Philosophies of Nature*

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At the recent International Congress of Philosophy held in Vienna in September, 1968, Wissenschafstheorie and Naturphilosophie constituted two separate divisions, even though the papers submitted to each appeared to cover more or less the same range of topics. This separation between a "philosophy of science" and a "philosophy of nature" raises the question of what is meant by a "philosophy of nature" today. The label itself is an unfashionable one in Anglo-American philosophy. Historians of philosophy in this tradition are wont to assume that the "natural philosophy" of the Greek and medieval type finds its historical continuation in today's empirical science, leaving little or no residue of a specifically "philosophical" sort in the discussion of nature. There is no question, of course, about the existence of a "philosophy of science" which takes as its starting point the methods and theoretical results of the sciences, though estimates will vary widely among philosophers in the different major traditions of contemporary thought (analytic, existentialist, Marxist . . .) about its significance. But can one "philosophize" about nature today? Is not a more exact approach available? Is there an authentic area of philosophical discussion about nature which is distinguishable from empirical science on the one hand and philosophy of science on the other? In this essay, I propose to look at some of the reasons why this question is so hard to answer, and then to describe

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some of the affirmative answers currently given it. But before doing so, some preliminary clarifications are needed.

**What Is Philosophy of Science?**

Contemporary philosophy of science seems to involve two rather different sorts of enquiry. The first is the analysis of the methods of the scientist, with a view to discovering their epistemological and logical significance. What one attempts to illuminate here is the activity of the scientist. This can be done in either of two ways. In his *Analytics*, Aristotle attempted to provide a philosophy of science based on a prior theory of knowledge and of natural essence, which would serve as an ideal for the scientist. It bore very little relation to the actual procedures of the natural sciences of the day, to the procedures of Aristotle's own biology, for example, or to those of the astronomy of Eudoxus. Yet it had a considerable degree of plausibility, deriving from the general metaphysical and epistemological features of Aristotle's system, and from analogies with the highly successful axiomatic geometry of the time. This sort of philosophy of science can be called "normative," because it is supposed to serve as a norm for the scientist, suggesting to him, on grounds prior to the specifics of his own scientific inquiry, what he should aim at and how the main logical structures of his work should appear. This type of philosophy of science was dominant until well into modern times, until in fact there was a successful and methodologically intricate working science which defied normative treatment of this sort, and demanded investigation in its own terms. "Descriptive" philosophy of science, as we may call it, is

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1 For an analysis of the different categories of writing covered by the label, 'philosophy of science,' see my article "Recent Work in the Philosophy of Science," *The New Scholasticism*, XL (1966), 478-518.

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not much more than a century old, though the beginnings of it may be found in the seventeenth century.

Descriptive philosophy of science analyzes what the contemporary scientist actually does, as well as what he claims to do. It is thus a relatively empirical inquiry; if disagreement arises, resort must be had to what is actually going on in the science of the day. Special attention will be paid to the descriptive meta-language of science, much of it vague or even inconsistent in its ordinary usage. Concepts such as law, experiment, cause, validation, explanation, will be analyzed and sharpened so as to function in a complex theoretical framework describing what the scientist does in presenting something as "science." Some element of evaluation will inevitably be present at this stage; a "descriptive" philosophy of science fully carried through will not be exclusively descriptive. What separates it from a "normative" philosophy of science (which will not be exclusively normative either, but will ordinarily involve descriptive elements) is that the evidence for its claims is drawn primarily from the actual practice of empirical science rather than from some prior and more general philosophical theory of knowledge. It is implicitly assumed that this practice has been warranted by several centuries of success, and is thus worth investigating on its own merits.

But what is called nowadays "philosophy of science" has another facet too. Many high-level scientific theories, such as Newtonian mechanics or neo-Darwinian evolutionary theory, have far-reaching consequences for philosophy. The discussion of these consequences, the modification of older philosophical views in the light of these newer scientific results, this too can properly be called philosophy of science, though here science plays a rather different role. Specific scientific results, not the general procedures by which the results were arrived at, are the starting-point of this inquiry. And it terminates, as science itself does, in some kind of claim about the physical world,
about the nature of time or of interaction, for example. It differs from science in its reflective emphasis, in its reliance upon conceptual analysis, in the fact that its findings cannot be enforced in the immediate empirical manner (via inductive or hypothetico-deductive modes of validation) which is ordinarily characteristic of science.

The relationships between these two types of philosophy of science and a “philosophy of nature” are obviously going to be very different. The former, which we can abbreviate as PSM (philosophy of science: method and meta-language) makes no immediate claim about nature in the way in which the latter, abbreviated as PSN (philosophy of science: nature) does. Yet even PSM may be held to have some consequences for our knowledge of nature. For example, the notions of lawfulness and determinism in some of the early modern philosophies of nature derived primarily from the analysis of some central features of the PSM of Newtonian science. This may serve to remind us that the distinction between PSM and PSN is ultimately not as clear-cut as the discussion above might suggest. But for the purposes of this paper, the distinction between the two is a valid and indeed an indispensable one.

**Two Questions for the Philosopher of Nature**

When someone writes something he describes as “philosophy of nature” today, there are two questions he must be prepared to answer if his choice of label is to prove at all helpful in marking off his inquiry from that of others, from science on the one side and philosophy of science on the other. The first question is this: is it being assumed that a source of insight other than that afforded by natural science is available to the philosopher in his discussions of nature? The “strong” claim for a philosophy of nature is that it rests upon such an insight, so that it is in some sense prior to the constructions of empirical science and able, therefore, to pass judgement on
their adequacy, at least in some respects. Such a philosophy of nature can be called a "first-order" one (PN1), because the warrant on which it rests purports to be a "philosophic" one distinct from, and prior to, that of science. On the other hand, a philosophy of nature may be derived from, or at least be suggested mainly by, the theories of contemporary natural science. Its warrant is thus a "second-order" one; it rests upon an already-validated science. Such a second-order philosophy of nature (PN2) claims no source of evidence prior to or independent of empirical science. What it has to say about nature ultimately stands or falls with the validity of the science from which it derives.

This distinction between a PN1 and a PN2 is, in principle, a reasonably sharp one. The basis of the distinction is the type of warrant on which the PN is alleged to rest: it may be direct, i.e., not mediated by or dependent upon the findings of empirical science (PN1), or indirect, i.e., deriving its force from a science, itself in need of a further methodological justification (PN2). In some cases, however, as we shall see, those who do what they call "philosophy of nature" appear to rely on both sorts of warrant. They call upon the findings of contemporary science, and in addition they explicitly draw upon a broader epistemological or ontological position in their support. We shall call such a warrant a "mixed" one, and the philosophy of nature deriving from it a philosophy of nature of "mixed" type (PNM). The methodological status of PNM is ordinarily very complex indeed, because the two types of evidence must somehow be brought into relationship with one another. This will further imply, as a rule, that each will affect and be affected by the other, so that the philosophical warrant may in this instance be modified in the light of the scientific evidence. PNM does not, then, make the "strong" claim of autonomy for itself that PN1 does, although it does make the interesting claim that in constructing an adequate
world-view, use may be made not only of contemporary scientific
theories, but also of broader metaphysical views. A positivist
can allow a PN2, because the "philosophical" considerations
it employs are likely to be logical ones, intrinsic (it could be
claimed) to the subject-matter. But he cannot allow a PNM,
or a fortiori a PN1.

What is the point of this somewhat artificial distinction
between different types of philosophy of nature? Why should
the dependence or otherwise of the PN upon science provide,
of itself, a relevant basis of division? The answer is that the
lively controversy about the validity of a PN today can best
be handled in terms of this distinction. To put it very simply:
there is no disagreement about the possibility of a valid PN2;
there is some disagreement about the possibility of a PNM;
there is a very great deal of disagreement about the possibility
of a PN1. Those who affirm or deny the existence of a valid
philosophy of nature in the context of contemporary science
nearly always have a PN1 in mind. Likewise, those who affirm
or deny the existence of a "philosophy of nature" over and
above what is currently called "philosophy of science" (PSN,
to be specific) always have PN1 in mind. The issue of the exist-
ence and nature of PN hinges, therefore, around PN1. Those
who describe themselves as "philosophers of nature" today are
doing either PN1 or PNM. Those who restrict themselves to
PN2 will ordinarily describe themselves as "philosophers of
science," and no particular methodological challenge is raised
by their work.

PN1 makes a far more ambitious claim than does PN2. A
PN2 lays no claim to an autonomous source of insight; it there-
fore never appears as an independent corrective of empirical
science. But the defender of PN1 has to locate and justify
some pre-scientific mode of evidence which retains its integrity
over against the reconstruction of experience characteristic of
contemporary science. A PN1, if such there be, would also be
more interesting than a PN2, because it would provide an understanding of nature prior to, and thus to some extent, at least, unaffected by, developments in science. It might, in consequence, serve a normative role where science is concerned; it could, perhaps, provide the basis for a criticism of concepts or types of explanation utilized in a particular scientific theory. In this way, it might even aid the way to a major theoretical advance in science. The first question we must pose, therefore, to someone who describes his work as "philosophy of nature" is whether he means PN1, PNM or PN2. And if he means PN1, we must press him about the exact source and the epistemological reliance of the pre-scientific insight he is claiming.

The second question we must have ready for him is what he means by "philosophy," and what precisely marks it off from science. This question is especially acute for the defender of a PN2; PN1 is unlikely to be mistaken for science, because of its (allegedly, at least) quite different starting-point. The major problem of a PN2 (and to a lesser degree of a PNM) is why it should be called "philosophy" in the first place. Is it a matter of generality, of speculative character, of a lack of hypothetico-deductive warrant or of any ready means of empirical falsification? What precisely leads one to use the term 'philosophical' here? And how is one to justify this particular use of the term within a general framework of types of knowledge? If 'philosophy' means no more than the construction of categorial systems of higher generality than the usual scientific theory and thus less easily tested in an empirical way than the ordinary "scientific" claim, then there is no sharp distinction either of method or aim between a PN2 and the sciences of nature from which it takes its origin. There would thus be a continuum ranging from Boyle's Law to the general theory of relativity, shading over into "philosophy," presumably, about the time the latter is reached. There is a danger in such a case that what is put forward as "philosophy of nature"
will be nothing more than an extremely speculative categorial scheme whose applicability to Nature can neither be disproved nor supported in any of the conventional "scientific" ways, yet which seems to its originator to have a cogency of some vague intuitive sort sufficient to merit the apparently less exigent title of 'philosophy.' The problem of adequate criteria of demarcation is, therefore, crucial for those who speak of a "philosophy of nature." Even more pressing, however, than the definition of a methodological criterion characteristic of "philosophy," would be the justification of "philosophy," thus understood, as a worthwhile and methodologically respectable enterprise.

In the remainder of this paper, we will pose the two questions formulated above first to some major figures in the history of philosophy and then to contemporary philosophical schools. This seems, perhaps, the best way of answering the question: "What, after all, is the philosophy of nature?"

**Distinguishing "Philosophy" from "Empirical Science"**

Let us look first at some early theorists of science, none of whom situate the distinction between "philosophy" and "empirical science" quite as this would be done today. Aristotle's "physics" is part of his "philosophy," and though he distinguished mathematics and mathematically-expressed physics from "straight" physics, a distinction between "philosophy" and "science" would have made no sense to him. Thus in his writings, physics is identical with one part of the "philosophy of nature," biology with another, psychology with another. To qualify as "science," i.e., as knowledge in its stablest and highest form, an item of natural knowledge would have to find a place somewhere in a tight structure of syllogistic demonstration in which an intuitive group of natural essences played
the crucial role. Aristotle knew perfectly well that most of what he had to say in biology, for example, was far from being demonstrative in the way that true "science" required. But instead of making a distinction on that account between two domains of knowledge, one stable and capable of intuitive justification and the other approximate, empirical, contingent, he preferred to regard the latter sort of knowledge (of which he amassed so extraordinary an amount in biology) as a stage on the way to the first, not worthy therefore of a name and a methodology in its own right.

When Aristotle's physics was rejected in the seventeenth century in favor of the new mechanical science of Galileo and Newton, there was a tendency on the part of the defenders of the older view to treat their opponent's theories as a mere "saving of the appearances," by contrast with the "natural philosophy" of Aristotle which still (to their mind) provided the true account of Nature. This was the beginning of one catastrophically misguided notion of PN, which was nothing more than ancient physics no longer capable of justification in the face of challenge from its own descendants, but unwilling to give up its claims nevertheless. This is the sort of PN which has been described as "fossilized science," where an earlier, largely discarded, theory of nature is defended in the name of a "philosophy" whose methods of validation and mode of evidence are never quite clarified.

In Descartes, one finds the beginning of the separation between philosophy and empirical science that will become more marked in Kant and Hegel. Descartes' goal was to derive physics from the metaphysical starting-point of the Cogito, making use of the method of clear and distinct ideas defined in the Regulae and the Discourse. But he soon found out that the complexity of the natural world was far too great for any such program to have any hope of working. In the final section of the Discourse, he wrote, rather wistfully one feels:
I have first tried to discover generally the principles or first causes of everything that is or can be in the world ... (deriving them from) certain germs of truth which are naturally existent in our souls. After that I considered which were the primary and most ordinary effects which might be deduced from these causes, and it seems to me that in this way I discovered the heavens, the stars, and earth, and even on the earth, water, air, fire, the minerals, and some other such things ... Then when I wished to descend to those which were more particular, so many objects of various kinds presented themselves to me that I did not think it was possible for the human mind to distinguish the forms of bodies which are on the earth from an infinitude of others which might have been so if it had been the will of God to place them there, if it were not that we arrive at causes from effects and avail ourselves of many particular experiments ... The power of nature is so ample and vast, and these principles are so simple and general, that I observed hardly any effect ... which could not have been deduced from the principles in many different ways; and my greatest difficulty is usually to discover in which of these ways the effect does depend on them. As to that, I do not know any plan but to try to find experiments.ª

It is worth quoting this famous passage in detail because it set the tone of one of the two major traditions of PN for over two centuries. Notice that metaphysics is held to be able to provide a general schema of Nature from its own resources. It is only when one gets into the particularities of the kinds of animal and mineral that the method of the Discourse (i.e. the "philosophical" method) appears to fail, and one has to have recourse to experiment to discover which of the possible states of affairs is in fact realized. The transition from the epistêmê or necessary principles and theorems proper to the philosopher only occurs when one has to fall back upon the empirical evidence of experiment. There is, thus, for Descartes a philosophy of nature which is "philosophical," in that it provides stable and certain knowledge derived from a rational analysis.

ªDiscourse on Method, in The Philosophical Works of Descartes, ed. by Haldane and Ross (Cambridge, 1931), vol. 1, p. 121.
of "certain germs of truth naturally existent in our souls." This PN is also relatively specific about Nature, since it can provide a mechanics, a cosmology, and a physics of the elements. It is contrasted with an "empirical" science, whose basic warrant is experiment rather than self-evidence.

One finds the same distinction throughout the rationalist tradition, notably in Kant, who distinguishes "physica pura" (which is synthetic a priori in character) from "physica empirica" (which, because of its contingency, can only be derived inductively from the phenomena).4 Newtonian mechanics belonged to physica pura because of its intuitive force and apparently a priori character. But the laws of gases (like Boyle's Law), of materials (like Hooke's Law), and a host of other specific phenomenal regularities had to be left to experiment, both for their discovery, and for their ultimate warrant. Kant's cut between PN and empirical science would, therefore, not coincide with the cut between philosophy and science that we recognize today, because he would place the more general parts of what we would now regard as "science" (notably mechanics) on the side of philosophy, i.e., on the side of synthetic a priori knowledge. Hegel's division is of the same kind, since he too wishes to separate off a necessary and intuitive categorization of Nature from the specific contingent natural regularities which the scientists of his day were discovering in great number. So he situates the more general knowledge of Nature in mechanics, optics, and astronomy, on the side of Naturphilosophie, just as Kant had done, though (as we shall see) from an entirely different methodological standpoint.

Meanwhile, over against the rationalist tradition a very different idea of the relationship between philosophy and em-

pirical knowledge was beginning to crystallize. One finds no grounds for a distinction in Newton between "philosophy" and "empirical science;" all systematic knowledge of nature ("natural philosophy") is one, though some of it is much more contingent than are its most general principles. Unlike Aristotle and Descartes, Newton did not find it necessary to mark off an ideal of scientific knowledge which would begin from intrinsically self-evident first principles. It is true that he does not explicitly dissociate himself from this ancient quest; it is also true that his mechanics came to possess for him (though not to the extent that it did for many of his later followers) something like a conceptual necessity. He is not nearly as tentative in his descriptions of his science as were Boyle and Huyghens about theirs; they qualified their claims with phrases like "temporary superstructure," "probability little short of certainty," and so forth. But it would nevertheless be risky to try to separate off a "philosophy of nature" in Newton's work on the score of some Cartesian-sounding claim to apodictic character. There are too many places where he recalls the necessarily tentative and revisable character of any science whose primary warrant is experiment.

So that when we speak of a "philosophy of nature" in his writings, it is not because one part of his work is clearly marked off by its methodology, nor by any a priori certainty claimed for it. Rather, we mean to single out the very general categories of nature and of natural explanation that he fashioned so skillfully into an inter-connected whole. The distinction between PN and the science of which it is the speculative prolongation or the categorial framework is thus not at all sharp in the empiricist tradition, of whom Newton may be taken to be the prototype. It is still easy, of course, to separate

off discussions of scientific method, but these are philosophy of science (PSM), not PN. The scientists themselves will find no reason to label part of their effort as "philosophical" and part as "scientific." And the notion of philosophy entertained by the empiricist followers of Locke and Hume will give no grounds for supposing that a separation between a "philosophy" of nature and a "science" of nature is worth making.

This point needs stressing, because it suggests that the notion of a separable "philosophy of nature" is, to a large extent, a product of the rationalist (and later the romantic) tradition. This goes a long way to explaining the antipathy in English-speaking countries to any enterprise under this label. And it reminds us that when we speak of the "philosophy" of nature of scientists after Newton or of philosophers outside the rationalist and romantic schools, we must be careful to moderate the sense of the term "philosophy" to an appropriately muted level.

**First-order and Second-order Philosophies of Nature**

Having put one of our two questions to some of the major thinkers in the history of "philosophical" thinking about nature, let us now go on to the second. Do any of the five discussed above have a first-order PN? This question is not without ambiguity when put to the earlier authors, because when one asks whether they have a PN which is prior to science, it must be recalled that they did not have a developed empirical science in the sense in which we do. This is especially true if we go back as far as Aristotle, long before the methodological components of philosophy and science had begun to separate. Nevertheless, there is a point in asking whether Aristotle's Physics would define a PN in some way prior to what we would now call "empirical science." Several answers could be given. In Book One of the Physics, Aristotle provides an analysis of the necessary and sufficient conditions for the
correct use of the word "change," noting that something can be correctly said to "change" only when there is a subject of which the change can be predicated, and a predicate which at first cannot, and later can, be correctly attributed to the subject. The most interesting feature of this well-known development of the matter-form doctrine is that it rests not upon a direct appeal to the nature of change but upon the way in which change is spoken of: the evidence in support of it (Aristotle goes out of his way to stress) is a linguistic structure or more exactly, the conditions for the correct use of a general linguistic structure. It is true that in a couple of passages a special problem is mentioned concerning "unqualified" change, one which raises a difficulty for the matter-form analysis he has just given, since there appears to be no linguistically-designatable subject of such changes. But apart from the few brief allusions to what later came to be called "primary matter," it can be said that the PN of Physics I is a PN1, resting upon an appeal to some basic features of ordinary language.\(^6\)

Elsewhere in the Physics, and in his other works on Nature, Aristotle more often relies upon very generic concepts abstracted from everyday experience, whose interrelations he tries to trace and to sharpen. When he speaks about the "natural" motion of fall, for instance, and of its dependence upon weight and the density of the medium, the evidence for his claim lies in our ordinary experiences of force and resistance, plus a sophisticated ontology of cause and nature. Is this a PN1? It seems that it must be left unclassified because it could later develop into a PN1 or a PN2 or a PNM, when faced with the techniques of developed empirical science. Aristotle could regard the fundamental conceptual revisions of seventeenth-century physics either as a continuation of the movement away from primitive experience already evident in the Physics (in

\(^6\) For a fuller treatment, see the Introduction and the essay on "Matter as a Principle" in The Concept of Matter, supra.
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which case he would probably arrive at a PN2), or more likely he would (like his seventeenth-century followers) take the Galilean idealization and quantization to be a removal from nature into pragmatic hypothetical construction. In the latter event, he would then have to ask what the status of his own Physics would be: a rival of the Galilean physics or a sort of pre-scientific PN1, defining the prior conditions of possibility for a natural science. The decisions for the modern Aristotelian between a PN1, a PN2, or a PNM (a decision Aristotle himself never had to face) will rest largely on his handling of the notions of experience and nature. But to develop this point further would lead us too far afield.

Cartesian physics can be regarded as the last great attempt to carry through the methodological program of Aristotle’s Posterior Analytics. If there are degrees of “firstness” among first-order philosophies of nature, Descartes’ “physics” is surely the PN1 par excellence. It equates, on metaphysical grounds, material being with the object of geometry; since matter and extension are equivalent, Descartes is forced to construct a mechanics without mass, and therefore with no way of accounting for density differences or for even the simplest facts of dynamics. Since extension is matter, a strict vacuum is impossible; all action must therefore be by contact. Thus his terrestrial mechanics is one of percussion, and his celestial mechanics has to make use of vortices and the like. His laws of percussion run counter to experience at every turn; his vortices have no operational “hold” of any sort. They cannot predict, nor can they be quantitatively specified in any way. His physics was thus nothing short of catastrophic from the empirical standpoint, yet such was its conceptual elegance, its “philosophical” or “explanatory” rightness, the strength of its deductive link with a prior metaphysics, that it dominated European science for almost a century.

Here we have the first major type of PN1, which we can
call the "Cartesian" or rationalist type. It derives its warrant from its metaphysical starting-point rather than from any specific appeal to experiment or even to everyday experiential concepts. It is alleged to be prior to experimental science and to be normative in its regard, dictating its general categories as well as its methods of approach. When we call Cartesian physics a PN1, several possible ambiguities must be avoided. Descartes himself did not mark it off in this way; for him there was just one science of mechanics—his. We could use the expression 'philosophy of nature' of his work in either of two senses, as noted in the last section; we could contrast his "physics" with the specific details of contingent experimental science, as he himself does. In this case, the distinction between his PN1 and empirical science would be primarily one of generality. Or we could stress that his PN1 derives from an a priori standpoint in the Cogito, "philosophical" not because of its generality but because of the sort of evidence it is supposed to rest on. In this event, the Cartesian PN1 becomes a competitor of more empirical modes of establishing specific physical theories, and not simply complementary to them at a higher order of generality.

Descartes himself never seems to have taken the dangers of such potential conflict seriously. He dismisses those of his scientific contemporaries who, like Galileo, work from the empirical level upwards, and constantly implies that a PN1 like his which works from the top down can always override the hypotheses and even the empirical findings of the lower order. Here we have, then, the rationalist PN1 in its most ambitious and also its most ill-fated manifestation. It is distinct from both metaphysics and empirical science; from the former, because though derived from it, it is specifically concerned with the motions of matter; from the latter, because it has a conceptual necessity deriving from its a priori starting-point. Descartes concedes, as we have seen, the existence of a "non-
metaphysical” experimental science of nature where the more detailed structures of the universe are unravelled. But he would have hotly denied the possibility of an altogether experimentally-based science at a sufficiently high order of generality to constitute, say, a mechanics or an optics, and thus to become a competitor with his own a priori mechanics and optics. He would have had to exclude such “sciences,” presumably on the grounds that the only adequate warrant for a general account of Nature is a priori metaphysical one which gives the account a demonstrative necessity that no revisable, experimental science could match.

Here then was the arena prepared for the great debate between Cartesians and Newtonians over natural philosophy. Here too was a PN which was held to be prior to empirical science in the strongest possible sense of over-riding it wherever conflict between the two occurred. Descartes never foresaw the sort of science Newton presented in his *Principia*; had he done so, would he have modified his methodological bias against the kind of evidence precisely controlled experience can provide? At any rate, Newton’s Cartesian contemporaries were unwilling to do so, and thus the notion of “philosophy of nature” as an a priori speculative account of Nature with no proper empirical foundation was firmly planted in the modern mind.

Newton’s PN is usually thought of as a second-order one, although (as Burtt and others have stressed), it was probably indebted to the neo-Platonist metaphysics of Newton’s Cambridge teachers and colleagues, More and Barrow, for some, at least, of the inspiration for its doctrines of space and time. Yet the absolute space and time of the General Scholium are conceptually indispensable for the consistency of the three famous “Axioms;” the first axiom requires a mode of time-measurement that presupposes an “absolute” (causally undisturbable) motion as its base; the second axiom suggests that if forces are to be thought of as “real,” then there must be an
absolute spatial reference-frame for estimating the accelerations relatively to which force is to be measured. So that here the categorial framework is closely tied to the empirical theory; in fact, the two cannot be sharply separated. One can speak here of a "philosophy of nature" (as we have already noted) in the sense of a set of very general mechanical concepts and a definite view of what it is to "explain" a motion; such a "philosophy" to some extent guides Newton in his construction of the intricate and elegant network of the *Principia*, but to an equal extent is itself reached by the very attempt to formulate a consistent empirical mechanics. And its major warrant was twofold: predictive success of the mechanics it inspired, and the logical coherence of the construction itself.7

One has to be careful not to cast Newton's work (or any other major innovative work in science for that matter) in too empiricist a mold. It is not as though the only PN discernible in his writings is logically wholly subsequent to his science, as though he went to work on his calculations with no sort of conceptual expectations deriving from sources other than the observational results he was working with. Mach and some of the early positivists made a heroic attempt to read the history of science in this way, but they did not even begin to make sense of what really happened in the major moments of conceptual revolution. Newton's insight drew upon resources as disparate as his own bodily experience of force and resistance, his appreciation for the "God of space" in Henry More's natural theology, and above all his "feel" for logical consistency, conceptual adequacy, and aesthetic coherence. His PN is thus of a more "mixed" character than the later empiricist account of the nature of science tended to allow.

Ought we then characterize it as PNM? There is a special difficulty about applying our distinction between types of PN

7See "Empiricism and the Scientific Revolution," supra.
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to the work of scientists who do not advert explicitly to the
presence in their work of considerations methodologically suf-
ficiently diverse from the main lines of their empirical science
to be titled under a different heading. In his book, Newton's
Philosophy of Nature, H. S. Thayer put together passages
of three different sorts from Newton's writings. There are
comments on problems of method (PSM); there are sections of
the Principia, the Optics, and of his correspondence that have
specific consequences for the philosophy of nature (PN2).
Finally, there are passages where one can discern the influence
of prior metaphysical views on Newton's formulation of his
theory of mechanics (PNM). Though all three types have in
common some sort of broadly "philosophic" character, it seems
important for many purposes not to blur the methodological
distinctions between them.

If a scientist does not himself prescribe for us what his
"philosophy of nature" is, serious ambiguities may arise in
our effort to associate a PN with his work. One can look in the
direction either of a PNM or a PN2. The former will include
all those autonomous "philosophical" considerations that
played a role in the construction and validation of the system.
It can be argued that a world-view, partially, at least, "philos-
ophical" in origin, implicitly guides the expectations of the
scientist, and frequently plays a part in determining whether
a specific scientific theory will be adjudged a "good" one or
not. If this be so, then a PNM will be implicit in any piece
of scientific work. But since the scientist himself is unlikely
to specify it in any detail, the attribution to him of a specific
PNM must inevitably be quite tentative. The search for a PN2
may not be any easier. What one seeks here are the "philoso-

*New York, 1953.
*I have developed this point more fully in "Creativity and Scientific
Discovery," in Freedom and Man, ed. by J. C. Murray (New York, 1965),
pp. 105-30.
phical” consequences of the scientific theory, “consequences” not usually in any strict deductive sense but rather in the sense of resting on the theory for whatever warrant is claimed for them. The danger here is of reading in, of attributing a PN2 to the scientist which might indeed be supported by his theory but of which he himself was unaware. To say that Newtonian mechanics suggests a particular PN2 is one thing, but to show that this PN2 was in some meaningful sense Newton’s own is an entirely different affair. The attribution of a PN (whether PNM or PN2) to a scientist who has not himself at least partially specified it is thus quite clearly a hazardous and ambiguous affair.

Kant’s philosophy of nature is a first-order one, though of quite a different kind from that of Descartes. It is an ingenious attempt to transform the PN2 of Newtonian mechanics into a PN1 which would stand on its own, without the need for specific experimental support. Kant’s “physica pura” is asserted to consist of synthetic a priori truths; the general categories of the Newtonian world are established via a transcendental deduction. Kant was much too good a physicist to want to construct, as Goethe and other eighteenth century Romantic philosophers had attempted to do, an alternative of a more “philosophical” (i.e., humanistic) sort to Newtonian physics. Rather, he was so convinced of its merits and of the permanence of its insights, that he felt the need of a metaphysics which would safeguard it against the dissolvent of Hume’s phenomenalism. The first Critique can best be read as an attempt to find a metaphysics for Newton’s science, or (in our terms) to transform the vulnerable PN2 of Newton into an invulnerable PN1.10

It might be objected that Kant’s PN is really a second-order one, since it is so totally dependent on Newtonian science for

10 For an excellent discussion of this, see G. Martin, Kant’s Metaphysics and Theory of Science (London, 1955).
its conceptual structure. But to assert this would be to mistake what it is that ultimately separates a PN1 from a PN2, or for that matter rationalism from empiricism. It is true that Kant takes over the entire top-level of Newton's physics. But he then tries to convert it into an interlocking set of necessary truths, which immediately makes his philosophy of nature a first-order one. If the argument of the first Critique be accepted, the reason for accepting the top level of Newtonian mechanics as "science" is no longer the original empirical warrant on which Newton himself had mainly relied, but rather the insight that the Newtonian categories are the subject's necessary way of structuring his phenomenal world. The ontological basis for Kant's PN lies in the subject, not in the world as it had done for Aristotle. His PN is derived from a metaphysical starting-point just as Descartes' had been, but unlike Descartes, he had the main lines of his physics fixed in advance: he was aided far more than he admitted by his prior knowledge of just what it was that the transcendental deduction had to uncover. So that his PN, unlike that of Descartes, did not produce a rival mechanics, nor did it introduce any new tensions between the findings of philosophy and those of empirical science. His PN was simply an overly-ambitious transformation of the main lines of the science of his day into a necessary structure of the most general knowledge of Nature.

Hegel's Naturphilosophie is (as indeed its author claimed it to be) the climax of the development we have been tracing. Hegel was the first to speak of a separate domain called "philosophy of nature" which would be distinct from metaphysics, from empirical science and from philosophy of science (as we would now call it). Our task is thus much easier here than it was with his great predecessors, none of whom use the phrase "philosophy of nature" as we do, and whose "philosophy of nature" must therefore be approached in a roundabout way. The Cartesian split between philosophy and em-
pirical science, which had been greatly intensified by the Romantic reaction against the apparently unhuman categories of the Newtonian world-view, comes to its sharpest focus in Hegel's work. He sees in Spirit the source of all intelligibility; matter is the locus of negation only. Thus it is through the structures of Spirit that we must seek to understand Nature. Philosophy of nature must, therefore, be phenomenological in its method; it must turn inwards rather than outwards, since it is in subjectivity rather than in the raw facticity of experiment or induction that the clues to all understanding, including the understanding of Nature, must be sought.

Instead of taking over the categorial structure of Newtonian physics as Kant had done, Hegel's PN constituted an emphatic rejection of that structure. Its abstract idealized quantitative concepts are remote from the qualitative immediacy in which man "naturally" knows his world. Consequently, Hegel viewed Newtonian science as a step away from reality, pragmatically justifiable perhaps, and certainly interesting in its own right, but no substitute for the "real" knowledge of Nature its method of abstraction had forced it to abandon. Naturphilosophie was thus conceived by its originator not as arising from, nor even as complementary to, the empirical science of his day, but rather as a critique of that science, providing an alternative science of Nature much more basic than any experimental-mathematical method could attain. Here we see Romantic philosophy at its most normative and imperious.

All three types of PN1 discussed above, the Cartesian, the Kantian, and the Hegelian, run into serious difficulties. The first and third tend to become involved in conflict with empirical science, and have to rely on a very dubious methodological claim to an autonomy and a primacy for a "philosophical" knowledge of Nature, sufficiently specific to challenge the physical theories of the experimentalist. The second goes in just the
opposite direction by taking empirical science too seriously, and by freezing it at one contingent stage of its development into a structure of immutable truths. It must be admitted, therefore, that the precedents in classical philosophy for a first-order philosophy of nature are scarcely such as to inspire confidence. If there is a valid PN1, it must surely be found along lines somewhat different from those followed by Descartes, Kant, or Hegel. A PN2, on the other hand, appears to run into far fewer problems of method even though its results are much less strikingly "philosophical," at least if we take philosophy to be a way of attaining knowledge that is beyond the reach of limited empirical enquiries.

CONTEMPORARY PHILOSOPHIES OF NATURE

In the remainder of the paper, we shall glance at the work of some recent writers who have laid claim to constructing a "philosophy of nature." No attempt will be made to give a complete review, nor to analyze any of these philosophies of nature in detail. Rather, the distinctions already drawn above between various classical approaches to the problem will be utilized in order to draw up a simple schema of the contemporary scene in philosophy of nature.

From what has been said, it is clear that the two major types of "pure" PN1 are the Kantian and the Hegelian. The Cartesian one was viable only in an age when empirical science had not yet attained confidence nor a high degree of complexity. A quick survey of the philosophy of the last few decades shows at least one major Kantian PN1, and quite a number of Hegelian efforts, at least one of them a major one. The Kantian philosopher of nature was, of course, Sir Arthur Eddington whose posthumous Fundamental Theory came closest to doing for relativistic mechanics and even for atomic theory what Kant did for Newtonian mechanics in the Critique of Pure Reason. In a series of well-written and highly plausible
earlier works, which enjoyed great popularity in the 'thirties and the 'forties, Eddington had attempted to show that a careful epistemological analysis of the complex nature of measurement, involving as it does dual reference-frames of instrument and observer, yields the four "cosmical constants" around which the Universe is built (the number of particles in the universe, the radius of the universe, the time-structure constant in spectroscopic theory, and the ratio of the electron and proton masses). Like Kant, Eddington knew the answers before he set up the sophisticated and extraordinarily ingenious analysis of the experimental observer which was designed to lead to these answers. And his "freezing" of the science of his day led to exactly the same results as did Kant's earlier. One by one the "constants" were found to have a value somewhat different from that originally assumed by him; the final blow was the discovery of the meson which at one stroke populated the universe with an immense new array of mysterious semi-particles. The Kantian seems to be ever doomed to see his "necessary truths" become historical curiosities.

The most influential contribution to the Hegelian tradition of Naturphilosophie in this century was probably that of Husserl, who in his final work, Die Krisis der Europäischen Wissenschaften und die Transzendentale Phänomenologie, undertook a critique of physics of what he called the "Galilean style," arguing that the PN associated with it is seriously deficient, and that a new physics which will do justice to the requirements of an adequate phenomenology is needed. He did not himself attempt to construct a PN1, contenting himself with an epistemological argument in favor of renewing the Hegelian effort to find one. Husserl was well-aware of the shortcomings of

Hegel's own *Naturphilosophie*, but agreed with him in holding that a phenomenologically-based PN can be normative in imposing its own categories upon the theories of empirical science. So far, no full-scale effort has been made to carry this ambitious program through. Bergson, of course, tried to do so for the notion of time. And some recent writers have interpreted the continuing controversy concerning the conceptual structures of quantum mechanics as an encouragement to reformulate quantum theory along more phenomenologically-acceptable lines.\(^{12}\)

One of the major criticisms of the various existentialisms of the past forty years has been their almost universal failure to provide an adequate account either of Nature or of the contemporary science of Nature. This neglect stems partly from an exaggerated humanism, as though man could be understood apart from the rest of Nature by employing the comfortable categories of subjectivity, and partly from an acceptance of the Hegelian-phenomenological critique of Newtonian science. The major existentialists (Sartre, Marcel, Heidegger . . .) have tended to regard physical science as a means of power over Nature, a pragmatic device, rather than a mode of insight. Their preoccupation with the negative effects of technology on man often leads them to confuse technology with theoretical science. Jaspers has been one of the few representatives of this powerful modern school who has made an effort to come to grips with physical science as a cognitive structure. The *Lebenswelt* does not have the tightness of texture that would be necessary if it were to support the sort of PN that contemporary science requires. Even though existentialism does not concern itself with the structures of the non-human world except insofar as they become *pour soi*, structures-for-man, this leads to difficulty on occasion where the sciences of man are concerned. Merleau-Ponty makes use of the findings of abnormal psychology and

neuro-physiology, for instance, but he nowhere provides a philosophy of science that would legitimize the implicit interpretation he adopts of the import of empirical theory in psychology or physiology. In short, then, we can say that though the existentialists would lean to first-order philosophies of nature of the Hegelian type, none of any consequence have in fact been constructed by them. It takes a good deal of courage, not to mention skill, to construct phenomenological alternatives to relativistic theories of space-time and quantum theories of the subatomic. The pour soi of orthodox existentialism is not a very promising starting-point for such an effort.

In listing the "pure" types of PN1, we omitted the Aristotelian, simply because (as we have seen), Aristotle's physics is prior to the sorts of distinction between philosophy and science that have made the problem of a PN so complicated today. But there are at least two sorts of philosophizing about Nature going on at present that could reasonably claim continuity with the Aristotelian effort. First is the attempt on the part of some of the British "ordinary-language" school to unveil certain pervasive structures of Nature (e.g., the "flow" of time, the space-time individual) by referring to structures of language. The underlying assumption of this approach would seem to be that a universal or even a frequently found linguistic structure must have an ontological counterpart, otherwise it would have no pragmatic support and would tend to be eliminated. The postulated structures are of such an unspecific sort that they can plausibly be asserted to be "prescientific," a presupposition of the language in terms of which the most general features of our experience are expressed, before the exigencies of empirical science force a sharpening or even a re-structuring of language upon us. There is a clear affinity

between this sort of PN1, whose warrant is ordinary language-use at such a level of generality that it can plausibly be asserted to be pre-scientific, and the hylemorphic doctrine of Physics I.

Some neo-Aristotelians have adopted a somewhat broader basis for a PN1. They would argue that even though Aristotle made no distinction between PN and empirical science in his work, the means to do so in retroactive fashion are readily available. Much of the analysis of the Physics is at such a high level of generality—the discussions of motion, of cause, of chance, for example—that it rests upon the "common core" of human experience in a way that modern science has in no way invalidated (so the argument runs). In this view, there would still be a way of constructing a PN1 on the basis of man's ordinary pre-scientific experience of nature; the expectation is that one would in this way come up with something like the general categorial structure of Aristotle's Physics. This suggestion was one frequently made in the later seventeenth century by the scholastic natural philosophers who saw the need of dissociating themselves from the repudiated Aristotelian scientific doctrines in mechanics, while somehow retaining the more "philosophical" part of the Physics; in this way they could insulate it against future catastrophe by confining it to generalities of the widest sort, like the matter-form-privation distinction. It is likewise among scholastic writers that this neo-Aristotelian PN1 has been popular in this century: Renoirte, Maritain, Van Melsen, De Koninck, to name only a few. These authors disagree on many important epistemological issues, but they are at one in supposing that a PN1 could be constructed, of a broadly Aristotelian sort, on the basis of pre-scientific experience.

14 The phrase is Mortimer Adler's in his recent book The Conditions of Philosophy (New York, 1965), where a "first-order" philosophy of nature is strongly defended. Adler's distinction between "first-order" and "second-order" philosophy of nature has a quite different basis than the one we have defined here.
The popularity of this approach has recently been on the wane. Among the reasons for this, three may be noted. It is difficult to justify in a rigorous epistemological way the isolating of the structures of pre-scientific experience at no matter how high a level of generality; it would seem that the precisions worked by science on key natural categories like force and time have got to be taken into account in any adequate PN. And the notion of a "common core of experience," independent of linguistic or cultural changes, runs into a host of difficulties, as Feyerabend and many other analytic philosophers have recently been stressing. Furthermore, there is a danger that the pushing of PN to higher and higher generality will end either by making it completely trivial, or by making it an analysis of predication about nature instead of nature. Thus, instead of "substance" we end with "subject of predication," "whatever is being talked about;" instead of "form," we have a "predicate," "something that is said of something else." It is easy enough to make this sort of analysis invulnerable, but also totally vacuous. Finally, some of the crucial Aristotelian concepts, notably those of substance and primary matter, have been in serious ontological difficulties for a long time past; if they are to be rescued, it seems fairly clear that an appeal to pre-scientific generalities will not suffice. The concepts of a non-vacuous PN cannot, it would seem, be sealed off from the questions raised by specific scientific advances.

If one wishes to reconstruct a PN of a generally "Aristotelian" sort today, it could be argued that what we have called a "mixed" PN would some closest to what he was trying to accomplish in his day. In a work like Errol Harris' The Foundations of Metaphysics in Science, philosophy and science go on side by side, so to speak, each supporting the other so that one cannot validly distinguish between them in

classifying the final resultant. It is a genre of argument that does not appeal to the average modern scientist who is likely to feel that constant conceptual refashioning and enquiries into the epistemological implications of every step taken are not going to be conducive to empirical results. But Harris argues that far-reaching philosophical issues hang from almost every statement made by the scientist, and that the great modern theories of relativity, of quanta and of evolution, contain the germs of a cosmology which is never made explicit in science, and thus has to be elucidated by the philosopher.

What is meant by “philosophy” here? It would seem that there is not much difference between what Harris does and what Einstein does in his theories of relativity. Yet we would classify these latter as “science,” not as “philosophy.” The question is not an easy one to answer, though even a superficial glance at Harris’ work somehow leads one to think of it as not a work of “science” in the ordinary sense. It would seem that it is thought of as “philosophy” mainly for two reasons. First, it dwells on matters of conceptual consistency, on problems of method, on unsuspected presuppositions, far more than would an account of the same topic by a “scientist.” Second, it raises issues that can be resolved only by constructing a more general theory of knowledge or an ontology. Because of this, it can be properly classified as PN rather than as science, and it is of “mixed” warrant, since Harris would call in for support both the empirical data underpinning the various theories whose results he generalizes, and also the metaphysics whose internal coherence and adequacy give it a claim that transcends that of the scientific theories from which it originally took its origin. This is not a PN2, then, strictly speaking, because explicit “philosophical” criteria are alleged in its support, criteria that are broader than (though not indeed, altogether dissimilar to) the criteria that are used in evaluating the run of scientific theories.
It may well be asked at this point whether the most general theories of science itself: the general theory of relativity, and the theory of evolution, for example, are not PNM rather than "science," in the narrower empirical sense in which we have been using this term. By this we mean that some of the criteria one is forced to call on when evaluating the theories seem to be necessarily "philosophical," in the sense defined above. It is possible to limit evaluation of these theories to a review of the empirical evidence, either inductive or hypothetico-deductive, that one can call in for their support. But this is certainly not what Einstein did: he relied upon considerations of conceptual coherence and of aesthetic appeal, and he attempted to relate epistemology and the ontology underlying his theory to a wider context. Had he done this with the emphasis and the thoroughness of Harris, it is indeed possible that his work would have been classified as "philosophy of nature," even though it was empirically testable through the verification of specific predictions.

This points up the fact that the major modern innovations in theoretical physics have been accompanied by meta-argument that can only be described as philosophical. Bohm's *Causality and Chance in Modern Physics* is surely not just another work of empirical science. The determinist-indeterminist controversy in quantum theory has involved issues of a properly philosophical sort, so that the contributions to it of Bohr, Heisenberg, Schrödinger, de Broglie, Vigier, and many others can properly be called a PNM, rather than just "straight" science. Scientists themselves, uneasy at the unusual complexity of the methodological issues involved in this controversy, have very often retreated to a neutral empiricist standpoint, saying that the issue must ultimately be settled in "purely empirical" terms, by that meaning verified or invalidated in terms of

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experimental predictions. Yet it is notable that scarcely any of
the main scientific protagonists in the controversy have been
content with this stance. Their adherence to the "indeter-
minist" or the "determinist" side is surely conditioned by
criteria that are much more on the side of "philosophy" than
of "empirical science." Whether the issue is or is not ulti-
mately settled in approved "empirical" fashion, the fact re-
mains that in the present stage of the debate it is quasi-
philosophical in character, and each "side" proceeds from a
quite definite PNM. If this be considered a defect, it is more
than odd that similar situations seem to arise in all the really
significant moments of conceptual movement in science, the
formulation of Newton's General Scholium being one other
notable example.

It may be objected that in this entire discussion we are
using the term 'science' in too narrow an empiricist sense
to begin with, thus transforming all "science" into PNM in
the sense of involving criteria that go beyond the immediate
empirical order. This objection has some force, and it should
remind us that there is no sharp distinction between PNM
and "science;" rather, one shades into the other. Yet even
though the boundary between the two is a hazy one, it is still
ture that the ordinary work of science can be defined quite well
in terms of inductive and hypothetico-deductive modes of vali-
dation that owe little or nothing to an explicit philosophical
viewpoint, other than a general respect for empirical evidence.
Whereas in times of "scientific revolution," or when the
scientific theory is of a sufficiently high order of generality,
"philosophical" considerations will inevitably enter into the
discussion. Whether the resultant will be called "science"
(like the General Scholium or the General Theory of Rela-
tivity) or "philosophy of nature" (like Harris' book), or
something in between (like Bohm's book) will depend entirely
on the relative emphasis placed on the "philosophical" and the
empirical criteria in stating the merits of the theory.
Ernan McMullin

Where ought one look for a PN2 today? We have surveyed the various types of PN1, and have just explored some of the problems of PNM. Is it worth distinguishing between PN2 and the PNM's above? It would seem so, although once again the boundary-line is not a clear-cut one. There are two sorts of context where the notion of a PN2 seems worth separating off. When Julian Huxley, for instance, elaborates a philosophy of evolution, its warrant appears to lie entirely in the neo-Darwinian theory which inspires it. It is not the case that Huxley's speculative evolutionary philosophy is required as a partial support for the neo-Darwinian theory from which he begins, nor is it the case (let us suppose, at least) that Huxley could call in other more properly "philosophical" evidence in support of his theory. In order words, one can reasonably say that the entire (or almost entire) warrant for his PN is the evolutionary science inspiring it. In this case, one can legitimately describe it as a "second-order" PN, logically subsequent to the scientific theory which originally inspired it. Such second-order philosophies of nature are legion: the myriad speculations of those whose imaginations are sparked by some new scientific advance, so that they leap to a speculative claim about Nature whose only real warrant is the original piece of empirical science.

The other sense in which we can, it would seem, profitably call upon the notion of a "second-order" PN, is when we disentangle the philosophical implications of a particular piece of science, say for example Einstein's equations of special relativity. We might say here that the theory shows that simultaneity of distant events is purely relative to the observer, a result of a quite unexpected kind. In describing a philosophy of time built on special relativity theory as a PN2, we mean, then, that the warrant for it is relativity theory alone.17 It

17 See, for instance, A. Grunbaum's work, Philosophical Problems of Space and Time (New York, 1963), which would be a straightforward PN2 (and PSN).
Philosophies of Nature

does not matter whether or not the discoverer of the theory (in this case Einstein) had this philosophical implication in mind as he worked. Nor does it matter whether broadly "philosophical" considerations affected the formulation of the theory, provided that the ultimate theory can be called "science" in the sense defined above.

Evolutionary Philosophies of Nature

We have left until the end the two commonest types of PN in vogue today: evolutionary philosophies of nature and Marxist philosophies of nature. They have one notable characteristic in common: they face in two directions, methodologically speaking, and the ensuing tension often leads them into ambiguity or even incoherence. It is not hard to see why the notion of evolution had such a profound effect upon philosophers. Plato and Aristotle had agreed in seeing in form the basic principle of intelligibility of the things of nature. The cosmic order was built around the continuing recurrence of form, embodied over and over in matter. To understand a nature was to grasp its formal element, to see how the normal activity of that nature fitted into the cosmic order generally. The forms themselves were regarded as given, as permanent features of the universe, and thus a permanent basis for science, understood as a stable knowledge of unchanging principles of activity. They were of the kind they were because this was what the order of the universe required. But there was really no way of answering questions like "why should there be oak trees in the first place?" or "why should oak trees be of precisely the kind they are?" The form, oak, was an ultimate, beyond which no question could be scientifically pushed. In this perspective, time, though a necessary condition for physical

activity and thus for the operation of bodily natures, was not itself an explanatory principle.

The first challenge to this conception of nature came from the Christian notions of creation and history, which seemed to imply not only that the forms were not ultimate but that time played an essential role in bringing them gradually to be. But it was only with the triumph of evolutionary ideas in geology and in biology that a basic shift in the notion of scientific explanation itself began to occur. Form was no longer the ultimate it had been for Aristotle or for the seventeenth-century pioneers of the new science. One could ask how it had come to be, and in this way understand it in a new way. To Aristotle's four canonical types of explanation was added genetic explanation, which rapidly came to seem the most fundamental. Plato had regarded complexity as a negation of the basic intelligibility of the One, so that a movement towards complexity was a movement away from intelligibility. Whereas the new evolutionary modes of thought encouraged people to see in this very movement itself a pattern which enabled the complexity to be understood. Process thus becomes the key to understanding, rather than a barrier to it; time is the essential matrix where the intelligibilities of nature develop, rather than a mere featureless arena where instances of Forms, themselves timeless, recur.

The consequences of this shift for the classic ideas of nature, time, purpose, motion, and the rest which formed the framework of the older philosophies of nature are obviously quite far-reaching. Philosophy of nature could be carried on as before only at the cost of disconnecting it entirely from empirical science, either by making it a PN1 of an isolated sort or else by relegating science to a phenomenal and superficial level of understanding. Few recent philosophers have cared much for

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"See E. McMullin, "Religion and the Natural Sciences" Bulletin of the University of Kansas School of Religion, 5 (3), (1988), 1-6."
either of these alternatives. But to say that their philosophies of nature, in consequence, took note of the importance of the evolutionary mode of development of natural structures is not to say that they would automatically be regarded as "evolutionary philosophies," in the sense in which we are here using this phrase. The matter is one of emphasis: for a philosophy to be described as an "evolutionary" one, it must be mainly governed by evolutionary modes of explanation; evolution must be for it, in the words of one of its best-known recent exponents, "not an hypothesis but a light illuminating all other hypotheses." 20

But there are two ways in which this might occur, and so we have two very different types of evolutionary philosophy, "naturalist" and "idealist," one corresponding to a PN2 and the other to a PNM. 21 The first is one that takes its origin in, and warrant from, evolutionary science. It argues from the success of evolutionary modes of explanation in biology to their centrality for any sort of understanding of being. It goes beyond the theories of the empirical scientist by a leap of generalization and the imaginative construction of categories and causal modalities that find their main support in analogies with science. It could be called either a "philosophy of science" (PSN) or a "philosophy of nature" (PN2), though the latter title is usually preferred because of the emphasis on the insight that nature itself is the product of evolution. It is, like all PN2, a prolongation of empirical science to a more reflective, more speculative, more general, and thus less easily verifiable level. Though it is spoken of as "philosophy," there is usually no clear-cut line of demarcation intended between it and the evolutionary science which is its ultimate

21 I have worked this out in more detail in "Teilhard as a Philosopher," Chicago Theological Seminary Register, 60 (1964), 15-28.
basis. As representatives of this kind of PN₂, we might take Herbert Spencer and Samuel Alexander; thinkers as diverse as John Dewey and Julian Huxley would also qualify.

On the other hand, a majority of those who have been called "evolutionary philosophers" start with the conscious life of man. A phenomenological analysis of the main features of consciousness discloses features like purpose and creativity. These are then extended outwards from man in an attempt to understand characteristics analogous to them in the universe disclosed by evolutionary science. Complexity is thus seen not as the automatic product of a mindless process of selection, but rather as the goal of spirit working gradually through the modalities of matter. The causal agency responsible for the apparently directed character of biological evolution is understood, therefore, as "psychic" energy in some analogous sense of that term, because what it brings about bears such a striking resemblance to the work of conscious purpose as we see this operate in our own lives. The leading categories and explanatory metaphors in such a PN will be phenomenological in origin; though they will be applied to biological and physiochemical processes, they will derive their initial meaning from the processes of conscious awareness. As a PN, it is thus of "mixed" warrant (PNM), because it rests not only on the original explanatory power of the phenomenological categories used, but also on their ability to synthesize and illuminate the data provided by the empirical sciences. The support for such a PN comes not only from the evolutionary sciences it unites and generalizes, but also from the plausibility it has, quite independently of, and prior to, these sciences as a descriptive analysis of the domain of consciousness.

Why call it a "philosophy?" Why should these phenomenological categories not be regarded as "scientific?" Husserl argued that they should be; in fact, he would insist that they are more "scientific" than the abstract concepts of energy and
momentum that have dominated physics since Newton's day. It is a matter of terminological convention, up to a point. But those who use the word 'scientific' today do not have the old Greek sense in mind, (as Husserl did). They have in mind, rather, an empirical finite mode of progressive and public validation of an intricately-connected conceptual framework, no one element of which can be anchored in the analysis of an element of consciousness in the way in which phenomenologists proceed. To say that phenomenological analysis is not "scientific" in the special sense that this term has come to have (in English, at least) is not to challenge its validity or its "scientific" character in the older Greek sense of the term.

Could not the work of Royce, of Teilhard de Chardin, of Morgan, and of the other major representatives of this type of evolutionary thinking, become "scientific" in this narrower sense, if modes of empirical validation of the claims made for the operation of psychic energy, élan vital, and the rest, are worked out? This is not impossible, though at this stage it does not seem to be likely. On the other hand, these thinkers themselves (Teilhard de Chardin, notably) would strongly argue that the narrower conception of "science" which would exclude their work is simply too narrow, and will have to be expanded by allowing for a mode of validation, through an immediate "seeing" of meaning, one which at present is left to the philosophers.

This sort of PNM has one great strength and one great weakness. Its strength is the analogies it can point up between the processes of evolution, from atom to ape, and the processes of conscious planning. The neo-Darwinian is committed to explaining every single developmental stage of every single natural structure in simple terms of adaptive advantage. This is an extraordinarily strong claim, one that in many instances cannot be directly verified. But even in such instances, the neo-Darwinist will assume that there must be an explanation for
the structure or behavior in question, which utilizes the ordinary energies of physics and chemistry and relies wholly on the mechanism of selective hereditary advantage. His main argument is that no other explanation is possible. But to this our evolutionary philosopher will object that he has a much more plausible account in terms of the familiar operation of conscious mind. And he will also object that the neo-Darwinian hypothesis becomes vacuous if it is incapable of being empirically falsified, i.e., if the neo-Darwinian when unable to specify an adaptive advantage in a particular case just insists that there "must" be one there.

The weakness of this "idealist" mode of evolutionary philosophy is that of idealism generally: How is it to be verified? Are plausible analogies enough? Is there not a great danger of anthropomorphism? How are the leading metaphors to be made precise, and inter-related with one another in a way that is open to some sort of progressive testing? Are there specific criteria for the application of these metaphors to concrete cases, so that we can say of one instance that it exhibits "purpose" or exemplifies the operation of "psychic energy" and of another that it does not? Or are these terms vaguely transcendent so that they can be affirmed, a priori, of all process and so are incapable of being empirically falsified? Is the "immediate grasp of significance," the "seeing" of which Royce and Teilhard speak, adequate for an intersubjective science? How are disagreements to be adjudicated if some see the operation of spirit in the coalescing of a nebula (as Royce did), and some do not? Finally, even if one were to concede the use of phenomenological categories as basic in understanding process, how could one show that the "spirit" operating in the course of evolutionary history is a single Spirit (as most defenders of this view maintain)? Or that the causal agency responsible for a significant mutation here and there across the surface of the globe is a single conscious intelligent agency?
How exactly does it operate as one? What physical energies are involved and how are they directed? We have here an analogue of the mind-body problem, but at an immensely more difficult and more precarious level.

Those who affirm this form of PNM must have very strong independent reason in advance for affirming the causal operation of mind throughout all matter. (They do, of course, rely on the extra warrant given their claim by the evolutionary evidence, i.e., their PN is a PNM, not a PN1.) Two types of thinker are especially attracted: idealist philosophers who see in this view the natural extension of their metaphysics, and those religious thinkers who hold for an immanent operation of the Divine mind throughout the universe. The Christian philosopher who stresses the transcendence of God and the unity and sufficiency of his creation is not likely to be sympathetic; but if he stresses immanence more strongly than transcendence, or if he leans to a "split-level" explanation of natural agency, in which two different sorts of energy, one Divine (or "psychic" or "spiritual") and the other physical (or "mechanical" or "tangential"), are claimed to play a part, then an evolutionary PNM will strongly commend itself to him.22

Many writers commonly classed as "evolutionary" pose a special problem for our classification. Hegel, for example, denied the existence of evolution in Nature, restricting it to the social realm only. His PN (as we have already seen) was thus a PN1 which, far from relying upon empirical science, set it aside. Bergson allows more intelligibility to Nature than did Hegel; his élan vital brings about increasing complexity of natural structure. But his famous analysis of time puts far more reliance on phenomenology than on the findings of evolutionary science. Bergson takes relatively little account of the findings of Newtonian and Darwinian science generally, and

22""Teilhard as a Philosopher," supra.
cannot be said to be consistently evolutionist in his cosmology. His PN would be nearer a PN1 than a PNM. Whitehead, on the other hand, did extensive original work in physical science, making controversial use there of the causal categories of his philosophy of organism. His metaphors refer much more, however, to ordinary organic process than they do to evolutionary process. Though the "consequent nature" of God plays a role in his system not unlike that of Spirit in some of the evolutionary writers, it would be misleading to classify him as an evolutionary philosopher. Many aspects of his metaphysics are, in fact, barely consistent with an evolutionist standpoint. Is his PN a PN2 or a PNM? This is a point he himself discusses in *Process and Reality*. In the Introduction, he asserts that his cosmology is a generalization from a starting-point in empirical science (thus a PN2), but it is quite clear that many of his most important categories, distinctions, and principles, cannot possibly be understood in this way, and must be taken to have been in some sense evidentially prior for him (thus indicating that his work ought be taken as PNM).

**Philosophy of Nature in Marxism-Leninism**

Of the papers submitted to the *Naturphilosophie* section of the recent International Congress of Philosophy, more than one-half (28 out of 51) came from Marxist-Leninist philosophers, most of them (16 out of 28) from the Soviet Union. As a group, the Soviet papers in philosophy of nature were almost unbelievably bad, confused in methodology and naive in their grasp of contemporary science. It was no great loss to the Congress when not a single one of the sixteen Soviet speakers listed for *Naturphilosophie* showed up on the podium.\(^2\) What the reasons were for this mass defection can be surmised, and it is unlikely that the low worth of the papers was one of them.

\(^2\) An informal check of registrations showed, in fact, that only one of them seemed to have reached Vienna.
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The Vienna debacle was one more unhappy chapter in the story of Soviet philosophy of nature. In the same month in which Soviet philosophers exhibited an unforgivable lack of critical standards in the text they communicated concerning matters of science, their colleagues in science put a space-ship in lunar orbit. What is one to say, then, of the relations between theory and practice in Russia? In the understanding of nature, at least, they seem far removed from one.

Yet it cannot be ignored that the largest single group who claim to be doing "philosophy of nature" today is the Soviet one. We shall see that the theoretical categories we have defined above will make it much easier to grasp the dilemma this group faces. It would take much too long to chronicle the curious and significant history of Naturphilosophie in the Soviet Union since the official imposition of Marxism-Leninism in 1919. It would also be difficult to determine just what is the situation today. Soviet scientists on the whole seem to be paying no more than lip-service nowadays to the principles of dialectical materialism, while the intellectually least creative among the philosophers (or so it would seem) are taking on the ungrateful task of declaring what the consequences of a faithful adhesion to Marxist-Leninist orthodoxy would be for the knowledge of nature. In this concluding section of our essay, we will content ourselves with a sketch of the three main attitudes towards PN that developed in the early days of the Soviet state, all three of which still have their defenders.24

But first let us glance at the works of Marx and Engels from which Soviet orthodoxy has somewhat hesitantly derived. One notes in both writers an obvious indecision as to what attitude to take towards empirical science. They sometimes

speak as positivists, suggesting that the method of natural science is the proper method for all knowledge; this would exclude a philosophy of nature entirely. At other times, they insist upon the existence and even the primacy of a metaphysics in the knowledge of nature, a metaphysics whose own warrant (they emphasize) does not derive from science; this suggests a PN1, or perhaps a PNM. Finally, they sometimes speak of a division of labor, with dialectics handling all social questions and providing the general categories for the understanding of process, and science taking care of the more specific issues; this would give a PNM of a particularly uneasy sort. The reasons for this rather extraordinary vacillation lay in the tension between the original components of Marxism: the "dialectical" (metaphysical) component borrowed from Hegel and the "materialist" component which was Feuerbach's contribution. The former is indispensable to Marx's social doctrine. The certainty of the triumph of the proletariat rested on no mere inductive generalization from history or from science, but upon a confident metaphysical a priori. The central part played by the concept of revolution in Marxist theory could not be justified by a materialist or a positivist philosophy; yet upon this notion rested much of the attraction of Marxism for dissident elements in society. On the other hand, Marx's "materialism" was equally important in the plan to remake society, because it excluded religion and all other forms of "idealist" (i.e., the view that spirit is in some sense prior to matter) which could act as a brake on revolutionary changes in the material or social order. And a "materialism" seemed to involve a special position for the sciences of matter and their methodology. The term, 'materialism,' was in fact a misnomer here; 'naturalism' (or even 'anti-supernaturalism') would have been more correct. Neither Marx nor Engels wished to reduce mind to matter; to do so would have been to sacrifice the privileged place of man at the forefront of the dialectical
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process. What they wished to safeguard was the absolute priority of "matter" (the this-worldly) over "mind" (the other-worldly), while also holding the qualitative irreducibility of the mental to the physical.

Marx himself realized the ambiguity of the label he had chosen, and felt obliged to campaign against the "vulgar" materialism of some of his German contemporaries, i.e., against materialism in the normal reductionist sense of that term. Furthermore, he rejected the positivism of Comte with disdain, seeing that it would eliminate the philosophical warrant for his own social doctrine. Finally, he also rejected idealism (despite the dependence of his own dialectical theory upon it), and with it all forms of metaphysics which are not continuous with, and implemented in, human practice. By eliminating these three classical philosophical positions, he not only left himself without any possibility of a consistent PN, but ensured that this would become a crucial issue, by emphasizing on the one hand the importance of science in the economic transformation of society and on the other the subservience of all knowledge (including presumably science) to political praxis. Subsequent reliance on the "classics" would thus bring about a vacillation between positivism, materialism, and dialectical metaphysics, with quotable texts in favor of, or in criticism of, any one of the three. And the touchstone was inevitably to be whether or not a PN1 or PNM was put forward, i.e., a philosophical position prior to science which could validly claim to be normative in respect to the work of science. According to both positivists and materialists, this was impossible; according to the followers of Hegel, it was essential. And the positivists and materialists themselves differed as to whether the lesser claim of a PN2 could be allowed.

In such a case, political considerations were likely to be decisive in the debate at any particular period among Marxists over the merits of PN. And this has in fact consistently been
the case. This is especially clear in the case of Lenin himself. The Menshevik-Bolshevik split in early Russian Marxism was purely political in origin, with no philosophical overtones. When the Menshevik spokesman, Plekhanov, attacked the Bolsheviks for their alleged positivism, Lenin attempted to dissociate his faction from this stigma in his book, *Materialism and Empiriocriticism* (1908), a polemical and at times extremely confused work. In it, he supported the Hegelian line and condemned positivism and mechanism. But he also refused to make political orthodoxy (Bolshevism for him) a touchstone of philosophical orthodoxy, as the Mensheviks had begun to do. His relative pluralism reflected nothing more fundamental, however, than the total lack of correspondence between the philosophical and political groupings of his day, especially within his own Bolshevik party. This pluralism was to become official, under his name, in the early days of the Soviet state, until ended by Stalin in 1929.

In 1922, Lenin called upon Soviet philosophers to give explicit consideration to the problem of a philosophy of nature, on the plea that without it the all-out campaign against religion then beginning would lack a proper theoretical basis. But his essay supports two quite different approaches to the issue. One is that scientists are "natural" materialists, and only need some protection against the "deviations" of religion and positivism. The other is that science itself has to be based on what he calls "the solid philosophical ground" of "Hegelian dialectics materialistically interpreted." Just how different these two approaches were, the next few years would show. They would also show that the theoretical problem of the existence of a PN1 is equivalent to the highly practical problem of the autonomy of science. It is this equivalence which has made the whole issue such a crucial, and also such an insoluble one for the Marxist thinkers.

In the 'twenties, most of the leading Russian scientists were
still "pre-revolutionary" in their training and sympathies. They naturally tended to reject any claim that dialectical metaphysics had a right to lay down the main categories and lines of development for science. Their view had strong support from Party theorists (Trotsky and Bukharin among others), who felt that it was sufficient if the primacy of dialectical reasoning was upheld in social theory, and who did not want to alienate the scientists on whose research the technological progress of the State depended. But some of the professional Marxist philosophers took alarm at what they considered to be the compromising of true Hegelian principles. Deborin led the faction that from 1924 onwards attacked the drift towards the mechanism and positivism that Lenin had earlier criticized. As the debate developed, it centered more and more on the precise methodology appropriate to a Marxist PN. Both sides agreed on the universality of dialectics. But the mechanist group thought that it should come as a PN2 from a study of concrete processes in nature, through the medium of the sciences. Whereas Deborin argued that a PN1 is an essential part of dialectical materialism, a PN1 which cannot, in his words, "be overthrown by particular contingent facts which are themselves subject to critical examination from the point of view of general methodology." It is no surprise to see him quoting Descartes in support of this view of the nature of PN.

At first, the Deborinists were a tiny minority. But they had a powerful weapon: the appeal to Marxist-Leninist orthodoxy. Despite the fact that "orthodoxy" on this issue was far from clear-cut, it was easier to picture the mechanists as deviationists who were implicitly calling in question the activist role of ideology and "partyness" in intellectual matters and substituting for it the neutral objectivity of scientific reasoning. In 1929, the debate ended with the outlawing of the mechanist view; the pluralism and the hesitations of the 'twenties were
over. Soviet society had no longer to depend on the "bourgeois" scientists, and a more dialectical line could be safely enforced. But 1929 also saw the ascent to power of Stalin and the "Great Break." In a short time, Deborin and the Hegelians were likewise overthrown (1931), on the grounds that they were "ivory-tower" thinkers, lacking in sufficient respect for the realities of political practice. What this actually meant was that the Party was taking over, and Stalin's new concept of "partyness" would see to it that impractical debates about PN would be abandoned in favor of more immediate political activity.

We could continue this chronicle, but enough has been said to show why Marxism-Leninism is faced with an insoluble problem in the domain of PN. Fidelity to the dialectical metaphysics which is essential to Marx's social thought, forces a PN1 on science which scientists reject, and which can on occasion be harmful to the progress of science, as Soviet biologists were to learn. Whereas the encouragement of a strong science and the extolling of scientific method in the spirit of Marx's materialism suggests that empirical science rather than dialectics ought to be made the arbiter of all rigorous thinking.

It is no wonder, then, that the ablest Soviet philosophers shun the domain of PN, central though it is for classical Marxism-Leninism, and that Soviet scientists pay less and less heed to the demands of dialectical orthodoxy. No wonder either, then, that Soviet philosophers of nature when they do turn up at international congresses sound so uncommonly like the faithful but equally distressed Aristotelians of the latter part of the seventeenth century.

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