The Structure of Biological Science

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Preface

This book is an introduction to the philosophy of biology as well as an extended defense of a particular philosophy of biology. The two endeavors go together: The second will not persuade unless it offers a coherent view of the life sciences that examines and sheds light on most of their epistemological and metaphysical problems. And the first will be little more than a disconnected series of insights into isolated puzzles that do not add up to an improved understanding of the subject as a whole unless it manifests a unifying theme. Despite their many virtues, previous introductions to this subject suffered for want of such a theme, which shows how the problems of each subject are interconnected and, more important, how the solutions to each of them constrain the treatment of others. A second feature of these works is that they reflect a philosopher's agenda of problems in the philosophy of science, instead of a biologist's concerns with understanding biology. The present work aims at meeting these two needs. There is a third reason why a new introduction to this subject is required. In the decade or more since introductions to the philosophy of biology first appeared, biology itself has undergone revolutionary development, especially in its biochemical division. Meanwhile, new controversies surrounding evolutionary theory have also arisen. The details of neither of these two developments could have been anticipated, but they have materially influenced the agenda of the philosophy of biology in the years since the first introductions appeared.

In one more respect, this work differs from previous introductions to the philosophy of biology, for it presumes more biological sophistication. It does so for three reasons. First, it has had to come to terms with so much that was unknown a decade or more ago. Second, it focuses expressly on what I take to be biological concerns. Third, it constitutes a philosophy, an organized system, a definite position about the nature and extent of biological knowledge. To the extent that it offers a distinct thesis about biology it is not a presentation of all sides on current and past controversies in the discipline. To carry out the extended argument for this thesis that the work constitutes, I have had to abbreviate the exposition of certain philosophical matters. Much of the philosophical stage setting I have curtailed is available in previous introductions to the subject, and at the end of each chapter I have provided an introduction to this literature. Because the book is meant for biologists I have tried to say enough, especially about philosophical motivation, to show what the philosophical problems are and why they are biologically serious. But because the work may also be a convenient way to report to philosophers the striking achievements of the most recent period of biological research, I have tried not to stint on biological details, especially in discussing the ramifications of methods and findings in the study of macromolecules.

My aim in undertaking this project was to provide a physicalist, materialist, reductionistic account of all biology, subject only to the constraint of doing as much justice to its achievements as they deserved. That is, I have made as few concessions to the material or formal distinctness of biology from the physical sciences as is consistent with its actual character. The result has been rather different both from what I expected and from what those familiar with my previous views will predict. For it turns out that doing justice to the science of biology results in what is at best a very limited, indeed hollow, vindication of reductionism and materialism, and a refutation of antireductionists that leaves biology with as much autonomy as their view of it really requires and rather more than many a reductionist is comfortable conceding.

Among those in whose debt the pursuit of this project placed me I thank first the authors of those introductions to this subject that I now ungratefully claim to supplant: David Hull and Michael Ruse. Their books, then their stimulation, and finally their encouragement have led me both to formulate a philosophy of biology and to expound it in the present terms. I am grateful to both for detailed comments on earlier versions of many of the discussions and arguments that follow, and I am especially indebted to David Hull for reading and improving the whole of a previous draft of the work. For help of equal magnitude, and for saving me from several blunders, I must thank Philip Kitcher. I am only sorry that I could not produce the result that would fully repay his painstaking help. Additionally, portions of this book are heavily indebted to Philip Kitcher's own work. Indeed, a crucial portion of Chapter 4 reports his results, and fully half of Chapter 7 recapitulates his insights (with conclusions he would not endorse, however). Similarly, I am indebted to David Hull and Michael Ghiselin for the latter half of Chapter 7, and in the first half of that chapter I am indebted to Elliott Sober's insightful treatment of essentialism. Chapter 5 is informed by Mary Williams's approach to evolution, and I am indebted to her for encouraging me to exploit it. Parts of this chapter were also heavily influenced by published and unpublished writings of John Beatty. For using so much of the intellectual capital in which these seven philosophers have so heavily invested, I am grateful almost to the point of embarrassment.

For reading and commenting on various parts of this book, I also thank Jonathan Bennett, Daniel Hausman, Richard Burian, William Wimsatt, William Starmer, Joan Straumanis, Peter van Inwagen, Paul Teller, William Bechtel, Alan Nelson, Donald T. Campbell, Stuart Kauffman, Jaegwon Kim, and Peter Richarson. For what virtues of readability this work has I am indebted to Jonathan Bennett's encouragement and Alfred Imhoff's copy editing.

What I understand of biology and its latest accomplishments I owe to William Starmer, John Vournakis, Calvin Vary, David Sullivan, Richard Levy, Thomas Fondy, Darrel Falk, Barbara Vertel, and Samuel Chan.

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Foundation, and brought to completion through a grant from the National Science Foundation. I am grateful to these institutions for supporting my research.

Finally, I have learned much about heterozygote superiority from Bloomsbury and even more about Gene-regulation and Gene-expression from my son.

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CHAPTER 1

Biology and Its Philosophy

In August of 1838, after hitting upon a mechanism for evolution, Charles Darwin confided to his notebook: "Origin of man now proved. – Metaphysics must flourish. – He who understands baboon would do more towards metaphysics than Locke" (Barrett, 1974:281). Any philosopher – and many a biologist – coming upon this prediction over the next century and more would certainly have thought it quite false. Metaphysics, the philosophical examination of the ultimate nature of reality, did not flourish during the hundred years after Darwin published *On the Origin of Species*. Indeed, it came close to vanishing. And the causes for the disappearance of philosophical and theological speculation throughout this period were to be found in the influence of Darwin's own theory.

If ever there was a theory that put an end to traditional philosophizing, it was the one Darwin expounded. By providing a single, unified scientific theory of "the origin of man" and of biological diversity generally, Darwin made scientifically irrelevant a host of questions that philosophers and scientists had taken seriously since long before the time of John Locke. The theory of natural selection has put an end to much speculation about the purpose of the universe, the meaning of life, the nature of man, and the objective grounds of morality. It has grievously undermined the theologian's most compelling grounds for the existence of God, the argument from the earth's design to the existence of a designer. Philosophers and biologists certainly recognized this effect of Darwinism, and over the course of the decades after 1859 some of them made great efforts to refute the theory as much on philosophical grounds as on biological ones. Among biologists, this work has had ever-diminishing influence, and antievolutionary philosophy has almost completely disappeared within biology. As indeed has almost all philosophy as traditionally conceived.

By making the traditional questions of philosophy biologically irrelevant, Darwin also helped make them philosophically disreputable. But when the grand questions of metaphysics were expunged from philosophy, there seemed to be nothing left to the subject but "logic chopping" and "mere semantics." Thus philosophy as a whole lost its interest for most scientists. The conclusion seems inescapable that Darwin put an end to philosophizing, at least about biological matters. By and large, Darwinians and anti-Darwinians have agreed on one thing: If Darwin was right about the origin of man, metaphysics should vanish, not flourish. For a long time, therefore, Darwin's prediction about his revolution's effects on philosophy seemed quite wrong.

But the more recent history of philosophy, and especially the philosophy of science, has vindicated Darwin after all. This chapter traces the course of the reflections that did so. This brief history of how traditional philosophical issues became respectable again in philosophy is at the same time the best argument for biologists taking the philosophical examination of their subject matter with the utmost seriousness. The history to be briefly surveyed is that of Logical Positivism – or Logical Empiricism, as some of its proponents called it. The rise and fall of this movement in the philosophy of science has revealed that the philosophy of a science is part and parcel of that science itself. The questions philosophers deal with do not differ in kind from those scientists face. Some differ in generality and in urgency, but none is a question that scientists can ignore as irrelevant to their discipline and its agenda. This means that the justification for pursuing the philosophy of science is nothing more or less than the justification for science itself.

Those who do not need to be convinced of the importance to science of philosophy and those eager to come immediately to grips with the philosophy of biology may safely leave this chapter to another occasion. Doubters may, however, profit from reviewing the argument of this chapter, for it provides the strongest basis possible for the biological relevance of the philosophy of science and does so through the examination of doctrines to be met again later in this work. Nevertheless, it is worth noting that this chapter proceeds at a level of generality much more removed from biology than the rest of the book. Indeed, the level is general enough that, if the argument to be presented is correct, this introduction can serve as the last chapter of this book instead of its first.

1.1. The Rise of Logical Positivism

Logical Positivism has certainly been the most important movement in the twentieth-century philosophy of science. Let us trace its motives, chief doctrine, and gravest difficulties. The motives were laudable, the doctrines striking, and the difficulties insurmountable. In surrendering the doctrines of Logical Positivism while honoring its motives, the philosophy of science transformed itself into something indistinguishable from science itself.

It is convenient to begin our exposition of Positivism with an important achievement of nonevolutionary biology. Throughout the latter half of the nineteenth century, embryology was at the forefront of experimental research. Among the most important of embryological experimentalists was Hans Driesch. Two striking laboratory discoveries are associated with his name. Working with sea-urchin eggs and embryos, he was able to demonstrate that the physical deformation of the egg and the subsequent rearrangement of the blastomeres – the cells produced in the first few stages of fission – had no effect on the normal development of the embryo. This experiment suggests that spatial relations among early blastomeres are irrelevant to normal development. Even more strikingly, Driesch went on to show that a single blastomere isolated from the rest at the two- or four-cell stage can give rise to a complete sea-urchin embryo normal in every respect except size.

Driesch is honored in every account of embryology for these crucial experimental discoveries. But he is ridiculed for the explanatory theory that he offered to account for them. The fact that an embryo, or indeed a single cell, can regulate its development to compensate for missing cells suggested to Driesch the operation of an organizing principle, which he dubbed an "entelechy" (after a similar notion in Aristotle's philosophy), and which he held to determine the harmonious development of living things and to distinguish them from inanimate ones. Because spatiotemporal location and physical mass seemed irrelevant to development, physics could not account for embyrological phenomena. Their causes must, he thought, be sought in nonmaterial forces. Therefore he adopted the view that entelechies have a nonspatiotemporal mode of existence, although they act "into" space and time. Entelechies are elementary "whole-making" factors that have no quantitative characteristics, are unanalyzable, and, according to Driesch, are knowable to the scientist only by reflection on the orderliness of direct human experience. It was perhaps inevitable that the temptations that led this important experimentalist to adopt such speculative explanations for the startling observations he made eventually overcame his biological interests altogether. Driesch ended his days as a professor of philosophy. Contemporary works still reprint his most important experimental papers but add cautions like the following: "Most embryologists, however, have had no difficulty in explaining regulation in terms of known physiological processes, making superfluous Driesch's mystical interpretations" (Gabriel and Fogel, 1955:210).

Driesch's entelechy is just the sort of occult entity that has long bedeviled all the natural sciences. The Logical Positivist philosophers of the first half of the century expounded a philosophy of science that would eliminate such speculative metaphysics from legitimate science, that would enable us to objectively distinguish empirical claims from disguised pseudoscience like astrology and antiscience like special-creationism, and that would also determine the scope and form of intellectually respectable philosophical examinations of science. Because, according to these Logical Empiricists, knowledge is either based on observation and experiment, as in the sciences, or on formal deduction from definitions, as in mathematics, whatever transcended these limits could be safely disregarded as scientifically, or cognitively, meaningless – indeed, in the view of some, as quite literally nonsense. In the view of some of these philosophers, a claim like Driesch's that nonphysical entelechies control the development of embryos was on a par with Lewis Carroll's nonsense verse from Alice in Wonderland: "Twas Brillig and the Slithy toves did gyre and gimble in the wabe . . ."

What Logical Positivists required to eliminate metaphysical nonsense from empirical science was an objective principle or test that could be applied to statements and terms from any discipline and that would decide about the cognitive significance of the claim or concept. These philosophers searched for a principle of meaningfulness that made no demands on the specific *content* of scientifically legitimate statements but required them to have a specified relation to actual and possible empirical evidence that could test them. The history of the school of Logical Positivism is the history of attempts to find the correct formulation of such a principle. Positivists knew roughly what it had to look like, and they knew broadly what systems of statements clearly passed its standard as meaningful and what sets of statements plainly failed as meaningless. Paradigm cases, of meaninglessness like Driesch's entelechies on the one hand, and meaningfulness like Rutherford's electrons on the other, were employed to calibrate varying candidates for a satisfactory principle of "cognitive significance." Such a principle had to rule the former as meaningless and the latter as meaningful. Because the mark of science is that its claims are controlled and justified by experiment, observation, and other forms of data collection, Positivists held that, to be meaningful, expressions have to be empirically testable by observation and experiment. Those that are not have no more role to play in science than the statement that "green ideas sleep furiously." They may look respectable, and satisfy the rules of grammar of the languages they are couched in, but these pseudosentences on whose truth or falsity the empirically ascertainable facts cannot bear are literally *non*sense, or at any rate without scientific significance.

Problems arose for Positivists in formulating a manageable principle that operated along these lines and gave the right answers for the calibrating samples. Consider what is required for empirical testability. If complete verification by observations is required for testability, almost no sentences except those reporting immediate sensations are testable. Statements of physics about unobservable entities like electrons and quarks will turn out to be meaningless. Even general laws about regularities among observable phenomena will fail the test because they cannot be strictly verified, expressing as they do a claim about an indefinitely large number of events.

Accordingly, the notion of empirical testability was revised and weakened to allow for the theoretical entities of science and for the generality of its most characteristic claims, its laws and theories. Instead of strict and direct verifiability, Positivists opted for indirect confirmability: A statement is scientifically meaningful if and only if there is actual or possible empirical evidence that tends to confirm, though perhaps not completely verify, the statement. But the notion of confirmation is an unsuitably vague one, so vague that Driesch's entelechy theory might even pass its muster. Therefore many philosophers, as well as sympathetic scientists, were attracted to another formulation of cognitive meaningfulness, one due originally to Karl Popper. Its particular attraction is its ability to pass the general laws and theories characteristic of science as meaningful while excluding Driesch's entelechy theory. Verifying a law requires an indefinitely large number of positive instances, but only one negative instance seems required to falsify a law. By contrast, on Driesch's own exposition of his theory, claims about entelechies are unfalsifiable by experiments because entelechies have no quantitative properties, nor even a spatiotemporal location for that matter. Thus, it has long and widely been held, especially by scientists themselves, that the mark of a scientifically respectable proposition is that there be actual or possible empirically detectable states of affairs that could *falsify* it.

1.2. The Consequences for Philosophy

Following through on Positivist strictures on the meaningfulness of statements had the profoundest consequences for philosophy and especially for the philosophy of science: These disciplines were restricted largely to the treatment of purely "semantic" questions, in the most pejorative sense of that term. Philosophy is not an experimental science; it can claim neither a special range of facts as its subject matter nor any nonempirical mode of knowledge of the facts the "real" sciences study. It must, in the Positivist view, limit itself to the provision and examination of definitions, stipulations, and conventions about language, and to the study of their formal relations. Any other philosophical enterprise was condemned to intellectual disreputability, to the cognitive meaninglessness that characterized so much pre-twentieth-century metaphysics. It was for this reason that twentieth-century philosophy became largely the philosophy of language and that the philosophy of science became the study of the implicit and explicit definitions of the terms ubiquitous in science – like 'law,' 'theory,' and 'explanation' – and of the terms of the special sciences – like 'mass,' 'element,' and 'phenotype.' The outcome of such investigations could at most be increased clarity about usage or proposed improvements in terminology, justified by considerations of convenience and simplicity.

So circumscribed, the philosophy of science has little to offer the sciences. It may show that the way in which physicists employ the term 'law' differs from the way biologists do, or that what the latter call 'explanations' differ from what the former do. But it can hardly assess or adjudicate substantive matters within or between the sciences. According to Positivist teachings, even the linguistic differences philosophy might uncover, and the distinct patterns of reasoning it can reveal, have no factual import, for they reflect conventions utterly independent of any fact of the matter. Such linguistic differences between sciences cannot constitute or reflect anything about the nature of the sciences' subject matter.

Philosophy, along with mathematics and logic, had long been a priori disciplines, domains in which truths have always been deemed *necessary* ones. It is just because of the necessity of mathematical truths, and for that matter philosophical ones, that they had to be known a priori: Experience never reveals the necessity of any truth it communicates. This is because claims of experience are falsifiable: Things can always be conceived to be different from the way they are experienced. But now the Logical Positivists thought they knew why mathematics and philosophy were a priori and necessary. It was not because the philosopher and mathematician had a special faculty of insight into necessary truths more firmly fixed, more secure, and more important than the merely contingent findings of empirical science. The truths of mathematics, and those philosophical claims left after the banishment of metaphysics, are necessary and a priori because they are disguised or undisguised *definitions* and the logical consequences of definitions. These truths are necessary because they have no content, restrict no factual possibilities, and merely express our conventions to use words in certain ways. They are vacuous trivialities. Philosophy provides a priori knowledge because it provides linguistic knowledge, not factual knowledge. As such, it does not compete with or cooperate with the sciences in providing factual knowledge. Because its only legitimate claims are not falsifiable, philosophy was condemned to a derivative role of clarifying and reconstructing the expression of factual knowledge, but not adding anything to it.

Positivists were willing to bear the high cost of casting down philosophy from its throne as queen of the sciences mainly because in doing so they were also ending the baleful effect of metaphysical speculation and pseudoscience on the real advance of knowledge.

For all its neatness and rigor, the Positivists' program fell apart in the immediate postwar period. It did not come unstuck through the attacks of its opponents and detractors, disgruntled metaphysicians who thought that philosophy did provide an alternative route to real knowledge that science could not reveal. The Positivists' program came apart at the hands of the Positivists themselves and of their students.

They found that its fundamental distinctions could not be justified by Positivism's own standards of adequacy. The collapse of Logical Positivism is best illustrated for our purposes by examining more closely the claim that scientific knowledge must be falsifiable. More than any other slogan, this one has become the outstanding shibboleth of contemporary biological methodology.

1.3. Problems of Falsifiability

A proposition is scientific if and only if it is falsifiable. This is the criterion or principle of falsifiability. Falsifiability must be distinguished from falsity, of course. To *falsify* a proposition, that is, to show it is false, it is sufficient to infer from it some implication that is in fact not borne out by observation or experiment. For a proposition to be *falsifiable* it must only be logically possible to do this, not actually, physically possible; otherwise we should have to say that a true empirical law is unfalsifiable because it cannot in fact be shown to be false.

Consider such an expression as, say, Ohm's law, which states the relation between resistance, voltage, and amperage: $R = E/I$. To test the simple claim that, for a potential-difference of E volts, and a current of I amperes, the resistance, R, in ohms, is equal to E/I, we require an ammeter, a voltmeter, an ohmmeter, a conductor, a resistor, and a source of electrical potential. Testing Ohm's law by setting up the appropriate circuit and observing the deflection of the point on the ohmmeter while varying the voltage and amperage requires a host of subsidiary, auxiliary hypotheses be true: not just assumptions about the presence of an electrical potential, or that the meters are functioning properly. What is assumed when Ohm's law is put to the test is the whole body of physical and electrical theory that, first, underwrites the construction and reliability of the meters; second, enables us to alter the amperage and voltage; third, assures us we can ignore certain forces acting on the circuit; and, fourth, adjusts for other forces. In particular, trusting the voltmeter involves embracing Maxwell's equations, which describe how the electric field generates a magnetic field, which twists the needle on the meter's dial. Additionally, we must implicitly appeal to Newtonian mechanics, which governs the needle's resistance to a spring and its deflection of a pointer. Accordingly, all these assumptions, hypotheses, and background theories meet the test in a body, together with the law we set out to test. Science meets experience not sentence by sentence, but in large blocks of theories and laws, blocks that are themselves divided from others by only constraints of practical manageability. Adopting these constraints constitutes substantial contingent theoretical commitments.

Suppose, now, that in our test of Ohm's law the meters do not read as the law predicts. Where does the fault lie: What proposition is falsified? Ohm's law? The assumptions about the construction and reliability of the meters? The assumption that there are no relevant intervening forces, or that they can be neglected? Are Newton's laws or Maxwell's equations at fault, or is the special theory of relativity that lies behind them? Of course it will be replied that none of these wider theories is thrown into doubt by such a test. Good sense directs that we check the wiring, the conductance of the metal it is made of, the springs in the meters, etc. So far as practical matters are concerned, once defects at this level are excluded, it is Ohm's law that would be suspect. But so far as matters of *strict* falsifiability are concerned, we see there is no such thing. For a disconfirmation does not point the finger at one particular statement under test; there is no one statement under test, for the entire conjunction of propositions is required for the prediction that fails. We are free to give up any one of the conjuncts and preserve all the rest. And this is not a mere matter of logic; the actual practice of scientists interpreting their data often reflects this freedom. Indeed, the most radical of scientific revolutions results from a scientist finding the fault to which an experimental anomaly points deeply in the center of a research program, instead of at its peripheral assumptions about the accuracy of measuring instruments.

How deeply can the falsification of a test, or of several of them, point? In the history of science it has certainly pointed at least as far as the falsity of Newtonian physics and its "philosophical" assumption of causal determinism. The discovery of the irreducibly random phenomena of radioactivity in effect falsified the belief behind Newtonian mechanics that every event has a cause that produces it in accordance with strict and exceptionless laws. Quantum mechanics rests on the rejection of a Newtonian principle that physicists and philosophers spent two hundred years attempting to prove as a necessary truth of metaphysics. In fact, difficulties in reconciling quantum mechanics with the most fundamental aspects of physical theory and its mathematical structure have led to the questioning of even more central and more "metaphysical" assumptions. In particular, some philosophers and physicists view the Heisenberg uncertainty principle of quantum mechanics as good reason to surrender the logical principle of bivalence, that every meaningful proposition is either *true* or *false*. Even more radically, responsible physicists have held that recent experiments require either the surrender of quantum mechanics or the "metaphysical" thesis that there is a world of enduring physical objects that exist independently of our knowledge of them. If these two proposals are coherent, then the experimental evidence that tests quantum mechanics can lead us to surrender, for factual reasons, principles of logic and mathematics we supposed to be necessarily true, and metaph 'cal theses Positivists supposed to be without empirical significance.

If any proposition can be surrendered as a result of a falsifying experiment, and if in the actual history of science the most central and firmly held of our beliefs have sometimes been surrendered, then we cannot identify propositions as necessarily true – as propositions we embrace *come what may* – that are known a priori. We cannot draw a contrast between such statements and contingent factual propositions – statements that may or may not survive attempts at falsifications – and so have scientifically significant empirical content. Similarly, any proposition, no matter how apparently factual, no matter how apparently vulnerable to falsification, can be preserved in the face of any possible falsifying experiment. We may in all consistency maintain that the earth is flat, attributing all apparent evidence against this belief to the falsity of one or another of the auxiliary assumptions that, together with it, are jointly falsified in photographs of the earth taken by an astronaut. Similarly, claims that Positivists stigmatized as pure metaphysics may also be surrendered in the aftermath of a falsifying experiment. Is the thesis of thoroughgoing universal determinism one of metaphysics? Is it scientifically empty speculation to assert that every event has a cause? It has certainly been a traditional thesis of philosophy, and yet it is one that has certainly come to be doubted as a result of the discovery of quantum-mechanical phenomena.

If we are to conclude that quantum phenomena have falsified metaphysical determinism, then we must conclude that metaphysical principles are testable after all and therefore cognitively significant. The only way to deny this power to experiment and observation is to deny that they ever falsify any single proposition at all. Either way, falsifiability no longer distinguishes between meaningless metaphysics and factual science.

Testing Ohm's law involves adopting Maxwell's equations for electromagnetism, and adopting these involves buying into the relativistic electrodynamics that accounts for them. And behind this theory stands the post-Newtonian "world picture," the research program that has animated modern science since the seventeenth century. It would of course be fatuous to hold that all this is at risk when an experiment does not corroborate Ohm's law. Any concern that would give an experimentalist real pause must be livelier than this abstract possibility. Even a theorist need not lose any sleep over the furthest mathematical, conceptual, and logical foundations of modern science. But the theorist cannot hold them logically irrelevant to his or the experimentalists' day-to-day concerns, and he has assuredly taken sides on their truth. What is more, at least sometimes in the history of science, and the lives of scientists, these broadest theoretical concerns do take a serious turn – either because they are called into question or because they suggest a direction for research.

These conclusions provide cognitive legitimacy to the speculative philosophy from which the Logical Positivists thought themselves to have freed "real" science. The justification for eliminating or embracing such notions as Driesch's entelechy is no different in kind from that employed to assess claims about the existence of electrons, magnets, or virons. It differs from them by degree, and very great degree at that. But ridding biology of such notions is not after all a matter of applying some rule against useless metaphysics. For deciding on the existence or nonexistence of entelechies is nothing less than questioning the adequacy of competing embryological theories altogether. But because this question is surely not an excursion into cognitively meaningless speculation, it follows that disputes about entelechies are not scientifically idle after all. Driesch's vitalism or the mechanism it opposed are indeed metaphysical theories, but they do not stand apart from "real" science. For better or worse, they stand on a continuum from sheer speculation through research programs and grand unifying theory to general theory and special models, all the way across to particular empirical findings. Unpalatable as this conclusion may be for empiricist philosophers and empirical scientists, to deny it without providing a workable distinction somewhere along the continuum would be unprincipled dogmatism – a dogmatism that the Positivists and their students would not accept.

1.4. Philosophy of Science Without Positivism

The end of Positivism means an end to philosophy's proscriptions against either treading on the subject matter of the empirical sciences or engaging in empty metaphysics. For metaphysics can no longer be distinguished from theoretical science. And neither can be distinguished from logic, linguistic conventions, or their analysis. For the necessity and unrevisability that was supposed to mark these subjects also fails to distinguish them from science or metaphysics. Although the fall of Positivism frees philosophers (and scientists for that matter) to turn their attention to more exciting activities than the study of language, it also transforms the significance of the very study. It turns the linguistic and logical analysis Positivists produced into the kind of metaphysical and epistemological exploration of the foundations of science to which philosophy has traditionally attended. It reveals that the analysis of concepts is just metaphysics carried out under a different name.

This change is well illustrated in the philosophical problems generated by the apparent goal-directedness, or purposiveness, of living things. The *teleology* (from the Greek words for "ends" or "goals" and their study) of the animate world has always been a focus of philosophical debate. Vitalists held that the purposiveness of things could only be the result of special forces, like Driesch's entelechies; mechanists insisted that teleology was only a special and complex form of mechanical causality, ultimately to be understood through the application of physics and chemistry alone. Materialism is of course just as metaphysical a thesis as vitalism. So Positivism invoked a plague on both these houses and enjoined philosophers to turn their attention to the purely linguistic question of giving the *meaning* of characteristic teleological expressions of biology. A cottage industry sprang up, in which philosophers provided definitions of terms like 'goal,' 'purpose,' and especially 'function'; these definitions were in turn rejected by other philosophers on the strength of counterexamples – clear cases of teleology that did not satisfy the definition or, still worse, nonteleological phenomena that did; the result was a cycle of revisions, qualifications, and reformulations that elicited another round of counterexamples, and so on.

With hindsight, however, philosophers came to see that the question of whether teleological expressions are definable in nonteleological physical terms is really just the ancient debate between vitalists and materialists carried out under the guise of linguistic analysis. If teleological statements can be translated into nonteleological, causal ones, then teleological processes are causal ones. If there is no difference between the formal claim about translation and materialists' allegedly factual one that living systems are just physical systems, then the linguistic question is identical to the metaphysical question of whether vitalism or materialism is correct.

In fact, the distinction between linguistic, metaphysical, and methodological problems and empirical issues is groundless. Biologists' attempts to uncover the purely causal mechanism of an apparently goal-directed activity like photosynthesis may or may not succeed. If it does, then this may strengthen a materialist metaphysical view. It will certainly encourage the continued exploitation of a methodology of searching for causal mechanisms to explain teleological behavior. But, of course, success in any one area of investigation cannot establish the general claim that all purposive phenomena are really causal. Nor does it establish the universal propriety of the methodology of searching for such mechanisms. What would? Well, nothing can ever be *established* in science. Nevertheless, a cogent explanation of why this method works will certainly strengthen the confidence of one biologist's particular account of photosynthesis.

On the other hand, suppose no causal mechanism for some goal-directed phenomena is detected, despite great effort. Under such conditions, biologists would be within their rights to insist that, nevertheless, further industry – better experimen-