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# External and Internal Factors in the Development of Science

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## I

According to a familiar traditional view, science is a distinctive enterprise, demarcated sharply from all other human pursuits. The exact nature of that line of demarcation was conceived differently by different thinkers or schools. Depending on whom one read, the demarcation might be held to lie in its experimental method, or in its building upon irrefutable observational facts, or in the distinctive rules (deductive or inductive) by which it adopts a theoretical superstructure on the basis of those facts, or in its goals of attaining knowledge of nature, or in a combination of several or all of these factors. But despite all such important differences of detail, according to all versions of this traditional sort of view, it was the unalterability, the inviolability, the eternity, attributed to the distinguishing marks of science, that most strikes us today. For according to those approaches, the distinguishing characteristics of science must characterize it forever, unaffected by any results of scientific inquiry. If it was its method that demarcated science from other sorts of human activities, that method was conceived as having since its introduction been applied to gain further and further truth, without itself ever having been altered or alterable by any of the discoveries to which it led; and it would always continue to be unalterable in its future applications. The situation was supposed to be similar in the case of rules of scientific inference: they were "logical" (either deductive or inductive), which implied both that they were unalterable and that their status was independent of the world of facts to which they were applied. As to facts themselves, much of that tradition held them to be irrefutable also.

That tradition, as is well known, is now a shambles, rejected almost universally. Already by the 1950s it had become clear to many that all attempts to characterize such eternal and inviolable

demarcators had failed. The notion of "observational facts" as brute undeniable givens, wholly independent of our fragile and insecure interpretations of them, was all but surrendered, and the idea that there was anything "given" in experience was thrown in jeopardy. All attempts to give a precise characterization of scientific method, whether experimental or logical, whether in terms of deductive or inductive logic, were, if not wholly abandoned by then, at least viewed with extreme pessimism by a growing number of those concerned with the study of science. New philosophical analyses emphasized that theories are radically underdetermined by observation; that what are called observations, far from being interpretation-free, are heavily theory-laden; that if indeed, as appeared to be the case, it was impossible to specify any unladen "given" in experience, then perhaps theory-ladenness even predetermined the outcome of experimental tests; that for any given body of what are taken to be data, alternative explanations, perhaps even an infinity of them, are always possible, and that perhaps *any* alternative can, with sufficient ingenuity, be defended "come what may," in the face of *any* body of what are alleged to be observational data.

The rebellion against tradition was not confined to philosophers: an even more serious threat came from the newly-professionalized discipline of the history of science. A torrent of historical studies indicated more and more convincingly that changes over the development of science have gone far deeper than mere change of theory. The changes seemed to extend also to what was counted as evidence, as observational, as factual; to criteria of adequacy of explanations of that evidence, or those observations or facts, and even to what counted as an explanation; and to method, which seemed not to be a single thing after all, but a multiplicity varying from period to period and subject to subject. The studies exposed the presence of broader and deeper "interpretative frameworks" which guided the construction of evidence, observation, fact, explanation, and theory, and which even determined the methodological rules, the criteria of scientific adequacy and the meanings of scientific terms, and even

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the goals of science itself. And it appeared that such interpretative frameworks differed in fundamental, perhaps incommensurable, ways from tradition to tradition or from group to group.

The implications seemed profound. If facts underdetermine theories—if indeed the sum total of scientific constraints, whether factual (observational), methodological, logical, criteriological, or whatever, underdetermine scientific belief—then some other, non-scientific, considerations must enter in to fill the gap between scientific constraints and scientific belief. Since science is a group endeavor, the most promising candidate would be social considerations. An even more extreme implication threatened: for if fact or observation, method, logic, and criteria all depend on presuppositions which vary from one tradition or group to another, then there are *no* independent scientific constraints standing above ultimate interpretative frameworks, and there would remain *no* intrinsically scientific constraints on belief; *everything* about science would be a matter of “external” considerations, perhaps socially conditioned. If for example observational facts are interpretation-laden through and through, then how could they be expected to enable us to decide for or against the interpretation to which they owe their very status, indeed their very meaning? Thus one degree or another of social relativism loomed: either social (or other non-scientific) considerations made up the difference between scientific constraints and scientific belief, or else there were no scientific constraints which were not themselves socially conditioned or determined. There was nothing intrinsically “internal” to the scientific enterprise which distinguished its procedures and conclusions from “external” ones.

There can by now be no reasonable doubt of the pervasive role of presupposition, of interpretation, in science and scientific change. There are no brute facts which confront us and force our theory choices in certain obligatory directions; there is no “given” which does not involve interpretation. Nor is there a single scientific method which is applied unambiguously across the board in all science, past, present or future. The extraction or testing of theories and hypotheses is far more complicated than can be captured by rules of any formal logic. What counts as an observation; conceptions of the objects or processes under study (and even whether what we study are appropriately characterized as “objects” and “processes”); the problems arising with respect to those objects and processes; the characteristics expected of answers to those problems, and the criteria of adequacy by which one answer is selected from the body of possible ones; the methods by which those answers are to be attained; the goals

of inquiry—all these are shaped in the light of background beliefs, background beliefs which are different at different periods of history and in different domains of inquiry. Science is built upon a background of presupposed beliefs, beliefs which form the basis of our interpretations of nature and our methods of studying it, and which are in principle always subject to alteration, abandonment, or replacement.

Nevertheless, it remains another question whether some of the conclusions which have been drawn really do follow from this pervasiveness of presupposition and interpretation. More specifically, we need to examine more closely whether either of the following theses follows from that pervasive role:

- (1) that there are no “internal” factors guiding scientific development independently of non-scientific factors (for reasons obvious to sociologists, I will call this “the strong thesis”); or
- (2) that, while there are such internal factors, they are insufficient by themselves to guide science, and must be supplemented by “external” factors. I will call this “the weak thesis.”<sup>1</sup>

In what follows, I will argue that neither of these theses follows from the pervasive employment in science of background beliefs and the deep changes that have affected the beliefs so employed. In other words, I shall try to show, firstly, that there is an important distinction between internal and external factors in science—though that distinction has to be radically recast in the light of the way science relies on antecedent beliefs and undergoes pervasive changes in those beliefs; and secondly, that the internal factors are generally sufficient to guide science in its inquiries—even though the claims it builds on those background beliefs are always in principle subject to possible doubt—and that that sufficiency has, as a matter of contingent fact rather than a necessity of logic, tended to increase over the history of science.

## II

According to the view of science which I shall present, the distinction between external and internal considerations guiding the development of science, far from being an *a priori* and essential characteristic present in science from its inception, has itself been a product of that development. In early periods of the history of efforts to understand nature, there was precious little guidance as to what, precisely, required investigation, what was relevant to the investigation, how to go about the investigation, and what a conclusion of the investigation would be

like. The Milesian philosophers of the sixth century B.C., who perhaps more than anyone deserve the reputation of founders of the knowledge-seeking enterprise, do not seem to have focussed on particular problems concerning particular sorts of substances or processes, but took all existence, all change, as their domain of inquiry, and everything—or rather nothing in particular—was relevant to their efforts. What methods they had were of the most simplistic and intuitive logical and analogical sort; and as to what they demanded in an explanation, that was little more than that an explanation should show nature not to be capricious.

Clarification with regard to these four aspects of inquiry—what to study, what was relevant to the study, the appropriate methods for that study, and the character of an explanatory conclusion to the study—required *learning how to learn* about nature. Later I will mention some other aspects of the scientific enterprise with respect to which we had to learn how to learn, but for the present let us focus on these and ask this question: How did we, in seeking knowledge about nature, come to learn how to go about determining what needs to be studied, what is relevant to that study, the methods of going about that study, and the character of explanations which would be the goals of those studies? How did we get from Miletus to today's science, not just in respect of the profound changes of beliefs that have come about, but in respect of how we go about studying nature to obtain those beliefs, and how did that profound transition produce a distinction between considerations "internal" and "external" to science? Of course in this short paper I can only give a sketch, an overview, of the process; but perhaps that will be enough to provide a basis for some conclusions about the debate concerning the nature and relative status of internal and external considerations in the development of science.<sup>2</sup>

We can begin by considering one important development which gradually assumed centrality in the scientific enterprise during the sixteenth through eighteenth centuries: the approach of examining specific subject-matters, such as moving bodies, salts, gases, in isolation from others—what we may call the *piecemeal approach to inquiry about nature*.<sup>3</sup> Although it had been present far earlier, it was then only one of many, not coming to prominence in the search for knowledge until the early modern period. In particular, it replaced an older holistic approach, stemming from Milesian philosophy, of trying to explain everything at once—for example, of trying to explain the nature of change or of substance *in general*. The specific subject-matters for study in this new approach may be referred to as *domains of investigation*.

Early domains of investigation were necessarily those of common, everyday classifications, based on considerations such as sensory similarities, use, place of discovery of a substance, and the like. Thus certain metals were given the same name and considered one sort of substance on the basis of their appearance, and similarly for certain transparent crystalline substances. Views of what the problems concerning those classifications were, of the methods by which those problems were to be approached, and of what sorts of answers to try to provide, what it was to give an explanation of the items of a domain, were likewise based on prevailing views, of which there were many contradicting one another, with few having any clear advantage over their competitors; and all were ill-formulated and vague. What was and was not relevant to the classifications and the attempt to deal with them was obscure.

But with the beginnings of intensive study of those presumed types of substances, distinctions were found between members of the types which led to distinct classifications. Domains were thus reconstituted and restructured, old bases of classification coming to be seen as superficial, other bases formerly seen as superficial, or new ones not known before, coming to be seen as of fundamental importance. What had previously been classified indiscriminately as "salts" became separate domains for investigation; on the other hand, early differences found between electricity and magnetism, the former, for example, acting on light bodies of all sorts, the latter only on heavy iron ones, came to be seen as superficial, and the two types of phenomena ultimately became unified in Maxwell's theory. And as the domains of scientific investigation were thus shifted and altered, so too were the items making them up reconceived, reinterpreted, and often redescribed and renamed. The most far-reaching of such reconceptions occurred in the so-called Chemical Revolution of the late eighteenth century, in conjunction with which Lavoisier and his associates proposed the renaming of all chemical substances according to their elemental constituents. And of course new discoveries added new items to domains, and even new domains.

But how did these restructurings of scientific fields come about? I suggest that the very adoption of the piecemeal approach to inquiry—the laying-out of boundaries of specific areas of investigation—automatically produced a standard against which theories could be assessed. Whatever else might be required of an explanation of a particular body of presumed information (domain), that explanation or theory could be successful only to the extent that it took account of the characteristics of the items of that domain. This is of course rather vague. What

were to be counted as “characteristics” of the domain items: How were we to decide what it was about, say, salts or magnetism, that had to be “accounted for”? To what extent did we have to “account for” them: with complete exactness, or was nature just not so precise? And in what would such an “account” consist? Should it tell what the perfect state of the substance was, as in some alchemical views (I will call this the *perfectionist approach* to understanding material substances)? Or should it take what might be called a *compositionalist approach*, deriving the properties of the domain items from their constituent parts, the arrangement of those parts, and the forces holding those parts together—a view propounded in chemistry in (among others) Stahl theory and coming to full expression in the theories of Lavoisier and his associates?

In all these respects, what are naturally called “hypotheses” played a role; and there was, in earlier phases of science, little to go on in selecting these hypotheses. Or more exactly, the motivating considerations in selecting explanatory approaches might come from just about anywhere. Antagonism to Aristotelian forms, natures, and final causes, rather than the dictates of nature, entered into adoption of the mechanistic and atomistic approaches of the middle and late seventeenth century; Newton developed his theories of motion (and thus of space and time) at least partly in the light of theological considerations, objecting to Cartesian physics on such grounds just as his own views were deemed atheistic by Leibniz and his followers. And in general, the large gap between scientific ambition and scientific conclusion had to be filled, under such circumstances, by considerations which we today would consider non-scientific, external, though at the time there was little or no ground to so distinguish them. Indeed, even the ambitions of science at such stages were dictated, at least partly and perhaps largely, by considerations which would today be called external. For the distinction between the external and the internal to science was at best only rudimentary and in many cases did not exist at all.

Under such circumstances, the only available option, however little it was apparent to the participants, was to wait and see which if any of the various hypotheses as to domain classification, presumed distinctions between superficial and really important domain-item characteristics, and the nature of “accounts” would hold up—which if any, that is, would *allow* successful account to be given of the domain in question. As it turned out, certain approaches—for example, the mechanistic approach to the study of the motions of planets, falling bodies, and projectiles by the late eighteenth century, and the compositionalist approach to the study

of material substances by the 1860s or so—did show themselves to account to a considerable degree for their respective domains. Let us examine some of the factors involved in this.

One particular achievement of the mechanistic approach was to show that accounts of domains could achieve *precision*, and that such precision was, at least often, expressible in mathematical terms. Things might not have been that way. Indeed, for Plato, although mathematics (geometry) was a necessary ingredient for understanding the world of experience, of change, it was not sufficient, for that world was intrinsically indeterminate; and Aristotle held that mathematics, the category of quantity, could not provide understanding of the essence of things. The idea that all details of experience could be explained precisely originated with Kepler. With him, it was based on a rationalistic theology which implied that there is (*i.e.*, that God must have had) a sufficient reason for everything in nature, a principle with which Kepler associated geometrical exactitude. Within the limits of available observational accuracy, it was shown to be an achievable ideal by the mechanical philosophy.

The Newtonian version of the mechanistic approach was able to do what it promised to do better than its rivals (Scholastic Aristotelianism, Cartesianism) were able to do what they promised to do; and because of the role played by mathematical precision, it was able to do what it did better than they did what they did. Some of this power was evident from the work of Newton; but its full force came out only with Laplace, who brought together and extended the work of a century and molded mechanics into an impressive tool for the analysis of problems of celestial and terrestrial motions and forces. But from the perspective of our present problems, Laplace’s work was important for another reason: with him, theological hypotheses began to become external to science. For Newton had not merely postulated attributes of the Divine Will in constructing his theories; he also found in the laws of nature on which he based his system reasons which he believed required God to intervene in the events of His creation. On various occasions he appealed to three implications of his laws. Thus, he pointed out, bodies lose momentum in impacts: the universe would run down unless there were intervention beyond the laws of nature to maintain that momentum—a job left by the laws of nature to God. Again, if the solar system were left to the governance of the law of gravitation, the mutual gravitational perturbations of the planets would pull them out of their neat Keplerian orbits; God would have to intervene every so often to maintain the order. And finally, there were the disturbing contradictions, pointed out so tentatively

(and incompletely) by Richard Bentley, concerning the possibility of universal gravitation in either a finite or an infinite universe; only God, Newton sometimes proposed in response to these difficulties, could maintain a universe in which universal gravitation operated.

Though a fully satisfactory treatment of the argument from loss of momentum would have to await the nineteenth and twentieth centuries, it had already been rejected by most people by Laplace's day: momentum is conserved, at least in collisions of the elementary atomic constituents of things which were conceived as "perfectly elastic" (even if absolutely rigid); and if momentum appears not to be conserved in collisions of ordinary bodies, that must simply be because it has been transferred to those inner, invisible constituents. As to the stability of the solar system, though he did not complete the job, Laplace laid down the basic arguments to demonstrate that gravitational perturbations are in the long run self-correcting: the law is, after all, sufficient to account for the orderliness of the solar system.

The Bentley paradoxes of gravitation were overlooked, indeed forgotten until the end of the nineteenth century; they were resolved only with the theory of general relativity. (Einstein, in pardonable ignorance of the history of the problem, christened it "Seeliger's Paradox.") Whether the story of his encounter with Napoleon is apocryphal or true, Laplace at least had solid arguments (as opposed to philosophical generalities) to offer for showing that science had no need of such hypotheses by counter-arguing specific arguments purporting to show that it did. Though the matters involved were not yet completely settled, and would not be for a long while, arguments that theological considerations were relevant to science had been deflated; such considerations were external to science precisely because the laws of science had been shown (even if as yet imperfectly and incompletely) to be sufficient to account for certain phenomena which had previously seemed to require divine intervention.

A similar portrayal could be offered for the development of the compositionalist approach to chemical explanation: as the nineteenth century wore on and questions concerning the nature of acids, the chlorine problem, and a multitude of other issues fell into place, the compositionalist approach came to be seen as clearly able to do a great deal toward the understanding of material substances—far more than its by-then-dead rival, the "perfectionist" approach.

The nineteenth century saw the vindication of many ideas for many domains: electricity, magnetism, heat, light, many areas of chemical investigation, and so on, these vindicated beliefs being of all levels of generality, from very specific beliefs about,

*e.g.*, particular substances or particular behavior of compass needles in particular circumstances to very general theories such as Maxwell's. Theoretical accounts of domains were shown to be highly successful with regard to their domains: the theories had a responsibility for accounting for a body of putative information which was, in general, if not always, well-delineated, and did so effectively within the range and limits of experiment and observation. Though there were problems with regard to many of these ideas, in many cases they had to do with specific reasons for supposing the theories to be *incomplete* rather than *incorrect*: they were not contradicted by any domain information, they just failed to take account of some item of their domain, and so, there was reason to believe, merely had to be extended rather than rejected or changed in fundamental ways. (Electromagnetic theory still had to be extended to cover the electrodynamics of moving bodies, for example.)

By now, several such theories had another appeal besides their success in accounting for their own circumscribed domains. For since Newton had fused terrestrial and celestial mechanics into a single theory, there had been increasing reason to believe that theories of different domains might well be the same. Electricity and magnetism, despite the differences noted by Gilbert and his successors, were gradually fused in a historical process beginning in the 1820s and culminating in Maxwell's theory; and that theory also incorporated light. That unification itself required the prior unification (mainly by Faraday) of various alleged kinds of electricity. Beginning with Davy and Berzelius, chemical bonding also began to be seen in terms of electrical attraction and repulsion. The process continued in the twentieth century, with the resolution of conflicts between electromagnetism and mechanics by special relativity, the unified accounts of spectra, atomic structure, chemical valency, and so on by quantum theory and ultimately by quantum mechanics; the quantum field-theoretic treatment of the domains of weak, electromagnetic, and strong interactions, resulting in the unified electroweak theory and movement toward grand and still grander unification. The trend was already evident in the nineteenth century, even though it was not always clear which type of theory should provide the basis for unification: should we continue to move toward a more complete mechanical world-view, absorbing electromagnetism and other areas into it, or vice-versa? (We see similar problems in the 1950s and 1960s: should field theory be abandoned as the guiding program for unification, and replaced by something else, like S-matrix theory? But notice that the questions were, in the twentieth century case as in the late nineteenth, resolved.)

From the viewpoint of our topic, though, the important facet of such unification was this: that *in addition to its success in accounting for its domain, a theory could be judged in terms of its compatibility with theories of other domains.* (There were, however, developing constraints on such judgments: not all domains could be reasonably expected to be explained in the same terms. I shall ignore such qualifications here, though.) In addition to doubts based on its failures to account for its domain of responsibility, a theory can also be doubted on the ground that it fails to conform to a type of theory with which we believe it ought to conform—for example, because that type of theory has been successful in several other domains.

Over the course of the centuries (the last few especially), a great many beliefs about nature have been found to have been both *successful* with regard to their domains of explanatory responsibility, and *coherent* with theories of other domains. This is not to say that there were not problems with regard to some of these theoretical beliefs. But in assessing the significance of such problems, we must be careful in three important respects. First, we must remember that problems of incompleteness are not reasons for rejecting a theory, and do not become reasons even for doubting a theory until a great deal of effort has been put into attempting to extend it to cover the recalcitrant domain items. Second, the fact that a theory has been tested and found successful and coherent with other theories only within certain experimental limits (of accuracy, velocity, mass, and other relevant parameters) is not by itself a reason for doubting the theory—though of course it may be a reason for extending the tests of the theory beyond those limits.

The third point is perhaps the most important. However much success and coherence a theory may achieve—however much it may be free of the *specific* doubts imposed by its specific failures to account for domain-items and to cohere with other theories with which we believe it should cohere—such doubts *might* arise in the future: there is always the *general possibility* of doubt. But the general possibility of doubt arising, insofar as it is a *mere possibility*, is not itself a ground for doubting any particular theory. The same may be said of all general possibilities as “I may always be dreaming,” or “A demon might be deceiving me in all my beliefs,” or “Perhaps I am only a brain in a vat.” Applying as they do equally and indiscriminately to *any* claim whatever (including the negation of the claim itself), such *universal doubts* cannot count against any one claim in preference to another, and *therefore in the actual knowledge-seeking enterprise they do not count as legitimate grounds for doubt.* Many philosophers and sociologists of science have failed to recognize this point in their accounts of

the dubitability of scientific claims; but it is ignored only at the peril of having failed to move beyond the approach to knowledge represented by Descartes’ dream and demon arguments in the seventeenth century—arguments which play no role in science, except as reminders that specific doubts *may* always arise in the future.

Keeping these three points in mind, we can see that it makes sense to speak of an accumulation of beliefs which, by virtue of their success and coherence, could be trusted by scientists. Many of these beliefs were free of any specific doubts in the senses I have just discussed, or at least close to it. The growth of such a body of beliefs was not a simplistic march of accumulation: many of the beliefs did fall subject to doubts, and had to be rejected. Other beliefs had their ups and downs: I have mentioned in passing the fortunes of quantum field theory in the 1950s and 1960s; more generally, that approach may be seen as promising in the early 1930s, with doubts increasing, satisfaction with regard to many of them in the late 1940s, doubts sufficient to lead many physicists to reject the approach in the 1950s and 1960s, and triumphant successes in 1971 and thereafter from which it will not easily be displaced. But even though such doubts can arise with regard to any theory, even those regarding which there are at a given time no specific doubts, *that body of beliefs which are, at a given time, free of such doubts (or even reasonably close to being free) constitute a basis on which science can alter its domains and build further hypotheses, methods, rules of reasoning, and goals.*

What has been developed through this process is a distinction between beliefs which can serve, from a scientific standpoint, as background in terms of which scientific change can proceed—in terms of which new scientific ideas, methods, goals, and so forth can be built—and those which it cannot employ in such building, at least in the sense that they have not been *scientifically* legitimated. It is, in short, a working distinction—one which has a function in the knowledge-seeking enterprise—between internal and external considerations in science. But it is a distinction which has been forged in the very process of investigation of nature, not laid down in some edict from heaven or philosophy which determines what counts as scientific and what does not. The process is one of a gradual discovery, sharpening, and organization of relevance-relations, and hence of a gradual separation of the objects of its investigations and what is directly relevant from what is irrelevant thereto: a gradual demarcation, that is, of the scientific from the non-scientific. Those considerations become internal, scientific, which *have been found*, as a matter of contingent fact, to be doubt-free (successful and coherent) and relevant to the



domain under investigation. All other considerations become external, non-scientific.

In the light of this process of internalization, it is easy to see why and how scientific change is so pervasive. In the course of inquiry, domains come more and more to be formulated in the light of background beliefs which have proved doubt-free and relevant to the domain being investigated, rather than in terms of such criteria as sensory similarities. But the background beliefs which lead to such changes in domain structure and conception also lead to alterations, sometimes profound, in other parts of the fabric of science. Sometimes it becomes necessary, in the light of problems that arise concerning a restructured domain or a new theory of the domain so restructured, to reject or modify some of the very background beliefs which led to the restructuring. The problems associated with particular domains become altered, as do the lines between recognized "scientific" problems and questions that are classed as "non-scientific." What counts as "observation" of a subject-matter, too, may be altered; both the entities or processes to be studied, and the methods of observing them, can develop with the acquisition of new successful and doubt-free beliefs. New background information, or old information formerly considered irrelevant, is found to be relevant to a particular domain. Old methods are rejected or reinterpreted, new ones introduced; new standards of possibility and acceptability arise. Even the goals of science may alter, as in the transition from perfectionist to compositionalist matter-theory.

Still, at any given stage, there is a body of background beliefs on which science relies. The hypotheses of current particle physics are not deduced or induced from a brute given; still less are they constructed in an intellectual vacuum. Their mode of formulation and their plausibility alike, despite their problems, are obtained from a prior background. Grand Unified Theories (GUTs) and their application to cosmology apply the idea of symmetry breaking using the Higgs mechanism previously employed in the highly-successful electroweak unification to join the latter with the newly functional theory of the strong interaction, quantum chromodynamics; the electroweak theory and QCD in turn were based on the gauge-theoretic (more specifically, Yang-Mills) approach whose importance became clear with the establishment of the renormalizability of such theories in the early 1970s. That approach itself was developed in the light of a long background including the enormously successful theory of quantum electrodynamics and the even more central place which group theory, especially Lie groups, had come to play in physical theory. All these pieces of background traced their pedigrees ultimately to quan-

tum mechanics and its necessary logical extension through the application thereto of special relativity, the latter two theories, at least, particularly when so joined, having proved free of specific and compelling reasons for doubt, and thus having the status of scientific background beliefs suitable as bases on which to build.

To make this process of scientific change clearer, it is necessary to call attention to one way in which reaction to traditional views of science has gone too far: namely, the rejection of the "given." For there is an important sense in which there is a "given" after all. True, it is not the given as imagined in classical and positivist myth; it is not, that is, a given in the sense that it is found as a result of pure perception, pure, that is, of any prior belief. Much less is it a given in the sense that, once recognized, it can tell us automatically the character of the world by which it is given. Nor is it even necessarily the product of a single effort, a look, but is often the product of many efforts, complete with the open possibility of error. Rather it is a given in the following three respects: that, (1) having been marked out as significant by our best available background ideas, (2) having been appropriately described in terms of those background ideas, and (3) having been made accessible by application of background ideas (a "theory of the instrument"), the specific character or value we find it to have is independent of—not determined by—those background ideas. Even with regard to it, doubt might always arise; but in its absence, the character or value of the "given" can lead to modifications in other beliefs.

### III

Through what I have called a process of internalization, science has found it possible more and more to achieve autonomy from external influences in building its future beliefs, methods, problems, rules of reasoning, explanatory patterns, standards, and goals. It has done so by relying on a body of background beliefs selected for their success in accounting for their domains of responsibility and for their coherence with theories of other relevant domains. It has learned how to learn. Things need not have turned out that way. Connections between things in nature might have been so tight that a piecemeal approach to inquiry would have failed; theories of different domains might not have been coherent with one another; and so on. The achievement of internalization is a contingent matter, not one of logical necessity or of the nature of science.

But because the degree of such autonomy is a function of the available background beliefs, it is evident that the information will, in at least some cases, to



some extent, be insufficient to guide the construction of new beliefs and research programs. And where our legitimized “internal” background beliefs are inadequate, we must look elsewhere for guidance. Sometimes we will have to look to less well-founded beliefs, or to beliefs which, however well-founded, have not been shown to be unambiguously relevant to the domain under consideration. And although it is highly unlikely, for example, that new approaches in contemporary particle physics, if they do not come from within physics itself, will come from anywhere else except mathematics, there is nothing to rule out occasional success of purely external appeals.

But the situation in modern science is radically different from what it was in early periods. Then, as we have seen, choice of what background considerations to appeal to or rely on was very much open: the distinction between internal and external considerations, between the scientific and the non-scientific, had not yet evolved. To say that the hypotheses proposed, the problems conceived, the standards applied, and so forth, were shaped or even determined by external (philosophical, political, economic, social, psychological) considerations, even in the majority or perhaps all cases, can be quite convincing. But to make similar all-embracing claims about modern physics, for example, is to ignore the substance of the subject. Sociology of science does itself a disservice when (and if) it makes the blanket claim, as some adherents of the “Strong Program” frequently appear to do, that all alleged internal considerations in science are externally determined, or even the “weak” blanket claim that, though there are internal considerations, they must always be supplemented by external ones to permit scientific development. One of the reasons why such extreme views have been advocated has been the difficulty of seeing how the internal-external distinction could be formulated given the rejection of the traditional view of science which I surveyed at the beginning of this paper. But the view I have outlined here provides a formulation of that distinction which is completely coherent with the newer view of the pervasiveness and depth of scientific change.

With this new formulation of the distinction in hand, it is possible to conceive more clearly the tasks of the sociology of science. To legitimize itself as a subject it does not need to deny that there are internal considerations at work in science, or to claim that such internal considerations as there may be are inadequate to do the job of science. But the present view, while showing how the internal-external distinction is viable, and how much of science—increasingly through the process of internalization—is directed by internal considerations, leaves open the possibility that in *some* cases,

even in the most modern circumstances, the internalized background of science *is in fact* inadequate. What sociology of science needs are sharpened tools for deciding, *on a case-by-case basis rather than in the light of a general thesis*, if and when this happens. That is, rather than presupposing, on the basis of very general philosophical arguments, that *all* science is (or should be viewed from the sociological perspective as) shaped by external considerations, it needs to be able to decide *when* “negotiation” is concluded in the light of scientific considerations and *when and to what extent* such conclusions are arrived at on the basis of factors other than scientific. (It might of course conclude that whether the factors involved in a particular case are external or internal is ambiguous—a possibility clearly permitted by my formulation of the distinction.) In other cases, the immensely important traditional task of the sociology of science remains: to show how extrascientific considerations often combine with internal ones to direct science, sometimes aiding, sometimes conflicting with, the direction that would be taken if internal considerations alone were operative. (Whether and how, for example, social and political conditions in England in the late seventeenth and eighteenth centuries<sup>4</sup>, or in Germany in the 1920s<sup>5</sup>, *combined with* internal factors to influence the course of science, rather than determining that course by themselves.) But for this purpose the sociology of science *requires* the distinction between internal and external factors, and that is why it does itself a disservice by denying it—in addition to the fact that denying it appears so perverse.

For the sociology as for the philosophy of science, the day is past for sweeping universal theses about the nature of scientific thought based on a few general and questionable philosophical or methodological contentions, or on case studies which arguably presuppose rather than support those contentions. We need a closer look at the subject purportedly under investigation, one which recognizes the patent differences between the considerations relied upon in sophisticated science as scientific, and those which, while often influencing science (as science influences them), are *de facto* non-scientific, external to science. And we need a look which recognizes that the extent and character of those interactions may be quite different from case to case, and in some cases may even be non-existent. In short, it is time at last for a piecemeal approach.

## NOTES

1. A good survey of current approaches in the sociology of science is Michael Mulkay, *Science and the Sociology of Knowledge*, (London: Allen and Unwin, 1979). The “Strong Program” is

advocated in the writings of David Bloor, Harry Collins, Trevor Pinch, and many others.

2. Further details of the view outlined here may be found in the essays collected in my *Reason and the Search for Knowledge*, (Dordrecht: Reidel, 1983); "The Concept of Observation in Science and Philosophy," *Philosophy of Science*, March, 1982, 485-526; and "Objectivity, Rationality, and Scientific Change," in *PSA 1984*, P. Kitcher and P. Asquith, eds. (East Lansing: Philosophy of Science Association, 1985); and "Method in the Philosophy of Science and Epistemology: How to Inquire about Inquiry and Knowledge," in *The Processes of Science*, ed. N. Nersessian, (Dordrecht: Nijhoff, forthcoming). In those and other works I argue, beyond the theses of the present paper, that the "internal" considerations, upon which science increasingly is able to rely, count as "rational" considerations, and that they can, and sometimes do, provide "knowledge." Those further contentions, however, are not required here, where the sole purpose has been to outline the basis of the internal-external distinction and its relevance to the sociology of science as I see it.

3. It would be impossible to list all the primary and secondary references relevant to the cases discussed in the following pages. Many such references are given in other writings of mine, especially *Reason and the Search for Knowledge* (see footnote 2). A few major books on the cases discussed in this

paper are the following: W.K.C. Guthrie, *A History of Greek Philosophy*, 6 volumes (Cambridge: Cambridge University Press, 1962-1981); G. Vlastos, *Plato's Universe* (Seattle: University of Washington Press, 1975); A. Koyre, *The Astronomical Revolution* (Ithaca: Cornell University Press, 1973); R.P. Multhauf, *The Origins of Chemistry* (London: Oldbourne, 1966); M.P. Crosland, *The Language of Chemistry* (Cambridge: Harvard University Press, 1962); R.S. Westfall, *Never at Rest: A Biography of Isaac Newton* (Cambridge: Cambridge University Press, 1980); S.G. Brush, *The Kind of Motion We Call Heat: A History of the Kinetic Theory of Gases in the 19th Century* (Amsterdam: North-Holland, 1976); E.T. Whittaker, *History of the Theories of Aether and Electricity*, 2 volumes, (London: Thomas Nelson & Sons, 1951-1953). Needless to say, the interpretations of cases and the conclusions which I have drawn from them, which are expressed in this paper, have derived not only from these, but also from many other books and articles. That is not to say, of course, that those authors would necessarily subscribe to my interpretations and conclusions.

4. Margaret Jacob, *The Newtonians and the English Revolution* (Ithaca: Cornell University Press, 1976).

5. Paul Forman, "Weimar Culture, Causality and Quantum Theory, 1918-1927," in *Historical Studies in the Physical Sciences*, ed. R. McCormach, Vol. 3 (Philadelphia: University of Pennsylvania Press, 1971), 1-115.

Comments and Reply follow on pages 10-23.