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CHAPTER 2

Second Prolegomenon: Science and Philosophy

If wisdom be attainable let us not only win it but enjoy it.

Cicero

§ 1. The Sciences

The distinction drawn between science and philosophy is of relatively recent origin. Prior to the nineteenth century, science was considered to be a part of philosophy and not an intellectual endeavor separate from it. What we know today as the scientific method began to take on its modern form in the period from the mid-1500s to the early 1700s, and what we now call science was known until into the nineteenth century as natural philosophy.

The term science far predates the developments of the sixteenth through eighteenth centuries, and many different intellectual endeavors have been called science. In the history of science the roster includes topics such as ethics, theology, astrology, and poetry, which are no longer considered to be sciences, and topics such as biology and astronomy which kept the name. This fluidity in what is or is not to be considered a science raises a question we should consider at the very beginning: What *is* "science"?

Perhaps a good place to begin our consideration of this question is to take a role call of those disciplines and areas of scholarship recognized by the modern university system. The so-called 'natural' sciences of physics, chemistry, biology, and the sub-disciplines which arose from them, are accepted by one and all as being sciences. Indeed, some people hold the view that these are the *only* sciences, although this view is probably in the minority today and perhaps always has been.

Alongside the natural sciences, we may place the so-called 'social' sciences, represented by such fields of study as psychology, economics, sociology, anthropology, archaeology, and political science. Are these sciences? Unlike physics, chemistry, and biology, each of the social sciences has had its right to be called a science challenged at one time or another by a considerably large number of people. It is probably accurate to say that the majority view today holds that economics and psychology (with some disputation over psychoanalysis) are indeed sciences, and that anthropology, with its ties to biology and paleontology, is at least 'scientific'.

Political science, more so than any of the other social sciences, finds many detractors who challenge its right to the name science. One thing that makes political science interesting with regard to this question is that some of the people who claim it is not a science are themselves political scientists. Writing in 1929, Charles A. Beard, a professor of history and political science, said, “No science of politics is possible; or if possible, desirable.[1]” It is possible to raise numerous objections and arguments against the possibility that political science can be a science. Most of these objections cite such factors as the uniqueness of each political situation, the fact that different people hold different political views (and do so more or less unpredictably), and that experimentation, in the sense of physics or chemistry, is not possible for political science. On the other hand, there is no shortage of political scientists willing to step forward to refute these charges. Esslinger[2] argued that a political *science* was possible by arguing that Beard and others of a similar mind did not understand what a science really is. Esslinger’s own view of what constitutes a science is somewhat self-serving to his argument (for example, his view that science is not a noun, but a verb, i.e., an activity he calls “sciencing”). The case he makes in favor of the possibility of developing a science of politics does not seem to be particularly strong, but he does make some good points. Johnson and Joslyn[3] make a somewhat stronger case by distinguishing between “traditional” political science (prior to 1950) and modern or “behavioral” political science (post 1950) and by thoroughly discussing various research methods that a science of political science can / must employ. It is clear that they view political science as a science, but one in which not all of its practitioners necessarily practice the scientific method.

In public, and in polite conversation, most ‘natural’ scientists (and, for that matter, most engineers) do not challenge the claim of any of these disciplines to be a science, but in private one finds a diversity of opinion among physicists, chemists, biologists, etc. as to whether the social sciences, considered individually, are sciences. One sometimes hears these groups referring to “exact” science (e.g., physics) and “inexact” science (e.g., economics) as a polite way of distinguishing between natural and social science.

Is history a science? In many universities history is classified among the “social science electives” in the core curriculum. Yet it is probably true that the great majority of people, including some historians, do *not* consider history to be a science, and very few natural scientists are willing to call history by the name of science. The study of history presents an interesting border case for the question of what is or is not a science. In the first place, history has much in common with literature or epic poetry (such as Homer’s *Iliad*). Like the novelist or the epic poet,

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1 C.A. Beard in *Research in the Social Sciences: Its Fundamental Methods and Objectives*, (ed.) Wilson Gee et al., NY: Macmillan, 1929, Chap. IX.

2 William Esslinger, *Politics and Science*, NY: Philosophical Library, 1955.

3 Janet Buttolph Johnson and Richard A. Joslyn, *Political Science Research Methods*, Washington, D.C.: Congressional Quarterly, Inc., 1986.

historians tell a *story*. Unlike the novelist or epic poet, however, the historian is vitally concerned that his story relates *facts* and that what is contained in this history is an account of true and actual past events. The Greek root of the word history is *istoría*, "knowledge by inquiry", i.e., *research*. Now, research in and of itself does not constitute a science. Something else is necessary if history is to be a science; but what that something must be is the point where the debate over history's status turns. J.B. Bury pulled no punches: "History is a science, no less and no more.[4]" In his view, "History has really been enthroned and ensphered among the sciences; but the particular nature of her influence, her time-honored association with literature, and other circumstances, have acted as a sort of vague could, half concealing from men's eyes her new position in the heavens." The basis of Bury's view of history as science lies "with the fact that man's grasp of his past development helps to determine his future development."

H.S. Hughes[5] puts forth a less partisan, and much better defended, view that history is *both* an art and a science. History's claim to science, in his view, stems from the historian's ability to find the *meaning* in past events, rather than to simply be a chronicler of those events. This he does by operating "on the boundary where the concerns of anthropology, biology, the humanities, and psychology meet and blend."

But on the other side of the debate, E.M Hulme[6], who was professor of history at Stanford, flatly disagreed with the idea that history either is or can be a science. He bases this view on three major points. First, it is impossible to include *every* fact of past events in any narrative of history and, consequently, the subjective human element of *selection* of which facts are to be included is too great. Secondly, the historian cannot avail himself of experimentation and, consequently, can take no steps to confirm an historical theory. Finally, Hulme accepts the assumption that man has free will and, consequently, the number of possible cause and effect connections at work in every historical event is virtually unlimited.

Finally, as our last commentator on history, Rev. H.B. George[7] rejects the notion that history is a science in extraordinarily strong terms: "To sum up briefly the results of our investigation into historical evidence. There is no such thing as historical knowledge in the strictest sense of the word . . . It is, strictly speaking, belief based on the testimony of others: and that belief may be of any degree."

Moving away from the problematical status of the social sciences, let us look next at the field of engineering. Is engineering a science? Here we have a case where the general public does

[4] J.B. Bury, *An Inaugural Lecture*, Cambridge, UK: Cambridge University Press, 1903.
[5] H. Stuart Hughes, *History as Art and as Science*, NY: Harper & Row, 1964.
[6] Edward Maslin Hulme, *History and Its Neighbors*, N.Y.: Oxford University Press, 1942.
[7] Rev. H.B. George, *Historical Evidence*, Oxford, UK: The Clarendon Press, 1909.

not typically hold a strong opinion and usually does not have a clear idea of what the difference, if any, is between an “engineer” and a “scientist”. But within the technical community, and even within engineering itself, there is a sharp division of opinion – one which is reflected within the university by the fact that while the natural and social sciences are often placed together in the same college (e.g. “the College of Letters and Sciences”), engineering is typically placed in a college of its own, separated from the others.

Furthermore, the typical natural scientist does not go to any great effort to conceal his belief that engineering is not a science. “You do not study science in order to add to our knowledge of nature,” he will say. “You only study science in order to exploit nature for commercial purposes.” In this view, the physicist or the chemist is not far from being right. The roots of engineering lie firmly in the soil of what was once known as the *technical arts*, and it is undeniably true that the majority of practicing engineers serve out their entire technical careers applying what they know to solve problems and produce goods and services. These practitioners rarely, if ever, engage in what would be considered research, applied or otherwise, by the natural scientist.

But, on the other hand, it is *also* true that *some* engineers (usually those with Ph.D. degrees) do engage in research and do carry out investigations on topics in a fashion that is practically indistinguishable from the working practices of the physicist. Let us look at a few examples of this.

The great physicist and Nobel laureate Richard Feynman once made the observation:

> Physicists always have a habit of taking the simplest example of any phenomenon and calling it "physics," leaving the more complicated examples to become the concern of other fields - say of applied mathematics, electrical engineering, chemistry, or crystallography. Even solid-state physics is almost only half physics because it worries too much about special substances [FEYN4: chap. 31: 1].

Even when the physical first principles of a phenomenon are known (or thought to be known), it may well be the case that the equations involved are solvable only for a very few special cases that are of theoretical interest only, and then only because of the fact that the equations *can* be solved in closed form. In the field of solid-state electronics, the electrical, dielectric, and optical properties of basic electronic devices (such as the transistor), as well as the physical processes of crystal growth, are of great commercial interest, and a great deal of the research carried out in this area is carried out by electrical or chemical engineers. “Yes,” the physicist might say, “but the work is still *physics*.” Perhaps. The fact remains that, to the engineer doing this work, the physics is usually considered to be only *part* of the research problem. What is the rest of it? In a word, engineering. That new discoveries emerge from this work is undisputed, and that these discoveries are scientific discoveries can hardly be seriously disputed. If one wishes, it can be argued that in this role the engineer is ‘really’ an ‘applied’ physicist, and there is truth in this assertion. The fact remains, though, that the person *doing* the work, if he or she is an engineer, thinks of his or her work as *engineering* and not as a physics.

As a second example, let us look at the science of system theory. System theory is the study of the behavior of complex systems. Its modern form can be traced to the work of R.E. Kalman, R.E. Bellman, and T.R. Bashkow, all in 1957. System theory is a discipline that arose entirely from the field of engineering (and, in particular, electrical engineering). System theory as a science is concerned with the analysis of systems having multiple input variables, multiple output variables, and whose dynamics are describable as systems of coupled (and often nonlinear) differential equations. Today we find it applied to such diverse applications as aircraft flight control systems, instrumentation failure monitoring and compensation in nuclear power plants, signal processing and recovery in communication systems, and blood flow measurements in medical diagnostics. Some system theorists regard all of science as system theory's domain.

As another example, we can consider *information theory*. The foundation of this discipline is credited to the work of two men: Claude Shannon in 1948, who is generally credited as the founder of mathematical communication theory, and Richard Hamming (also in 1948, although patent considerations delayed publication of his work until 1950), who is widely regarded as the father of the field of error control coding. The development of Shannon theory and its application in coding theory revolutionized the technology of communication systems, and it is no exaggeration to say that all of our modern digital communication technologies in use today owe their existence to this science[8]. What is particularly peculiar about information theory is that virtually no one considers it to be a 'natural' science because its object ('information') is not a physical entity. To use our term from Chapter 1, "information" is a supersensible object. Information theory is concerned, in part, with the measurement of the "information content" within "information-bearing signals" (which *are* invariably physical entities such as voltages, currents, sound waves or, for that matter, smoke signals).

We could continue this discussion of "is *X* a science?" for many more pages. Is medicine a science? Almost everyone will agree that medical science *is* a science; what is interesting here is that the very same criticism leveled against the status of engineering as a science applies nearly verbatim to medical science. Most physicians do not do research; they practice medicine, etc. Is *grammar* a science? Phrased this way, the great majority of people would answer with a resounding, "no!" But if the question is phrased: is *syntax* (i.e. linguistics) a science? then very strong arguments can be made in favor of a "yes" answer. To quote the renowned linguist Noam Chomsky, "Syntax is the study of the principles and processes by which sentences are constructed

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[8] There is virtually no disagreement that information theory is a science. What little argument there is over its status is whether or not information theory is "really" applied mathematics. The situation, in this sense, parallels our earlier discussion about solid-state electronics.

in various languages" [CHOM1: 11]. One need only study Chomsky's work in transformational grammar [CHOM1], [CHOM2] in order to see an extraordinary similarity between the methods of syntax research and the methods of empirical natural science.

It is quite evident from these examples that the question "what is science?" is much less clear-cut than one might initially suspect. In part, this is a consequence of different groups of people employing different definitions or descriptions of what constitutes and characterizes a science. Yet, if the term "science" is to have any meaning for us, we must find a proper definition for this term. Let us now look at some of the issues involved in doing so.

§ 2. Positivism and the Natural Sciences

The natural sciences, as disciplines distinct from philosophy, took on their present form primarily in the nineteenth century. While the methods and subject matter that distinguish science from philosophy made their appearance as early as the seventeenth century, the distinction between "empirical science" and "rational science" was not made explicit until Kant formalized these ideas in the latter half of the eighteenth century. In Kant's view, this distinction is not a distinction between two *disciplines* but, rather, a distinction between the *principles* from which the basic elements of empirical vs. rational science are drawn.

The outright divorce of science from philosophy did not occur until Auguste Comte formulated his doctrine of "positive philosophy" in the first half of the nineteenth century. Comte's work resonated with the discontent with and backlash against Hegel's philosophy which, as we saw in Chapter 1, took an extreme rationalist position that was impossible to reconcile with the methods of scientific inquiry. The central tenet of Comte's theory was that only the "positive" sciences (i.e., the study of natural, mental, and social phenomena by empirical methods) deserved to be called *science*. The "rational sciences" of logic and mathematics were no longer considered to be sciences, and philosophy was explicitly denied the title of science. To Comte, all philosophy was merely *speculation* – a term he intended as only slightly less derogatory than *superstition* (which is where Comte placed religion). Henceforth, decreed positivism, there would be no place for "metaphysics" within science.

Comte's views converged with many other nineteenth century attitudes to produce the following picture of science:

1. Science is identical to knowledge of facts;
2. Knowledge of facts is restricted to only that knowledge obtained and verified experimentally;
3. Mathematics and logic are merely formal disciplines and not science;
4. Philosophy and religion are merely conjectural, consisting only of opinions and

beliefs.

In his *New Introductory Lectures on Psycho-Analysis*, Freud described positive science in the following way.

> It asserts that there is no other source of knowledge of the universe, but the intellectual manipulation of carefully verified observations, in fact, what is called research, and that no knowledge can be obtained from revelation, intuition, or inspiration [FREU1: 874].

In Freud's view, science alone can "arrive at correspondence with reality" and "this correspondence with the real external world we call truth."

Comte saw *sociology*, the science of which he was the founder, as one of the two highest forms of science (the other being biology). He saw psychology as being merely a subdivision of biology. There is some irony in this because Freud, who was an unabashed positivist, saw sociology as merely a subdivision of psychology. This conflict of views is merely one of many such contradictions that plague the attitude of positivism. We could dismiss such disagreements as mere quibbling over words if it were not for the importance that precisely understood words have in science. In his *Elements of Chemistry*, the great eighteenth century chemist Antoine Lavoisier wrote

> The impossibility of separating the nomenclature of a science from the science itself is owing to this, that every branch of physical science must consist of three things: the series of facts which are the objects of the science, the ideas which represent these facts, and the words by which these ideas are expressed. Like three impressions of the same seal, the word ought to produce the idea, and the idea ought to be a picture of the fact. And, as ideas are preserved and communicated by means of words, it necessarily follows that we cannot improve the language of any science without at the same time improving the science itself; neither can we, on the other hand, improve a science without improving the language or nomenclature which belongs to it. However certain the facts of any science may be and however just the ideas we may have formed of these facts, we can only communicate false impressions to others while we want words by which these may be properly expressed.

The problem with Comte's positivism goes much deeper than any mere squabbling over terminology. The fundamental problem with positivism is its internal inconsistency. On the one hand, positivism bans metaphysics from any role in science; at the same time, positive science proceeds from a set of assumptions about nature that are nothing else than metaphysical assumptions or prejudices. The difference between positivism and Hume's empiricism is that Hume is fully aware of his assumptions and their effect on theories developed by empirical means, while the positivist refuses to examine his critical presumptions and remains, by choice and doctrine, ignorant of them and their implications. The positivist's professed indifference to his basic principles by which he draws his "knowledge of nature", and the consequences of this attitude, were clearly foreseen by Kant:

> (It) is of course in vain to feign *indifference* in regard to [metaphysical inquiry], the object of which cannot be indifferent to human nature. Besides, these pretended *indifferentists*, however much they may try to disguise themselves by the assumption of a popular style through changes in the language of the schools, unavoidably fall into metaphysical declarations, which they profess to despise so much [KANT1: 4 (A: x)].

Arguing in favor of the rational and *a priori* element of thought having a legitimate place alongside experiment in the business of science, Kant wrote

> Reason . . . must approach nature so as to be instructed by it, not, however, in the character of a pupil, who listens to all his master chooses to tell him, but like an appointed magistrate who compels witnesses to answer those questions which he himself puts to them [KANT1: 14 (B: xiii)].

Time and history have vindicated Kant's view so much that at the close of the twentieth century positivism as a doctrine was dead in the natural sciences. This is not to say that all the influences of the positivist attitude are gone; quite to the contrary, there are a number of attitudes in science that still bear the marks of the positivist era. But it *is* to say that the dogma of positivism has been recognized as untenable by the natural sciences. Let us look at a couple of examples of how the natural scientist viewed his profession by the latter half of the twentieth century. Our first example is taken from an introductory college-level chemistry textbook[1].

> From the earliest times, answers to questions about nature have been sought in two general ways. One way is by pure thought and logical argument, the method of *rationalism*. The other is by gathering facts and making observations, the method of *empiricism*. In science, neither of these methods is wholly independent of the other: One must have some facts before he can rationalize; and one must be logical and rational if he is to interpret his empirical observations. Yet, in a given situation, one method can be emphasized more than the other. . .
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> Chemistry is the rational and empirical study of the structure of matter and the changes that matter undergoes in natural processes and in planned experiments. In chemistry, as in all natural sciences, we rely on empirical tests of our ideas; and, in all sciences except perhaps astronomy and geology, we focus our attention on *reproducible* facts, that is, on events or occurrences that take place in the same way when all conditions are the same.

Note in the above description how empirical methods (observation and experiment) and rational *a priori* principle (the presumption that given the *same* condition, the same thing *should* happen) are tied together, neither taking precedence over the other. Physics was probably the first of the natural sciences to move away from positivism. This change in attitude was more or less forced upon physics in the early twentieth century, primarily by the revolution in physics brought about on the one hand by Einstein's relativity and, on the other hand, by the development of the theory of quantum mechanics. The present day view of physics was well stated by Feynman:

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[1] Charles W. Keenan and Jesse H. Wood, *General College Chemistry*, 4th ed., N.Y.: Harper & Row, 1971.

> The principle of science, the definition, almost, is the following: The test of all knowledge is experiment. Experiment is the *sole judge* of scientific "truth." But what is the source of knowledge? Where do the laws that are to be tested come from? Experiment itself helps to produce these laws, in the sense that it gives us hints. But also needed is *imagination* to create from these hints the great generalizations - to guess at the wonderful, simple, but very strange patterns beneath them all, and then to experiment again to check whether we have made the right guess. . .
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> A few hundred years ago, a method was devised to find partial answers to such questions. *Observation, reason*, and *experiment* make up what we call the *scientific method* [FEYN3: chap. 1: 1, chap. 2: 1].

Accurate as these descriptions are for chemistry and physics, there is still a difficulty when it comes to accepting, for instance, Feynman's "almost" definition as the "general" definition of science. For example, what experiments can *astronomy* conduct? Astronomy can, and does, carry out observations, and it does attempt to explain its observations on the basis of accepted laws of physics. But the simple truth of the matter is that astronomy cannot reproduce stars and galaxies in the laboratory and must substitute computer *simulation* in place of experiment to test its theories of how the universe developed. But simulation is *not* experimentation; the computer will faithfully produce calculations that conform to the mathematical models it is given, but its results are products of *rational thought* and not observations of "nature itself." Are we to conclude that astronomy is not a science?

§ 3. Laboratory and Nonlaboratory Sciences

Astronomy is not alone in facing the situation where its object denies it the possibility of obtaining direct verification of its theories through reproducible controlled experiments. Other disciplines such as paleontology, economics, sociology, and political science face this same situation. Even physics itself faces formidable major challenges in applying Feynman's dictum of "observe, reason, experiment" when it comes to such theories as the theory of quarks in elementary particles[2]. And when theory in physics reaches the point where confirming experiments require energy levels beyond any hope of producing on earth in order to carry out, then physics as a science will have reached the same situation as astronomy finds itself in currently.

All of this is to say that there are some very good reasons to distinguish sciences in terms of *laboratory* sciences (those where controlled experiments in the laboratory environment are

[2] Quarks are entities postulated to be the elementary constituents of the class of "particles" known as hadrons [FEYN1: 131-150]. Quarks, if they do indeed exist as presently thought, may only be "observed" indirectly because, according to the theory, they are bound together in the "particle" they form by the strongest force known in nature (called, appropriately enough, 'the strong force'). This is called the "quark confinement" theorem of quantum chromodynamics. Thus, it has been practically impossible so far to verify the individual existence of quarks directly by isolating them.

possible and practical) and *nonlaboratory* sciences (those where controlled experiments are either impossible or completely impractical). As a less esoteric example, let us consider the science of economics. Economics can be defined as[3]

> the study of a society's use of its scarce resources with reference to (1) the extent to which they are used, (2) how efficiently they are used, (3) the choice between competing alternative uses, and (4) the nature and consequences of changes in productive power of the resources over time.

The economist would love to have the comparative luxury of conducting controlled laboratory experiments to test his theories. Unfortunately, it is extremely rare that he or she is able in practice to conduct controlled experiments of the sort available to chemistry and physics. Since economic theories have been, and continue to be, used by governments in making policy decisions, it is extremely important that economic theories be correct. The consequences of a mistaken policy decision by a government can be profoundly bad for its citizens. But the economist is largely confined to only making observations of what happens and must rely on circumstances beyond his or her control to supply the data required to test and refine economic theory. While it is true that, in the words of Yogi Berra, “you can observe a lot by watching,” economics is and will likely remain primarily a nonlaboratory science.

The study of geology provides yet another example. Geology is the study of the planet earth. It is usual to logically divide this study into (1) physical geology, the study of the origin, classification, and composition of earth materials and the processes which occur on the surface and in the interior of the earth; and (2) historical geology, the study of origins, evolution, changes in the distribution of the lands and seas, the growth and destruction of mountains, and the developmental history of plants and animals through time. It is from the geologist that we hear the earth is some 4.5 billion years old, that from time to time life on earth has experienced mass extinction events, that the seemingly solid continents beneath our feet are actually drifting slowly, and that there once was a time when all of the land mass of the earth formed a single enormous continent. How does the geologist come by such findings?

The geologist does not work so much in a *laboratory* as he or she does *in the field*. On and in the earth he or she finds a wealth of data which, using an eclectic mix of knowledge from various other sciences, can be studied and which provides material for the formulation of hypotheses. However, the geologist can work only with the tangible results of the past, and must work backwards in time to try to find the causes that produced these results. The number of controlled experiments available to the geologist are severely limited and so, like the economist, the geologist must primarily rely on observation, and it is the earth itself that determines what the

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[3] Richard G. Lipsey and Peter O. Steiner, *Economics*, 2nd ed., N.Y.: Harper & Row, 1969.

geologist will be allowed to see and study. To say that the earth is the geologist's *laboratory* is therefore somewhat misleading; the earth is the geologist's antique shop – filled with treasures in odd and mislaid places, which require seeking out, collecting, and analyzing.

Unless we deny the title of science to all endeavors such as economics, geology, astronomy, paleontology and the like, we must clarify what we mean by this word "science" to provide it with the scope necessary to take in the nonlaboratory sciences. This, in turn, requires us to clearly understand the role and proper use of the rational element of science.

§ 4. Paradigms

What is it that transforms what is more or less a mere groping to understand certain facts into the intellectual discipline we call science? What is it that makes an alchemy into a chemistry, the craft of sword-making into metallurgy, or a curiosity about lightning or lodestone into the physics of electricity and magnetism? The real history of the development of a science seldom matches the portrait painted of that history in the usual science textbooks (when these textbooks bother to discuss the history of the field at all). Unintentionally or not, science has somehow gained the reputation of coming to its knowledge by slow and steady accumulation – a new fact here, a new theory there – constructing its understanding of the world "brick by brick" in a manner of speaking. Nothing could be farther from the truth. Historian Thomas Kuhn writes

> In recent years . . . a few historians of science have been finding it more and more difficult to fulfill the functions that the concept of development-by-accumulation assigns to them. As chroniclers of an incremental process, they discover that additional research makes it harder, not easier, to answer questions like: When was oxygen discovered? Who first conceived of energy conservation? Increasingly, a few of them suspect that these are the wrong sorts of questions to ask. Perhaps science does not develop by accumulation of individual discoveries and inventions. . . The more carefully they study, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today. If these out-of-date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge. If, on the other hand, they are to be called science, then science has included bodies of belief quite incompatible with the ones we hold today. Given these alternatives, the historian must choose the latter. Out-of-date theories are not in principle unscientific because they have been discarded. That choice, however, makes it difficult to see scientific development as a process of accretion [KUHN: 2-3].

Kuhn found that the image of science as progressing step-by-step like the second hand of a clock is nothing but a myth. He identifies two "states" in which science operates. The first of these is what he calls "normal" science, and normal science does indeed conform to the step-by-step model of science, often for long periods of time. From time to time, however, every science experiences a period where long-held assumptions, models, and interpretations are overturned by the weight of evidence to the contrary. It is during these periods that old theories, such as phlogiston in chemistry, are discarded and new theories take their place. Kuhn calls these events scientific *revolutions*.

The basis of "normal" science is one or more past scientific achievements that a particular community of scientists accepts for a time as the foundation for its further practice. These achievements usually gain acceptance from their ability to explain past observations and experimental results and to predict *new* results that subsequent investigation verifies. In this way the assumptions, axioms, and principles that constitute the basis of these seminal achievements become the *paradigm* of the science.

> Effective research scarcely begins before a scientific community thinks it has acquired firm answers to questions like the following: What are the fundamental entities of which the universe is composed? How do these interact with each other and the senses? What questions may be legitimately asked about such entities and what techniques employed in seeking solutions?
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> . . . Normal science, the activity in which most scientists inevitably spend almost all their time, is predicated on the assumption that the scientific community knows what the world is like. Much of the success of the enterprise derives from the community's willingness to defend that assumption, if necessary at considerable cost [KUHN: 4-5].
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> . . . The commitments that govern normal science specify not only what sorts of entities the universe does contain, but also, by implication, those that it does not. [KUHN: 7].
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> Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, i.e., for the genesis and continuation of a particular scientific tradition.
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> . . . Acquisition of a paradigm and of the more esoteric type research it permits is a sign of maturity in the development of any given scientific field [KUHN: 11].

The adopting of a paradigm is essential to the development of a science as a discipline. It provides a fundamental organizing principle around which the flood of data and observations can be channeled, charted, and understood in a broad and general context. A paradigm stipulates what the scientist can take "for granted" and what he or she cannot. It supplies norms for the judgment of empirical outcomes and for recognition of "surprising" phenomena.

> In the absence of a paradigm or some candidate for a paradigm, all the facts that could possibly pertain to the development of a given science are likely to seem equally relevant. As a result, early fact-gathering is a far more nearly random activity than the one that subsequent scientific development makes familiar. Furthermore, in the absence of a reason for seeking some particular form of more recondite information, early fact-gathering is usually restricted to the wealth of data that lie readily to hand. . . Because the crafts are one readily accessible source of facts that could not have been casually discovered, technology has often played a vital role in the emergence of new sciences [KUHN: 15-16].

While paradigms govern the course of normal science between “revolutions,” these paradigms are generally not “eternal truths” and, from time to time, events or discoveries occur that drastically and fundamentally alter the basic paradigm of a science. Feynman notes

> (The) philosophy or idea around a theory may change enormously when there are very tiny changes in the theory. For instance, Newton's ideas about space and time agreed with experiment very well, but in order to get the correct motion of the orbit of Mercury . . . the difference in the character of the theory needed was enormous. . . In stating a new law you cannot make imperfections on a perfect thing; you have to have another perfect thing. So the differences in philosophical ideas between Newton's and Einstein's theories of gravitation are enormous [FEYN2: 168-169].

§ 5. Rational Principles

If paradigms play such a vital role in normal science, we must ask ourselves: Where do we get our paradigms? Certainly our observations and data play some role in the formation of paradigms. But we must also remember the lesson that Hume taught: *Empirical data contain no general principles*. Every observation, every fact gleaned from study or experiment, is *particular*. It announces nothing other than itself and it carries no family album that identifies other facts to which it is related. To this raw data something else must be added, and since a datum is only the matter of experience and not experience itself, this something-else must be *a priori* (prior to experience) and, therefore, a product of reason. It must be, in other words, a *rational principle*.

> No natural history can be interpreted in the absence of at least some implicit body of intertwined theoretical and methodological belief that permits selection, evaluation, and criticism. If that body of belief is not already implicit in the collection of facts - in which case more than "mere facts" are at hand - it must be externally supplied, perhaps by a current metaphysic, by another science, or by personal and historical accident [KUHN: 16-17].

Contrary to the myth of positivism, the inference of reason, i.e., the principle derived from reason *a priori*, plays a central role in the establishment of scientific paradigms. These rational principles are not in the least mystical or incomprehensible. They originate, rather, in our ideas of how things “ought to be” or how things “must be” in the world. The mere fact that such ideas are *a priori* and, for the most part, are descriptive of supersensible objects, does not make them either infallible or innate in the mind (in the sense of innate ideas). Quite to the contrary, they are frequently fallible and never innate in the sense that Locke or Leibniz used that term.

How shall we describe these rational principles? The formal and rigorous exposition of such principles will take us most of the rest of this treatise to establish. But that does not mean we cannot describe the ‘feel’ of such principles. Let us take two examples to illustrate what we are talking about here. The first is taken from Bernstein’s biography of Albert Einstein. In a letter to an Illinois historian written in 1954, Einstein described why the famous Michelson-Morley experiment, which shocked the world of physics in 1887 by destroying the mechanistic paradigm that supported the prevailing view of Maxwell's theory of electromagnetism, had no important influence on his development of the theory of relativity.[4]

> In my own development Michelson's result has not had a considerable influence. I even do not remember if I knew of it when I wrote my first paper on the subject (1905). The explanation is that I was, for general reasons, firmly convinced that there does not exist absolute motion and my problem was only how this could be reconciled with our knowledge of electro-dynamics. One can therefore understand why in my personal struggle Michelson's experiment played no role or at least no decisive role.

Einstein was one of the first of the great twentieth century physicists to abandon the positivism of the nineteenth century in favor of the role that rational *a priori* principles can play in science. He was famous for his "thought experiments" in which he followed lines of reasoning that compared theoretical consequences of physical theories against rational concepts founded on "general reasons." The conclusions he drew from such thought experiments led to the revolutionary developments of the theories of relativity and quantum physics.

Richard Feynman also described, in somewhat more ambiguous terms, the role that "philosophies" played in his approach to science [FEYN2: 169-171].

> What are these philosophies? They are really tricky ways to compute consequences quickly. A philosophy, which is sometimes called an understanding of the laws, is simply the way that a person holds the laws in his mind in order to guess quickly at the consequences.
>
> . . . One of the most important things in this 'guess-compute consequences-compare with experiment' business is to know when you are right. It is possible to know when you are right way ahead of checking all the consequences. You can recognize truth by its beauty and simplicity. . . When you get it right, it is obvious that it is right - at least if you have any experience - because usually what happens is that more comes out than goes in. Your guess is, in fact, that something is very simple. If you cannot see immediately that it is wrong, and it is simpler than it was before, then it is right.

Neither Feynman nor Einstein state in detail what their "general reasons" or "philosophies" are, and the vagueness with which they express this factor of their thinking prevents us from discovering in their words the principles actually used by these renowned scientists. Even so, it seems evident that both men are speaking of a set of "laws about laws" that governs or guides their intellectual process of discovery.

Such general principles cannot be given empirically because empirical data contain no general principles, as we have said and as Hume argued in painstaking detail. The word for a system of such principles is **metaphysic**. This brings us to the question of how metaphysics stands in relationship to the science for which it is the source of rational principles.

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[4] Jeremy Bernstein, *Einstein*, N.Y.: Viking Press, 1973.

§ 6. Kant's Philosophy of Science

We now return to the main question posed at the beginning of this chapter: What is science? From what we have already discussed, we have seen that science involves both empirical and rational factors, that it requires a paradigm, and that somewhere in the roots of this paradigm it requires rational *a priori* principles, but that these principles cannot be the innate ideas of the rationalist philosophers. The observation of empirical facts and their subsumption under generalizing theories is a necessary character of a science. We have seen that laboratory experimentation is enormously useful as a method of testing the conclusions of a theory, but that there are some topics of disciplined human inquiry that do not lend themselves to study by the method of controlled experimentation. Either we must deny to these fields the name of science, or we must admit that the controlled experiment is not a necessary factor in science.

It is clear that the result or outcome of a science is an organized, self-consistent, and unified body of knowledge. Obviously such a body of knowledge for any active science is not complete; if it were, the science would be done with its work. Such is the state, in the view of physicists, of what is now called classical physics, i.e. physics prior to relativity theory and quantum theory[1]. Scientific knowledge is *systematic* knowledge, and it is this systematic, interlocked character, and not its completeness, that gives scientific knowledge its power and importance. But what is it that brings this systematic character into the body of scientific knowledge? Kant explained it in the following way.

> I understand by a *system* . . . the unity of manifold knowledge under one Idea. This is the rational idea of the form of a whole so far as through this the scope of the manifold as well as the place of the parts with respect to each other is determined *a priori*. The scientific rational idea therefore contains the purpose and the form of the whole which is congruent with it. The unity of the purpose, to which all the parts are related and in the Idea of which they are related to each other, allows any missing part to be noticed by cognizance of the rest, and there can occur no contingent addition or undetermined magnitude of perfection that does not have its boundaries determined *a priori* [KANT1: 532 (B: 860-861)].

Any scientific study has an *Object*, i.e. that which constitutes the purpose of the study. This purpose limits the scope of the investigation. For instance, if I wish to study the properties of bacteria, I will not ponder the effects of disease on the economy of nations, nor will I concern myself with what would be the best material from which to construct bathtubs. The purpose of the study defines the type and forms of knowledge *relevant* to the purpose of the study. I may not,

[1] This view is, of course, not shared by engineers, most of whose work involves the application of classical physics - what one might call "filling in the gaps." As the practice of Kuhn's "normal" science, the applied research carried out by engineers using classical physics aims to increase the *breadth* of knowledge that can be gained under the paradigms of classical physics, whereas physics largely concerns itself with the *depth*.

and probably will not, know at the beginning what constitutes the complete set of relevant factors that will eventually come into my study of bacteria, but my purpose will help me to determine whether or not some particular fact fits in the proper scope of the study. Those facts that have a relationship to my purpose also, *ipso facto*, have a relationship with each other. This is the *rational idea* that underlies the concept of a system. From this idea it also follows that if I cannot see how two or more parts of the system relate to each other, other than by the mere fact that both relate to my purpose, then I know that something is missing from my system.

With this reminder of the systematic character of scientific knowledge, we may now better grasp Kant's dissection of the idea of natural science:

> Every doctrine constituting a system, that is, one in accordance with the principle of a disciplined whole of knowledge, is called a science; and as its principles may be either principles of the *empirical* or *rational* connection of knowledge in a whole, so natural science . . . would have to be divided into *historical* or *rational* natural science, were it not that the word *nature* (as implying the deduction of the manifold, pertaining to the *Dasein*[2] of things, from its inner principle) necessitates a knowledge through reason of its context if it is to deserve the name natural science. Hence the doctrine of nature may better be divided into *historical doctrine of nature*, comprising nothing but systematically-ordered facts respecting natural things . . . and *natural science*. Natural science . . . would be either natural science *properly* or *improperly* so-called, of which the first would treat its object wholly according to principles *a priori*, and the second according to laws derived from experience [KANT15: 137-138 (4: 467-468)].

Let us wade through the tangle of Kant's complex, run-on grammar and extract his essential ideas. First, we find that *any* science is "a doctrine constituting a system." We have just examined what it means for a body of ideas to be a *system*. The word "doctrine" refers to "teachings" or instruction, and so we have a science whenever that which our body of facts teaches us forms a disciplined and orderly system of knowledge.

Now, our "facts" may be either empirical (given through experience) or rational (given through reasoning according to *general* principles). If our facts are systematically organized but have no necessary connection with each other outside of their common connection in the purpose of the topic (for example, if our doctrine were merely a catalog of different kinds of birds), we have a merely *historical* doctrine. The science of such a doctrine is not given by the body of knowledge itself but by the methodology or *system* under which the facts are collected and organized. The unity is in the *method* rather than the *outcome*. The nineteenth century spectroscopists – who measured and cataloged the wavelengths of light emitted by gases when these gases were suitably excited by electric currents, and who were able to fit their observations

[2] Kant's word means 'existence' with the connotation of denoting a subject in the sense of its being present or actual, i.e., to be a being or entity (e.g., "being" in the sense of "a human being"). In German, *Dasein* is distinct from *Existenz*, which means existence in the context of a manner or mode or *structure* of being, as in "he lived a miserable existence." *Existenz* has the connotation of denoting a predication on a subject.

using empirically derived formulas – were therefore scientists, but the body of knowledge they collected constituted merely an historical doctrine of knowledge.

In 1913, a young physicist named Niels Bohr combined the empirical theory of Planck (who formulated the empirical law that radiation could be emitted or absorbed only in discrete packets called quanta) and the empirical theory of Rutherford (who formulated the idea that electrons in an atom orbit a positive nucleus) and developed an empirical law (known today as the "Bohr model"). This model succeeded in uniting the empirical formulas obtained by the spectroscopists under a generalizing *theory*. In doing so, Bohr transformed the historical doctrine of the spectroscopists into what Kant would call a natural science. But since, at that time, Planck's theory was derived solely from empirical experience and lacked an *a priori* principle from which it could be derived, Bohr's theory fell into the category Kant called natural science *improper*. Indeed, Bohr's theory actually stood in direct contradiction to another well-established law of physics, namely the Maxwell theory of electromagnetic radiation, and Bohr was forced to postulate that the Maxwell theory simply didn't apply for some reason to the atom.

This contradiction was one of the great problems with the Bohr model of the atom. Over the next eighteen years, physicists studied this theory relentlessly *because it was unacceptable to them* that the atom should be immune to the laws of electromagnetic phenomena. But this principle – that there could not be such a disconnect between quantum theory and Maxwell's theory – is a *rational principle* of the *a priori* type. Experience did not demand that the basic contradiction between these two theories be removed; instead, it was the *sense of how things must be*, i.e. a *metaphysical* principle, that demanded the unification of quantum theory and the classical theory of electromagnetism.

Does this rational principle transform the scientific doctrine of quantum theory into a proper science, in the Kantian sense of that term? According to Kant's classification, a proper science is one which "would treat its subject wholly according to principles *a priori*." Did the new quantum theory do this? If we examine this theory, we find that its first principles were based on such ideas as electric charge, probability amplitudes, potential and kinetic energy, and so forth. The objects of these ideas *are all supersensible objects*. There is no possibility whatsoever that these objects, *as things in themselves*, could ever be objects of *experience*. They comprise a *metaphysical substratum*, derived through reasoning, in which *the phenomena of experience are united*. It was at this point where the physics of the atom became a *proper* science in Kant's sense of that term. The fact that it was the phenomena of experience that led physicists to the conception of these ideas of supersensible objects is not relevant to this classification. The reason for this is a simple one: no amount of experience could ever *provide* us with the idea of electric charge or probability amplitudes or any of the other supersensibles at the rational base of this science. The ideas of these objects are products of the mind, and they owe their existence (in the

Existenz sense of the word rather than the *Dasein* sense of the word) solely to the power of the mind to produce such ideas.

It is now an easier task to extend Kant's definition of science to fields other than "natural" science. Science in general is a doctrine constituting a system. Any system must present a unified body of knowledge. If this unity is provided solely by the method of obtaining the facts of this body of knowledge, our science is an *historical* doctrine. If the facts themselves are united by a principle, but this principle itself is only an empirical principle, brought into being for no other reason than as an *ad hoc* statement needed to reconcile otherwise diverse experiences, we have an *improper* science. If the unity of the system is brought about through *a priori* principles dealing with supersensible objects of reason (that is, through a metaphysic), we have a *proper* science.

It is of the greatest importance for us to recognize that nothing in these definitions requires or implies our scientific knowledge must be true *to an absolute certainty*. "Truth" is, in Kant's words, "the congruence [*Übereinstimmung*] of a concept with its object" [KANT1: 73 (B: 82)]. "Things" are not true or false; our *concepts* of things are true or false according to whether experience and reason supports or gainsays the agreement between a concept and the appearance of the object of the concept. If the concept arises from experience, experience could later be found to contradict the concept. For example, the concept of *mass* is an idea that unifies the concept of a force acting on a corporeal body and the concept of the acceleration undergone by that body. In seventeenth century physics this idea of the "mass" of a corporeal body was *made to include* the notion that a body's mass was representative of the "amount of matter" in that body. Therefore a given body was thought to have a definite and fixed amount of mass. With the coming of the relativity theory the idea that mass was strictly a property of a corporeal body came into conflict with the relativity theory *and with the experimental experience* that supported Einstein's relativity theory. The old concept of mass was thus demonstrated to be *false* and the offending idea – that mass represents somehow the quantity of matter in an object – was dropped, giving us a new idea of mass.

"Truth" and "certainty" are two quite different ideas. The degree to which we *hold* a concept to be true is a kind of measure of the perfection of the concept. If we call this the degree of certainty, we can distinguish concepts according to whether we regard them as *knowledge*, as *belief*, or merely as *opinion* [KANT8: 72-80 (9: 65-81)]. But so long as and so far as the concept and its object are congruent, regardless of the degree to which we hold the concept to be true, there is truth in the concept.

This distinction between truth and certainty must receive a fuller treatment in order to be properly appreciated, but we will postpone that treatment until later in this treatise. However, the fact that there *is* such a distinction raises another question concerning the nature of science. A proper science rests on *a priori* principles, and such principles always involve ideas concerning supersensible objects. While we have yet to establish whether it is possible to be certain of the truth of such ideas, it seems obvious that there are some such ideas, and very important ones at that, of which we cannot be *absolutely* certain. If we make absolute certainty a criterion for naming a doctrine a science, would any science survive this test? Physics would not since, by the frank admission of the physics community, there are plenty of concepts within physics of which the physicist is not *absolutely* certain. But if physics falls, it takes chemistry, biology, and the other "natural" sciences with it because *all* these sciences draw some part of their paradigms from physics. This is too high a price to pay for absolute certainty.

If we do not require absolute certainty as the distinguishing mark of a science, does this make all intellectual endeavors, whether scientific, religious, or speculative, equal? Let us take, for example, the claim of so-called "creation science" that it stands on equal footing with the theory of evolution and, consequently, that biology classes taught in the public schools should be required to include a component of "creation science" as a counterpoint to the theory of evolution. Stripped of the wearisome rhetoric in the public dispute on this question, the fundamental justification for the position of the creationists seems to be simply that "evolution is only a theory" and, because it is not known to be true with absolute certainty, the opposing viewpoint of creationism merits an equal place in science classes.

This point of view seems to me to confuse "truth" with "certainty." The evolution theory has proven itself to be quite fecund in the science of biology and, while not an absolutely certain truth, the theory of evolution and its object are in congruence and, therefore, true. Furthermore, the degree of certainty in which it is held to be true by the community of scientists is great. Contrast this with "creation science". In order to hold creationism to be true, it is necessary to substitute various miracles as *ad hoc* principles for overcoming fundamental contradictions between the creationist timeline and extremely well established laws of both *physics* and *geology*[3]. Against the wealth of facts united under these sciences, creationism can only point to the Bible as its first principle. Does creation science merit the same degree of holding-to-be-true that these other sciences have achieved? Physics is a proper science; "creation science" is not and its reliance on *ad hoc* miracles renders it unsystematic and, therefore, it does not merit the title of science at all. This example, then, gives us the answer to the question raised above. All intellectual endeavors are not equal and no such endeavor can base its claim to be a science, in even the smallest part, on the ground that some *other* doctrine recognized as a science does not possess certainty in its theories. It must win the title strictly and solely on its own merits.

[3] c.f. Donald U. Wise, "Creationism's Geologic Time Scale," *American Scientist*, vol. 86, no. 2, Mar.-Apr. 1998, pp. 160-173.

§ 7. Science, Metaphysics, and the Critical Philosophy

The positivists of the nineteenth century would have found it intolerable to think that a proper science must have a metaphysical basis in its paradigm. Given the reputation of metaphysics, then and now, this attitude is completely understandable. Moreover, I would not feel shocked if a majority of the readers of this treatise experienced the same reaction. One need only visit the "philosophy" section of the average bookstore and view the collection of eastern mysticism and so-called "new age metaphysics" to gain a sharp lesson in the kind of mumbo-jumbo and trash that popularly passes itself off today as metaphysics. If mysticism is metaphysics then metaphysics is nothing but a word meaning any harebrained opinion. But before we commit this treatise to the flames, let us ask yet another question: What is metaphysics?

§ 7.1 The General View of Metaphysics

We owe the classification of metaphysics as a separate branch of study to Aristotle, whose name for it was "first philosophy." Aristotle did not originate metaphysics; if anyone can claim to be the inventor of metaphysics, it would be Parmenides (*c*. 540-470 B.C.). But it was Aristotle who first gave a systematic structure and discipline to the study of metaphysics, and provided a definition of this study as "the science which considers What Is simply in its character of Being, and the properties which it has as such." Aristotelian metaphysics is ontology.

In Aristotle's view, the primary mode of "Being" is called "substance" and so for Aristotle metaphysics is the science of the characteristics and properties common to all substances. Unlike the special sciences, which always limit the scope of their inquiries to a specific topic, the scope of metaphysics in general is *unlimited* since it must apply to all things as objects of both the sensible and supersensible types. Metaphysics, in other words, must supply the grounds or "first principles" that underlie all other branches of knowledge and which provide the basic set of acroams from which the other branches of knowledge must proceed. Put another way, metaphysics examines the presuppositions of all other sciences.

Aristotle divided his "first philosophy" into two parts: logic and "the science of substance." This division mirrors his doctrine of substance as the union of matter and form. Logic is *form*: it provides the "rules of demonstration and argument" pertinent to the subject matter. The science of substance (today called ontology) is *material*, i.e., metaphysics deals with the *subject matter* to which the forms of logical argument are applied. To paraphrase Joad, "The logician is interested in how the scientist thinks; the metaphysician [is interested in] the general nature of that which [the scientist] thinks about" [JOAD: 159].

Within this general description of metaphysics, we find through history that various philosophers have held greatly differing views about the *details* of metaphysics. Indeed, as Joad has observed, someone who undertakes to study philosophy ought to be warned “that he will be asked to take part not in a steady and ordered advance from speculation to knowledge but in a series of marches and counter-marches, in the course of which he will traverse and retraverse the same territory in the company of travelers whose concern seems less to arrive at a goal than to obliterate the footsteps of their predecessors” [JOAD: 9]. For example, the controversy between rationalism and empiricism can be traced back at least as far as Plato (Aristotle’s teacher and the foremost classical philosopher of rationalism) and Aristotle (the foremost classical empiricist).

This character of the historical method of the study of philosophy is perhaps one of the things about this subject that the scientist (who cherishes the ideal of the steady advance of knowledge) finds most objectionable. So it is that Hume, the naturalist, wrote that any writing which concerned nothing but metaphysics should be “committed to the flames” and why the positivists tried, unsuccessfully, to stamp out metaphysics altogether. So, too, it was that Kant wrote [KANT1: 3 (A: viii-ix)]:

> There was a time when she was called the *queen* of all the sciences; and, if we take the will for the deed, she certainly deserves, so far as regards the high importance of her object, this title of honor. Now, it is the fashion of the time to heap contempt and scorn upon her and the matron mourns, forlorn and forsaken, like Hecuba:
>
> Modo maxima rerum,
> Tot generis, natisque potens . . .
> Nunc trahor exul, inops.
> OVID, *Metamorphoses*[1]

The standing of the philosophy department in most American universities – whether measured in terms of prestige, funding, the number of students enrolled in it as their major, faculty salaries, and so forth – bears witness to the aptness of Kant’s simile in today’s culture. Failure to achieve is an unforgiven sin in American culture, and so Bloom could write[2]:

> Most interestingly of all, lost amidst this collection of disciplines, modestly sits philosophy. It has been dethroned by political and theoretical democracy, bereft of the passion or the capacity to rule. Its story defines in itself our whole problem. Philosophy once proudly proclaimed that it was the best way of life, and it dared to survey the whole, to seek the first causes of all things, and not only dictated its rules to the special sciences but constituted and ordered them. The classic philosophy books are philosophy in action, doing precisely these things. But this was all impossible *hybris*, say their impoverished heirs. Real science did not need them, and the rest is ideology or myth. Now they are just books on a shelf. Democracy took away philosophy's privileges, and philosophy could not decide whether to fade away or to take a job.

[1] "Only yesterday the greatest of wealth, strong in so many sons . . . now I'm dragged an exile, destitute."
[2] Allan Bloom, *The Closing of the American Mind*, N.Y.: Simon and Schuster, 1987, pg. 377.

If normal science, in Kuhn's terminology, does not need philosophy, it is only because its current paradigm (which it gets in part from philosophy) is still adequate. But let us not forget that *every* branch of science (with the exception only of those sciences, such as biochemistry, that form out of two or more previously established sciences) originated in philosophy and, like Hecuba's sons, are philosophy's children. Bloom summarizes the condition of current-day philosophy in America in the following way[3]:

> Philosophy was architectonic, had the plans for the whole building, and the carpenters, masons, and plumbers were its subordinates and had no meaning without its plan. Philosophy founded the university, but it could no longer do so. We live off its legacy. When people speak vaguely about generalists vs. specialists, they must mean by the generalist the philosopher, for he is the only kind of knower who embraces, or once embraced, all the specialties . . . and not just a collection of the matters of the specialties. . . An American high school student knows only the word "philosophy," and it does not appear to be any more serious a life choice than yoga. In America, anyhow, everybody has a philosophy. Philosophy was not ever a very powerful presence in universities . . . We began with a public philosophy that sufficed for us, and we thought that it was common sense. In America, Tocqueville said, everyone is a Cartesian although no one has read Descartes. . . One need not have read a line of philosophy to be considered educated in this country. It is easily equated with hot air, much more so than any other humane discipline.
>
> But it has succumbed and probably could disappear without being much noticed. . . Positivism and ordinary language analysis have long dominated, although they are on the decline and evidently being replaced by nothing. . . in sum, the philosophy landscape is largely bleak. That is why so much of the philosophic instinct in America used to lead toward the new social sciences and is now veering off toward certain branches of literature and literary criticism. As it stands, philosophy is just another humanities subject, rather contentless, without a thought of trying to take command in the crisis of the university.

That positivism succeeded in its revolution to the extent of dethroning philosophy, at least in America, as a serious discipline of science is manifestly obvious. But, as I have argued above, to understand how science works we must understand what metaphysics *was and should be again*, and have a clear view of its relation to science in general. In view of Joad's apt description of the philosophical landscape quoted above, our first question should be: which one, if any, of the many approaches to metaphysics can serve as our foundation? In Chapter 1, we discussed at length the difficulties that arise from a purely rationalist or from a purely empirical philosophy. To avoid these particular mistakes of the past, we must look to the unified themes.

§ 7.2 Kant's Classification of Metaphysics

Kant's Copernican hypothesis – the hypothesis that the object must conform to our knowledge rather than knowledge conforming to the object – requires a different view of metaphysics from that of Aristotle. In Chapter 1, we saw that Kant's theory holds that mind is an active agency in the process of perception, and the phenomenon of mind manifests itself in the construction of a

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[3] *ibid*.

"world model" from the data of experience. This transcendental perspective requires a different definition of metaphysics from that of Aristotle's substance-centered view. With Kant, metaphysics in general is "the system of pure rational knowledge through concepts" [KANT19: 418 (29: 945)].

This terminology requires some discussion. First of all, metaphysics is a *system* – a unity of various concepts under one idea. The objects with which this system is concerned are the "cognitions of reason" – a phrase Kant uses to refer to *functional* knowledge of *theoretical* knowledge[4] without necessarily implying that the latter is *certain*. Reason is the ability of mind to employ all one's powers of perception, feeling, thinking, etc. by which empirical knowledge is represented, judgments are made, conclusions are drawn, and actions are decided upon.

The knowledge with which metaphysics is concerned is *pure* – a term Kant uses to denote mental powers of representation that contain nothing empirical. In other words, *pure* cognitions of reason denote those transcendental factors in the phenomenon of mind supplied and added to the empirical in perception in formulating one's "world model." These are the factors from which *generalizing* ideas spring. We have already discussed at some length the fact that empirical data of sense are particular and contingent, carrying no universal or generalizing principles. The generalizing principles must be supplied by and arise from the innate workings of mind; they are the principles and functions that *make* experience possible.

Finally, these pure cognitions of reason build empirical concepts – a term Kant uses to mean an objective conscious representation at a level of representation higher than that of the immediate representations of sense. A concept, then, is almost but not quite synonymous with what is usually meant by the term *a thought*. Kant's theory employs a veritable zoo of different terms describing various shades of meaning given to the general term *mental representation*, and we shall later devote considerable time to explaining the idea of a "concept." For our present purposes, it is enough if we understand Kant's definition of metaphysics in the following terms. Metaphysics-in-general is the theory (system) explaining the process of mental activity by which an Organized Being comes to possess empirical knowledge. The elements of pure metaphysics are those innate laws of pure intellect (*nous*) that describe the phenomenon of mind in operation and which explain the manner by which we come by all empirical knowledge. The distinction between Kantian and Aristotelian metaphysics is an important one. The object of Aristotle's metaphysics was *substance*. The object of Kant's metaphysics is "the material of pure philosophy" – that is, those mental rules and capacities that define how knowledge is constituted and how knowledge is *made* possible. **Kant's metaphysics is epistemology-centered**.

Within this broad definition of metaphysics-in-general Kant identifies particular subdivisions

[4] In Latin, *cognitio* is "knowledge of or acquaintance with" a person or thing.

of metaphysics. His three main divisions are called *transcendental metaphysics*, *metaphysics proper*, and *applied metaphysics*. We next consider the first two of these.

Transcendental Metaphysics

The term "transcendental" denotes the conditions necessary for the possibility of experience and for describing what we experience. Transcendental metaphysics is therefore the core of metaphysics and "rests on the dissection of reason according to all the elementary notions contained in it" [KANT19: 427 (29: 956)]. This core is the part of Kant's system that concerns first principles of the phenomenon of mind. Because mind is to be regarded as a supersensible object, the principles of transcendental metaphysics are entirely concerned with the necessary conditions that must exist to establish the possibility of our four common themes of mind – thinking, knowledge, purpose, and self-consciousness. Because transcendental metaphysics is concerned with that which makes experience possible, the principles and ideas of transcendental metaphysics are entirely pure (i.e., do not draw on the data of empirical sense) and *a priori* (prior to experience). It is no less than a theory of *pure epistemology* – the study of the *Existenz* of knowledge as it must be viewed under Kant's Copernican hypothesis.

Transcendental metaphysics is a *rationalist* view in the sense that it concerns pure reason, but it also contains an *empirical* premise inasmuch as it sets down general principles necessary for the possibility of the phenomenon of mind to appear to us in the way that it does. But its object – mind as *nous* – is not an object of the external environment; it is part of "who we are" and although *nous* is the idea of a supersensible object, each of us knows no other supersensible object in a way more *immediate*, more *given*, and more *certain*. The empirical premise of transcendental metaphysics is only this: the actual existence (*Dasein*) of the phenomenon of mind. This empirical premise – alone among all empirical premises – occupies a unique position in human understanding. Transcendental metaphysics is thus also *empirical realism*.

Within transcendental metaphysics Kant distinguishes two divisions. First, it contains a doctrine of *elements* – the *a priori* principles of pure reason in general. These elements are the constituents of Kant's transcendental epistemology. Second, it contains a doctrine of *method* – the determination of the formal conditions of a complete system of pure reason. The doctrine of method is the idea of a system of regulative principles for the systematic construction of metaphysics proper. Given the epistemological elements and their extents and limitations set forth in the doctrine of elements, the doctrine of method prescribes how these elements must be employed in order to construct a rational theory of metaphysics. In other words, the doctrine of method does not concern itself with the phenomenon of mind; it concerns itself with: 1) a *discipline* as a system for examining and testing how we ought to employ reason in rational theorizing, 2) a *canon* of regulative principles for proper rational theorizing, and 3) an *architectonic*, or art, for constructing rational systems.

Metaphysics Proper

Metaphysics proper is metaphysics applied to general objects – the objects of one's constructed world model. Put another way, this is the division of metaphysics concerned with the *outcomes* produced by the phenomenon of mind (whereas transcendental metaphysics is concerned with the first principles of the phenomenon of mind). Metaphysics proper is, therefore, more closely aligned with traditional metaphysics than is the core system of Kant's transcendental metaphysics. Kant subdivides metaphysics proper into two major subdivisions: the metaphysics of sensible objects, and the metaphysics of rational objects of pure reason. We can think of these subdivisions as the metaphysics of Critical empiricism and the metaphysics of Critical rationalism. The Critical theory unites rationalism and empiricism in one system.

The metaphysics of sensible objects can be divided between a doctrine of objects of inner sense (Rational Psychology) and a doctrine of objects of outer sense (Rational Physics). These two doctrines, respectively, treat the *subjective* factors of *nous* – the manner in which mind is affected by sensible objects – and the *objective* representation of sensible objects – i.e., the conceptualization of the concepts in one's "world model" built up by the thinking Subject [KANT19: 427 (29: 956)]. In Rational Physics the matter of our objects of knowledge come to us through the *receptivity of the senses*; Rational Physics contains the formal principles by which objects *in general* are represented. Rational Physics does not deal immediately with the affective aspects of mind; it deals with how we know about objects rationally and logically given the epistemological limitations of our knowledge in the phenomenon of mind. Similarly, Rational Psychology deals with the *a priori* principles governing the objects of inner sense, i.e. with the mental *state* of a mind-model. While Rational Physics contains the regulative principles for conceptualizing the idea of an external object in accordance with the data of outer sense (*somatology*), Rational Psychology contains the regulative principles for the relationship of the representation of an object to the "thinking nature" of the Organized Being.

The metaphysics of objects of pure reason, on the other hand, is concerned with that character manifested in the phenomenon of mind we call *speculation*. Our ideas of supersensible objects clearly do not spring from sensation. That is, after all, why we call them supersensible objects in the first place. Therefore, these ideas can only be the product of the *creative mind*. Now, what is the nature or character of an idea of a supersensible object? In other words, what do ideas of the supersensible *do*? If we examine this question abstractly, Kant tells us, we find that there are two main *results* such ideas achieve.

In the first place, we use supersensible objects to *unify our empirical concepts under general principles*. While Rational Physics is that subdivision of metaphysics concerned with the formulation of the mental representation of *specific* objects of outer sense, generalizing ideas tie these objects together into a picture of a "world" (i.e., a unified *whole* – Nature). The subdivision of metaphysics proper concerned with *the process by which this unification is accomplished* is called Rational Cosmology. Rational Cosmology, in other words, contains those *a priori* principles for conceptualizing the idea of an objective world as the idea of the embodiment of all objects in accordance with the idea of a necessary *whole*. It is one thing for mind to bring forth the concept of a *particular* object (Rational Physics). It is quite another thing for it to come forward with the *general* idea that all such objects are necessarily united in a single Nature. Rational Cosmology deals with the process by which such an idea is to be brought forth.

In the second place, the phenomenon of mind also manifests itself by ideas concerning that which we call *purposes*, *intentions*, *values*, and *behaviors*. These striking aspects of human character are made manifest in our ability to decide upon our own courses of actions, to interpret our own behavior in terms of "right" and "wrong," and to declare certain things to be "good" or "evil." It is obvious that no empirical datum of sense brings in with it such notions as "good" or "evil." We *decide for ourselves* that some thing *X* is "bad" and some other thing *Y* is "right." How do such non-physical ideas come into being? That question is a concern of this second subdivision of the metaphysics of objects of pure reason. The ideas of supersensible objects of this sort might be termed moral ideas, as in the sense of a system of personal ethics, insofar as one can regard personal morality as one's logic of actions.

But this topic goes even further than what the words "morals" and "ethics" suggest. When one "sets a purpose" for some particular action, and then further decides on the means by which the end purpose is to be achieved, one develops *for oneself* an interlocking *system* of ideas, judgments, and decisions within which these notions of right, wrong, good, bad, and so on *have a context*. Put another way, creativity of mind constructs what we usually call a *system of values*. But, just as the metaphysic proper of Rational Cosmology must provide the rational principles that bring unity and order to our idea of Nature, metaphysics when applied to this idea of a system of values must provide the principles that make the very ability to produce *any* personal system of values at all *possible*.

Furthermore, mind does not stop even with this. From the development of individual personal values, ethics, purposes, and so on, one can bring oneself to an idea of a universe in which one believes to exist a *necessary natural order* – a notion that all which happens in the universe happens *for a reason*. The name Kant gave to this subdivision of metaphysics proper was Rational Theology – a term that does not denote God or theological doctrine in any religious sense but, rather, denotes the principles of pure reason through which the phenomenon of the mind's capacity for self-legislation, through the *mental construction* of a system of values, is possible.[5] Rational Theology contains the regulative principles for conceptualizing the idea of a necessary natural order in Nature in accordance with the idea of the possibility of a supreme condition from which *coherence in a natural order* necessarily follows.

An objection sometimes raised regarding this subdivision of Kant's metaphysics is that "morals" and "values" are *learned* and are in no way innate to the mind. So far as by "morals" one means *specific* moral ideas such as "do not steal" or "do not kill" and so on, this appears to be true. However, the metaphysic of Rational Theology does not deal with specific 'moral' ideas or concrete behaviors since all ideas of this sort are rooted in sensible concrete experience. Rational Theology concerns pure and *a priori* principles. Put another way, Rational Theology is concerned only with how it is even possible for the intellect to have *any* idea of right, wrong, good, evil, or to even pose the question, "What is this all about?" That we *are capable* of developing such ideas, and that this development appears to follow a particular process of mental development, is not a mere conjecture, but is, indeed, a verifiable *fact* (c.f. [PIAG7], [PIAG16]).

§ 7.3 The Critical Philosophy and Science

Transcendental metaphysics and metaphysics proper, taken together, make up Kant's Critical Philosophy. Now we need to ask the question: *what has all this to do with science proper*? To answer this question, we must explore the manner in which this Critical philosophy might supply the groundwork for *empirical* knowledge of the specific object of a science.

At the foundation of this philosophy is the critique of knowledge in terms of its origin, extent, certainty, and limitations. This constitutes the epistemology provided under transcendental metaphysics. The principles contained in this doctrine are the grounds for all doctrines of *applied* metaphysics, which are constrained *a priori* to be in accordance with the system of Critical metaphysics. These first principles of the entire system of philosophy *and* science are the subject of Kant's three great Critiques: *Critique of Pure Reason*, *Critique of Practical Reason*, and *Critique of Judgment*.

By following the doctrine of the Critical metaphysics, one can develop a specific *applied* metaphysic for a particular doctrine of science. But such a specific metaphysic must conform to the general principles, necessary for *any* such system, outlined in Kant's metaphysics proper. The

[5] Kant's four major essays on Rational Theology, and its relationship to religion, were published in 1793 in his *Religion Within the Limits of Reason Alone*. The book created a firestorm among Prussian religious authorities and was severely censured. On October 1, 1794, King Friedrich Wilhelm II issued the following order to Kant: "Our Most High Person has for a long time observed with great displeasure how you misuse your philosophy for distortion, debasement and degradation of many cardinal and fundamental doctrines of the Holy Scriptures and Christianity . . . in the future, toward the avoidance of our highest disfavor, you will give no such cause for offense . . . failing which, you may surely expect consequences inevitably unpleasant to yourself" [KANT20a: 485 (11: 525-526)].

general principles of Rational Physics, Psychology, Cosmology, and Theology, collectively, are regulative principles – guidelines for the *formulation* of *any* rational theory. Let us recall that we have taken Kant's definition of science as any doctrine that constitutes a *system*, and that the elements of a system are tied together under a unifying general idea. When this unity is the product of a principle that unites these elements *necessarily*, we have a proper science.

But *generalizing* ideas are never given *in* empirical facts. All such facts are merely contingent, and could be overturned by some future experience. Therefore, given the object of the science, its generalizing principles must have a *rational* basis. A system of such rational principles, limited by and applied to the object of the science, constitutes an *applied metaphysic*. Philosophical issues for any science arise from the confidence we have in the soundness of its applied metaphysic. If, as in positivism, this metaphysic is merely an aggregate of ideas and prejudices – a hodgepodge of assumptions and presumptions without a single unifying ground – then we may call this metaphysic mere *speculation* at best and a pseudo-metaphysic at worst. To have a proper applied metaphysic of a science, this metaphysic must be deduced from the principles of the Critical Philosophy. Even a proper science stands on merely speculative grounds *if it lacks a Critical applied metaphysic*.

In saying this, it is important we recognize the role that an applied metaphysic deduced from a Critical foundation would play. Every science has its *fundamental objects* – the things it regards everything else within the doctrine of the science to be based upon. In the science of mechanics this fundamental thing is the corpuscle. In economics we have the rational man. In Toynbee's view of history we have societies. A proper applied metaphysic makes explicit the standing of the fundamental objects with respect to what we can know objectively to be true on grounds of its accordance with the laws of human understanding. Furthermore, a proper applied metaphysic establishes the ontological basis of the laws by which the science posits relations among its objects from empirically-discovered facts. The more firmly these foundations are established, the more solid is the basis for the paradigm of the science. Applied metaphysics is ontology.

Kant developed an applied metaphysic for each of several different sciences, although his exposition in each of these cases was, surprisingly, rather less than rigorous (as noted by many subsequent commentators and critics). Kant's applied systems were published under the titles *Metaphysical Foundations of Natural Science*, *Metaphysics of Morals*, and *Religion Within the Limits of Reason Alone*. To a certain degree, his *Logic* also contains explicit elements of what might justifiably be called an applied metaphysic of logic, although Kant himself stopped short of calling this a metaphysic for the science of logic.

Logically, Kant's extensive works suggest a *sequence* in the development of a proper science. By this I do not mean to imply that Kant held that sciences *do* develop in the way I am about to suggest; rather, it is my opinion that Kant held science *ought to develop* itself in this fashion. The sequence suggested by Kant's system is this: 1) The objective of the science should first be described and its general object of study posited; 2) It should develop a rational applied metaphysic of its object, using Critical metaphysics to deduce what can be said objectively about its object *a priori*, and formulating the constituent principles of this applied metaphysic in accordance with the general principles of metaphysics proper; and, 3) in deducing the empirical principles of the science, these principles should themselves be analyzed and shown to be in accordance with both transcendental metaphysics and metaphysics proper.

A doctrine of how this is to be accomplished presents us with certain difficulties. A rational applied metaphysic may take into itself nothing that is merely empirical (aside from the basic exposition of the object of the science, which serves this metaphysic only in establishing the context of the science). However, this leaves open a crucial question: *How is it possible for a* rational *metaphysic to make a transition to the* empirical *science itself*? As Kant himself commented, the principle of such a transition cannot be contained within the applied metaphysic itself because such a principle must precede the metaphysic. Still less can such a principle be discovered in the empirical realm of the science itself.

The answer to this question is not contained within Kant's works of the Critical Philosophy; that is, it is not found within his doctrines of either transcendental metaphysics or metaphysics proper. This strongly suggests that there is a missing piece in Kant's theory – a missing system of method that would contain the doctrine for making the transition from a metaphysic to its science. There is some reason to believe that Kant was working on such a doctrine until the infirmities of old age set upon him, and that death overtook him before he was able to complete this work. In a letter dated January 20, 1792, Kant wrote to J.S. Beck

> You have presented me with your thorough investigation of what is just the hardest thing in the *Critique*, viz., the analysis of an experience in general and the principles of its possibility. I have already planned a System of Metaphysics to overcome these difficulties. . .
>
> You have hit upon it quite well when you say, "The quintessence of representations is itself the Object, and the act of the mind through which this quintessence of representations is presented is what is meant by 'referring them to the Object'." But one may ask: How can a *complex* essence of representation be presented? Not through consciousness that it is given to us; for a quintessence requires *composition* (*synthesis*) of the manifold. It must thus (as quintessence) *be created* and this through an inner act which is valid for a *given* manifold in general and which precedes *a priori* the manner in which this is given [KANT20: 182-184 (11: 313-314)].

Kant's final work, the unfinished *Opus Postumum* [KANT10], appears to contain Kant's final efforts to bring this System of Metaphysics into being [KANT10: xxix-xxxviii]. In this work, we find a number of passages which seem to support this hypothesis:

> Since both these parts of natural science in general [physics and metaphysics] are nevertheless allied to each other so closely that the former cannot but have regard for the latter, and the latter for the former, the idea of a transition is an idea given *a priori* in the doctrine of the elements of natural

> science in general, and requires stating the idea of a special discipline of its own [KANT10: 36 (21: 524-525)].
>
> The transition from one science [metaphysics] to the other [physics] must have certain intermediate concepts, which are given in the one and applied in the other, and which thus belong to both territories alike; otherwise, this advance is not a law-like transition but a leap in which one neither knows where one is going, nor, in looking back, understands from whence he came [KANT10: 37 (21: 525-526)].
>
> If [the transition from the metaphysical foundations of natural science to physics] took place by means of experience, it would be physics itself; if it takes place, however, by means of principles of the possibility of experience [transcendental metaphysics] it precedes physics *a priori* and contains principles *a priori* it delivers [to physics]. This is, however, a particular part of natural science which contains its own principles and founds its own system - although a merely formal one. . . . This transition is not merely propaedeutic; for such a concept is precarious and concerns only the subjective aspect of knowledge. There is a not merely regulative but also constitutive formal principle of natural science, existing *a priori* for a system [KANT10: 56 (22: 239-240)].

Elsewhere, Kant described this final project in various letters. In 1789 he wrote to Marcus Herz explaining that he could not comply with Herz's request that he critique a book Herz had sent him because

> . . . I who in my 66th year am still burdened with the extensive work of completing my plan (partly in producing the last part of the critique, namely, that of the power of judgment, which should appear soon, and partly in completion of a *system* of metaphysics, of nature as well as of morals, in conformity with those critical demands). [KANT20: 151: (11: 48-49)].

Nine years later, in a letter to Christian Garve, we find Kant in faltering health but still at work:

> I am as it were paralyzed for mental work even though physically I am tolerably well. To see that the out and out close of my bill still lies ahead - in things which affect the whole of philosophy (both as regards its purpose and its means) - and is yet after all unfinished, though I am aware of the feasibility of this task: It is a pain like Tantalus though for all that not hopeless. The task on which I am now working concerns the "Transition from the metaphysical foundations of natural science to physics." It must be completed, or else a gap will remain in the critical philosophy." [KANT20: 251 (12: 257)].

In that same year, 1798, he writes to J.G.C.C. Kiesewetter

> The state of my health is that of an old man, free from illness, but nevertheless an invalid . . . who nevertheless feels a little bit of strength still remains within him to complete the work at hand wherewith the critical business will be completed and a gap now open will be filled; namely, the *transition* from the metaphysical foundations of natural science to physics as an actual part of natural philosophy that must not be left out of the system [KANT20: 252 (12: 258)].

Kant's health continued to deteriorate, and death finally took him in 1804 with this gap left unfilled. It is part of the task of this treatise to provide a direction for finishing off this remaining "unpaid bill" of the Critical Philosophy. That will be taken up in the final Chapter of this treatise, but before we can come to address this issue there is much for us to examine. Only after this examination will we be ready to deal with the issue of the Critical transition.

§ 8. The Development of Theory

The speculation I gave in the previous section, regarding what I think may have been Kant's view of the sequence in which a science ought to develop, has not been followed by any science that has developed since Kant's lifetime. Yet sciences have *been* developed since that time. In some cases, such as information theory or microbiology, these new sciences derived from older well-established sciences. In other cases, such as psychology, sociology, and many of the other social sciences, we see new disciplines arising without the benefit of such inheritance. In most such cases these newly-born sciences are unable to claim the same scope of success as the "natural" sciences[1] such as physics. Is this due to the relative youth of these sciences, to the difficulty of their objects, or to some other factor such as the influence of positivism? Since we have no examples of any science that has developed along Kantian lines, and because such a development would in any case require the completion of Kant's system and the method of transition from applied metaphysics to its special science, how *have* the sciences developed?

For students and for the practitioners of what Kuhn terms normal science, such a question need not be answered in order to learn or practice a science. It has been given to very few people to have been present at, and able to contribute to, either the genesis of a new science or the re-making of an established science at the time of a scientific revolution. Yet our understanding of what science *is* can benefit from having the historical perspective of how these intellectual accomplishments came to be made. Such a perspective might go far towards advancing our ability to judge what makes a science, might perhaps even help in some ways to improve those sciences which already exist, and might help pre-paradigmatic studies become full-fledged sciences in their own right. A full treatment of this perspective would easily make up a complete treatise in its own right, and such a full treatment is not part of the scope of this present work. Even so, let us at least look at how some of the pioneers of science described the perspective from which they worked.

One such pioneer was Copernicus (1473-1543). Copernicus falls into the category of what we might call a "revolutionist" scientist since his work overthrew a previously existing paradigm, the Ptolemaic system of astronomy. Copernicus seems to have been a reluctant revolutionist who,

[1] The reader might well wonder why I seem to insist of putting quote marks around "natural" when I refer to the "natural" sciences such as physics, chemistry, or biology. I do not do so out of any pejorative motive or to imply any judgment that these disciplines suffer from an excess of conceit. Rather, I simply have great difficulty with the connotation that if there are "natural" sciences, then perhaps there are "unnatural" sciences as well. I see economics, psychology, and so forth as being quite "natural" fields for scientific study. Why, then, should only certain slices of the study of what we find in Nature as a whole be deemed "natural" sciences?

by his own admission in the “Preface and Dedication to Pope Paul III” of his *On the Revolutions of the Heavenly Spheres*, kept his work hidden from view “for almost four times nine years.” His reluctance to publish seems not to have been due to self-doubt about the correctness of his theory, but rather to “the scorn I had to fear on account of the newness and absurdity of my opinion” from “those who know that the opinion that the earth rests immovable in the middle of the heavens as if their center had been confirmed by the judgment of many ages”. How did such an apparently shy and reluctant man came to overthrow the accumulated wisdom of fourteen centuries?

> But perhaps Your Holiness will not be so much surprised at my giving the results of my nocturnal study to the light . . . as you will be eager to hear from me what came into my mind that in opposition to the general opinion of mathematicians and almost in opposition to common sense I should dare to imagine some movement of the Earth. And so I am unwilling to hide from Your Holiness that nothing except my knowledge that mathematicians have not agreed with one another in their researches moved me to think out a different scheme of drawing up the movements of the spheres of the world. For in the first place mathematicians are so uncertain about the movements of the sun and moon that they can neither demonstrate nor observe the unchanging magnitude of the revolving year. Then in setting up the solar and lunar movements and those of the other five wandering stars, they do not employ the same principles, assumptions, or demonstrations for the revolutions and apparent movements. . . Moreover, they have not been able to discover or to infer the chief point of all, i.e., the form of the world and the certain commensurability of its parts. But they are exactly in the same fix as someone taking from different places hands, feet, head, and the other limbs - shaped very beautifully but not with reference to one body and without correspondence to one another - so that such parts made up a monster rather than a man. And so, in the process of demonstration which they call "method," they are found either to have omitted something necessary or to have admitted something foreign which by no means pertains to the matter; and they would by no means have been in this fix, if they had followed sure principles. For if the hypotheses they assumed were not false, everything which followed from the hypotheses would have been verified without fail . . .
>
> Accordingly, when I had meditated upon this lack of certitude in the traditional mathematics concerning the composition of the movements of the spheres of the world, I began to be annoyed that the philosophers, who in other respects had made a very careful scrutiny of the least details of the world, had discovered no scheme for the movements of the machinery of the world, which had been built for us by the Best and Most Orderly Workman of all. Wherefore I took the trouble to reread all the books by philosophers which I could lay hold of, to see if any of them even supposed that the movements of the spheres of the world were different from those laid down by those who taught mathematics in the schools. And as a matter of fact, I found first in Cicero that Nicetas thought that the Earth moved. And afterwards I found in Plutarch that there were some others of the same opinion . . .
>
> Therefore I also, having found occasion, began to meditate upon the mobility of the Earth. . . I thought that I too would be readily permitted to test whether or not, by the laying down that the Earth had some movement, demonstrations less shaky than those of my predecessors could be found for the revolutions of the heavenly spheres.
>
> And so, having laid down the movements which I attribute to the Earth farther on in the work, I finally discovered by the help of long and numerous observations that if the movements of the other wandering stars are correlated with the circular movements of the Earth, and if the movements are computed in accordance with the revolution of each planet, not only do all their phenomena follow from that but also this correlation binds together so closely the order and magnitudes of all the

> planets and their spheres or orbital circles and the heavens themselves that nothing can be shifted around in any part of them without disrupting the remaining parts and the universe as a whole.

It is worthwhile to compare Copernicus' words with the previous section in the discussion of Kant's metaphysics proper. In Copernicus' description we see, first of all, dissatisfaction with the lack of a *system* – a unity of principles – in the Ptolemaic science of astronomy, and with the failure of existing theories to explain the *whole* of what was empirically known at the time. There is "annoyance" with the philosophers (i.e., the scientific community; remember that philosophy and science were not separate in the sixteenth century) for not discovering a "scheme." Finally, we see that Copernicus began with a hypothesis, compared its consequences with "long and numerous observations," and found "accordance" that "binds together so closely" the observed and the expected that "nothing can be shifted around in any part . . . without disrupting the remaining parts and the universe as a whole." We see Copernicus objecting to theory omitting "something necessary" or adding in "something foreign which by no means pertains to the matter." Nor is Copernicus reluctant to consider the ideas of men as distant from himself in time as we today are from the Roman emperor Hadrian.[2]

Copernicus was also possessed by a motive that speaks to the point we were making earlier regarding the propensity of the phenomenon of mind to construct systems. At the beginning of Book I of *On the Revolutions of the Heavenly Spheres*, he writes "Among the many and varied literary and artistic studies upon which the natural talents of man are nourished, I think that those above all should be embraced and pursued with the most loving care which have to do with things that are very beautiful and very worthy of knowledge. . . Many philosophers have called the world a visible god on account of its extraordinary excellence. So if the worth of the arts were measured by the matter with which they deal, this art – which some call astronomy, others astrology, and many of the ancients the consummation of mathematics – would be by far the most outstanding. This art which is as it were the head of all the liberal arts and the one most worthy of a free man leans upon nearly all the other branches of mathematics. Arithmetic, geometry, optics, geodesy, mechanics, and whatever others, all offer themselves in its service." [3] It would seem that there is, for Copernicus, a value system admixed with the development of, if not directly a part of, science and that this value system need not be a strictly commercial system of values. Inasmuch as such a system can exert an influence on the way a scientist thinks, it is to be wondered at why modern scientific textbooks rarely mention the artistic value of scientific theorizing.

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[2] Copernicus cites Philolaus the Pythagorean (late 5th century B.C.), Ecphantus of Syracuse (*c*. 390 B.C.), and Heraclides of Pontus (*c*. 360 B.C.).

[3] Prior to the divorce of science and philosophy in the nineteenth century, what we now call *the* sciences were more often called the *arts* - a title which seems quite appropriate to me, and which it would do science no harm to remember.

Let us turn from Copernicus to another father of science. Perhaps no pre-twentieth century scientist's name is more widely known than Sir Isaac Newton (1642-1727). Like Copernicus, Newton let his work on mechanics, gravitation, and the calculus lie unpublished for two decades before, at the insistence of Edmund Halley and with the work of other scientists closing in on the independent discovery of what Newton had already accomplished, he consented to publish *Mathematical Principles of Natural Philosophy* in 1687. Unlike Copernicus, Newton's work is less the product of one daring idea than the great *synthesis* of many ideas and observations. Let us look at how Newton described his science.

In the preface to the first edition, he writes

> Since the ancients (as we are told by Pappus) esteemed the science of mechanics of greatest importance in the investigation of natural things, and the moderns, rejecting substantial forms and occult qualities, have endeavored to subject the phenomena of nature to the laws of mathematics, I have in this treatise cultivated mathematics as far as it relates to philosophy. The ancients considered mechanics in a twofold respect; as rational, which proceeds accurately by demonstration, and practical. To practical mechanics all the manual arts belong, from which mechanics took its name. . . (The) description of right lines and circles, upon which geometry is founded, belongs to mechanics. Geometry does not teach us to draw these lines, but requires them to be drawn, for it requires that the learner should first be taught to describe them accurately before he enters upon geometry, then it shows how by these operations problems may be solved. To describe right lines and circles are problems, but not geometrical problems. The solution of these problems is required from mechanics, and by geometry the use of them, when so solved, is shown; and it is the glory of geometry that from those few principles, brought from without, it is able to produce so many things. Therefore geometry is founded in mechanical practice, and is nothing but that part of universal mechanics which accurately proposes and demonstrates the art of measuring. But since the manual arts are chiefly employed in the moving of bodies, it happens that geometry is commonly referred to their magnitude, and mechanics to their motion. In this sense rational mechanics will be the science of motions resulting from any forces whatsoever, and of the forces required to produce any motions, accurately proposed and demonstrated. . . But I consider philosophy rather than arts and write not concerning manual but natural powers, and consider chiefly those things which relate to gravity, levity, elastic force, the resistance of fluids, and the like forces, whether attractive or impulsive; and therefore I offer this work as the mathematical principles of philosophy, for the whole burden of philosophy seems to consist in this - from the phenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the other phenomena.

The three books contained within Newton's *Mathematical Principles* are entitled, respectively, *The Motion of Bodies*, *The Motion of Bodies (In Resisting Mediums)*, and *The System of the World (In Mathematical Treatment)*. In introducing the third book, Newton writes

> In the preceding books I have laid down the principles of philosophy; principles not philosophical but mathematical: such, namely, as we may build our reasonings upon in philosophical inquiries. . . It remains that, from the same principles, I now demonstrate the frame of the System of the World.

This introduction is followed by four "rules of reasoning in philosophy" that consist of four non-mathematical "metaphysical" principles. In rule IV, we find, interestingly, a reference by Newton to "experimental philosophy" in which "we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions." Later on, he elaborates a little bit about his "experimental philosophy":

> Hitherto we have explained the phenomena of the heavens and of our sea by the power of gravity, but have not yet assigned the cause of this power. . . But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy.

It is with Newton that we see mathematics given apriority "as that we may build our reasonings upon in philosophical inquiries." If we substitute the word "science" for "philosophy" in Newton's book[4], we see in this what is almost the trademark of science – its use of mathematics as the formal *language* of science and in which extremely *precise* statements of scientific empirical principles may be made. This does not mean, however, that science is a *purely formal* doctrine in which everything that must be said can be said only via mathematics. Newton himself laid down a number of fundamental principles that were *not* stated in mathematical terms, such as his statement above regarding hypotheses illustrates. Today there is a presupposition shared by some people that everything contained within a science must be framed in formal mathematics. But this is not so if by this one means the positivism of modern-day mathematical formalism:

> Pure rational knowledge from mere *ideas* is called pure philosophy or metaphysics, while that which only bases its knowledge on the *construction* of concepts, by means of presenting the object in an *a priori* intuition, is termed mathematics. . . But I maintain that in every special natural doctrine only so much science *proper* is to be met with as there is mathematics in it. For, in accordance with the foregoing, science proper, especially of nature, requires a pure portion, lying at the foundation of the empirical, and based upon an *a priori* knowledge of natural things. Now to know something *a priori* means to recognize it from its mere possibility. But the possibility of determinate natural things cannot be known from mere conceptions; for from these the possibility of the thought (that it does not contradict itself) can indeed be known, but not that of the Object as natural thing which can be given (as existent) outside the thought. Hence, to the possibility of a determinate thing, and therefore to know it *a priori,* is further requisite that the *intuition* corresponding *a priori* to the concept should be given; that is, that the concept should be constructed. But rational knowledge through construction is mathematical. A pure philosophy of nature in general, that is, one that only investigates what constitutes the idea of a nature in general, may thus be possible without mathematics but a pure doctrine of nature respecting *determinate* natural things . . . is only possible by means of mathematics . . . a doctrine of nature can only contain so much science proper as there is mathematics in it [KANT15: 139-141 (4: 469-470)].

[4] Recall that in Newton's day, what we call science today was then called "natural philosophy."

A "doctrine of nature" contains more than just "science proper" and it is only with "science proper" that we meet applied mathematics. Positivist mathematics cannot tell science what sort of mathematics the science requires, nor can science determine this for itself. It is the underlying system of principles (i.e., the metaphysic of the science) that determines this, and this determination comes from the acroamatic[5] (i.e., non-mathematical) principles of the metaphysic [KANT1: 479-483 (B: 758-765)]. No one regards the modern axioms of mathematics, such as the Zermelo-Fraenkel-Skolem axioms of set theory, as fundamental postulates of nature. Yet if it were really possible to *base* all of physics on mathematics, this is precisely what these axioms would have to be. In real practice the questions of science drive the constructions of applied mathematics. Nor was Newton trying to say all the principles of philosophy must be mathematical or else mechanics would be part of geometry, and not "geometry . . . that part of universal mechanics," as he stated in his preface to the *Mathematical Principles*.

In great contrast to Leibniz, Newton took care to avoid rational extensions of empirical principles beyond what could possibly be given, directly or indirectly, in experience. His remark concerning hypotheses illustrates this quite clearly. For Newton what was convincing evidence for a theory was not Leibniz' dictum of the sufficient reason but, rather, the scope of different phenomena that could be explained by a single theory. Here we gain a glimpse or a suggestion that there might exist for the human mind more than one type of "sufficient reason." There is nothing presented in empirical nature demanding that a single law unite a wide diversity of what appear to be entirely different phenomena. But the degree to which a scientific doctrine is held to be true depends greatly upon precisely this sort of fecundity in a theory. The greater the scope of a theory proves to be, the more perfect the theory is regarded to be. Leibniz wanted a direct and unbroken path leading straight to a primal object whose existence could slam the door against any possible miraculous explanation being required – an "objectively sufficient" reason. Newton was content to let the fecundity of the theory stand as a measure of its correctness – a "subjectively sufficient" reason grounded in a belief in an ultimate order to be found in things.

Such a subjectively sufficient reason is not a proof of correctness but a faith in the basic correctness of the theory, and often raises a new question for every question the theory puts to rest. In Newton's case the new difficulty raised was "action at a distance" and subsequent generations of physicists labored mightily to find a way to get rid of action at a distance before eventually largely (but not completely) conceding the issue in the twentieth century.[6]

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5 An acroamatic principle is a fundamental and *a priori* principle of pure reason - a "first principle" (primitive element) of transcendental metaphysics. As such it is logically prior to the axioms of mathematics because in a Critical mathematics these axioms are constructed *from* acroamata.

6 Action at a distance is still with us today in quantum mechanics. See F. Rohrlich, "Facing quantum mechanical reality," *Science*, vol. 221, no. 4617, 23 Sept., 1983, pp. 1251-1255.

Let us now turn our attention to another famous revolutionist, the great naturalist Charles Darwin (1809-1882). Among the general population Darwin is perhaps as well-known a name as Newton and the title of his classic work, *The Origin of Species by Means of Natural Selection* (1859), is much more widely known by the general public than is Newton's *Mathematical Principles*. What is perhaps less widely known is that Darwin did not originate the idea of evolution, nor was he the first to postulate the mechanism of natural selection. As far as natural selection is concerned, Darwin lost the publication race to Aristotle by the not-so-narrow margin of some twenty-two centuries.[1] Still, in Darwin's day the dominant view was that every species had been individually created in the beginning and that species were immutable. That view, however, was being increasingly questioned by early nineteenth century naturalists, as outlined in the historical sketch that prefaced the second edition (1860) of *The Origin of Species*.

It is probably safe to say that if Darwin had never written *The Origin of Species* someone else would have soon published a similar theory. Be that as it may, Darwin *did* publish this work and, in doing so, overturned the paradigm of biology. In his case the strength of the revolution he is credited with lies in the weight of his arguments in favor of the theory and his forthright discussion of the difficulties and issues that remained outstanding at the time.

In searching for the thoughts that shaped Darwin's theory, Darwin himself is of little help to us. *The Origin of Species* is, as he put it, "one long argument" reciting "facts and inferences." However, we can find a few instances in this work that provide some amount of insight into Darwin's development of the theory of natural selection. For Darwin the starting point came in 1831 when he boarded the H.M.S. *Beagle* – as an unknown and little-regarded naturalist devoted primarily to collecting beetles – for a five year voyage of geological research. Darwin returned to Great Britain in 1836 as one of the best qualified naturalists of his day, filled with such a new sense of purpose that his father, who had worried considerably about his son's future, remarked, "Why, the shape of his head is quite altered." In *The Origin of Species* Darwin tells us

> When on board the H.M.S. *Beagle* as naturalist, I was much struck with certain facts in the distribution of the organic beings inhabiting South America, and in the geological relations of the present to the past inhabitants of that continent. These facts, as will be seen in the latter chapters of this volume, seemed to throw some light on the origin of species - that mystery of mysteries, as it has been called by one of our greatest philosophers. On my return home, it occurred to me, in 1837, that something might perhaps be made out of this question by patiently accumulating and reflecting on all sorts of facts which could possibly have any bearing on it. After five years' work I allowed myself to speculate on the subject, and drew up some short notes; these I enlarged in 1844 into a sketch of the conclusions, which then seemed to me probable: from that period to the present day I

[1] "Wheresoever, therefore, all things together (that is all the parts of one whole) happened like as if they were made for the sake of something, these were preserved, having been appropriately constituted by an internal spontaneity; and whatsoever things were not thus constituted, perished, and still perish." (Aristotle, *Physics*, Bk. II, ch. 8).

have steadily pursued the same object. I hope that I may be excused for entering on these personal details, as I give them to show that I have not been hasty in coming to a decision.

In considering the Origin of Species, it is quite conceivable that a naturalist, reflecting on the mutual affinities of organic beings, on their embryological relations, their geographic distribution, geological succession, and other such facts, might come to the conclusion that species had not been independently created, but had descended, like varieties, from other species. Nevertheless, such a conclusion, even if well founded, would be unsatisfactory, until it could be shown how the innumerable species inhabiting this world have been modified, so as to acquire that perfection of structure and coadaptation which justly excites our admiration. Naturalists continually refer to external conditions, such as climate, food, &c., as the only possible cause of variations. In one limited sense, as we shall hereafter see, this may be true; but it is preposterous to attribute to mere external conditions, the structure, for instance, of the woodpecker, with its feet, tail, beak, and tongue, so admirably adapted to catch insects under the bark of trees. In the case of the mistletoe, which draws its nourishment from certain trees, which has seeds that must be transported by certain birds, and which has flowers with separate sexes absolutely requiring the agency of certain insects to bring pollen from one to the other, it is equally preposterous to account for the structure of this parasite, with its relation to several distinct organic beings, by the effects of external conditions, or of habit, or of the volition of the plant itself.

It is, therefore, of the highest importance to gain a clear insight into the means of modification and coadaptation. At the commencement of my observations it seemed to me probable that a careful study of domesticated animals and of cultivated plants would offer the best chance of making out this obscure problem. Nor have I been disappointed; in this and in all other perplexing cases I have invariably found that our knowledge, imperfect though it may be, of variation under domestication, afforded the best and safest clue. I may venture to express my conviction of the high value of such studies, although they have been very commonly neglected by naturalists.

No one ought to feel surprise at much remaining as yet unexplained in regard to the origin of species and varieties, if he make due allowance for our profound ignorance in regard to the mutual relations of the many beings which live around us. Who can explain why one species ranges widely and is very numerous, and why another allied species has a narrow range and is rare? Yet these relations are of the highest importance, for they determine the present welfare and, as I believe, the future success and modification of every inhabitant of this world. Still less do we know of the mutual relations of the innumerable inhabitants of the world during the many past geological epochs in its history. Although much remains obscure, and will long remain obscure, I can entertain no doubt, after the most deliberate study and dispassionate judgment of which I am capable, that the view which most naturalists until recently entertained, and which I formerly entertained - namely, that each species had been independently created - is erroneous. I am fully convinced that species are not immutable; but that those belonging to what are called the same genera are lineal descendants of some other and generally extinct species, in the same manner as the acknowledged varieties of any one species are the descendants of that species. Furthermore, I am convinced that Natural Selection has been the most important, but not the exclusive, means of modification.

Following this introduction, Darwin launches into his prolonged presentation of fact and observation after fact and observation, intertwined with the questions these facts raise, and the competing postulates that both he and others raised as potential explanations for these facts and answers to these questions. He discusses the difficulties contained in the theory, many objections that can be raised against his conclusions, and sources of “imperfection” in the study of the subject. We can gain an appreciation for the scope of *Origin of Species* by examining its chapter titles: Variation under domestication; Variation under nature; Struggle for existence; Natural selection or the survival of the fittest; Laws of variations; Difficulties of the theory; Miscellaneous objections to the theory of natural selection; Instinct; Hybridism; On the imperfection of the geological record; On the geological succession of organic beings; Geographical distribution (two chapters); Mutual affinities of organic beings, morphology, embryology, rudimentary organs; and, finally, Recapitulation and conclusion. In its breadth, detail, and frank discussion of difficulties and important but unresolved issues, *The Origin of Species* is a masterpiece of intellectual honesty and scientific observation.

It is also a preeminent example of what Kant would call an historical doctrine of nature. Darwin does not present a mathematical theory, nor does *The Origin of Species* represent the final word on a complete theory of evolution by natural selection. Rather, he presents the *first* word of such a theory. Within the vast diversity exhibited by life on earth, Darwin focuses on the existence of "affinities" – small variations from one species to another – and he asks if the appearance of such affinities might not be evidence of some overriding natural unity in the development of life on earth. He finds in natural selection a possible explanation that takes in both the manifold diversity of life and the existence of close similarities between heterogeneous forms of life. *The Origin of Species* is biological cosmology in its infancy.

Let us now take a look at a lesser known revolutionist. The life sciences as we know them today are not quite two centuries old. Considering the long history of medicine and biology, which can trace their ancestry to before the time of Hippocrates and Aristotle, this may seem at first to be a surprising statement. But for most of history from the ancient Greeks until the middle of the nineteenth century, the life sciences were based on rudimentary observation and more or less accidental discovery of medical treatments. This was due primarily to a prevailing opinion that life was dependent on some "vital force" and that one could learn nothing about life processes through the study of tissues and organs. When parts are separated from the living body as a whole, the thinking went, this vital force is lost and therefore one could learn nothing about life by the study of "dead tissue".

That this prevailing viewpoint changed was due in considerable measure to the French physiologist Claude Bernard (1813-1878). It was Bernard's ground-breaking essay, *An Introduction to the Study of Experimental Medicine* (1865), that led the way to providing the life sciences with an experimental foundation.

> To conserve health and to cure disease: Medicine is still pursuing a scientific solution to this problem, which has confronted it from the first. The present state of medical practice suggests that a solution is still far to seek. During its advance through the centuries, however, medicine has always been driven into action and from numberless ventures in the realm of empiricism has gained useful information. Though furrowed and overturned by all manner of systems so evanescent that, one by one, they have disappeared, it has none the less carried on research, acquired ideas, and piled up

> precious materials which in due time will find their place and meaning in scientific medicine. Today, thanks to the great development and powerful support of the physico-chemical sciences, study of the phenomena of life, both normal and pathological, has made progress which continues with surprising rapidity.
>
> It is therefore clear to all unprejudiced minds that medicine is turning toward its permanent scientific path. By the very nature of its evolutionary advance, it is little by little abandoning the region of systems, to assume a more and more analytic form, and thus gradually to join in the method of investigation common to the experimental sciences.
>
> In order to embrace the medical problem as a whole, experimental medicine must include three basic parts: physiology, pathology, and therapeutics. Knowledge of causes of the phenomena of life in the normal state, i.e., PHYSIOLOGY, will teach us to maintain normal conditions of life and to conserve HEALTH. Knowledge of diseases and of their determining causes, i.e., PATHOLOGY, will lead us, on the one hand, to prevent the development of morbid conditions, and, on the other, to fight their results with medical agents, i.e., to cure the diseases.

Medicine today, taken as a whole, is still an improper science in the Kantian sense that its principles are for the most part empirical. This does not mean, of course, that there does not exist within it certain narrow specialties that have achieved the Kantian status of science proper. But medicine as a whole *does* lack an explicit applied metaphysic. Bernard's specific contribution in his essay was to put experimental practice on its proper footing. He discusses the distinction between mere *observation* and *experimentation* and the role each has in the science of medicine. He discusses the role and use of rational inference made on experimental facts, and the necessary role of specialized instrumentation for obtaining these facts. Most of all, he describes an overall methodology for the conduct of experimentation vs. the role that observation plays in guiding the development of experimental research.

> Observation, then, is what shows facts; experimentation is what teaches us about facts and gives experience in relation to anything. But as this teaching can come through comparison and judgment only, i.e., by the sequence of reasoning, it follows that man alone is capable of gaining experience and perfecting himself by it.

> We can learn - i.e., gain experience of our surroundings - in two ways, empirically and experimentally. First there is a sort of teaching or unconscious and empirical experience, which we get from dealing with separate objects. But the knowledge which we gain in this way is also accompanied necessarily by vague experimental reasoning which we carry on quite unawares, and in consequence of which we bring together facts to make a judgment about them. Experience, then, may be gained from empirical and unconscious reasoning; but the obscure and spontaneous movement of the mind has been raised by men of science into a clear and reasoned method, which therefore proceeds consciously and more swiftly toward a definite goal. Such is the experimental method in the sciences by which experience is always gained in virtue of precise reasoning based on an idea born of observation and controlled by experiment. In all experimental knowledge, indeed, there are three phases: an observation is made, a comparison established and a judgment rendered. By the experimental method, we simply make a judgment on the facts around us, by help of a criterion which is itself just another fact so arranged as to control the judgment and to afford experience. Taken in this general sense, experience is the one source of human knowledge. The mind in itself has only the feeling of a necessary relation between things: it can know the form of that relation only by experience.

> Two things must, therefore, be considered in the experimental method: (1) The art of getting accurate facts by means of rigorous investigation; (2) the art of working them up by means of experimental reasoning, so as to deduce knowledge of the law of phenomena. We said that experimental reasoning always and necessarily deals with two facts at a time: observation, used as a starting point; experiment, used as conclusion or control. In reasoning, however, we can distinguish between actual observation and experiment only, as it were, by logical abstraction and because of the position in which they stand.
>
> But outside of experimental reasoning, observation and experiment no longer exist in the abstract sense; there are only concrete facts in each, to be got by precise and rigorous methods of investigation. We shall see, further on, that the investigator himself must be analyzed into observer and experimenter; not according to whether he is active or passive in producing phenomena, but according to whether he acts on them or not, to make himself their master.

Bernard's essay is a treatise on the *science proper* of experimental methodology, as this methodology can be applied to the field of medicine. In it he presents a number of maxims that he argues must be employed in experimental research. His deductions of these maxims have a profound philosophical flavor inasmuch as he deduces them on the basis of pure *a priori* principles of the sort expounded in Kant's critiques.[2] As an example, Bernard writes

> The first condition to be fulfilled by men of science, applying themselves to the investigation of natural phenomena, is to maintain absolute freedom of mind, based on philosophic doubt. Yet we must not be in the least skeptical; we must believe in science, i.e., in determinism; we must believe in a complete and necessary relation between things, among the phenomena proper to living beings as well as in all others; but at the same time we must be thoroughly convinced that we know this relation only in a more or less approximate way, and that the theories we hold are far from embodying changeless truths.

For a person today trained in science it might seem as though Bernard is setting down no great insight in this work. I have no difficulty imagining a contemporary reader of Bernard's essay dismissing it as "all fluff and obvious bromides." While it can be justly argued that the essay stops well short of formal completeness as a doctrine, I say that such a dismissal would reveal that the person making it has missed the entire significance of its contribution to medical science. By the definition we have adopted in this chapter, a science is *any* doctrine that constitutes a *system*. Bernard's essay is on the doctrine of *the science of experimentation in general*.

It was precisely this doctrine that was missing from medical science up to the first half of the nineteenth century. That such a doctrine *needed to be propounded* illustrates that it was far from being merely a collection of self-evident generalities. Commenting on *An Introduction to the Study of Experimental Medicine*, Pasteur wrote, "nothing so complete, so profound, so luminous has so far been written on the true principles of the difficult art of experimentation."

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[2] It cannot be judged, on the basis of Bernard's essay alone, whether or not the striking similarity between the principles he employs in deducing his maxims and the principles Kant outlined in his theory is accidental or whether Bernard himself had been influenced by Kant's critiques.

In most cases it is historically incorrect to view the development of theory as being due to one person alone. In the great majority of cases where a new science has developed or an old science has been changed in a revolutionary way, we find a number of "pioneers" making specific key individual contributions. Usually, however, it is the work of only one or a few key individuals who bring together these contributions in a great synthesis *as a system*. One good case in point is the science of electromagnetism, where we find two key individuals whose work resulted directly in the form of that science as it still appears today. The first of these men is the great experimentalist, Michael Faraday; the other is the great mathematical physicist, James Clerk Maxwell.

Michael Faraday (1791-1867) was a remarkable, largely self-taught scientist. He was the son of a blacksmith and his scientific career began as a laboratory assistant to the British chemist Sir Humphrey Davy at the Royal Institute in 1813. Faraday's experimental studies of electricity and magnetism began in 1821. He first collected and repeated every known experiment that had been reported on electromagnetism. His original investigation continued from this point, and his experiments were published under the title of *Experimental Researches in Electricity* in three volumes, published in 1844, 1847, and 1855.

Everyone is probably familiar with everyday manifestations electromagnetism, such as static electricity, where one can get a slight electric shock from touching an appliance after walking across a rug. In more violent form, we have all seen static electricity manifested as lightning. Most of us are probably less conscious of the phenomena of electromagnetism manifested in common gadgets such as our electric lights, our televisions and radios, computers, and so on – particularly since we tend to take these appliances for granted and, below a certain age, some people now alive can not remember a time when all of these common products did not exist. It is difficult for most people to imagine how freakish and rather "magical" the phenomena of electricity and magnetism truly are. We cannot see, hear, touch, feel, or taste electromagnetic fields; yet this ghostly phenomenon is one of the four great "fundamental interactions" (in today's view of physics) that constitutes our physical universe.[1] At the beginning of the nineteenth century the nature of electromagnetic phenomena was not at all understood, and no one did more to bring us to our present understanding of electric and magnetic phenomena than Faraday.

Faraday's *Researches* is a model illustration of the empirical method in science. Of particular interest to us here are Faraday's thoughts and views expressed in his *Researches*. In

[1] The other three are: gravitation, the weak nuclear force (which is responsible for a particular type of radioactivity known as beta decay), and the strong nuclear force (which is responsible for binding protons and neutrons together in the nucleus of the atom).

1833 it was not yet known if there was more than one kind of electricity. In pursuing an answer to this question, Faraday wrote[2]

> 265. The progress of the electrical researches which I have had the honor to present to the Royal Society, brought me to a point where at which it was essential for the further prosecution of my inquiries that no doubt should remain of the identity or distinction of electricities excited by different means. It is perfectly true that Cavendish, Wollaston, Colladon, and others, have in succession removed some of the greatest objections to the acknowledgement of the identity of common, animal and voltaic electricity, and I believe that most philosophers consider these electricities as really the same. But on the other hand it is also true that the accuracy of Wollaston's experiments has been denied; and also that one of them, which really is no proper proof of chemical decomposition by common electricity . . . has been that selected by several experimenters as the test of chemical action. It is a fact, too, that many philosophers are still drawing distinctions between the electricities from different sources; or at least doubting whether their identity is proved. Sir Humphrey Davy, for instance, in his paper on the Torpedo, thought it probable that animal electricity would be found of a peculiar kind; and referring to it, to common electricity, voltaic electricity and magnetism, has said, "Distinctions might be established in pursuing the various modifications or properties of electricity in these different forms, &c." Indeed I need only refer to the last volume of the *Philosophical Transactions* to show that the question is by no means considered as settled.
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> 266. Notwithstanding, therefore, the general impression of the identity of electricities, it is evident that the proofs have not been sufficiently clear and distinct to obtain the assent of all those who were competent to consider the subject; and the question seemed to me very much in the condition of that which Sir H. Davy solved so beautifully . . . The same necessity that urged him to decide the doubtful point, which interfered with the extension of his views, and destroyed the strictness of his reasoning, has obliged me to ascertain the identity or difference of common and voltaic electricity. I have satisfied myself that they are identical, and I hope the experiments which I have to offer and the proofs flowing from them, will be found worthy the attention of the Royal Society.

In the paragraphs that followed Faraday takes us step-by-step through the problem description, the experimental methods to be employed in answering the questions, and the actual experiments themselves – both his own original experiments and his own repetition of experiments reported by other researchers, which Faraday repeated for the purposes of establishing either confirmation or denial of the reported results. From this there came, in paragraph 360, "The *general conclusion* which must, I think, be drawn from this collection of facts is, that *electricity, whatever may be its source, is identical in its nature*. The phenomena in the five kinds or species quoted, differ, not in their character but only in degree; and in that respect vary in proportion to the variable circumstances of *quantity* and *intensity* which can at pleasure be made to change in almost any one of the kinds of electricity, as much as it does between one kind and another."

Faraday also, from time to time, gently reminded the 'philosophers' there is a place at the table of science for the experimentalist. In 1834 he wrote[3]

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[2] Michael Faraday, *Experimental Researches in Electricity*, Third Series, § 7, ¶ 265-266.
[3] op. cit., Eighth Series, § 14, ¶ 875-876.

> 875. The great question of the source of electricity, in the voltaic pile has engaged the attention of so many eminent philosophers, that a man of liberal mind and able to appreciate their powers would probably conclude, although he might not have studied the question, that the truth was somewhere revealed. But if in pursuance of this impression he were induced to enter upon the work of collating results and conclusions, he would find such contradictory evidence, such equilibrium of opinion, such variety and combination of theory, as would leave him in complete doubt respecting what he should accept as the true interpretation of nature: he would be forced to take upon himself the labor of repeating and examining the facts, and then use his own judgment on them in preference to that of others.
>
> 876. This state of the subject must, to those who have made up their minds on the matter, be my apology for entering upon its investigation. The views I have taken of the definite action of electricity in decomposing bodies and the identity of the power so used with the power to overcome, founded not on a mere opinion or general notion, but on facts which, being altogether new, were to my mind precise and conclusive, gave me, as I conceived, the power of examining the question with advantages not before possessed by any, and which might compensate, on my part, for the superior clearness and extent of intellect on theirs. Such are the considerations which have induced me to suppose I might help in deciding the question, and be able to render assistance in that great service of removing *doubtful knowledge*. Such knowledge is the early morning light of every advancing science, and is essential to its development; but the man who is engaged in dispelling that which is deceptive in it, and revealing more clearly that which is true, is as useful in his place, and as necessary to the general progress of the science, as he who first broke through the intellectual darkness, and opened a path into knowledge before unknown by man.

Along similar lines, in 1837 Faraday writes[4]

> 1161. The science of electricity is in that state in which every part of it requires experimental investigation; not merely for the discovery of new effects, but what is just now of far more importance, the development of the means by which the old effects are produced, and the consequent more accurate determination of the first principles of action of the most extraordinary and universal power in nature: and to those philosophers who pursue the inquiry zealously yet cautiously, combining experiment with analogy, suspicious of their preconceived notions, paying more respect to a fact than a theory, not too hasty to generalize, and above all things, willing at every step to cross-examine their own opinions, both by reasoning and experiment, no branch of knowledge can afford so fine and ready a field for discovery as this.

When Faraday uses the word "philosopher" he generally means "mathematician" in the sense of those individuals who today would be called "theoretical" physicists. But that Faraday himself was guided by what we could call a metaphysical intuition is without doubt, as his own words make clear. For one thing, Faraday held a James-like attitude of "what difference does it make?" pragmatism. In 1838 he writes[5]

> 1667. The theory of induction set forth and illustrated in the three preceding series of experimental researches does not assume anything new as to the nature of the electric force or forces, but only as to their distribution. The effects may depend upon the association of one electric fluid with the

[4] op. cit., Eleventh Series, § 18, ¶ 1161.
[5] op. cit. Fourteenth Series, § 20, ¶ 1667.

> particles of matter, as in the theory of Franklin, Epinus, Cavendish, and Mossotti; or they may depend upon the association of two electric fluids, as in the theory of Dufay and Poisson; or they may not depend on anything which can properly be called the electric fluid, but on vibrations or other affections of matter in which they appear. The theory is unaffected by such differences in the mode of viewing the nature of the forces; and though it professes to perform the important office of stating *how* the powers are arranged (at least in inductive phenomena), it does not, as far as I can yet perceive, supply a single experiment which can be considered as a distinguishing test of the truth of any one of these various views.
>
> 1668. But, to ascertain how the forces are arranged, to trace them in their various relations to the particles of matter, to determine their general laws, and also the specific differences which occur under these laws, is as important as, if not more so than, to know whether the forces reside in a fluid or not; and with the hope of assisting in this research, I shall offer some further developments, theoretical and experimental, of the conditions under which I suppose the particles of matter are placed when exhibiting inductive phenomena.

On the other side of Faraday's pragmatism we find him driven by considerations that Kant might have called rational cosmological ideas. Thus in 1845 he writes[6]

> 2146. I have long held an opinion, almost amounting to conviction, in common I believe with many other lovers of natural knowledge, that the various forms under which the forces of matter are manifest have one common origin; or, in other words, are so directly related and mutually dependent, that they are convertible, as it were, one into another, and possess equivalents of power in their action. In modern times, the proofs of their convertibility have been accumulated to a very considerable extent, and a commencement made of the determination of their equivalent forces.
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> 2147. This strong persuasion extended to the powers of light, and led, on many a former occasion, to many exertions, having for their object the discovery of the direct relation of light and electricity, and their mutual action in bodies subject jointly to their power; but the results were negative and were afterwards confirmed, in that respect, by Wartmann.
>
> 2148. These ineffectual exertions, and many others which were never published, could not remove my strong persuasion derived from philosophical considerations; and, therefore, I recently resumed the inquiry by experiment in a most strict and searching manner, and have at last succeeded in *magnetizing and electrifying a ray of light, and in illuminating a magnetic line of force*. These results, without entering into the detail of many unproductive experiments, I will describe as briefly and clearly as I can.

We can hardly find a better example of the coordinate partnership between rational and empirical philosophy than the one just quoted. Other than the fact that lightning (or a spark) manifests itself by the presence of both light and electricity (which does not necessarily mean that light is a property of electricity or vice versa), there was no *empirical* reason to believe these two phenomena are necessarily connected in any way. But to Faraday's mind it seemed that they *should* be connected, and this conviction drove him to establish, after much labor, such a connection *in an empirical demonstration*. Empirical facts may drive rational theorizing, but reason may also drive experimentation. Put in a more Kantian manner of expression, the empirical and the rational are equal partners – coordinates – in the development of knowledge and neither is strictly subordinate to the other.

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[6] op. cit., Nineteenth Series, § 26.

Although Faraday was pragmatic about such questions as whether or not electricity was a fluid of some sort, this does not mean that he did not visualize electromagnetic phenomena in sensible terms. Faraday originated the concept of "lines of force" – what we today call the electromagnetic *field*. The mathematical notion of a "field" is so ingrained in physics today that it is taken more or less for granted as being the best way of looking at many kinds of phenomena. But such was not the case in Faraday's time, particularly by mathematically well-trained physicists. It was to fall to Faraday, the experimentalist, to conceive this idea.[7]

> 3070. From my earliest experiments on the relation of electricity and magnetism, I have had to think and speak of lines of magnetic force as representations of the magnetic power; not merely in the points of quality and direction, but also in quantity. The necessity I was under of a more frequent use of the term in some recent researches (2149 &c.), has led me to believe that the time has arrived, when the idea conveyed by the phrase should be stated very clearly, and should also be carefully examined, that it may be ascertained how far it may be truly applied in representing magnetic conditions and phenomena; how far it may be useful in their elucidation; and, also, how far it may assist in leading the mind correctly on to further conceptions of the physical nature of the force, and the recognition of possible effects, either old or new, which may be produced by it.
>
> 3071. A line of magnetic force may be defined as that line which is described by a very small magnetic needle, when it is so moved in either direction corresponding to its length, that the needle is constantly a tangent to the line of motion; or it is that line along which, if a transverse wire be moved in either direction, there is no tendency to the formation of any current in the wire, whilst if moved in any other direction there is such a tendency; or it is that line which coincides with the direction of the magnecrystallic axis of a crystal of bismuth, which is carried in either direction along it. The direction of these lines about and amongst magnets and electric currents, is easily represented and understood, in a general manner, by the ordinary use of iron filings. . .
>
> 3073. A point equally important to the definition of these lines is that they represent a determinate and unchanging amount of force. Though, therefore, their forms, as they exist between two or more centers or sources of magnetic power, may vary greatly, and also the space through which they may be traced, yet the sum of power contained in any one section of a given portion of lines is exactly equal to the sum of power in any other section of the same lines, however altered in form, or however convergent or divergent they may be at the second place. The experimental proof of this character of the lines will be given hereafter (3109 &c.).
>
> 3074. Now it appears to me that these lines may be employed with great advantage to represent the nature, condition, direction, and comparative amount of the magnetic forces; and that in many cases they have, to the physical reasoner at least, a superiority over that method which represents the forces as concentrated in centers of action; such as the poles of magnets or needles; or some other methods, as, for instance, that which considers north or south magnetisms as fluids diffused over the ends or amongst the particles of a bar. No doubt, any of these methods which does not assume too much will, with a faithful application, give true results; and so they all ought to give the same results as far as they can respectively be applied. But some may, by their very nature, be applicable to a far greater extent, and give far more varied results, than others. . .
>
> 3075. I desire to restrict the meaning of the term *line of force* so that it shall imply no more than the condition of the force in any given place, as to strength and direction; and not to include (at present) any idea of the nature of the physical cause of the phenomena; or be tied up with or in any way dependent on, such an idea. Still, there is no impropriety in endeavoring to conceive the method in which the physical forces are either excited, or exist, or are transmitted; nor, when these by experiment and comparison are ascertained in any given degree, in representing them by any

[7] op. cit., Twenty-Eighth Series, § 34.

> method which we adopt to represent the mere forces, provided no error is thereby introduced. On the contrary, when the natural truth and the conventional representation of it most closely agree, then are we most advanced in our knowledge.

Faraday goes on to make comments about various speculative "natures of physical causes" of the magnetic 'power' – such as the notions of fluids, action-at-a-distance, and the "ether" medium, and confesses his own preference for the action-at-a-distance view – but remains steadfast in his position that such speculations are no more than *representations* of the observable phenomena – simply ways in which to *think about* electromagnetic effects. This is a distinction that modern day scientists (like scientists of the past) are sometimes prone to forget.

Faraday lacked the mathematical training to formalize his many findings in a concise mathematical system. This task fell to a younger man, the Scottish physicist James Clerk Maxwell (1831-1879). Maxwell's epic work, *A Treatise on Electricity and Magnetism*, was written in 1873 and still constitutes our present day formalism of the classical theory of electromagnetism. Maxwell did not shy away from giving Faraday his just credit for the theory. In the preface to the first edition Maxwell writes

> There is . . . a considerable mass of mathematical memoirs which are of great importance in electrical science, but they lie concealed in the bulky Transactions of the learned societies; they do not form a connected system; they are of very unequal merit, and they are for the most part beyond the comprehension of any but professed mathematicians.
>
> I have therefore thought that a treatise would be useful which should have for its principal object to take up the whole subject in a methodical manner, and which should also indicate how each part of the subject is brought within the reach of methods of verification by actual measurement.
>
> The general complexion of the treatise differs considerably from that of several excellent electrical works, published, most of them, in Germany, and it may appear that scant justice is done to the speculations of several eminent electricians and mathematicians. One reason of this is that before I began the study of electricity I resolved to read no mathematics on the subject till I had first read through Faraday's *Experimental Researches in Electricity*. I was aware that there was supposed to be a difference between Faraday's way of conceiving phenomena and that of the mathematicians, so that neither he nor they were satisfied with each other's language. I had also the conviction that this discrepancy did not arise from either party being wrong. . .
>
> As I proceeded with the study of Faraday, I perceived that his method of conceiving the phenomena was also a mathematical one, though not exhibited in the conventional form of mathematical symbols. I also found that these methods were capable of being expressed in the ordinary mathematical forms, and thus compared with those of the professed mathematicians. . .
>
> I also found that several of the most fertile methods of research discovered by the mathematicians could be expressed much better in terms of ideas derived from Faraday than in their original form. . .
>
> I have confined myself almost entirely to the mathematical treatment of the subject, but I would recommend the student, after he has learned, experimentally if possible, what are the phenomena to be observed, to read carefully Faraday's *Experimental Researches in Electricity*. He will there find a strictly contemporary historical account of some of the greatest electrical discoveries and investigations, carried on in an order and succession which could hardly have been improved if the results had been known from the first, and expressed in the language of a man who devoted much of his attention to the methods of accurately describing scientific operations and their results. . .

Maxwell's mathematical synthesis of Faraday's work stands as one of the great achievements in the history of science. In just four equations, plus a few auxiliary equations, Maxwell was able to summarize all that we know about electromagnetic phenomena. Furthermore, Maxwell's synthesis led to the prediction of a new phenomenon – the generation and propagation of electromagnetic waves – and established that light is itself an electromagnetic wave (thus establishing the general connection between light and electricity that Faraday had "felt" necessarily existed).[1]

While most of Maxwell's *Treatise* consists of a sophisticated mathematical treatment of the topic, it is interesting for us to take note of Maxwell's commentary in which he compared the method of Faraday with that of another great theoretical "electrician," André Marie Ampère (1775 - 1836).[2]

> The method of Ampère, however, though cast into an inductive form, does not allow us to trace the formation of the ideas which guided it. We can scarcely believe that Ampère really discovered the law of action by means of the experiments which he describes. We are led to suspect, what, indeed, he tells us himself, that he discovered the law by some process which he has not shewn us, and that when he had afterwards built up a perfect demonstration, he removed all traces of the scaffolding by which he had raised it.
>
> Faraday, on the other hand, shews us his unsuccessful as well as his successful experiments, and his crude ideas as well as his developed ones, and the reader, however inferior to him in inductive power, feels sympathy even more than admiration, and is tempted to believe that, if he had the opportunity, he too would be a discoverer. Every student should therefore read Ampère's research as a splendid example of scientific style in the statement of a discovery, but he should also study Faraday for the cultivation of a scientific spirit, by means of the action and reaction which will take place between the newly discovered facts as introduced to him by Faraday and the nascent ideas in his own mind.
>
> It was perhaps for the advantage of science that Faraday, though thoroughly conscious of the fundamental forms of space, time, and force, was not a professed mathematician. He was not tempted to enter into the many interesting researches in pure mathematics which his discoveries would have suggested if they had been exhibited in a mathematical form, and he did not feel called upon either to force his results into a shape acceptable to the mathematical taste of the time, or to express them in a form which mathematicians might attack. He was thus left at leisure to do his proper work, to coordinate his ideas with his facts, and to express them in natural, untechnical language.
>
> It is mainly with the hope of making these ideas the basis of a mathematical method that I have undertaken this treatise.

In the course of modern science education, the 'scaffolding' by which accepted theory has been built up is almost always omitted from the science textbook, leaving the student to gain the impression that science is *founded* on the rational method. In fact the pioneers of science do not work this way in their processes of discovery and synthesis. Rather, nascent science develops as a

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[1] The experimental confirmation of the existence of Maxwell's waves was made nine years after Maxwell's death.

[2] James Clerk Maxwell, *A Treatise on Electricity and Magnetism*, ¶ 528.

result of the on-going coordinate partnership of the empirical and the rational, each taking its turn in leading the development at the appropriate moment.

§ 9. Common Characteristics in the Development of Science

In the examples we have discussed in Section 8 there are several common themes relating to the intertwining of philosophy and science that merit our attention. These themes speak to both the typical stages in the development of science and the role that philosophy, consciously or otherwise, plays in that development. In this section, we examine these themes and explore how the structure of science and the structure of metaphysics can be related to each other.

Theme 1: Every Science Has a Limited Topic

The first characteristic, common to all the examples presented in the previous section, is so common as to almost pass unnoticed when one reads any scientific textbook or paper. This is: that every science is specific to a carefully delimited set of phenomena which the scientist seeks to understand. We do not, for example, find economic theory discussed in a physics textbook. This specific set of phenomena constitutes the *topic* of the science.

The topic of a science is typically not static over the course of time in the development of a given science. In the early years of development, before the science has developed its doctrine very far, the phenomena regarded as topical to the science may be rather broad. Usually, though, the range of phenomena under investigation shrinks, in a sense, as the science posits fundamental principles and relationships uniting phenomena that, at first inspection, seem to have little or nothing to do with one another. As this happens the focus and attention of the science's practitioners becomes more and more directed to specialized phenomena that are regarded as being fundamental to the original broad range of phenomena. It is in this sense, i.e. where the scientist actually concentrates his efforts, that I say the topic shrinks, even though the original set of phenomena still lingers – for the most part in the 'background' of the science. Put another way, as the doctrine of the science develops more and more fundamental relationships, the original set of phenomena tends to be regarded as the domain of *application* of the science rather than the direct object of the research activities that make up the practice of the science. This domain may even increase in scope as the direct topic of the science shrinks, inasmuch as the scientific doctrine may come to take in phenomena that were not originally seen as part of the topic. Thus, today, physics claims (perhaps a bit arrogantly from a pragmatist's viewpoint) to explain chemistry.

For example, in Faraday's work we find references to different "kinds" of electricity – voltaic, common, animal, etc. At one time these were viewed as quite separate phenomena.[3] Faraday's research established, once and for all, that these manifestations of electric phenomena were in fact merely different appearances of a single thing. Today it is rare to even find any reference to "voltaic" or to "animal" electricity in a physics or an electrical engineering textbook.

On the other side of the picture, a science may achieve great success in explaining in considerable detail some subset of its original topic but find less success in other parts of this topic. In such cases we may find the practitioners of the science *dividing* the original topic, thus allowing the more successful portion to become the concern of specialists while the less fecund segment of the topic receives its own special treatment by those scientists interested in it.

What is noteworthy in all this is the plasticity of the topic of the science. It is easy for the undergraduate student to get the impression that a science (that is, a topic) was somehow set down once and for all in the beginning – perhaps by some famous past scientist – and from that time has never changed and never will. This is, of course, not true. A science is defined by its practitioners – the scientists – throughout its history. It may keep, as physics does, its original name as an umbrella under which different specialists operate (e.g., mechanics, thermodynamics, solid state physics, elementary particle physics, plasma physics, astronomy – all under the umbrella of "physics"; internal medicine, surgery, pathology – all under the umbrella of "medicine"). But in such cases, the majority of the practitioners become specialists in one of the special subdisciplines, each of which may justifiably claim to be a special science in its own right by virtue of delimiting its own particular topic. The most extreme example of this in the history of science is philosophy – once the incubator and birthplace of every science, regarding itself as science, but now seemingly self-banished from further participation, as if everything that could be a science either already *is* a science or could be made a science by cross-pollination of two or more existing sciences.

Theme 2: Every Science is Constructed

By Kant's definition, which we have adopted in this treatise, a science is any doctrine that constitutes a system (a whole of knowledge). The phenomena of nature (including human nature in the case of the social sciences) do not present us with a system; they present us with *data* – a term I use in the general sense of "something that is given." The doctrine of a science must be constructed from this data, and the builders of doctrines are those people we call scientists.

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3 The idea of "animal electricity" even inspired Mary Shelley to write *Frankenstein*.

Every science is explanatory, but explanation has two quite different aspects. In the first place, we can have explanations of *relationship*, e.g., if *A* then *B*. It is with this aspect that science has dealt most successfully and which is, in a view widely held among scientists, the correct and proper sort of explanation for science. Explanations of relationship tend to deal with the "how?" question. As Feynman said on more than one occasion,

> I'd like to talk a little bit about understanding. When we have a lecture, there are many reasons why you might not understand the speaker. One is, his language is bad . . . and it's hard to understand. . . Another possibility, especially if the lecturer is a physicist, is that he uses ordinary words in a funny way. . . The next reason that you might think you do not understand what I'm telling you is, while I am describing *how* Nature works, you won't understand *why* Nature works that way. But you see, nobody understands that. I can't explain why Nature behaves in this peculiar way [FEYN1: 9-10].

The "why?" question leads us to the second aspect of explanation. This aspect is the one in which we try to explain the *possibility* of relationships. Speculative metaphysics, e.g. Leibniz' monads, can historically be seen as the effort to address the "why?" question. In Kantian terminology, the "how?" question inquires into relationships and the "why?" question inquires into *modalities* (a term I will describe more fully in Chapter 3). Inquiries of relation, generally speaking, concern judgments of a categorical, a hypothetical, or a disjunctive form. Inquiries of modality concern judgments of a problematical, assertoric, or apodictic form. Viewed materially, judgments of relation concern inherence and subsistence, causality and dependency, or relations of reciprocity (e.g. relativity). Judgments of modality, on the other hand, concern possibility and impossibility, actuality and non-being, or necessity and contingency.

In all these cases the concepts and principles that go into the doctrine of a science are placed there by the scientist. Every science is *constructed*, and contains therefore the 'material' building blocks of experience and the 'formal' structures erected by human understanding. Neither generalizing relations nor generalizing modalities are presented to us by the phenomena; both are, in this sense, metaphysical inasmuch as they are rational constructs and have their source of origin in human thinking. Furthermore, both types of explanation set *limits* on the objective validity of the scientific principles they embody. Thus Darwin's natural selection speaks not to the moving earth of Copernicus, and Faraday's lines of force stand mute on the question of Bernard's determining causes of disease. Nature presents no roadmap to science, and science recognizes no bounds other than those its practitioners place for themselves.

Theme 3: Science is Methodical

Although science, in its construction, places for itself its own bounds, we must not construe this to mean that scientists are free willy-nilly to stake out whatever bounds they wish. The doctrine of a science must also constitute a *system* – a connected, self-consistent whole in which the appearance of internal contradictions cannot be tolerated indefinitely.

The construction of a science is methodical inasmuch as this construction is guided by some particular set of maxims – which, of course, are *also* constructed by science's practitioners. Hand in hand with the doctrine of any science we also find a doctrine of scientific *method*. In most cases this doctrine of method is not stated with the rigor one perceives in the scientific doctrine of its elements. Feynman gave the following description:

> The whole question of imagination in science is often misunderstood by people in other disciplines. They try to test our imagination in the following way. They say, "Here is a picture of some people in a situation. What do you imagine will happen next?" When we say, "I can't imagine," they may think we have a weak imagination. They overlook the fact that whatever we are *allowed* to imagine in science must be *consistent with everything else we know:* that the electric fields and waves we talk about are not just some happy thoughts which we are free to make as we wish, but ideas which must be consistent with all the laws of physics we know. We can't allow ourselves to seriously imagine things which are obviously in contradiction to the known laws of nature. And so our kind of imagination is quite a difficult game. One has to have imagination to think of something that has never been seen before, never been heard of before. At the same time, the thoughts are restricted in a strait jacket, so to speak, limited by the conditions that come from our knowledge of the way nature really is. The problem of creating something which is new, but which is consistent with everything which has been seen before, is one of extreme difficulty [FEYN4: Ch. 2: 10-11].

Here we have a statement of *constraint* – the explicit requirement that the science must form a system. We saw another statement of constraint expressed by Faraday (§1667) viz. not assuming new forces or "natures" of phenomena (e.g. 'electric fluid') as purely *rational* but experimentally (or observationally) *untestable* constructs. Similarly, we saw Copernicus' maxim regarding the need for not "[omitting] something necessary or [adding] something foreign which by no means pertains to the matter" under investigation. These constraints, and others, can be stated in the form of negative maxims – the "thou shalt nots" of scientific *discipline*.

From time to time a philosophically-minded scientist or a scientifically-minded philosopher undertakes an effort to formalize the maxims of scientific methodology. Inasmuch as these efforts are themselves systematic doctrines – what we could call science of *methods* in the tradition of, for example, Bernard – it would draw us too far from the topic of this treatise to treat these studies in detail. Examples, however, are easy to find. Imre Lakatos, a mathematically-minded philosopher (1922-1974), presented his views on the methods of science in his posthumous work, *The Methodology of Scientific Research Programmes* [LAKA2]. Lakatos is better known in mathematics for his masterpiece work, *Proofs and Refutations* [LAKA1], in which he argues that mathematics should regard itself as a science rather than in terms of the logical positivism of the formalist school of mathematical thought. Lakatos is concerned with the idea of *falsification* in scientific and in mathematical theory – when and how a theory is “falsified” *in the actual practice* of scientists or mathematicians – and with *dubitability* – when and to what degree a scientific theory is held-to-be-true *in actual practice*.

As an illustration of the importance of considering scientific methodology as it is actually practiced, we may cite one particular example. There is a popular dogma frequently expressed claiming that science follows a *dictum* which states that one experimental results that contradicts theory falsifies the theory. In actual practice this is hardly ever the case when the theory in question is one that has enjoyed long standing and which has proven to be fecund. This point is make by Kuhn [KUHN], by Lakatos [LAKA2][4], and by Feynman [FEYN2: Ch. 3]. This dichotomy between what the popularized dogma cited above claims and what scientists actually do points to the significance we find in the fact that methodologies of science are constructed along with the development of the science itself. Like the rational elements in scientific doctrine, the rationality behind the construction of scientific methodology takes its ground from metaphysical considerations, whether the scientist is aware of this or not.

Theme 4: Every Science Concerns Itself with Specific Objects

Most often we use the term “object” in the context of the object of a concept, i.e. the ‘something’ that a concept is a concept *of*. That we regard “object” in this fashion is one consequence of applying Kant’s Copernican hypothesis to ontology, by which our knowledge of objects must always be regarded from the viewpoint of the knowing Subject.[5] We *know* experience only through concepts; an object is a unity of concepts.

Sensible objects correlate concepts of experience. In this the knowledge of a sensible object can be traced back to sensorimotor sensations, by which information about the external world is conveyed to understanding. Sensible objects of experience constitute the objects of that which we call ***phenomena***. But the objects of phenomena are not the objects with which a science is primarily concerned in its theories and principles. Rather, the specific objects of greatest concern in a science are the supersensible correlates of those concepts we shall call *ideas*. For example, the object corresponding to the sensible concept of *weight* belongs to the class of phenomena and is an object of experience; the object corresponding to the concept of *mass*, on the other hand, is *supersensible* and, as such, can *never* be an object of experience. We could call such an object a

[4] As it happens, Kuhn and Lakatos clash on certain points in their views on methodology. In my opinion a great deal of this stems from the one misreading or misinterpreting what the other is saying. However, you may find it of great interest to read both *The Structure of Scientific Revolutions* and *The Methodology of Scientific Research Programmes* and compare the arguments of these two men.

[5] see Chapter 1, §6.1.

rational object, but in this treatise we will follow Kant's terminology and refer to such an object as a ***noumenon***.[6]

Science doctrine posits noumenal ideas in order to explain and unite the phenomena contained in its topic as well as to further unify its noumenal ideas. For instance, the sensible concepts of force and acceleration are united in theory by the noumenal idea of mass. Faraday's "line of magnetic force" is a noumenal idea that gives unity to both the observable patterns formed by iron filings in the presence of a magnet and the observable sensation of two magnets either attracting or repelling one another. Darwin's "principle of divergence of character" is an idea that connects the observable variation from one individual to another within the same species with his theory that new species arise from older species through natural selection.

Every science proceeds from its observable phenomena – the collection of facts available through experience – to ideas of noumenal objects that systematically organize and unify these facts by means of universal principles held-to-be necessary. The ideas of the objects of a science are therefore constructed by reason for the purpose of achieving this necessary and universal connection of facts in a system of phenomenal concepts and noumenal ideas.

Theme 5: A Science Begins as an Historical Doctrine

We discussed earlier Kant's classification of sciences as historical doctrines, natural science improper, and natural science proper.[7] With the possible exception of mathematics (whose origin is lost in the fog of prehistory), sciences, excepting those that come into being as hybrids of two or more existing sciences, tend to progress in order through each of these successive stages.

The development of a scientific topic always begins with the recognition of some interesting phenomena and the initial gathering of empirical facts that seem more or less directly related to these phenomena. In time this fact gathering expands to include empirical facts that are thought to be possibly related to the topic. The early fact-gathering activities are generally also accompanied by some initial efforts at cataloging and organizing the empirical facts. At this point in the development of a science the unity of the facts is given only by their inclusion within the topic of the science. There is an absence of even any empirical principles at this stage, which is the trademark of a science that is merely an historical doctrine.

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[6] The word "phenomenon" derives from the Greek word *phainomenon* - to place in the light. The word *noumenon* is a Greek word generally denoting anything perceived. Kant uses *noumenon* as a technical term denoting an object understood by the intellect without the direct testimony of the senses.

[7] From this point on, I will reserve the term "natural science" to denote Kant's classification. The sciences of physics, chemistry, biology, and so forth will hereafter be called the *physical* sciences rather than the "natural" sciences in order to reduce any confusion in terminology.

A science advances from historical doctrine to become a natural science when it is able to unite some subset of its topical phenomena with objective theory. To do this it requires both ideas of noumenal objects (as we discussed previously) and principles that unite these objects in theory. It is at this point where a metaphysic, usually in the form of the scientist's personal metaphysical presuppositions or prejudices, begins to shape the science. This is because ideas of supersensible objects always arise through reasoning rather than direct sensible experience. A scientist's metaphysical presuppositions always enters the development of his science at this stage through his notion that a necessary *objective* order (or "cosmology") exists that underlies the phenomena contained within the topic of his science.

When the principles of the science are merely empirical. as in Faraday's lines of force, the natural science is called a natural science *improper* (under Kant's classification). It is not until the "first principles" of the science – in the form of rational principles that consciously contain the grounds of *necessity* and *universality* for the empirical principles – arise from some set of ontological premises that the science becomes a natural science *proper*. In most cases in science this step is marked by the development of a formal mathematical theory from which principles that were previously merely empirical can be derived as consequences of the rational 'first' principles. Frequently these rational first principles often play a role in redefining the accepted methodology of the science as well, and they outline new questions the science can ask and, perhaps also, specify questions that the science cannot not ask, i.e. questions that are "meaningless" insofar as the ontological picture of this particular science is concerned. Put another way, we see the formal establishment of a rational *paradigm* (to use Kuhn's word) that will thereafter direct the activities of the science and refine its topic.

Among our previous historical examples the transition from Faraday to Maxwell exemplifies this step in the development of a science. Faraday, of course, developed abstract ideas to explain his experimental findings. However, he took great care that these principles were empirical and went no farther than his experimental facts justified. Even his idea of lines of force had a strictly *operational* definition given in terms of observable effects. With Maxwell we see Faraday's lines of force turned into the abstract idea of the "electromagnetic field." Maxwell formulated a set of equations (the famous "Maxwell equations") from which Faraday's empirical findings could be derived as consequences. Additionally, Maxwell was able to predict *new* phenomena (displacement currents and electromagnetic waves) that were not contained within Faraday's theory and which were subsequently confirmed by experiment. The Maxwell theory united the phenomena of electricity, magnetism, and light under the single concept of the dynamics of the electromagnetic field.

At this point, we need to ask if the picture I have just presented of the development of a science is necessarily the *only* way in which a new science evolves. In particular, we need to look at the possibility that a science can begin with some set of rational principles *without* first going through the stages of fact-gathering and the development of empirical principles. We can picture such a situation as one in which someone first “has an idea” for a theory and then proceeds to look for facts that support this theory *ex post facto*. Is this not possible?

“Creation science” seems to be an activity of this sort, but its reliance on miraculous events makes it a pseudo-science and therefore not an acceptable example. Nor should we look to the derived or “hybrid” sciences, which originate out of other already established sciences, as examples because these sciences are based on other original sciences. It is whether or not an original science can arise from pure rational grounds to which we direct our question.

It would seem that, if such a reversal of development is possible, the so-called rational sciences (logic, geometry, and mathematics in general) should provide the best evidence. What little we do know about pre-Ionian mathematics, however, seems to indicate an empirical origin for geometry and mathematics. There is little doubt that early Greek geometers and mathematicians based their science on knowledge obtained from the Egyptians and Phoenicians.

> On the subject of prehistoric mathematics, we may observe in the first place that, though all early races which have left records behind them knew something of numeration and mechanics, and though the majority were also acquainted with the elements of land-surveying, yet the rules which they possessed were in general founded only on the results of observation and experiment, and were neither deduced from nor did they form part of any science. [BALL: 1-2].

What little evidence we do have suggests that mathematics originated as practical arithmetic in which the abacus and the swan-pan served as instruments prior to development of mathematical theory. A thousand years before Christ an Egyptian scribe named Ahmes left a manuscript (thought to be a copy of another manuscript of a work that was already a thousand years old at the time) entitled “Directions for knowing all dark things.” Ahmes’ manuscript is a collection of rules and questions familiar to priests and a number of problems in arithmetic and geometry. The answers to these problems are given but not the processes by which these answers were obtained. What we do know is that mathematics seems to have been already rather highly developed, although still empirical and without the rigor the Greeks would later add to it, at the beginning of recorded Egyptian history.[1] There is some reason to think that arithmetic came to Egypt from western Asia, perhaps from Ur, where it appears to have grown out of the thriving trade and commerce which, according to clay tablets dating back to before 2800 B.C., existed at that time.

Egyptian geometry is also already well-developed by the beginning of recorded Egyptian history. There appears to be little doubt that Egyptian geometry grew out of land surveying,

[1] According to Egyptian legend, mathematics and the sciences were invented by Thoth, the Egyptian god of wisdom, around 18,000 B.C.

which was a highly developed art in ancient Egypt. Indeed, the Greek word for geometry (*geometria*) may have originated from γη ("the earth") and μετρεω ("I measure"). Taken altogether, it appears to be the case that even the rational sciences of geometry and arithmetic followed the course from historical doctrine to natural science proper outlined above.

As for the science of logic, there is really no doubt of its empirical origin. We owe the development of formal logic to Aristotle, but it is clear that logic as an *art* predated his formal treatment of this subject. Plato and Xenophon both write of Socrates' clever use of dialectic logic. In Aristotle's own "Organon" of logic (*Categories* [ARIS1], *On Interpretation* [ARIS2], *Prior Analytics* [ARIS3], *Posterior Analytics* [ARIS4], *Topics* [ARIS5], and *Sophistical Refutations* [ARIS14]), what we find is a thorough and formal treatment of what Aristotle viewed as the systematic study of *correct* argument and inference. Oratory (i.e., *politics*) played a central role in democratic Athens, and we can see Aristotle's logic growing from the practical soil of Athenian politics. What Aristotle did was to formalize the practices of *demonstration* and *dialectic argument*. Furthermore, although Aristotle's system is *formal*, it is important for us to realize that Aristotle's system of logic contains *material* as well as strictly formal elements, which are introduced by the ten "categories" (which he appears to have learned from Plato) and his "Five Words" (which appear to be original with Aristotle) [TAY: 22-24].

Since the physical sciences and the social sciences all have well-known empirical roots, it seems that we possess no examples of any *successful* development of a science that has *not* followed the path from historical doctrine to natural science proper.[2] Of course, this does not prove that a reversal of this order is impossible. But it does seem to be the case that no new science (i.e., no science that is not a hybrid) has followed any other course of development. Speaking from personal opinion, I do not see it at all likely that a successful science could develop in any other way.

Theme 6: A Science Admits of Dubitability

One of the strongest human needs seems to be the need to be *certain* about at least some things. This need is reflected by most of the history of philosophy up to the time of Kant.[3] The majority of the general lay public apparently believes that scientific knowledge is certain, but this view is not shared *at all* by the community of scientists in the present day. To quote Richard Feynman

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[2] For those of us willing to call philosophy (or, better, some particular doctrine of philosophy) a science, we must bear in mind the strictly empirical origins of Greek philosophy in the hands of the pre-Socratic philosophers. It will not do to regard, say, Kant as operating in a vacuum devoid of philosophical tradition.

[3] The Greek skeptics present us with an interesting special case. The skeptics claimed that nothing could be certain - a position they seem to have been more or less certain about. Hume was the last great skeptic, but at least he confessed that he was uncertain about his own skepticism.

again,

> Science is a way to teach how something gets to be known, what is not known, to what extent things *are* known (for nothing is known absolutely), how to handle doubt and uncertainty, what the rules of evidence are, how to think about things so that judgments can be made, how to distinguish truth from fraud, and from show.[4]

> You see, one thing is, I can live with doubt and uncertainty and not knowing. I think it's much more interesting to live not knowing than to have answers which might be wrong. I have approximate answers and possible beliefs and different degrees of certainty about different things, but I'm not absolutely sure of anything and there are many things I don't know anything about, such as whether it means anything to ask why we're here.[5]

It is one thing to have no doubt about some phenomenal fact, such as the fact that the sun rose this morning, and something quite different to be *certain* about the supersensible objects of rational ideas and theories. Indeed, one basic result of Kant's Critical Philosophy is that it is impossible for us to know, beyond any possible doubt, everything that might be contained in the essence of noumenal objects because such questions always *transcend* possible experience. This view, stated in quite different terms, is also held by Karl Popper and Imre Lakatos:

> In science, the search for "foundations" leads to the traditional problem of "inductive logic": how to derive general laws from particular experiments and observations. In 1934 there was a revolution in the philosophy of science when Karl Popper proposed that it is neither possible nor necessary to justify the laws of science by justifying inductive reasoning. Popper asserts that scientific theories are not derived inductively from facts: rather, they are invented as hypotheses, speculations, even guesses, and are then subjected to experimental tests where critics attempt to refute them. A theory is entitled to be considered scientific, said Popper, only if it is in principle capable of being tested and risking refutation. Once a theory survives such tests, it acquires a degree of credibility, and may be considered to be tentatively established; but it is never *proved*. A scientific theory may be objectively true, but we can never know it to be so with certainty [DAVI: 345].

I would add to this summary that an 'established' scientific theory is *always* either 1) objectively true – i.e., its concept and its object are congruent – so far as the facts *presently known* are concerned, or 2) apparently contradicted in some situation, but where there is some possibility that the situation itself might be explained in a way which, if verified, preserves the theory, or 3) apparently falsified under some circumstances. In the second case, the situation tends to be regarded with more suspicion than the theory itself. In the third case, the theory is regarded with suspicion, and tends to become a focus of attention, but generally is regarded as "probably" a special case of, or an approximation to, a more general principle yet undiscovered, particularly if the theory has been shown to be fecund in many cases.[6]

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[4] From James Gleick, *Genius: The Life and Science of Richard Feynman*, New York: Pantheon Books, 1992, pg. 285.
[5] *ibid*. pg. 438.
[6] In one famous example from physics, the laws of conservation of energy, linear momentum, and angular momentum all came under suspicion when it was discovered they were apparently violated in a particular

Because the actual resiliency of scientific ideas, in the face of what appears to be experimental or observational contradiction, is so at odds with the popular viewpoint that "one negative outcome of experiment refutes the theory," we should take a closer look at this characteristic of dubitability in science. Are scientists simply being dishonest or, more likely, simply being stubborn? Perhaps a little of the latter, but there is sound reason to be stubborn when it comes to questions regarding objects that transcend possible experience (such as energy or momentum). To appreciate this, we must look at the Kantian distinction between *definition*, *description*, and *exposition*.

In Kant's terminology a definition is "a sufficiently distinct and precisely delimited concept" [KANT8: 141 (9: 140)]. One class of examples of this is provided by mathematical definitions. Definitions are "made" (that is, they are *constructed*) concepts, formed from other concepts in a precise way. Because the object of a definition is 'the way that it is' because it is *dictated to be that way* and no other (by definition), a definition is always true. The concept can never disagree with its object because the object is simply forbidden to disagree with its definition.

Definitions are never possible for *empirical* concepts because it is not possible to precisely delimit concepts that owe their origin to experience alone. Rather, the concept of an object of experience is constructed in either of two different ways. First, the concept might merely be a catalog of particular empirical facts, in which case, the concept is called a *description*. The concept contains only that which is *given* through observation or experiment. The simple description is merely a concept constructed by combining successive presentations of empirically given characteristics.

The second way that an empirical concept can be constructed is through *exposition*. The exposition of a concept consists in the combination of successive representations of the characteristics of the object in which *not all of the characteristics included in the concept are given in an actual experience* of the object. Exposition, in other words, results in a concept in which both given (empirical) characteristics and made (rational or supersensible) characteristics are presented. The description is therefore a special case of the exposition inasmuch as the description contains no ideas of supersensible objects within it [KANT8: 141-144 (9: 141-143]. Faraday's line of force is a concept of description; Maxwell's electromagnetic field is a concept of exposition.[7]

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kind of radioactivity known as "beta decay." Rather than discarding these important principles, Wolfgang Pauli proposed that there existed a new but undiscovered "particle" - today known as the neutrino - which was responsible for the apparent violation of the conservation principles. The problem of beta decay was discovered experimentally in 1914, Pauli proposed the neutrino in 1930, and the existence of the neutrino was finally verified in 1955. Thus, the period in which the great conservation principles were in serious doubt spanned 41 years; in all this time physics never abandoned any of these principles.

[7] Faraday, we recall, refused to speculate about what a line of force "might be"; he personally was content to regard them as phenomena consistent with "action at a distance," but refused to allow the idea of action

A concept of description can never attain absolute certainty because we have no *a priori* way of knowing that the description of the object is completely characterized. Likewise, a concept of exposition can also never attain absolute certainty because we have no *a priori* ground for holding our ideas of supersensible objects to be complete insofar as their relation to possible future experience is concerned. For example, physics has already seen its idea of “mass” altered by the theory of relativity. Because we cannot be sure that some future experiment will not contradict the current idea of mass, we cannot know with absolute certainty that our idea of mass is necessarily final and complete. At some point in the future there might possibly be found some testable prediction of a possible experience which, when tested, turns out to be false but which can be explained by yet another modification of the idea of mass.

Natural science *requires* ideas of supersensible objects and the rational principles based on these ideas. But these ideas are *always* the product of experience. As Kant said,

> That all our knowledge begins with experience there can be no doubt; for how is it possible that the faculty of knowledge should be awakened into exercise otherwise than by means of objects which affect our senses, and partly of themselves produce representations, partly rouse our activity of understanding to compare, to connect, or to separate these, and so work up the raw material of our sense impressions into a knowledge of objects, which is called experience? [KANT1: 30 (B: 1)].

Our definitions can be made true, but definitions never give us objects of experience. Only descriptions and expositions produce concepts of objects of experience. Therefore the doctrine of a science *must necessarily* be uncertain *to some degree* regarding its theories. And it is the rational recognition of this uncertainty which introduces into the methodology of science the requirement that its theories be testable and exposed to possible falsification through observation and, where possible, through experiment. It is also why ‘falsification’ can be uncertain, i.e. why we are not immediately certain that an apparent contradiction between theory and experiment necessarily means that the theory is incorrect. The problem of deciding when a theory is to be regarded as ‘falsified’ vs. when we can regard its corroborative facts as ‘outweighing’ facts that are ‘anomalous’ under the theory is an important issue in science, e.g. *vide* [LAKA2: 8-101].

§ 10. Speculation

‘Speculation’ is a term often used pejoratively in science. We regard “mere speculation” as something that is somehow ill-founded or unjustified. A scientist may speculate as an aid to thinking or in suggesting particular experiments, but the speculation itself is not to be taken seriously and needs more evidence before becoming acceptable as a theory. The view that philosophy was

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at a distance to be incorporated into the concept. Maxwell, on the other hand, viewed electromagnetic fields as a "something" that filled space - a concept not given by sensible experience.

"mere speculation" was one factor in the prominence attained by positivism in the nineteenth century.

Many scientists regard theorizing without experimentation as a form of speculation. Often they are right, although the history of science has shown that from time to time speculative theories turn out to be much better descriptions of nature than had been initially thought. One example of this was Louis de Broglie's 1924 speculation – without one shred of experimental evidence calling for it – that particles should exhibit 'wave' properties. Dubbed "*la Comédie Francaise*" by some at the time, de Broglie's theory would have probably gone unnoticed had Einstein not given it his immediate endorsement. With the prestige of Einstein backing it, by 1925 de Broglie's thesis had been turned into a rigorous mathematical theory by Erwin Schrödinger and, independently, Werner Heisenberg; in 1927, the existence of "de Broglie waves" was verified experimentally; in 1929, de Broglie received the Nobel Prize in physics.

The case of de Broglie illustrates that speculation is not always a bad thing. Of course, when a speculative theory like de Broglie's succeeds in such a magnificent fashion we tend to call it a "brilliant insight" rather than a speculation. How did de Broglie come by his brilliant insight? He tells us in his own words[1]:

> When I began to consider these difficulties I was chiefly struck by two facts. On the one hand the Quantum Theory of Light cannot be considered satisfactory, since it defines the energy of a light-corpuscle by the equation $W = h\nu$, containing the frequency ν.[2] Now a purely corpuscular theory contains nothing that enables us to define a frequency; for this reason alone, therefore, we are compelled, in the case of Light, to introduce the idea of a corpuscle and that of periodicity simultaneously.
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> On the other hand, determination of the stable motion of electrons in the atom introduces integers; and up to this point the only phenomena involving integers in Physics were those of interference and of normal modes of vibration. This fact suggested to me the idea that electrons too could not be regarded simply as corpuscles, but that periodicity must be assigned to them also.
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> In this way, then, I obtained the following general idea, in accordance with which I pursued my investigations: - that it is necessary in the case of Matter, as well as of radiation generally and of Light in particular, to introduce the idea of the corpuscle and of the wave simultaneously: or in other words, in the one case as well as in the other, we must assume the existence of corpuscles accompanied by waves. But corpuscles and waves cannot be independent of each other: in Bohr's terms, they are two complementary aspects of Reality: and it must consequently be possible to establish a certain parallelism between the motion of a corpuscle and the propagation of its associated wave. The first object at which to aim, therefore, was to establish the existence of this parallelism.

Note that de Broglie argues from very general and *formal* notions. His wave-corpuscle duality argument is based on a formal notion of what *should be* rather than on any empirical fact coming out of the experimentalist's laboratory. De Broglie examines two somewhat successful theories as they existed at the time – Einstein's quantum theory of light (for which Einstein received his

[1] Louis de Broglie, *Matter and Light*, (tr.) W.H. Johnson, New York: W.W. Norton & Co., 1939.

[2] In this equation, W is the energy and h is a number known as Plank's constant.

Nobel Prize) and Bohr's "old quantum mechanics" model of the atom (for which Bohr received his Nobel Prize). What de Broglie finds objectionable is the physical picture in relation to the mathematical picture. De Broglie's idea, consequently, is driven by a *metaphysical* rather than a physical consideration – and, indeed, this idea of wave-particle duality is one of the most difficult to visualize features of the modern quantum theory.[3]

What is it that makes the metaphysical reasoning of a de Broglie a brilliant insight while making the metaphysical reasoning of a Leibniz (e.g., the monad) or a Hegel a "speculation" worthy only, in Hume's words, of being committed to the flames? One could answer pragmatically that the former is proven to be useful and fecund while the latter is sterile and practically useless, but this begs the question. What we really want to know is: did de Broglie just get lucky, or are there some rules we might discover that put speculation on a more sound footing?

When a scientist says (pejoratively) of some idea, "this is pure speculation," he usually means that the idea or hypothesis not only involves purely rational objects whose connection with empirical fact is problematic and not sufficiently supported by empirical evidence[4], but that it also involves some supposition or concept he finds unacceptable because it requires a view of nature that clashes with his own "sense of the way things are." Usually a speculative explanation is sufficient to explain some set of phenomena, but oftentimes these phenomena might equally well have some other explanation that cannot be ruled out at the time. All else being equal, the scientist will usually tend to favor those ideas that do the least violence to his own metaphysical views of Nature. Often these views are no more than metaphysical prejudices.

It can be said that a theory is viewed as speculative in a pejorative sense when the chain of reasoning leading to it is seen as containing arbitrary assumptions or some gap (*hiatus*) between established facts and the premise of the theory. Let us take, for instance, Leibniz' monad as an example. A monad is, by definition, a "simple" substance. Is the property of simplicity an established fact? No. Does the presumption that an absolutely simple substance exists lead to other even more unconfirmable consequences? Yes.[5] The theory of monads is seen as speculation because, first, it implies that the elementary things that comprise our universe are "formal atoms" which can feel happy or sad, and, second, because a *saltus* or "leap" can be found between the

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[3] Today one sometimes hears this idea referred to as a "wavicle" - as if inventing a new word could clear up the mystery of how a thing can be both "wave-like" and "particle-like" at the same time.

[4] Because the object of an idea always transcends direct experience, *no* idea is unequivocally established by empirical evidence. (Bear in mind that the word "idea" is used in its Kantian sense in this treatise; "idea" implies *noumenon*).

[5] If we are faithful to the notion of an absolutely simple substance, then the endowment of monads with the power of perception, and the necessary corollary that a "pre-established harmony" must exist, both follow when we try to use monads to explain the existence of "composite" entities - which is, of course, why monads were introduced by Leibniz in the first place.

idea of an absolutely simple substance and the facts of Nature as we know them. The first idea is repugnant to a physical scientist, and the latter fact provides a "sufficient reason" for giving in to one's feeling of repugnance and dismissing the entire theory out of hand. The practice of science is a very human activity.

It is a basic principle of deduction in formal logic that no *saltus* in the chain from premise to conclusion is allowable in a valid deduction. In symbolic logic the presence of a *saltus* is more or less easy to detect because the terms involved in the logical expression are abstract variables. But in scientific deduction the terms involved each carry a 'material' meaning, explicit or implied, which can make the detection of a *saltus* more difficult. To illustrate this, consider the formal symbolic deduction: *A* implies *B*, *C* implies *D*, therefore *A* implies *D*. That this conclusion is invalid (that is, not established) is easy to spot since the link from *B* to *C* is not established; we have a leap (*saltus*) in the argument. However, consider the following statement by Maxwell[6]:

> But in all of these theories the question naturally occurs: - If something is transmitted from one particle to another at a distance, what is its condition after it has left the one particle and before it has reached the other? If this something is the potential energy of the two particles, as in Neumann's theory, how are we to conceive this energy as existing in a point of space, coinciding neither with the one particle nor with the other? In fact, whenever energy is transmitted from one body to another in time, there must be a medium or substance in which the energy exists after it leaves one body and before it reaches the other, for energy, as Torricelli remarked, 'is a quintessence of so subtle a nature that it cannot be contained in any vessel except the inmost substance of material things.' Hence all these theories lead to the conception of a medium in which the propagation takes place, and if we admit this medium as an hypothesis, I think it ought to occupy a prominent place in our investigations, and that we ought to endeavor to construct a mental representation of all the details of its action, and this has been my constant aim in this treatise.

The *saltus* in this argument is not so easy to spot. Maxwell is arguing that an "æthereal fluid" must exist, through which electromagnetic forces are propagated. He had established earlier that the mathematical form of the equations of electromagnetism could be expressed in a manner identical to the equations that describe the state of stress in a mechanical medium.[7] Maxwell's argument proved to be persuasive, and the existence of the æther was accepted by physicists right up until the time when the famous Michelson-Morley experiment caused the entire idea to come crashing down.

As is often the case, the *saltus* hidden in Maxwell's argument involves the idea of a *noumenon* – in this case, the idea of *energy*. Potential energy was (and, by and large, still *is*) regarded as a 'thing' that must be a "contained in" some material body. Since, at the time, Newton's model of "light corpuscles" (what we today call *photons*) was a discarded theory, the æther had to exist as the medium in which the "electromagnetic stresses" were seated. But have we *really* established that "potential energy" is a thing that must *necessarily* be contained in a

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6 J.C. Maxwell, *A Treatise on Electricity and Magnetism*, Art. 866.

7 *ibid*. Art. 641-645.

material body? No. The idea is useful, and is likely to remain so for the foreseeable future; but it is not an established *fact* in the scientific sense of that word.

Speculation (in the neutral sense of this term) is unavoidable in science. Whether the speculative object is the *vis insita* (inertia) of a Newton, the æthereal waves of a Huygens[8], the Quantum of Action[9] of a Planck, or the exchangeable value of a Smith[10], the unifying objects of a natural science are, without fail, *noumena* – and for such supersensible objects no absolute surety is possible. If the idea of such an object is fecund – if it appears to explain a great variety of phenomena and seems uncontradicted by experience – its speculative underpinnings are soon forgotten through confidence and habit of use. For us to regard such an idea as pejoratively speculative it is necessary that we both recognize the *saltus* in its application *and* for us to make a judgment that this leap goes too far.

Physicists, for instance, seem to have no deep-seated objection to a *naive* notion of a "simple" substance *per se*[11], but they greatly object to the notion of a simple substance endowed with perception. By refusing to accept "perception" as a characteristic of any of its objects, physics places the phenomena studied by cognitive psychology outside its chosen topic. If the idea that an electron could be happy or sad were somehow found to be useful in producing a deeper unity in our understanding of physical phenomena, and if such an idea could be tested experimentally and stand up under this testing, I have little doubt that physics would adapt its topic to include the emotions of electrons however ridiculous this idea appears to be today.

Observation and experiment are often the activities that bring established ideas of a science into question, where the premises built into the idea are examined. But, while a *saltus* is more or less easy to perceive in formal logic, the recognition of a *saltus*, and agreement as to the seriousness of this *saltus*, is much less clear cut in natural science. For example, the great majority of cosmologists (those who speculate on the origins of the universe) hold the view that the universe originated in a 'Big Bang' where, for no known reason, all of the matter and energy present in the universe suddenly appeared from out of nowhere at a single point in some kind of

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[8] Christiaan Huygens, *Treatise on Light*, 1690.

[9] Max Planck, *The Universe in the Light of Modern Physics*, 1929.

[10] Adam Smith, *An Inquiry Into the Nature and Causes of the Wealth of Nations*, 1776.

[11] The naive idea of a simple substance in physics is illustrated by physics' idea of "elementary particles" - "particles" such as the electron, which, so far as we know, cannot be broken up into more "elementary" pieces. I use the term "naive" to describe the idea of elementary particles because these particles are not "windowless" (as a "truly simple" substance must be); the elementary particles of physics are said to interact with other particles through the "exchange" of other "particles" such as the photon. These "exchanged particles" are, however, not regarded as "part" of either of the particles said to interact. The idea of "interaction through the exchange of particles" summons to the imagination a picture rather like imagining the "exchanged particles" as some kind of currency in a sort of quantum mechanical banking system - although this analogy is a weak one, inasmuch as no "elementary particle" ever appears to "go broke" by running out of particles to exchange!

void of absolute nothingness. The universe then ‘expanded’ outward, as in a violent explosion, creating space and time in the process. Furthermore, during the first 10^{-30} of a second of the universe’s existence, physical processes were supposedly at work that no longer are present or possible in today’s universe. Among these occult processes is included a phase of rapid expansion where the size of the universe grew at a rate many times faster than the present speed of light and then, abruptly and for a conveniently untestable reason,[12] slowed down its rate of expansion to that thought to be observed today. This speculation is called an “inflation model.”

Speaking for myself, I think this isn't merely a *saltus*; I think it’s an entire track meet of very long jumps. Big Bang theory has a track record of piling one *ad hoc* speculation on top of another to patch various inconsistencies between theory and observation. The theory contains too many occult quantities and its theorists are too quick to invent new and untestable physics. I think there is far too much Plato found in it. But the Big Bang is the majority view held in astronomy today[13] and shows every sign of remaining the dominant view for some time to come. Many cosmologists tell us the Big Bang is a fact and their speculation is being written into today’s high school science textbooks. I say it is not fact but hypothesis, should be called that, and, like creationism, does not belong in a high school science textbook. We’ll discuss more of this in Chapter 24.

The point here is this: An idea is viewed as speculative or not speculative depending on the topic of the science and the metaphysical ideas (be they clear or vague, systematic or prejudicial) that underlie judgment in scientific assessment. The human elements – reasoning and judgment – can not be made separate from the doctrine of the science itself. There is no science without the scientist, just as there can be no observation without an observer. Speculation is always found in the theory that attempts to unite the phenomena, not in the given phenomena. Since speculation always involves the *rational* part of the doctrine, not the empirical part, it is this rational substratum of scientific doctrine that we must explore to understand speculation in science. And it is precisely this rational substratum of a scientific doctrine that we call its applied metaphysic.

While positivism can be accused (as I do in this treatise) of making us complacent about and ignorant of the metaphysical presumptions that are always at work in rational thinking, the absence of a *systematic* metaphysic based on Critical examination of the nature of knowledge has been a source of error throughout the history of science. Even those scientists in the days before

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[12] Big Bang cosmology is the handmaiden of what is known as the grand unified theory or GUT, the so-called “theory of everything”, to which it turns when it requires fixing up. Big Bang inflation is tied to Higgs field theory, for which there is as of yet not one shred of experimental evidence. GUT does not try to explain anything observed in nature; it merely tries to unite the ‘four fundamental interactions.’

[13] There is a theory, held by a tiny minority of mostly plasma physicists, that attempts to explain the observed structure of the universe in terms of gravitational *plus* electromagnetic effects. I am agnostic with regard to this second theory as well, but the interested reader may refer to Eric Lerner's *The Big Bang Never Happened*, New York: Vintage Books, 1992.

positivism, and who were well educated in philosophy, made speculations that seemed sound to them in their day but which are looked upon with amusement today. I think it is not the case that we today are smarter or more learned than these men; we simply have the benefits of a greater store of experimental facts and of hindsight. Consider the words of Huygens, written in his 1690 *Treatise on Light*:

> There will be seen in [*Treatise on Light*] demonstrations of those kinds which do not produce as great a certitude as those of geometry, and which even differ much therefrom, since, whereas the geometers prove their propositions by fixed and incontestable principles, here the principles are verified by the conclusions to be drawn from them; the nature of these things not allowing of this being done otherwise. It is always possible to attain thereby to a degree of probability which very often is scarce less than complete proof. To wit, when things which have been demonstrated by the principles that have been assumed correspond perfectly to the phenomena which experiment has brought under observation; especially when there are a great number of them, and further, principally, when one can imagine and foresee new phenomena which ought to follow from the hypotheses which one employs, and when one finds that therein the fact corresponds to our prevision. But if all these proofs of probability are met with in that which I propose to discuss, as it seems to me they are, this ought to be a very strong confirmation of the success of my inquiry; and it must be ill if the facts are not pretty much as I represent them.
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> I shall therefore essay in this book, to give, in accordance with the principles accepted in the philosophy of the present day, some clearer and more probable reasons, firstly, of these properties of light propagated rectilinearly; secondly, of light which is reflected on meeting other bodies. Then I shall explain the phenomena of those rays of light which are said to suffer refraction on passing through transparent bodies of different sorts; and in this part I shall also explain the effects of the refraction of the air by the different densities of the atmosphere.
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> Thereafter, I shall examine the causes of the strange refraction of a certain kind of crystal which is brought from Iceland. And, finally, I shall treat of the various shapes of transparent and reflecting bodies by which rays are collected at a point or are turned aside in various ways. From this it will be seen with what facility, following our new theory, we find not only the ellipses, hyperbolas, and other curves which M. Descartes has ingeniously invented for this purpose; but also those which the surface of a glass lens ought to possess when its other surface is given as spherical or plane, or of any other figure that may be.
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> It is inconceivable to doubt that light consists in the motion of some sort of matter. For whether one considers its production, one sees that here upon the earth it is chiefly engendered by fire and flame which contain without doubt bodies that are in rapid motion, since they dissolve and melt many other bodies, even the most solid; or whether one considers its effects, one sees that when light is collected, as by concave mirrors, it has the property of burning as a fire does, that is to say, it disunites the particles of bodies. This is assuredly the mark of motion, at least in the true philosophy, in which one conceives the causes of all natural effects in terms of mechanical motions. This, in my opinion, we must necessarily do, or else renounce all hopes of ever comprehending anything in physics.
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> We know that by means of the air, which is an invisible and impalpable body, sound spreads around the spot where it has been produced by a movement which is passed on successively from one part of air to another; and that the spreading of this movement, taking place equally rapidly on all sides, ought to form spherical surfaces ever enlarging and which strike our ears. Now there is no doubt at all that light also comes from the luminous body to our eye by some movement impressed on the matter which is between the two; since, as we have already seen, it cannot be by the transport of a body which passes from one to the other. If, in addition, light takes time for its passage - which we are now going to examine - it will follow that this movement, impressed on the intervening matter, is successive; and consequently it spreads, as sound does, by spherical surfaces and waves; for I call them waves from their resemblance to those which are seen to be formed in water when a

> stone is thrown into it, and which present a successive spreading as circles, though these arise from another cause, and are only in a flat surface.

In these passages by Huygens, we find a most marvelous admixture of scientific principles of reasoning still used today, childishly naive certainty in mechanistic theories we now know (or think we know) to be false, and vivid analogies between light, sound, and water. We see the luminiferous æther make its appearance as the medium of light, and the rejection of Newton's idea of light corpuscles. It seems obvious that Huygens, the "Dutch Archimedes" as he was called, regarded all this as a model of sound, rational, scientific thought. Huygens is not specific as to which "true philosophy" he subscribed, but we can be certain that, whatever it may have been[14], it predated Kant's 1781 Copernican Revolution and, consequently, did *not* contain a systematic Critical examination of the nature of what we *can* and *cannot* know.

§ 11. The Idea of a System of Metaphysics

The unifying ideas that constitute the applied metaphysic of a doctrine are of two general types. Ideas of the first type provide the ontological objects of the doctrine. These ideas are usually constructed on empirical foundations by exposition, and they serve as specific unifiers of various phenomena. Such ideas (e.g., Faraday's lines of force or Darwin's divergence of character) arise *a posteriori* from experience. If we hold two magnets in our hands we can feel the attraction (or repulsion) that acts to draw the magnets together (or to push them apart). From this experience it is seemingly but a small step to imagine lines of force acting as the efficient cause of this phenomenon. But there is nothing in the experience itself that presents this idea unequivocally. If I find to be repugnant the idea of action at a distance, which seems to be what a line of force implies (as it seemed to Faraday), I might choose to think of this phenomenon in terms of the magnets "struggling to embrace each other." (Ideas of this sort we say have an "animistic" character). The point here is this: The idea of a unifying *noumenon* is typically a product of *analogy* (from similarity in one characteristic to similarity in all characteristics), and is not given in the phenomena. Hence, such ideas become possible only via experience.

Ideas of this first type are constitutive of, in Kant's terminology, a rational cosmology. They are ideas of objects of which we suppose the "universe within the topic" is comprised. They seem to us to come out of the observed phenomena of experience. Ideas of the second type are wholly

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[14] Huygens' intimate familiarity with Descartes' achievements in geometry and optics, which is illustrated throughout the *Treatise on Light*, makes it reasonable to suspect the "true philosophy" to which he refers is that of Descartes. However, his statement that 'principles are verified by conclusions' is very Newtonian in character and places him on the side of Newton and Halley in the later famous controversy with Berkeley over Newton's Calculus. Huygens was not a philosopher or metaphysician but a mathematician, physicist, and astronomer, and he held a degree in law.

of a different character. These are concepts and principles that seem not to arise from experience but from within ourselves. Faraday wrote of his "opinion, almost mounting to conviction . . . that the various forms under which the forces of matter are manifest have one common origin." Darwin wrote, "Each of the endless variations which we see in the plumage of our fowls must have had some efficient cause." Einstein, in his "Foundation of the General Theory of Relativity," wrote, "the method hitherto employed for laying co-ordinates into the space-time continuum in a definite manner thus breaks down . . . So there is nothing for it but to regard all imaginable systems of co-ordinates, on principle, as equally suitable for the description of nature."

Each of these examples adds in something not at all given in experience and makes a statement about what *ought* to be the 'nature of nature.' Put another way, these are *regulative* principles, dealing not with the objects of the science but with the unity of the rational cosmology which these objects constitute. In short, the regulative ideas speak to the pure rational notion of an underlying *order* in nature – an idea which is given in no experience of external phenomena.

These two classes of ideas – the constitutive ideas of objects and the regulative ideas of a necessary unity among these objects – give us a metaphysical structure in terms of a composition in Nature and a *nexus* (or "connection") of the manifold of Nature. Without such a structure, such an applied metaphysic, no systematic doctrine – and therefore no natural science – is possible.

It is not so long ago that this conclusion would have horrified scientists and laymen alike, for the recognition of the dubitability of science is of fairly recent origin. But if the unifying objects and regulative principles of a science arise in the mind of the scientist – and not in the empirical phenomena of the external world – how can science answer the charge, if it be leveled, that all of its hard-won knowledge is chimerical and that all of its vaunted doctrine nothing but pompous posturing? Locke, Leibniz, and, yes, even Newton saw no recourse but to answer by an appeal to divinity. Let us allow Newton to speak for himself:

> All these things being considered, it seems probable to me that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures, and with other such properties, and in such proportion to space, as most conduced to the end for which He formed them; and that these primitive bodies being solids, are incomparably harder than any porous bodies compounded of them; even so very hard as never to wear or break into pieces; no ordinary power being able to divide what God himself made one in the first creation. While particles continue entire, they may compose bodies of one and the same nature and texture in all ages; but should they wear away, or break into pieces, the nature of things depending on them would be changed. . . And, therefore, that Nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations and motions of these permanent particles; compound bodies being apt to break, not in the midst of solid particles, but where those particles are laid together, and only touch in a few points.
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> It seems to me, further, that these particles have not only a *vis inertiae*, accompanied with such passive laws of motion as naturally result from that force, but also that they are moved by certain active principles, such as is that of gravity, and that which causes fermentation, and the cohesion of bodies. These principles I consider, not as occult qualities, supposed to result from the specific

> forms of things, but as general laws of nature, by which the things themselves are formed; their truth appearing to us by phenomena, though their causes be not yet discovered. For these are manifest qualities, and their causes only are occult. And the Aristotelians gave the name of occult qualities, not to manifest qualities, but to such qualities only as they supposed to lie hid in bodies, and to be unknown causes of manifest effects. Such would be the causes of gravity, and of magnetic and electric attractions, and of fermentations, if we should suppose that these forces or actions arose from qualities unknown to us, and incapable of being discovered and made manifest. Such occult qualities put a stop to the improvement of natural philosophy, and therefore of late years have been rejected. To tell us that every species of things is endowed with an occult specific quality by which it acts and produces manifest effects, is to tell us nothing; but to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from these manifest principles, would be a very great step in philosophy, though the causes of those principles were not yet discovered. And, therefore, I scruple not to propose the principles of motion above mentioned, they being of very general extent, and leave their causes to be found out.
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> Now, by the help of these principles, all material things seem to have been composed of the hard and solid particles above mentioned, variously associated in the first creation by the counsel of an intelligent agent. For it became Him who created them to set them in order. And if He did so, it's unphilosophical to seek for any other origin of the world, or to pretend that it might arise out of a chaos by the mere laws of Nature; though, once being formed, it may continue by those laws for many ages.
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> As in mathematics, so in natural philosophy, the investigation of difficult things by the method of analysis, ought ever to precede the method of composition. This analysis consists in making experiments and observations, and in drawing general conclusions from them by induction, and admitting of no objections against the conclusions but such as are taken from experiments, or other certain truths. For hypotheses are not to be regarded in experimental philosophy. And although the arguing from experiments and observations by induction be no demonstration of general conclusions, yet it is the best way of arguing which the nature of things admits of, and may be looked upon as so much the stronger, by how much the induction is more general. And if no exception occur from phenomena, the conclusion may be pronounced generally. But if at any time afterwards any exception shall occur from experiments, it may then begin to be pronounced with such exceptions as occur. By this way of analysis we may proceed from compounds to ingredients, and from motions to the forces producing them; and, in general, from effects to their causes, and from particular causes to more general ones, till the argument end in the most general. This is the method of analysis; and the synthesis consists in assuming the causes discovered, and established as principles, and by them explaining the phenomena proceeding from them, and proving the explanations [NEWT2: Bk. III, pt. 1].

This last paragraph is one of the most eloquent statements of modern scientific method ever given. Newton tells us that we have *no better alternative* in trying to make sense of the natural world, than to proceed step by step – always open-minded to the possibility that experience will prove our ideas to be flawed – but to strive anyway to perfect and generalize these ideas. Are the objects and principles of a scientific doctrine chimerical? Perhaps in an absolute sense, but not in a sense relative to the range of phenomena they adequately explain. Is science a fraud? No. Its activities are directed to the continual perfecting of our understanding of nature, a Sisyphean labor to satisfy the curiosity and the need to understand that is so much a part of our humanity.

But by what right are we to esteem our noumenal ideas so highly as to presume we can know our world through them? What right have we to expect truth – the congruence of our concepts with their objects? Kant answers: Man himself is *part* of Nature, and if the "things made manifest" in phenomena are natural, so equally is the manifestation we call reason. It is not Nature we seek, but *understanding*. And if, as Kant says, our objects conform to our concepts because it is the phenomenon of mind that produces knowledge of these objects through our concepts – as the best representations we can achieve of an experiential world made noumenal – we need only understand ourselves to find the scope and limits of what we can know of this thing we call Nature.

Every doctrine of science, willingly or unwillingly, contains in its foundation the elements of an applied metaphysic. This is the irremovable human factor in natural science. But historically metaphysics has been a hindrance as well as a helper. We cannot, as the positivists tried to do, throw out metaphysics and assert "there is no other source of knowledge of the universe, but the intellectual manipulation of carefully verified observations, in fact, what is called *research*, and that no knowledge can be obtained from revelation, intuition, or inspiration"[1] because "intellectual manipulation" calls forth those ideas of *noumena* we call metaphysical. If we can accept this situation as fact then we can also acknowledge that we may either: 1) pay little attention to the role played by our metaphysical presumptions, in which case we are likely to find ourselves saddled with a hodgepodge aggregation of more or less accidental metaphysical prejudices, or 2) we can give as serious an examination to metaphysics as we give to our sciences, in order to make a *system* of our metaphysical doctrine – in short, we can make metaphysics itself into a science. The latter option should, it seems to me, be found appealing by anyone who loves science.

But if metaphysics is found within every science, how can scientific methods make a science out of metaphysics? Is this not an infinite regression and an impossible task? Certainly this idea involves an *indefinite* regress. So what? This means nothing more than that we must apply the same methods of deduction to metaphysics as we apply elsewhere in science. An impossible task? Perhaps, but no more so and no less so than the task of any other science. And for this task we do indeed have material to work with: the phenomenon of mind. But, one may protest, our knowledge of this science is doomed to be uncertain, and so isn't this a waste of time? Yes, it is uncertain; no it is not a waste of time or else all science is a waste of time. If you must clutch to certainty, abandon science and go to the monastery, for absolute and total certainty is not to be found anywhere in science. The utility of a science of metaphysics lies in the hope that scientific metaphysics will lead to better science in general.

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[1] Sigmund Freud, *New Introductory Lectures on Psycho-Analysis*, lecture 35.

We may call an unexamined, uncritical aggregate of metaphysical ideas by the name pseudo-metaphysic. With few exceptions[2], this seems to be the only type of metaphysic that underlies the work of the vast majority of modern scientists. From birth every human infant is busy with the construction of a mental "world model" which has for its topic the "making sense" of the representations of sights, sounds, feelings, tastes, and smells that affect the baby's mind.[3] In spirit at least, every infant is a little scientist. Some of us never grow out of it. But it is also clear that the infant is a *realist*, that his examination of the world is *uncritical*, and that the way of viewing the world one develops early in life exerts a powerful hold on the way in which we interpret all of life. It requires education to develop the maturity of disciplined reasoning that is such a vital part of scientific thinking. And it requires attentive scrutiny of the topic to turn a merely historical doctrine into a natural science.

The modern day education of most scientists does not include the study of philosophy, and even if it did I doubt that the smorgasbord of semi-historical sound bites in the too-prevalent surveys presented to undergraduates by many philosophy departments could serve to do anything more than to turn scientist students into committed positivists. Typical course titles under philosophy in a typical college catalog read *Existentialism*, *Metaphysics*, *Theory of Knowledge*, etc., and the course syllabi seem to invariably promise the undergraduate a smattering of pieces from the works of a half-dozen or more different philosophers. We do not often find such titles as *Plato*, *Aristotle*, or *Kant*, and there seems to be no emphasis for studying in detail the specific works of great thinkers *treating their philosophies as they meant them to be read*. Many, perhaps even most, philosophy departments seem to treat the history of philosophy but not a theory of *how to philosophize*. This, at least, has been my own experience with philosophy departments.[4] This seems to me to be the pedagogy of "value relativism" in which no work is great and no system of ideas is profound. To offer students watered-down summaries piecemeal of what were once wholes, and to expect them to select for themselves an idea here and a thought there, all done in haste over one semester, can only promote philosophy as historical doctrine and not as a science. It seems no surprise, then, that no new science emerged from philosophy during most of the twentieth century. If we taught science this way astrology would rule the roost, we would balance our checkbooks using an abacus, and our surgeons would still give haircuts. Does it surprise anyone, therefore, that graduate students in science are neither inclined nor encouraged to take even one philosophy course? Is it surprising, rather than ironic, that the majority of those who hold the Doctor of Philosophy degree have taken no more than one course, if that, in philosophy?

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[2] see Henry Margenau, *The Nature of Physical Reality* [MARG].
[3] see Jean Piaget, *The Origins of Intelligence in Children* [PIAG1].
[4] This does not mean I have not met some true scholars of philosophy. But Allan Bloom, too, has a point.

In this environment we cannot assert with confidence that the practitioners of any given science share even a common *historical* metaphysic, much less a common metaphysical system. Too much is left to accidents of personal development – a Darwinian divergence of philosophical character. With no common ground in a shared metaphysic the delimiting of acceptable speculation from pejorative speculation follows its own metaphorical law of natural selection and philosophy fails to provide reliable guidance to science.

We cannot rid science of the effects of the phenomenon of mind and the metaphysical ideas that are among its products. But we can place our metaphysical ideas under Critical examination and try to bring order and a systematic architectonic to these ideas. If systematic organization and examination works to the benefit of the particular sciences, why should it not work when applied to the phenomenon of mind?

What can we say about this idea of an architectonic of a system of metaphysics? First, we need a foundation – a Critical epistemology concerned with the origins, scope, and limitations of human knowledge. This is what Kant's Critical philosophy claims to provide. Second, we need a system for the regulation of the *disciplined* development of an applied metaphysic – one that clarifies the *general* picture of what a proper applied metaphysic must contain. This is what Kant refers to when he speaks of doctrine of method. Third, we require *experience* – gained through observation and, when possible, experimentation – to summon our mental resources to the exposition of the object ideas that constitute an applied metaphysic. Finally, we require a system of regulative principles *a priori* by which we can judge this applied metaphysic against the standards and limitations imposed by the pure epistemology of the phenomenon of mind and the principles that regulate it. This would be the *transition to natural science* of which Kant spoke in his last years and which we will discuss at the end of this treatise (Chapter 24).

The topic in the remainder of this treatise is the Critical philosophy and its consequences for its systematic application to a science of mental physics – a proper science of the phenomenon of mind. Because psychology today declines to call itself the science of mind – it prefers to call itself the scientific study of the behavior of organisms, or the study of man with the aid of scientific methodology, or by some other phrase that strives to avoid giving too much of a sense of prominence to the *noumenal* idea of mind[5] – we find our topic to be more or less unburdened by current convention or recent tradition. But if I am to persuade you that the phenomenon of

[5] Psychologist Arthur Reber tells us, "Psychology simply cannot be defined; indeed, it cannot even be easily characterized. . . Psychology is what scientists and philosophers of various persuasions have created to try to fulfill the need to understand the minds and behaviors of various organisms from the most simple to the most complex. Hence it really isn't a thing at all, it is about a thing or about many things. . . It is an attempt to understand what has so far pretty much escaped understanding . . ." [Reber's *Dictionary of Psychology* under **psychology**].

mind is indeed a topic that can be treated scientifically, without pejorative speculation, it falls upon this treatise as a duty to lay well the metaphysical foundations, to cite supporting evidence from the empirical observations and experiments available, and to expose the problematical hypotheses and issues where our degree of certainty is at its worst. No science promises all the answers to all the questions nor claims immunity from doubt, and I make no such claim for this treatise. But if this treatise helps us to better understand the phenomenon of mind, this cannot help but benefit the broader community of the sciences through the betterment of our understanding of the metaphysical character of the products of the phenomenon of mind.

The task that lies ahead for us in this treatise is not an easy one. It is often very technical in detail. Along the way I think you will be surprised by the large number of ideas we typically take for granted that will not stand up under Critical scrutiny. We must be prepared to come away from this work with a very different way of looking at the world. Yet I think I can promise the person of serious and inquiring disposition: The effort expended will be very worthwhile.