argument on ground that is foreign to many of the contemporary debates on this issue.

¹⁷. The offending argument by Larry Laudan is touched on later in the paper. Bas van Fraassen offers his alternative, constructive empiricist gloss on the experimental manipulation of theoretical entities, in *The Scientific Image*, pp.75-77, and *Laws and Symmetry*, pp. 230-232.

The first caveat is that the issue ought not be tied too closely to contemporary science and especially not to the latest, most esoteric theories. Such theories may be the very best we have, but they are also the ones that stretch our knowledge to the limit and take it into risky territories. If Vannevar Bush's metaphor that equates the growth of scientific knowledge with the conquering of an endless frontier has anything to it at all, it implies that the farthest reaches of science are the least secure. If we wish to debate what entities science has discovered, let us be content to stay within relatively secure stockades and deal with what can be found there.

The second caveat emphasizes a point that was argued above: the issue ought not be couched in an overly theory-centered way. What is at stake is the reference of theoretical terms, not the truth of the most advanced theoretical claims. In particular, a complex and delicate balance exists between reference to entities and the theoretical generalizations that contribute to securing it. It is important to recognize *both* that we cannot refer to any entities unless we can make true claims about those entities *and* that we need not employ our most advanced theories to make true claims about theoretical entities.¹⁸ If one supposes that the reference of theoretical terms is strictly constrained by the logical structure of the theory in which the terms occur—a position taken by virtually all logical empiricist philosophies of science—one rules out the sort of progress we wish to explore *by definition*. This is illegitimate. For example, if it is thought that Newtonian mechanics defines or delimits the objects to which it can refer so as to exclude any objects failing to satisfy the equation

$$F = ma, \tag{2}$$

then current physics (insofar as it accepts relativity theory and quantum mechanics) must deny that Newtonian mechanics genuinely refers to any objects whatsoever. Thus if, say, relativity theory is true (or even, on most philosophical accounts, accepted-as-true), the theory-centered view would maintain that anyone who speaks a relativistic language must not only treat the claims of Newtonian mechanics as false but also hold that *strictly speaking, Newtonian mechanics fails to refer to any objects*.

An Interlude on Reference

If we develop a theory of, say, stars the central premises of which are wholly false (as, on some accounts, was the case with Ptolemy's theory), it does not follow that we cannot refer to stars. Ptolemy, for example, certainly was able to refer to stars even if the rigorous interpretation of his theory as strictly false. This is one source of the widespread discomfort with Hanson's and Kuhn's metaphor, according to which the proponents of fundamentally different theories "live in different worlds." If one switches cases to consider quasars, phlogiston, genes, or protons, a parallel point obtains: an argument based only on the ("internal") semantics of theories cannot legitimately show it to be impossible for radically different theories to corefer to the same unobserved or unobservable entities. Insofar as these entities enter into the causal order and can be tracked down by locating them in a causal chain, thus placing them in relation to experimental

¹⁸ The latter point is the central subject of the next section, but it is already clear from the discussion of the differences between Bateson and Morgan in n. 3. It is possible to locate the cause of a phenomenon (the odd eye color of a fruit fly or the incremental alteration of the electrical charge of an oil droplet in the Millikan apparatus) without presupposing the descriptive apparatus of the most advanced theory pertinent to those phenomena.

scientists, it is possible to obtain access to them in a variety of ways and to come to recognize that they have been radically misdescribed by our 'best' theories. It follows that semantic considerations of the sort illustrated above are not alone sufficient to prohibit radically mistaken theories from referring to unobservable entities. After all, there is an immense variety of devices for securing reference: definite description, indefinite description, indexicals, ostensives, self-referential locutions, spatio-temporal localizers, and so on indefinitely.

Of crucial importance is the fact that a fair number of these devices are independent of the descriptions employed in speaking of the entities in question. For example, the various *drosophila* genes for red eye color can be identified to a first approximation as those entities that cause the development and transmission of red eyes when present in a normal genetic background. Different eye color genes can be distinguished from one another (again, to a first approximation) by the unusual patterns of transmission that occur when mutant alleles affecting two distinct genes contributing to the same phenotype are present within a lineage. Such ways of identifying genes are, to be sure, not wholly theory free and not immune from total failure, but they are not dependent on particular features of one versus another theory of the gene or of the composition of genes. Thus it is not terribly difficult to refer to distinct genes while employing seriously discrepant alternative theories of the nature or structure of those entities, any or all of which might turn out to be mistaken.¹⁹ Similar claims pertain, of course, to other sciences; parallel points can easily be made about electrons or galaxies. The question whether there are such things as genes or electrons can ultimately be resolved only over a long term, but its resolution requires that there be means of referring to those entities, if only hypothetically, in ways that do not depend too deeply on the theoretical descriptions one employs in talking or writing about them.

Any semantic theory that by itself prohibits transtheoretical reference of this sort to such theoretical entities as quasars, genes, protons, or even phlogiston is seriously suspect. Such semantic theories have done a great disservice to philosophy of science by placing *a priori* obstacles in the way of those who have sought to make sense of the disagreements that arise between adherents of conflicting theories when they attempt to work out the structure, composition, and behavior of such entities. Indeed, conflicts of this character are extremely common, and yet they make no sense unless transtheoretical reference to the disputed entities is possible.

Ontological Progress Again

There is no need to provide a long list of candidates for the status of concern to us here—new level entities, beyond the reach of unaided perception, discovered by science. Such a list

¹⁹. To reinforce this point, an addendum regarding the outcome of the dispute between Morgan and Bateson is useful. With the victory of the Morgan school in mainstream genetics, by the late 30s and early 40s the dominant theory considered genes to be *protein* (or, perhaps, nucleoprotein) molecules located on chromosomes. The falsity of the protein theory of gene composition in no way undermined the immense progress made from (say) 1915 to 1940 in devising richer criteria of identity for genes and in locating and characterizing large numbers of new genes. There is no question but that geneticist's ability to refer *to the same genes* was in no way affected by the replacement of the protein theory with the theory that genes are composed of DNA. It is a matter requiring substantive historical argument, but the extraordinary theoretical developments from 1915 to the present have raised very few doubts about the success of the pioneers of genetics in referring to particular genes.

could easily be extracted from this paper, but every reader will be able to expand it greatly. What needs to be worried at is whether science has adequately established the existence of at least some of the putative entities in question. Subject to all the usual caveats regarding fallibilism—we may turn out to be mistaken in cases very dear to our hearts, cases that are firmly grounded in available evidence—the answer seems straightforward. Are there serious grounds for believing that there are no galaxies, no tectonic plates, no bacteria, no DNA molecules, no hydrogen and helium atoms, no electrons and protons? What arguments convincingly demonstrate that we are not justified in claiming that such entities exist? What criteria should we employ in deciding when such (putative) entities are properly viewed as hypothetical and when their existence can properly be taken as established?

Ian Hacking offers an answer to the last question²⁰ which points the way, though it is not sufficiently general. His answer is, roughly, that a putative entity has been shown to exist when we can manipulate it as a tool in dealing with other matters. (Electrons have been shown to exist because we can use them to bombard protons, thin films of metal, and a great deal else—in Hacking's word, because we can "spray" them.) But we cannot spray or otherwise manipulate tectonic plates or galaxies even though, if the above considerations are correct, we can establish (and in some cases have established) the reality of such entities as thoroughly as we have established the existence of electrons. Therefore, manipulability is sufficient but not necessary for existence.

Hacking's approach points the right way, however: the crucial realist step is establishing that well identified entities genuinely play a causal role in the unfolding of some well established phenomena. All the better if we can control the phenomena by controlling the entities in question or if we can visualize them or make them audible by controlling their interactions with experimental apparatus. But such feats are confirmatory gravy. In principle there is nothing mysterious about obtaining causal interactions with novel entities, even when those entities are above or below the threshold of unaided perception. To establish the reality of theoretical entities, we proceed in much the way that the European explorers of the late fifteenth and early sixteenth centuries did when they established the existence of North and South America; we manage to observe, run into, or manipulate objects in ways that reflect the causal role(s) of those entities in some domain that we are exploring. It may be that in the early phases of our exploration we confuse a new continent (South America) with another one (India) or an island with a continent, but such are the hazards of exploration. With sufficient time, sufficient luck, sufficient will, sufficient skill, and sufficient capital, in favorable cases such issues can be adequately resolved.

To be sure, explorations in contemporary science are usually done with the aid of instruments and theories. These are the tools by means of which we attempt to localize supposed theoretical entities and to figure out whether the evidence for the existence of those entities rests on artefacts, on unfamiliar aspects of familiar objects, or on features of genuinely novel things or processes that must be taken into account in order to understand how the phenomena of interest are caused. The less familiar and the further from our ordinary experience theoretical entities are (and they can be distant in a variety of ways), the more caution we must employ because of the likelihood of being deceived by artefacts, by defects in our tools, by unfamiliarity with the materials with which we are working, by deep differences in the character of the entities and phenomena we are exploring from those with which we have previously worked, or by our

²⁰. Cf. *Representing and Intervening*, pp. 22 ff.

natural tendency toward self-deception. Nor is there any guarantee that any particularly interesting case is a favorable one—we may, in the end, fail to discover any useful regularities or any significant causal processes. Nonetheless, when so-called theoretical entities and processes are adequately localized and individuated and, at the same time, shown to play a causal role in the domain of phenomena we are examining, they have, to that extent, been shown to be real. (The cases that Hacking explores, in which we can employ the entities in question to alter an otherwise predictable course of events, are particularly dramatic, but they represent only one way in which we can establish that there are identifiable causally efficacious entities entering into a particular class of interactions.)

This account allows us to proceed in an extremely cursory fashion, employing a schematic treatment of the history of science in order to support the claim that there has been extraordinary ontological progress in science. We shall carry out this treatment under three heads.

1. The theoretical forefront of science has proceeded, in stages, to deal with phenomena further and further removed from unaided human perception.
2. In this process, it has found that the world has intrinsic scale and, correlatively, that objects are hierarchically structured.
3. Behind the theoretical forefront, one finds a rich body of devices for checking whether different theories and different sciences refer to the same entities and, if they do, whether they concur or disagree in their accounts of the behavior and composition of those entities. In a significant number of cases the answers to these questions justify firm confidence in the existence of the entities in question.

There is little need to comment on the first point. From the ancient Greeks through the Renaissance, science was able to deal only with phenomena occurring on a time scale running from about one second to at most a few centuries. Now the time scale runs from nano- or pico-seconds to tens of billions of years. A similar change has occurred in the spatial dimensions and energy content of the events, objects, processes, and phenomena encompassed by science. Arguments of empiricist inclination often propose that, since we test the consequences of theoretical models at the same macroscopic scale available to the Greeks, all that has occurred is that theoretical postulates that *seem* to describe entities and processes on non-observable scales have been used to deduce observationally testable hypotheses. It seems to us that such accounts are radically mistaken in the way they handle certain of the images that, for example, microscopes, telescopes, cloud chambers, autoradiographs, and spectrometers have produced when the presence and action of specifically identified, hitherto unobservable entities was predicted.

The second point requires more attention. One of the major findings of the present century has been that the world is not Newtonian in the specific sense that objects, events, and processes exhibit intrinsic scale; the possibility of constructing geometrically similar figures cannot be taken for granted for all masses. Thus it is physically impossible to have Jonathan Swift's fleas upon lesser fleas ad infinitum. For such reasons, it is no accident that subatomic particles form stable atoms of just the dimensions that they do or that atoms form molecules alterable (as atomic nuclei are not) by chemical interactions, and that the discovery of the substructure of atomic nuclei required the recognition and use (or at least observation) of higher energy

interactions than those available to classical chemistry. Similarly, it is no accident that living cells and galaxies exhibit a limited range of dimensions.

These points give serious content to the claim that the entities and processes uncovered by science involve hierarchical structuring and various forms of emergence. They also give serious content to the claim that scientific ontology interacts with philosophical ontology. As we have worked our way out from dimensions of a human scale toward the very small and very large, we have discovered that at certain sizes, but not at others, there are stable entities of certain sorts waiting to be discovered. At different sizes the entities are intrinsically different in kind and enter—can enter or must enter—into intrinsically different interactions. We now understand something about the reasons for the sizes being what they are; they have something to do with intrinsic scale as revealed by the quantum of action. The reason that molecules, but not nuclei, can be perturbed by chemical interactions turns on the differences in the magnitudes and the effective distances of the forces involved. The reason that the collapse of some stars can produce neutron stars while that of others cannot is that the (known) forces separating stellar particles must be overcome by gravitational collapse in order to begin the relevant fusion reaction—which requires a minimum mass. And so on. As surprising as it is that there can be and are such things as black holes, it is equally surprising that stellar collapse cannot produce black holes of arbitrary size.

Even in the absence of genuine theoretical understanding (or in the face of the need to revise it significantly), it is possible to discover aberrant events, entities, and processes of various sorts and to identify and reidentify them as entering into novel classes of interaction, as withstanding certain sorts of perturbations but not others, and so on. This process is an extended one, often lasting many scientific generations, and draws on the technologies, theories, and resources of many disciplines. Multiple access, involving many techniques for reaching the same kind of entity, together with the reidentifiability of its peculiarities of interaction (exemplified by the resistance of chemically similar nuclei to chemical separation but their susceptibility to physical separation)—these are the sorts of features that allow confident determination that a new kind of entity has been discovered.

These last considerations already speak to the third point. Consider any appropriate instance of cross-disciplinary treatment of some particular entity. Our example will be DNA. How certain can we be that DNA is a unique and reidentifiable material, that it typically assumes the familiar double helical conformation in physiological contexts, and that it is one of the primary carriers of genetic information? On the one hand, the answer to such questions is a "local" matter; it turns on the details of the scientific work, not on philosophers' concerns about ontology. The questions just asked concern the actual, and only secondarily the possible or the necessary. They are answered by having recourse to an immense variety of techniques from biochemistry, crystallography, electron microscopy, genetics, physiology, and so on. On the other hand, the form of the answer is reasonably general; extreme confidence, such as we have in this instance, stems from our ability to gain access to, and ability to utilize, this substance and its behaviors from an incredibly large number of starting points using an incredibly large number of techniques, and to do so in a way which enforces concordance across a large number of disciplines with respect to such matters as the location and conformation of the DNA and the differential effects brought about by various alterations of the material.

The form of our answer will seem disappointing to some. It evades the general epistemological challenge of the antirealists by "retreating" to specifics. Yet we maintain that this is just right. One shows that scientific realism is *possible* by showing that it *makes sense* to

claim that theoretical entities are real and that one of the aims of science is to locate and understand such entities. One shows that realism about particular entities is *correct* by examining the evidence and the arguments brought to bear in support of particular claims about those entities.

This procedure eliminates the general arguments against realism by explaining why general sceptical arguments about theoretical entities are less compelling than the evidence for the reality of DNA. The latter evidence is so cogent not simply because arguments dealing with the specific evidence are by themselves more persuasive than general sceptical arguments of any form. Rather, the specific evidence for DNA is more rationally compelling than the general sceptical argument because there is a plausible naturalistic conception of the reliability of scientific methodology identifies the theoretically "relevant" or appropriate sceptical challenges concerning specific entities. This naturalistic conception respects the fact that we seek the best explanation of interesting behaviors as well as interactions and plausible abductive arguments regarding their causes including perceptually inaccessible causes). Even in the absence of a satisfactory *account* of inference to the best explanation, one can defend the epistemological status of such inferences. Thus, should an empiricist claim that we have given no non-question-begging reason against the sceptic—e.g., by indicating that the relevant *scientific* arguments for theoretical entities (as well as for unobserved observables) *are themselves abductive*—our response is that there is no scepticism about abductive arguments for theoretical entities that doesn't also lead to scepticism about observables.

Therefore, epistemological inductions²¹ from the continuing replacement (or abandonment as some have called it) of "highest level" theories damage only the most inappropriately formulated versions of scientific realism; the most important arguments for the reality of DNA (or of protons and other such entities) do not turn on the status of highest-level theories alone. As we have argued throughout this paper, that evidence stems from robust concordance of results across disciplines, ranging from electron microscope photomicrographs and autoradiographs of radioactive DNA, through transformation of plants and animals by use of especially prepared DNAs, to elaborate chemical analyses of cellular extracts and crystallographic analyses. What makes the evidence robust is that it is so multifarious and that it has withstood integration into a body of practice quite thoroughly removed from the theoretical frontiers. That DNA exists is the best explanation for this diverse evidence. There can be no clearer indication that the sceptic places exorbitant demands on the realist than the fact that, if those demands were honored, they would block the very arguments the (selective) sceptic wishes to make on behalf of inductive inferences concerning observables.

²¹. Such as have fascinated Larry Laudan and come to play an increasingly central role in his writings on these topics. (Cf. "A Confutation of Convergent Realism," *Phil. Sci.* 48 (1981): 19-49 and subsequent papers.) From the fact that all major theories (except, perhaps, some of the most recent ones that have not yet been adequately tested) have been shown to be false, Laudan argues to the pessimistic conclusion that we have to expect the same fate for current theories. He then draws from this result the moral that science does not make referential progress of the sort here claimed. It is the last inference that we maintain is inadequately, indeed, ill supported. One is invited to arrive at a similarly negative appraisal of claims concerning the typical reliability of scientific methodology in Arthur Fine's *The Shaky Game* (see esp. p.119). The basis for this pessimistic expectation is Hilary Putnam's famed meta-induction, set out in *Meaning and the Moral Sciences* (London: Routledge, 1978), pp.24-25.

Summary and Conclusion

It will be useful to conclude by characterizing the general form of the response to contemporary epistemological antirealists implicit in this paper. It can be put thus: the standards that antirealists put forward for scientific realism are exorbitant and utopian. When those standards are replaced by more sensible ones, most of their objections to scientific realism melt away, while the remainder are transformed into significant and appropriate objections to realism about particular entities.

It is exorbitant to expect blanket arguments for scientific realism in general. A great deal of science does not aim at ontological progress—modelling, control, and prediction are all legitimate goals. Not all attempts to achieve ontological progress in science are well grounded, and many of those that are well grounded are unsuccessful. Where there is ontological progress, it comes for the most part piecemeal and across a broad front. Typically, it involves cross-disciplinary and cross-theoretical work over several scientific generations. Thus Lakatos was right in rejecting exorbitant appeals to instant rationality.²² He would have been equally right to reject realisms that rested, in the end, on appeals to monotheoretic or monoprogrammatic research. Given the various scales and complex causal ordering of the universe, nature demands diverse methods for its investigation. And because the nature we explore is (largely) ontologically prior to our investigative programs, it is not especially surprising when the entities we discover and characterize do not respect disciplinary boundaries.

It is utopian to expect contemporary theories to offer nothing but the truth about theoretical entities. The idea that the success or failure of scientific realism should be pegged to the ability of theories to withstand all attempts to criticize or overthrow them is, therefore, one that must be rejected. And while it is correct that very many of the theoretical events, entities, and processes tried out and taken seriously in the course of historical science have not withstood critical empirical scrutiny, it is also true that critical scrutiny typically forces us to more robust realism about events, entities, and processes underlying the phenomena of concern. The rejection of phlogiston is intimately tied to the development of a doctrine of chemical elements. The rejection of atomism (in the sense of the discovery that chemical atoms are themselves complexly structured) does not remove chemical elements as distinct entities from the world; it does not even remove *chemical atoms* (such as Cl) from the world. The development of quantum mechanics, with its analysis of subatomic particles as having a substructure involving resonances and other unlocalizable entities and processes, does not remove electrons, protons, neutrons, neutrinos, photons, etc., from the world. Notwithstanding the rejection of such views by epistemological antirealists, the discovery and confirmation of the existence of chemical elements, chemical atoms, and even some subatomic particles exemplify ontological progress of the sort that we have been discussing.

²² This stance against such exorbitant demands on realism is closely similar to that arrived at by Alan Musgrave. Cf. "Realism versus Constructive Empiricism," in P. M. Churchland and C. Hooker (ed.), *Images of Science*, pp.197-221; also see "The Ultimate Argument for Scientific Realism," in R. Nola (ed.), *Relativism and Realism in Science* (Dordrecht: Kluwer, 1988), pp. 229-252. The concordance in our views is not entirely accidental—discussions with Musgrave played a significant role in the preliminary work on this paper.